no choice of candidates and must vote a straight communistic ticket is called "a highest form of democracy," it is clear that American and Soviet concepts of democracy are so far apart that there cannot be a constructive debate on that point.

Let us turn to another statement by A. Zhebrak, "that science can be free in a centralized socialistic state" and that the opinion of K. Sax that "science must conform to political philosophy" in USSR is wrong.

We must state most emphatically, on the basis of our personal experience in USSR, that K. Sax is absolutely right. All groups of population in Soviet Union are living under terrific political pressure and the state is regulating everything and interfering in everything, and this cannot be different because the state is supreme and absolute and the individual is just nothing. Freedom, as Americans understand it, is simply nonexistent in USSR. The scientists are not exceptions to a general rule, although they enjoy some privileges and their standard of living is much higher than that of other less fortunate subjects of Soviet Union.

Let us take a glaring example, about which A. Zhebrak keeps a modest silence. We mean the case of academician N. I. Vavilov, who "was arrested by NKVD in the summer of 1940 and has been kept in custody since then" (*Chronica Botanica*, 1941, 6, 429). We have now reliable information that Vavilov died in a concentration camp in Siberia in 1942.

We can understand the reasons for Zhebrak's avoiding the issue. What ironical commentary on the freedom of science in USSR is the fact that one of the most famous Russian scientists, who rendered outstanding service to his country and who was so respected in USA, could be put to a certain death in a concentration camp for no other reason than his scientific views were found not to be in conformity with Marxian ideology (Vavilov–Lysenko controversy)! But the most disturbing fact is that the case of Vavilov is by no means an exception. We know that hundreds of less-known Russian scientists are dying slowly in Soviet concentration camps which can compete quite favorably in atrocities with Belsen, Dachau, and other Nazi horror camps. Although we can cite the names of some of these unfortunates we have some very sound reasons not to do this. The first rule of all totalitarian states—silence is golden—is known not only to A. Zhebrak but also to us. We talk about Vavilov only because we are sure that he is dead.

K. Sax asks "why and how Vavilov died." He can find the answer to his second question in a book written by a well-known Russian scientist, Prof. Chernavin, *I speak for the silent*, which gives a true and very vivid picture of the life of scientists in the Soviet Union and explains why their careers end sometimes in jail. Prof. Chernavin spent considerable time in various concentration camps and eventually escaped from one of them to Finland, so he can be considered a specialist on this subject.

Although probably 12–15 years elapsed since Chernavin's book was written, our information gathered from the persons who returned only recently from Soviet Union convinced us that nothing was done there to promote the cause of freedom in general and freedom of science in particular. A few privileges granted to scientists are not enough to change the general picture. And although A. Zhebrak describes very eloquently the progress of science, especially in his field (plant genetics), we earnestly believe that the achievements of scientists in USSR would be much more impressive if the fundamental conditions for that—freedom of scientific research and freedom of discussion and criticism—existed in that state.

We are afraid that A. Zhebrak hardly will understand us. We are probably talking again different languages although we both were born in the same country. And this fact is significant in itself. The difference in concepts of such fundamental things as democracy and freedom, which appeared only after the revolution of 1917, is the result of 28 years of most persistent, shrewd, and vicious propaganda to which all citizens of the Soviet Union are subjected day after day. It is interesting to note that even scientists are not immune to this scourge of our time.

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Present Status of Foreign Herbaria and Museums

Although the war has brought about tremendous advances in most branches of science, it has resulted in irreparable losses to systematic biology. Losses of some of the reservoirs of basic data of these sciences, the herbaria and museums that have been destroyed or damaged, are like the results of the fall of the legendary Humpty-Dumpty—all the king's horses and all the king's men cannot restore them. Similar specimens may be accumulated to replace those destroyed, if the places from which they came have not been completely altered, but if a species has been based on a specimen, there is no conceivable way of filling its place if it is lost. Much destruction of this sort has occurred during the war just ended and is likely to continue to occur because of a lack of interest during the period of reconstruction, and because of loss and lack of replacement of competent curatorial personnel.

Systematic botany and zoology differ from most other branches of biology in that the sources of their data can be preserved in relatively permanent form in herbaria and museums. Collections of specimens, together with libraries in which are preserved the records of the circumstances under which the specimens were collected as well as the results of hundreds of years of study, form the essential equipment of the biological systematist. With these collections and libraries as the reservoirs of data, the student may identify plants and animals, construct classifications, work out comparative morphology, stabilize nomenclature, reconstruct phylogeny, plot and interpret geographical distribution, and with newer techniques study the structure and behavior of populations. Since the collections are carefully preserved and filed, the work in these fields does not rest merely on observations of ephemeral phenomena recorded by human hand

but on the actual material observed, which may be consulted and verified at will by future workers.

Classification and the identification of material is also fundamental to all other types of biological research. It is the foundation of the language used by the workers in other fields by which they can communicate and compare their results. The classification of the plant and animal kingdoms has well been compared to the map plus directory of a great city.

Within certain limits systematic biologists have traditionally carried on almost ideal cooperation. The doors of herbaria and museums have always been open to all qualified workers from whatever institution or country. Material has been lent back and forth in great quantity to facilitate the work of those who cannot travel extensively or who need to study more specimens than they have at hand. Exchanges and deposits of specimens build up the large collections needed for adequate study. Anyone who obtains a specimen and places it in a museum is assisting with the work of countless future workers, most of whom he will never see. This is the spirit upon which all science is founded, and it is especially well developed in the oldest branch of biology, systematics.

The losses sustained in the war are thus not of merely local interest to the institutions or countries which have incurred them. They are losses to biology as a whole, and all workers in systematic biology may well be seriously concerned.

It is our purpose as botanists here to call attention to the fact that no concerted effort has been made or is being made to ascertain the extent of these losses, the fate of specimens borrowed from the American institutions, or the steps needed to avert further losses and to repair, in so far as possible, the damage that has been done. The need for such an early inventory should be readily appreciated, and its practicality can hardly be denied in the face of the sending abroad of specialists commissioned to make inventories in the interest of salvaging all sorts of things: objects of art, historical documents, and monuments as well as military and industrial data. With further delay it will be a peacetime "too little and too late."

Systematic botany has traditionally been ill-supported, and most accomplishments over and above routine duties have been the result of the personal initiative of the botanists, and at their own expense or, rarely, at the expense of wealthy benefactors. The task of making an inventory of the losses sustained by, and the present condition of, botanical institutions in war-torn countries, however, might well be considered a legitimate project for a governmental agency. Because of the difficulties in travel, the expense involved, and the disagreeable living conditions in the countries concerned at this time, it is likely that no one would care to undertake such a task on his own initiative. Restrictions on foreign travel would, moreover, make it absolutely necessary that such a project have active governmental backing especially authorized in the Department of State. Obviously, no effort should be spared to secure for such work American specialists, both in cryptogamic and in phanerogamic taxonomy, technically qualified by a comprehensive knowledge of botanical history and by broad experience in herbarium practices. The task is scarcely one to be left to persons lacking these qualifications who may be found within the region.

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Recent Contributions to the Theory of Random Functions

There is a rapidly growing interest in statistical problems where one has not to deal with a single numerical random variable or a finite collection of them, but instead, with an infinite sequence of numbers given at random, or a continuous random function. One thinks of the coordinates of a particle in the Brownian motion, or the path of a molecule in a gas, or the "noise" potentials in electrical machines, as being random functions in this sense.

The recent work in this field provides an excellent example of the impediment which the common irrelation between allied disciplines may place in the way of scientific advance. A large fraction of the methods and ideas of this subject has been rediscovered since 1940 by physicists and electrical engineers, almost wholly unaware that the same problems had been raised and solved in the mathematical literature a decade before. What differences exist in treatment or proof are either notational, or minor ones dictated by the traditional opposition between the physicist's ability to make physical intuition bolster an heuristic argument and the mathematician's demand for maximum rigor and generality.

The particulars are these: In a series of papers, the results of which have been summarized (N. Wiener. *Acta Math.*, 1930, 55, 117–258; R. E. H. C. Paley and N. Wiener. *Amer. math. Soc. Colloq. Publ.*, 1934, 19), N. Wiener develops a theory of Gaussianly distributed random functions, both in the wholly independent case with a "white" spectrum and the more general one with an arbitrary "power spectrum." He derives a general formula, in the form of a definite integral, for calculating the average of any function or functional of one or two such random functions—or any number, by obvious extension (*Amer. math. Soc. Colloq. Publ.*, 1934, 19, 152). The discussion proceeds from a theory of formal Fourier series with Gaussianly distributed coefficients (pp. 147, 151). This is called the "Method of Rice" by M. C. Wang and G. E. Uhlenbeck (*Rev. mod. Phys.*, 1945, 17, 323–342) in view of the extensive use made of it in S. O. Rice's review of 1944 (*Bell Syst. tech. J.*, 1944, 23, 282–332; 1945, 24, 46–156).

All of the fundamental methods presented by Rice, except for the discussion of the shot effect, are to be found in this work of Wiener. A large part of the special results may be obtained easily by substituting in Wiener's