Letters

Educational Data Open Questions

Wolfle'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 Wolfle'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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Research Prior to the Pill

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