

RESPONSE TO DR. SCHIFF

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Dr. Schiff does not respond directly to the criticisms of the Study. The claim remains that it was undertaken to justify previously held positions and that its conclusions are not substantiated by its data.

I shall begin with a response to Dr. Schiff's concluding question, about the "bottom line" worth of the Study. Then I will respond to other of his comments, within the space allotted me.

Trow (1986) presents four models of educational research, and the Study is closest to the one he calls "political . . . the intentional use of research by political decision-makers to strengthen an argument, to justify positions already taken, or to avoid making or having to make unpopular decisions by burying the controversial problem in research" (p. 257). Some of this research tends to be weak in methodology and analysis, since the results are known in advance. It is clear to me that the Study is a prime example of political research. My critique was precisely of the Study's methodology and analysis. Nothing Dr. Schiff writes negates the conclusion that it is flawed on that score (see #1 below).

As for the Study's conclusions and recommendations, Dr. Schiff does not deny my assertion that they justify previously held positions rather than arising from the research. He touts the Study's influence on "movers and shakers," but has to admit that "an examination of [the Study's] impact has yet to be made."

Dr. Schiff would have us believe that the Study's purported political benefits somehow compensate for its flawed methodology. Unfortunately, there is as much potential for harm as good when leadership, unaware of the Study's weaknesses, moves to implement the Study's recommendations. For example, findings like "Conversational Hebrew is not being taught effectively or learned effectively" (p. 92) are likely to move shakers to erroneous policy conclusions, since the finding itself is almost baseless, nor did the Study investigate, or even indicate, that some language teaching approaches or curricula might work better than others.

The Study's "vast amount and variety of talent" could certainly have been put to better use than to take three years merely to repeat findings and conclusions (and not even that, in any rigorous way)

contained in previously published works. There is room for "quick and dirty" research, but what is the value of "slow and dirty" research? Indeed, let the reader judge.

Turning briefly to some specific items:

1. Dr. Schiff wants to know when vocabulary tests ceased being a measure of language comprehension -- about a generation ago. The change in linguistics began to appear in the early 70's (e.g. Spolsky, Murphy, Holm and Ferrel 1972). By the mid-80's there were already available to the classroom teacher numerous practical guides for testing language proficiency which were not vocabulary-based (e.g. Carroll and Hall, 1985; Shohamy, 1985). In any case, the paper-and-pencil test used in the Study could not measure conversational Hebrew, as it claims to.

Dr. Schiff now informs us that the Knowledge Inventory tested "curricular achievement based upon what the principals knew was being taught." That is an assertion not only not made in the original Study, but contradicted by it. The Study makes it quite clear that, for example, most schools teach very little Hebrew language or Jewish history (pp. 105-6), yet those two domains alone comprise 25% of the Knowledge Inventory.

Regarding the idiosyncratic scoring system on the attitude inventory, Dr. Schiff reassures us that "students generally responded to all the items. There were hardly any 'skipped questions'." Are there no data he can present to buttress this point? (In this regard, knowing how the students were instructed to answer is crucial. Thus, when writing my original article, I asked Dr. Schiff to supply the "Guidelines for Administration" of the Inventory. I was informed, in a letter of May 12, 1992, that he could not supply them.) He views the "critical finding of the Study" to be that most students were only passively involved in Jewish life, yet does not address the profile I presented of a "passive" student (using the Study's scoring system), who would likely be regarded as a prized, active graduate of most supplementary schools.

Dr. Schiff does not address my criticism about the Study's confusion between cross-sectional and longitudinal designs. He states the obvious (that each design has its own strengths). However, the Study conducted the former and interpreted it as if it were the latter. As for "changes" in scores among grades, nowhere does the Study test the grade means for statistical significance, so we have no way of knowing if there was meaningful variation or not -- even on a cross-sectional basis. (Thanks to Dr. Harold Himmelfarb for this observation.)

Stating the obvious is how Dr. Schiff tries to handle the question of the Inventory's reliability, rather than using statistical measures appropriate to heterogeneous tests (like the stratified alpha, Cronbach, 1990).

On the absence of comparison groups, I did not suggest inserting Inventory items into the Yeshiva High School entrance exams (as Dr. Schiff asserts), but the reverse! Comparison groups **might** make the supplementary school results look worse, but without such groups we just don't know what we are looking at. Well-designed research is meant to **answer** questions, not leave us to speculate about what might have been, or might yet be.

2. Dr. Schiff says I "err" when I characterize the recommendations as deemphasizing schooling in favor of family education programs. If that is not the case, what is the Study's **chiddush**? A dispassionate reading of the Study's recommendations (Chapter 9), indicates a clear tilt in favor of non-instructional programs (which is what "deemphasize" means).

3. Dr. Schiff sheds no additional light on reasons for the shift away from the effective schools paradigm. As to the proportion of field research time given to this phase of the work (which he says I blatantly misrepresent), my reading of the Study Timetable (page 56) shows nine of 24 months for a total of 37%, not five percent as Dr. Schiff asserts. Regarding the reporting of results from the effective schools phase, it is true that about half of one paragraph on page 50 does sketch some very general statements about what the research showed, without providing any substantive -- let alone quantitative -- findings. This example is symptomatic of a fundamental difference between myself and Dr. Schiff, regarding what the terms "results" and "research" mean, as well as the uses to which they are put.

Another telling example of Study "findings" which lack an anchor in data, is the "relationship of family to schooling." Dr. Schiff persists in the belief that "the Study demonstrated that the family is key to supplementary school effectiveness." Yet, neither the Study nor his rejoinder offers any evidence to that effect. Instead, he accepts uncritically the testimony of BJE staff and school personnel, that the major obstacle to school achievement is lack of parental support. Precisely such preconceptions can, however unconsciously, color the Study staff's "scanning" of Inventory results, which "revealed" the clear connection between parent commitment and student achievement. If anything, the Study itself is pessimistic about the benefits working with the parents will have on the students: "Based on the data available, it is not clear whether participation in parent education and adult

education programs has any impact upon classroom instruction and the learning behavior of pupils" (p. 101). If, as Dr. Schiff asserts, the Study really is "the largest scale and most elaborate investigation of its kind," where are the data to prove it?

Dr. Schiff urges dramatic improvement in Jewish supplementary schooling. I urge the same for "Jewish Supplementary Schooling." At least we achieve some measure of agreement in that.

References

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