socially conscious, that they may work for the constructions of peace as hard as they work for the destructions that are probably inevitable if we build further our already overwhelming military might.

HARLOW SHAPLEY

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It is not my purpose either to enter into a debate with Dr. Shapley concerning several of the points raised in his letter or to defend the statements I made in my article, but rather I desire to supplement these statements in order to clarify any misunderstanding.

In regard to the first of the two points raised, I certainly did not mean to imply a draft of scientists. I stated, "The medical profession, having failed to take similar action in the face of a parallel and long-standing problem, is now faced with a draft. Will a similar crisis be required to stimulate scientists?" I believe that a draft of scientists would be an admission of failure on the part of science as a whole and on the part of the military—failure to establish conditions conducive to research in the armed services. My feeling is that science has an obligation to see that the armed services are made attractive to scientists. Having done this, science has a further obligation to assist the services in staffing military research organizations by encouraging scientists to enter the armed forces and by assuring them that they will have the weight of organized science behind them. I have opposed (somewhat vociferously) a draft of medical personnel, and I would oppose equally a draft of scientists. However, I do believe that unless physicians in the field of medicine, and scientists in their field, take vigorous action soon, the physicians certainly and the scientists probably will be faced with a draft in the future.

Dr. Shapley's second point is an exception to my tacit assumption that there will be another world conflict. I have made this assumption on the basis of world events since the end of World War II which indicate that man has not changed his attitude toward war, and on the historical basis of man's previous approach to the solution of his problems in international relations. I will not say that World War III is enevitable, but based on these two observations, I do believe that it is likely. In view of this, it appears to me that scientists have a double obligation—to work for peace, but at the same time to prepare for national defense. I do not believe that these two obligations are in conflict.

I am in complete agreement with Dr. Shapley regarding the desirability and necessity of scientists' working toward a permanent peace not because they are scientists, but because they have been trained in a way of thinking that views war as the absurdity and illogicalness it is. It has been my experience, however, that scientists all too frequently shed their fine scientific way of thinking with their laboratory coats. I share Dr. Shapley's hope that "the scientific societies may get more socially conscious." My article is objective evidence of this.

I support wholeheartedly any logical unemotional efforts by scientists or anyone else to establish a permanent peace. This is consistent with a scientific training and particularly with a scientific way of thinking. At the same time, I am convinced that, for the preservation of our way of life, it is imperative that we provide ourselves with the strongest possible national defense until such time as a permanent peace is established. Such a strong national defense can come about only as the result of the cooperation of scientists and the armed forces.

I appreciate greatly Dr. Shapley's good letter and the motives which prompted him to write it.

HERMAN S. WIGODSKY

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The Probit Method

In "Application of Probits to Sweet Corn Earliness Data" (*Science*, May 27), Gordon Haskell proposes the use of the probit method to estimate 50 percent silking time of corn from data showing tassel date for each plant separately. This is a sufficiently common misapplication of the probit method to justify a correction.

In his opening remarks on the probit method D. J. Finney says on this point, (*Probit method*, Cambridge: University Press, 1947. P. 14) "If the tolerance [*read* tassel date, J. C.] of each subject has been separately and independently determined, the set of values obtained may be subjected to the same analytical processes as measurements of length or weight; the estimation of means and standard errors, the comparison of distributions, and the making of tests of significance present no new features." In short, when tassel dates are available separately for each plant, the 50 percent tassel date may be computed directly as the median of the recorded tassel dates. If the sample data have been drawn from a normally distributed population, as is assumed by Haskell, a more efficient estimate would be the mean tassel date. The mean tassel date can of course be computed by converting each observed tassel date to days from some convenient reference date, such as July 1 or July 15. Similarly, the slope of the probit line can be computed as the reciprocal of the standard deviation of the individual tassel times.

The basic reason for the inapplicability of the probit method in this and similar problems in which individual measurements are available for each experimental subject is that the assumptions on which it is based are not satisfied. The probit method assumes that the sample data showing percent having silked on or before a given date are statistically independent. That this assumption is not satisfied in the present example is apparent from the fact that if the sample showed that 10 percent of the plants had tasseled by July 20, it would of necessity show that 10 percent or more had tasseled by July 21. In the pharmacological and other applications in which the assumptions of the probit method are satisfied, no such necessity exists. It is possible, and frequently happens, that a larger proportion of animals in an experiment are killed by a low than by a high dose of a toxic substance. A linear relation between probits and stimulus is a necessary, but not, as is sometimes assumed, a sufficient condition for the applicability of the probit method.

The price one pays for using the probit method when the assumptions on which it is based are not satisfied is that one's estimates of the ED_{50} (e.g., 50 percent tassel time) is subject on the average to a larger error than the estimate yielded by the sample mean. In fact, when the underlying distribution is normal no estimate will have smaller average error than the sample mean. In addition the computation of the sample mean and standard deviation involves considerably less work than the use of the probit method. This pleasant concatenation of circumstances is not as frequent in statistics as it might be and should not be overlooked when it occurs.

JEROME CORNFIELD

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I am interested in the comments made by J. Cornfield on my note on the application of probits to sweet corn earliness data. I did assume that the population had a normal distribution for flowering date, but I based my application on the general shape of the original curves in Fig. 1.

My attention has also been drawn by Ray Barratt to his paper "Alternaria Blight versus the Genus *Lycopersicon*" (Tech. Bull. 82, N. H. Agric. Exp. Sta., 1944), in which he uses an arithmetic probability curve to allow comparison of percentage yield and percentage defoliation on the same scale. As my application is similar in principle to that of Dr. Barratt, the use of ordinary probability paper would suffice.

GORDON HASKELL

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On Gates' *Human Ancestry*

I should like to comment briefly upon W. R. Krogman's review of R. Ruggles Gates' *Human ancestry from a genetical point of view* (*Science*, July 1, 1949). A reviewer is certainly entitled to say what he thinks about a book; but at the same time he is, I believe, obligated to give his readers a fair and unbiased estimate of another man's work. This Krogman has not done.

Dr. Gates recognizes five species of mankind, which for him differ mentally as well as physically. He makes the following statements (all quoted by Krogman): "This eighteenth century doctrine [that 'all men are born free and equal'] is hopelessly at variance with the facts of science"; ". . . there is no question of the inheritance of mental abilities and disabilities"; ". . . the mental differences between races remain and cannot be gainsaid." Dr. Krogman objects to what he calls this "sequential build-up" on the ground that "the reader is led, even though perhaps unconsciously, into a *racist* patterning of thought, both culturally and biologically" (italics mine).

To this writer all of Gates' statements are accurate and are well authenticated by readily available evidence. Whether we like them or not, they are facts which we must honestly face and make the best of. Use of the term *racist* is, I believe, as unnecessary as it is irrelevant. This word, to be sure, is a favorite with literary anthropologists who write for popular consumption, and is sometimes effective, I suppose, with timid souls. But like *red* and *fascist* it is a scare word, an evaluative (and usually derogatory) rather than a scientific description.

I agree with Krogman that Gates' book is somewhat uneven in quality and is often hard reading. But I do not agree that it is a "bad" book or is "not a good" book. On the contrary, its emphasis upon biology provides a much needed and refreshing antidote to the wishful thinking of the apostles of the "new anthropology." It should be read by every psychologist, and should be required reading for all sociologists.

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I am accused of being biased. I admit it. I am biased against anything that will either directly or by implication give support to those who are prejudiced against other peoples. I do not retreat from my stand that this book trends in that direction. Mr. Garrett must please note that in my review I used the term *racist* only after the author had first implied it when he referred to "the mental differences between races." The author wrote from a "genetical point of view." This means a biological point of view. The logical assumption becomes that the so-called "mental differences between races" are genetically or biologically based. I wish to affirm my own feeling that no one has really proved mental differences on a racial basis; what they really mean is a cultural basis.

I agree with Garrett that psychologists and sociologists should read this book—with a good book on cultural anthropology as a quick antidote!

W. M. KROGMAN

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