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of specialists commissioned to make inventories in the interest of salvaging all sorts of things: objects of art, historical documents, and monuments as well as military and industrial data.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

general formula and evaluating or approximating the resulting definite integral by the usual methods of function theory. To this class belong, for example, the results of Sections 3.1 and 3.2 in Rice on the distribution of the values of the noise at various times, and those in Part IV on the average of various properties of the response of nonlinear devices to Gaussian noise. Indeed, the fundamental formula of the "correlation function method" (4.07, p. 132), ascribed to Van Vleck and North (1943) with references to Fowler and Rice (1942) and Fränz (1941), is Wiener's formula for the case of a function of two random functions.

It is, of course, true that much of this recent work has constituted a real advance. One need only cite the important work of Kac on zeros of random functions and that of Rice and others on the response of nonlinear devices with random inputs confined to narrow-frequency bands, the low-frequency component of the response, envelopes, etc. On precisely this account it is a greater pity that this further work could not have commenced when the basis was first obtained by the pure mathematician, in which case we should be a decade further today.

It is worth remarking in this connection that certain recent mathematical papers of R. H. Cameron and W. T. Martin on the evaluation of Wiener integrals (*Trans. Amer. math. Soc.*, 1945, 58, 184–219) are highly relevant to nonlinear noise problems.

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A Publicity Victim

In my book, *The natural gardens of North Carolina* (pp. 28 and 371), I reported that "In India there is a superstition that tea made from the leaves of the plant (*Centella Asiatica*) acts as a brain stimulant." Recently this story went out as a news release from the North Carolina Department of Conservation with the word "superstition" omitted and with my name introduced in such a way that, from numerous letters received, many people were induced to not only believe the story but to believe that I believed it.

In every communication in which I have related this story, I have called it a superstition. And to have the publicity people, by implication, get one lined up in support of a superstition becomes a serious matter.

Fortunately most scientists have so frequently suffered from publicity misrepresentation that they will be skeptical as to the authenticity of such a story. An occasional extreme examples of how tortuous publicity operates may help keep them on their guard. However, in this instance there was no chance given to prevent this unfortunate twist in the writer's relation to a botanical superstition.

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Blood Group Factors and Ethnic Relationships

Upon the basis of certain extremely interesting and valuable findings concerning the frequency distributions of certain blood group factors among the ethnic groups of mankind Dr. A. S. Wiener (*Science*, 1946, 103, 147) criticizes a diagram (Fig. 18) which appears in a recently published book of mine (*An introduction to physical anthropology*). Dr. Wiener writes: "In view of these findings [on the blood groups], the Australian aborigenes (*sic*) appear to be more closely related to the Mongoloid group than either the Caucasoid or Negroid groups, and the diagram should therefore be revised accordingly."

That Dr. Wiener would consider, as he appears to do, blood group factors alone sufficient to indicate the closeness of the genetic relationship between various groups of man is to me nothing short of astonishing. I had thought that all students of the subject, including Dr. Wiener, were agreed that the inferences drawn from such data could, at most, be regarded only as suggestive. I would be the last to underestimate the potential value of the blood group factors in helping us to untangle the skein of hominid ethnic relationships, but I cannot see that any useful purpose will be served by making these factors bear more than they can carry. As I have written in the book to which Dr. Wiener refers: "The non-adaptive, non-selective nature of the blood group genes renders them of great potential value in the tracing of ethnic relationships. It is, however, not to be expected that it will be possible to solve anthropological problems by merely turning to blood group tables, as one would look up a definition in a dictionary. This is particularly worth emphasizing in view of the fact that neither the evolutionary nor the ethnic implications of the blood groups can as yet be said to be quite clear" (*op. cit.*, p. 134).

I am convinced that the problem of human ethnic relationships will be most fruitfully attacked by the use of genetic methods of analysis, and that the blood group factors will play an important part in that attack, but *in conjunction with the analysis of a good many other characters, the genetic behavior of which is more or less understood.* Taken alone, blood group factors will not tell us very much, and any attempt, at the present time, to erect or criticize a classification of the ethnic groups of man on the basis of such factors alone would be, to say the least, premature. As Dr. Wiener may possibly have forgotten: "The blood tests have limitations, because peoples of the same race may have widely different distributions, while totally unrelated races may have a similar serological classification" (A. S. Wiener. *Blood groups and transfusion.* (3rd ed.) Springfield, Ill.: C. C. Thomas, 1943. P. 330).

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