Letters

NIH Grants and Contracts

Over the years *Science* has published many articles and letters on substantive issues related to the programs sponsored by the National Institutes of Health (NIH). The magazine has thus been a respected forum for the communication of a wide variety of views, suggestions, and criticisms related to NIH programs. As deficiencies have been pointed out we have attempted to provide appropriate responses, usually in the form of improved communication or the implementation of more effective administrative procedures. One such action was our expansion in 1971 of the then *NIH Grants Policy Guide* to the *NIH Guide for Grants and Contracts*. At that time we began publishing in the *Guide* current information on policies and procedures related to the contract mechanism as well as descriptions of the collaborative (contract) programs of each institute. We also began publication of supplemental issues of the *Guide*. The supplements contain requests for "Sources sought synopses," and announcements of the availability of "Requests for proposals." The purpose of this additional information is to supplement but not supplant the services provided by *Commerce Business Daily*, published by the Department of Commerce.

Our examination of the present mailing lists for the *Guide* has convinced us that they are inadequate. The lists lack specificity. All supplemental issues go to all addressees—for example, a viral oncologist receives announcements related to the need for improved materials for extraoral maxillofacial prostheses.

In an effort to provide current information on the broadest possible basis and to improve the targeting of our future mailings, we are publishing shortly a special issue of the *Guide*. The special issue will contain an updated description of the programs of each NIH institute and research division that has collaborative or contract components. We are sending this issue to all current principal investigators of NIH grants and contracts and to all institutional officials whose names appear on approved grant applications or contract proposals. In addition, we are utilizing a number of special mailing keys in order to reach other individuals who may be interested. Finally, we are attaching to the special issue of the *Guide* a request form that will give each recipient an opportunity to indicate specific mailings he wishes to receive.

We expect the special issue of the *Guide* to reach most of the addressees by the middle of October. Any scientist or administrator who has an interest in the collaborative programs of NIH, but does not receive this special issue may obtain a copy by writing to the Grants and Contracts Guide Distribution Center, Division of Research Grants, Westwood Building, Room 206, National Institutes of Health, Bethesda, Maryland 20014.

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Long-Distance Runners

Research on the controversial role of coffee drinking as a cause of coronary heart disease (T. Maugh II, Research News, 10 Aug., p. 534) will probably contribute little to prevention programs. Exercise is the key. When the level of vigorous exercise is raised high enough, the protection appears to be absolute. The American Medical Joggers Association has been unable to document a single death resulting from coronary heart disease among marathon finishers of any age (*1*).

In several rehabilitation centers, patients who have recovered from one or more myocardial infarctions are being trained to run marathon distances. Seven such patients finished the Boston Marathon this year. They ran with their cardiologist, Terrence Kavanagh, from the Toronto Rehabilitation Center (*2*).

If vigorous exercise continues to protect cardiac patients from myocardial infarction, there is no excuse for the rest of us to debate the hazards of the other more controversial risk factors. We should join the long-distance runners.

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1. T. J. Bassler, *Lancet* **1972-II**, 711 (1972).
2. T. Kavanagh and R. J. Shephard, *J. Amer. Med. Assoc.* **224**, 1580 (1973).

Quality of Statistics

While I wholeheartedly support Kruskal's call (22 June, p. 1256) for a "room of one's own" for statistics in the house of public policy, I must disagree with him on one point. He says that in government activity "Much of . . . statistical thought and action . . . is not carried out by people called statisticians, or trained as statisticians. Much of it is not regarded as having important statistical components. *Consequently*, much of it is of poor quality" (italics added).

This kind of causal inference on the basis of only empirical information is the sort of "trap" that Kruskal himself condemns. Has he in fact examined the two-row by two-column table of analyst (statistician or nonstatistician) by quality of work (good or poor)? One of the appealing aspects of his proposed ecumenism is that the open doors will allow statisticians to peer into other rooms wherein nonanointed scientists are developing and using "good quality" statistics.

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Pollock's thoughtful letter provides a welcome opportunity to discuss further some points from my article. First, the empirical information about who does federal statistics and the quality of that work came to me primarily during my membership on the President's Commission on Federal Statistics; more careful studies of such matters could and should be made—quality judgments are never easy—but I am satisfied that my statement is qualitatively correct; the facts about Pollock's two by two classification are roughly known.

Second, even if that table were exactly known, causation might still be in question; that lack of training in statistics tends to produce statistical work of poor quality is my own opinion.

Third, I regret that Pollock misinterpreted my view as being the pompous one that only trained statisticians can do good statistical work. On the contrary, many important advances in statistics have been initiated by nonstatisticians; that is quite consistent with the view that better training in statistics tends to lead to better statistical work.

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Heritability of Intelligence

Constance Holden, in her report on R. J. Herrnstein's research on the heritability of intelligence (News and Comment, 6 July, p. 36), notes that the IQ test "is now widely regarded as 'culture unfair,' " and goes on to say that Herrnstein and others deny that it is culturally biased. I cannot help noting that the term "unfair" vitiates the argument. The point is simpler and has nothing to do with fairness: there is no possibility of any "intelligence" test *not* being culturally biased. The content of an intelligence test must have something to do with the ideas or with the muscle habits or with habitual modes of perception and action of the people who take the test. All these things are culturally mediated or influenced in human beings (even man's actions as a mammal or a vertebrate are given cultural evaluations that influence the behavior itself). This is not a dictum or a definition—it is a recognition of the way in which cultural experience permeates everything human beings perceive and do.

The question that should be asked is, Why do serious students of human behavior fool around with a dated idea like "intelligence"? It is possible, of course, to measure performance; it is possible to deal with perception (either physiologically or insofar as it is turned into behavior); it is even possible to deal with hidden values and assumptions. But is "intelligence" an adequate concept for summarizing all that?

The fact that the results of IQ tests can be statisticized makes matters worse—it gives the figures something of the quality of scientific "data" and thereby implies a "reality" that the figures do not have.

Obviously, behavioral scientists badly need summarizing concepts or just shorthand terms with which to bring together some of the things they measure. But just as obviously, "intelligence" is a culture-bound western European idea that has been given far more scientific weight than it can bear.

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Differences in intelligence are, obviously, to some extent inheritable, just like physique, longevity, or skin color. But who is to define what *kind* of intelligence is best—any more than what kind of physiognomy or physique is best?

Let me illustrate by suggesting one reasonable definition of intelligence: Intelligence is that quality which best promotes the survival of the human race. Then we could argue that the greatest threat to survival of the human race is the existence of nuclear weapons. Since they were discovered and developed predominantly by white scientists, we obviously have an urgent imperative to stop the breeding of white scientists.

This is not a completely facetious syllogism. Obviously people who say there are no intelligence differences between races are not very realistic. Extensive studies are not needed to establish this point. But Herrnstein and others who do not recognize that the particular kind of intelligence which another race has might be of much greater importance to mankind than his own are promoting a particularly dangerous and distasteful kind of intellectual unreality.

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"Slow burn" is the technical psychological term which describes the feelings I experienced in mounting intensity as I read Constance Holden's report on R. J. Herrnstein. My quarrel is not, of course, with Holden who merely describes the irresponsible attacks on Herrnstein. I use the word "irresponsible" with a certain conscious irony since that is the descriptive label which has inaccurately been pinned on Herrnstein. What is there about Herrnstein's statement in the *Atlantic* (*1*)—"Although there are scraps of evidence for a genetic component in the black-white difference, the overwhelming case is for believing that American blacks have been at an environmental disadvantage"—to justify the accusations of "racism" leveled against him?

Let me make it clear that I not only disagree with Herrnstein's position, but think he is totally mistaken to say that: "People who work in this field are hiding under rocks." A careful reading of Crow's 1969 article in the *Harvard Educational Review* (*2*) and of Scarr-Salapatek's article (*3*) raises serious questions about Herrnstein's interpretation of the data. Thoday's cautious warning (*4*) about understanding phenotypic characteristics where the data occur in the form of continuous variables ought to give any serious investigator some second thoughts about long-term judgment. The situation is most properly characterized by Paul Meehl (*5*): "No one knows and, worse, no one knows how to find out to what extent the SES [socioeconomic status]-I.Q. correlation is attributable to environmental impact and to what extent it is attributable to genetic influence. This causal ambiguity, while rather obvious (and clearly pointed out over 40 years ago in Burks and Kelly, 1928) is, as I read the record, somewhat above the sophistication level of many sociologists and psychologists . . . ."

It makes no sense to me to argue that all subjects of investigations are on the same level with respect to the amount of evidence that justifies publication. It takes a certain amount of social imbecility to argue that the same amount of evidence would justify publishing that pressing a lever in a Skinner box is related to a reinforcement schedule, as would justify publishing that SES is largely determined by genetic factors. Nevertheless, it is irresponsible to malign an author for things he expressly did *not* say in print!

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1. R. J. Herrnstein, *Atlantic* **228**, 43 (September 1971).
2. J. F. Crow, *Harvard Educ. Rev.* **39**, 301 (1969).
3. S. Scarr-Salapatek, *Science* **174**, 1285 (1971).
4. J. Thoday, *J. Biosoc. Sci.* 1 (Suppl.), 3 (1969).
5. P. Meehl, *J. Soc. Issues* **27**, 65 (1971).