

Letters

Educational Data Open Questions

Wolfe's editorial (7 Apr., p. 19) states "James Coleman in his *Equality of Educational Opportunity* . . . presents massive support for the generalization that differences in school achievement are so closely related to differences in family background that *changes* [emphasis added] in school facilities and curricula have little effect in overcoming deep-seated environmental handicaps. He reaches the dismal conclusion that 'schools bring little influence to bear on a child's achievement that is independent of his background and general social context; and that . . . the inequalities imposed on children by their home, neighborhood, and peer environments are carried along to become the inequalities with which they confront adult life at the end of school.'"

Coleman's survey, which assesses only one moment in time, cannot conclude directly what effect *changes* in school facilities and curricula have had or will have. Consequently, even though the report is massive, its support for this generalization is not. It is true that the school-to-school variances found by Coleman seem disappointingly small to some people, but there is no independent yardstick with which to measure them. The statement quoted seems to imply that little would be lost if children did not go to school at all. If we are not prepared to accept this extreme, just how is the statement to be interpreted?

In a preceding paragraph the editorial states, referring to differences found by Conant among high schools, "These inequalities will persist so long as school budgets . . . are determined by local attitudes and financial resources." These same attitudes and resources form an essential part of what Coleman calls background and social context. When he says "schools bring little influence to bear on a child's achievement which is independent of his background and social context," he is, in effect, saying that after removing the differences associated with the fact that poor schools tend to be in poor

neighborhoods, and vice versa, there seems to be little school effect left. But this statement does little to confirm or deny the possible effectiveness of uniform budgeting suggested by Conant as a method of promoting equality of educational opportunity.

James Coleman and his colleagues have done a remarkable job in collecting and presenting this mass of data in the short time allotted to them. In addition to investigating the many problems upon which this study bears directly we must, as Coleman has in chapter 3 of the report, try to use these data to shed light upon related problems of concern. In so doing we run the risk that suggested hypotheses will be considered to be proven principles. Because of this danger we feel that great caution must be exercised in basing policies upon this part of the Coleman report. The Office of Education, Coleman, and the academic community must have more time to investigate the many facets of these data, not only by careful examination of the study itself, but also by carrying out some of the many experiments suggested by the results in the report.

None of these remarks is meant to contradict Wolfe's emphasis on the importance of our understanding the learning process. Our studies under the auspices of Harvard's faculty Seminar on the Equal Educational Opportunity Report have led us to believe that except for the obvious inequality of *attainment* of various ethnic and regional groups, the results of the Coleman report are extremely difficult to interpret. For example, little attention has been given to the fundamental question, "What is educational opportunity, and how shall we recognize its equality?"

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* An additional 23 persons cosigned this letter. Their names may be secured from G. M. Ambach of the Harvard Graduate School of Education.

That was a fine thing for Reynolds to call attention to Somers' publication on the suppression of ovulation in 1940 and the contributions of the big "guns" of the day Albright and Kurzrock ("The pill: early breakthroughs," *Letters*, 17 Mar., p. 1361). The basic principle of the feedback mechanism was clearly stated by Carl Moore and Dorothy Price at the meeting of the anatomists in 1931 and published in the *American Journal of Anatomy* [50, 137 (1932)]. At about this time (1932-33) I was working with the late J. A. Morrell who was furnishing me with a lot of Amniotin, a mixture of estrogens taken from amniotic fluid of cows. We discussed the possible use of Amniotin as a contraceptive, and I asked Morrell to write Lombard Kelly about the idea, which he did on 19 October 1933.

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The paper by Moore and Price is rather extended and winding in the light of today's knowledge, but clearly demonstrates that "testis hormone" and "estrin," given alone or in combination and under certain conditions to males or females, have an antagonistic action on the hypophysis. This was probably the earliest breakthrough in principle. Hartman's letter was probably the first suggestion that a follow-up of this action of estrin be used to test its effects on fertility. I have written to G. Lombard Kelly, with no reply. A proper view in the 1930's was that administered estrogens, except for deficiency, might be carcinogenic. The papers by Sturgis and Albright and the paper by Kurzrock referred to in Sturgis' letter do not refer to the earlier work by Moore and Price or to that of Makepeace, Weinstein, and Friedman [*Amer. J. Physiol.* 119, 512 (1937)] cited by Stein (*Letters*, 28 Apr., p. 457). They may have made the discovery *de novo* or they may have been subconsciously influenced by knowledge which they had but did not relate specifically to the earlier work of some 3 to 10 years before in rats and rabbits regarding the nature of the action of estrogen. With retrospective hindsight of a quarter of a century, Sturgis tells me that he and Albright were unaware of, and so uninfluenced by, earlier work on animals. It appears at this time that

the paper by Sturgis and Albright was the first paper to demonstrate that estrogen given at the appropriate time does prevent ovulation in women. I would also point out that their primary goal of discovering a method for control of dysmenorrhea, while explained in terms of the absence of ovulation, is still unexplained in terms of what we know of the physiology of the uterus.

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Microorganisms on Mars

Horowitz and his associates in the article "Planetary contamination I: The problem and the agreements" (24 Mar., p. 1501) present a series of arguments for the relaxation of the COSPAR (Committee on Space Research) recommendation on spacecraft sterilization. There are several points, in addition to the question of Martian environmental hostility, which may be debatable. Some are moot and others are a reflection of the authors' seeming unawareness of current interplanetary quarantine policy. Examination of these points in detail is not warranted here.

The American Institute of Biological Sciences' Spacecraft Sterilization Advisory Committee of the National Aeronautics and Space Administration has been considering for the past year and a half many of the questions raised by Horowitz. It has developed a dry heat sterilization cycle which can satisfy the COSPAR requirements and is believed to be compatible with present spacecraft engineering and design.

Horowitz' call to lower the standards is not based on any more specific data than was used for the COSPAR premise. The prime difference is that the COSPAR recommendations have taken a quantitative form in a simple model while Horowitz' suppositions are less clearly formulated.

The 1966 USA recommendation to COSPAR, that the probability of contaminating a planet be no more than 1×10^{-3} during the period of biological exploration, sets up a sterilization requirement that is considered to be a workable and acceptable probability. The existence of such a quantitative definition has permitted engineering development of interplanetary exploration

vehicles by setting the limits which the craft must meet. Such a definition is a continuing requirement.

Reducing COSPAR probability restraints is of lesser importance than a better understanding of sterilizing procedures. More precise sterilization requirements of time and temperature should be set in order to keep to a minimum the degradation of the reliability of the spacecraft and yet attain the desired probability of sterility. The committee has developed more precise dry heat sterilization data that have already significantly reduced these requirements without sacrificing or reducing the probability of attaining the desired sterility.

Horowitz does not specify a standard to be met. Can he suggest a more workable probability? He should specify the microbial burden to be allowed, the cleanliness requirements for his experiments, and the thermal tolerance of his equipment. These are practical problems urgently requiring resolution if the program is to be continued unimpeded. If this information is available, he can make valuable contributions. It is urged that he discuss these aspects with the AIBS committee. The problem can thereby be further removed from the area of rumination and supposition and lead to a rewarding scientific solution.

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The article which Bond's committee objects to is an examination of the basic assumptions of the current spacecraft sterilization policy in the light of new knowledge of the planet Mars. It shows that the validity of these assumptions is, at the very least, questionable. Our conclusions are based on a large amount of evidence which was not available in 1964 when the basic COSPAR resolution was adopted, including the data from Mariner IV and from recent earth-based observations. In view of this fact, the committee's assertion that our conclusions were "not based on any more specific data than was used for the COSPAR premise" is incomprehensible. Equally curious is the committee's declaration that "reduction of COSPAR probability restraints

is of lesser importance than a better understanding of sterilizing procedures." Surely the establishment of a sound policy is as important as the pursuit of technology for implementing that policy.

Contrary to what the Bond committee implies, we do not reject the recommendation that the probability of contaminating Mars not exceed 10^{-3} during the period of unmanned exploration. We accept this objective, but contend that, for the reasons detailed in our article, it can be attained without the adoption of extreme sterilization procedures.

If the Bond committee can substantiate its claim to have solved all spacecraft sterilization problems by a dry heat cycle, it will deserve the thanks of everyone who is interested in planetary exploration—providing, of course, that the process is reasonable in cost. If such a process exists, it has been a well-kept secret. Current estimates of the cost of sterilizing the Voyager series run into the hundreds of millions of dollars.

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Louis XV in a Dark Corner

My hat is off to the sharp historians of Harvard University and Norfolk State Prison who have finally fixed responsibility for the utterance "Après moi le déluge" on Louis V (The Slugard) of France (News and Comment, 31 Mar., p. 1653).

Despite the distress this will cause those of us who for years have attributed this remark to Igwald (The Witless) of Finnmark, we may be comforted by the fact that still another lamp is lighted in a hitherto dark corner of history.

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Pressures and Student Disorders

Abelson placed a finger on a sensitive spot in American secondary education in his editorial "Excessive educational pressures" (12 May, p. 741). As he states, responsibility for excessive