

**JEWISH SUPPLEMENTARY SCHOOLING MISPERCEIVED<sup>1</sup>**

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*Jewish Supplementary Schooling was a large-scale assessment of supplementary schools in greater New York. It consisted of interviews, on-site visits, and an Inventory of Knowledge, Involvement, and Attitudes. The main finding was that the schools were failing in all three areas; the main recommendation, to reorient the schools to family and informal education. A close examination of the research and the study report revealed that the data were deeply flawed, and the recommendations unrelated to the data. Flaws included poor Inventory items and scoring system, the absence of a comparison group, and a mistaken design. A closing discussion of assessment strategies highlights the need to conceptualize more clearly the nature of supplementary education, to develop relevant evaluation criteria, and to design a study process which capitalizes on supplementary schools' unparalleled autonomy. The weaknesses in the study were shown to be typical of research in Jewish education.*

A few key research articles have had an enduring effect on the direction of Jewish education, for better and for worse. Dushkin and Engleman (1959) highlighted the shallowness of American Jewish schools. Himmelfarb (1974, 1975) convinced an entire generation that supplementary schools were "for naught." Even when reanalysis of Himmelfarb's data led to a more positive assessment of the effectiveness of supplementary schools (Cohen 1988), the prior research held sway in the public's mind. Finally, Schoem's (1979) chilling ethnographic study provided the most detailed description of the malaise of supplementary schools.

*Jewish Supplementary Schooling: An Educational System in Need of Change* (1988) is likely to inherit the central position of the Himmelfarb research, in three questionable ways. First, it continues the tradition which finds supplementary schools to be of negligible worth. Hence its key -- albeit unsubstantiated -- recommendation is that schools should reorient themselves toward family education, alternative environments, and informal education. Second, while both studies called for substantial reform, their own pessimistic view of the situation may actually block the flow of added resources needed for change.

Finally, the broad distribution of popularized versions of both works, far beyond a mere scholarly article circulated only to specialists, reinforced the negative climate of opinion surrounding supplementary schools, without giving readers the data to let them formulate an opinion of their own.

Therefore, *Jewish Supplementary Schooling* (hereafter, "the Study") needs close scrutiny, on two accounts. First, I have used the Study in teaching university courses in recent years, and am convinced that the research itself is flawed, that the Study's conclusions and recommendations are not based on the data (a point of view shared by others, e.g. Israel, no date) and may rely on preconceived positions. Second, I have been involved with supplementary schooling in both research and policy roles (Resnick 1986; Resnick no date), and clearly see the need for experimentation and change. But the Study paints a monolithic, undeservedly negative view of the situation, and (as mentioned above) is more likely to preclude change ("Don't throw good money after bad") than to promote it.

This paper will focus on three basic questions. First, as a piece of educational research, are the Study's conclusions warranted? Second, in what ways is the Study characteristic of the problematics of contemporary research in Jewish education? Third, what can the Study teach us about which evaluation methods are appropriate for supplementary schooling?

## I

The Study was conducted on a grand scale. More than a dozen members of the staff of the New York Board of Jewish Education were mobilized to constitute the professional study team, guided by a fifteen-member BJE Board Task Force on the Jewish Supplementary School. The process lasted three years, including the design and administration of an "Inventory of Jewish Knowledge, Involvement, and Attitudes" to about 3700 pupils in 40 schools, together with more than 600 interviews of parents, principals, teachers, pupils, and lay and rabbinic leaders in these schools, as well as school observations. The 40 schools were chosen to reflect the profile of supplementary schools in Greater New York, in terms of ideology (90% of the students were in Reform or Conservative schools, with Orthodox and Reconstructionist schools accounting for the remainder); geography (nearly two-thirds of the schools were suburban, the rest were urban); and school size (forty percent of the schools were "small," with less than 100 students). Almost half the field research time was involved with defining,

identifying and profiling "effective supplementary schools," though the approach was abandoned and the results not reported. The Study set out to test eleven hypotheses (p. 58)<sup>2</sup>, but we will focus on three of the key ones.

**Hypothesis #1: The levels of Jewish knowledge, Jewish involvement and Jewish attitudes of pupils are significantly lower than the expectations of the respective principals.**

This hypothesis was tested by creating an objective test Inventory which was administered to all the students in the 40 schools in the sample. The Inventory contained 100 multiple-choice knowledge items in ten subject areas (e.g., Customs and Ceremonies, Jewish Holidays, Life Cycle) and ten items each for involvement and attitudes. The items were not based on the school curricula, but rather "upon the minimal levels . . . that principals expected pupils to exhibit by the time they became Bar/Bat Mitzvah" (p. 115), namely "100% or close to 100%" (p. 85).

Before discussing the results of the Inventory, it is important to examine the use of principals' expectations as the sole criterion of school effectiveness. The sample included schools of four denominations which differed in the number of class hours and subjects taught. That diversity was presumably the reason for testing principals' expectations, rather than curricular achievement, though the Study is silent on this point. However, that decision is problematic, since the Study itself shows that principals' expectations do not shape the bulk of the schools' instructional hours. For example, 87% of the principals indicate that Hebrew language is an important curricular goal, and Conversational Hebrew is the largest single test in the Inventory. Yet, as we shall see, many of the schools taught almost no Hebrew at all. Since the Inventory tests material largely unrelated to the students' actual school experience, the Inventory is an unreliable indicator of the effectiveness of school instruction. It is with that caveat in mind, that we turn to the Inventory results.

"The findings demonstrate that in all three areas pupils scored much lower (an average of 50% lower) than principals' expectations" (p. 115). This finding is then transmuted into a much broader, more critical statement that "schools do a very poor job in increasing Jewish knowledge in all subject areas; they show no success in guiding children towards increased Jewish involvement; and they demonstrate an inability to influence positive growth in Jewish attitudes" (p. 119). These erroneously negative conclusions arise from three defects in the research: the Inventory itself is poorly designed; the absence of a

comparison group makes it almost impossible to evaluate the results; and the results are interpreted as if the study were longitudinal, when it was actually cross-sectional.

*Poorly designed Inventory.* The Knowledge Inventory consisted of ten subtests, most consisting of only ten items each. Therefore, poor or overly-difficult items considerably affect the outcome on each subtest. For example, the Study emphasizes that only "9.1% knew that the Sanhedrin was the highest authority in Israel during the days of the Second Temple" (p. 87). With only four possible responses to that question, the 9% outcome is far below the chance response rate. Careful item development would have alerted the test designers to the presence of more attractive answer alternatives which distorted the results. For example, that item's full phrasing is "The highest *religious* [emphasis added] authority in the days of the Second Temple was the: (1) Sanhedrin; (2) Gaon; (3) Prophet; (4) Samaritan." In context, the response "prophet" would not be utterly inappropriate, and it likely accounts for the mistaken student responses.<sup>3</sup>

The Jewish Involvement and Attitudes Inventories are even more problematic. Ten items are simply not enough to arrive at any reliable profile of each of these complex areas. As troubling as many of the items are, the scoring system is even more misleading. On an item like "My parents (father or mother or both) attend synagogue: (1) Every Shabbat and Jewish Holiday; (2) On Jewish Holidays only; (3) Only for Bar/Bat Mitzvahs; (4) Never," alternative (1) received 10 points as being very positive; (2), 6.67 points; (3), 3.33 points as being mildly negative; and (4), no points.<sup>5</sup> "If no response was made, this was considered to be a neutral (uncommitted/indifferent) or passive response, and five points were awarded" (p. 82). The aggregate scores were then given labels describing the level of Jewish involvement/attitude, e.g., 80-100 is "very high/positive;" 40-59 is "passive/neutral;" and 0-19 is "very low/negative." The overwhelming majority of students rated in the "passive" range on involvement. On the attitude scale, younger students were weakly positive, with older students (sixth grade and above in public school), neutral.

Yet, here is one profile of a student who would be rated "passive" using this scoring system: attends synagogue every Shabbat; eats [only] Kosher food at home (but not away from home); sometimes says a Berakhah before eating; lives in a home where Shabbat candles are lit every Friday evening and the Hanukkah Menorah every year; attends a Passover Seder every year; sometimes gives Tzedakah (Keren Ami); once attended a Jewish summer camp; parents attend synagogue only for Bar/Bat Mitzvahs; never attended the Salute to Israel Parade.

There is no indication in the Study of how the criteria for the labels were devised.

Two problems in this scoring system account for the statistical distortion. First, applying numerical weightings to descriptive responses as if they constituted a true scale is always questionable, e.g. in the synagogue attendance question above, assuming that the numerical distance between responses (1) and (2) is the same "distance" as between responses (2) and (3). Even more troubling is to take these semi-quantitative findings and reconstitute them as qualitative states, complete with descriptive labels ("passive" or "neutral"). Awarding five points for no response -- assuming that it meant passivity -- distorts the results beyond recognition. For example, a student who **skipped** eight questions, and then answered two questions at the highest level (e.g. went more than two times each to the Salute to Israel Parade and to a Jewish summer camp) is scored as having "high" Jewish involvement.

A simple reporting of the percentages of students responding to each alternative, question-by-question, would have been a more straightforward way to present these data.

*Absence of a comparison group.* We have already seen that, based on the Inventory results, the Study concludes that supplementary schools are doing a poor job. Its performance seems even more lackluster because the supplementary school is often "compared to its more intensive sibling -- the Jewish day school" (p. 11). Yet the Inventory results -- even if they had overcome the substantive problems raised in the previous section -- really tell us very little about how well the supplementary schools are doing, because we have no basis on which to decide whether the results are poor or not. That is due to the absence of any comparison group, or baseline data, against which we can compare these results in order to interpret them.

Two obvious potential comparison groups come to mind. The first, and easiest group, would have been a sample of Jewish day school students from the same grades and geographic areas as the supplementary students. While such a comparison group would not have controlled for all the important variables which affect achievement (e.g. home influences), it still would have provided a framework for interpreting the supplementary school results. The fact that Bar Mitzvah aged children averaged only 60% on the Jewish Holidays subtest certainly *seems* like a poor score, but if day school students did not do much better, then we would have pause to consider what the results mean. In fact, there was a very easy way to make such comparisons, without having to create a special day school comparison group. Since the BJE administers city-wide entrance exams to elementary day school

students seeking admission to day high schools, it could have included some of those items in the supplementary school Inventory, and had a basis for comparison ready at hand.

The second possible comparison group, more difficult to recruit but potentially more important, are Jewish children not receiving any formal Jewish education. These might have been drawn from Jewish settings (e.g. JCC's) or from the totally unaffiliated. Here, too, there would be important extra-schooling variables (partly controllable with data on parent observance in the Involvement Inventory), but one wonders how many of them would know about Yom ha-Atzmaut or the Sanhedrin. Against such a comparison group, supplementary schools might look a lot better than the Study suggests.<sup>4</sup>

*Cross-sectional, rather than longitudinal, design.* The Study makes repeated, mistaken references to differences in scores between grades, "tracking the level of pupil performance from Hebrew grade one through Hebrew grade eight" (p. 86); "learning curves" (p. 88); and dedicates an entire section to "The Sixth-Grade Knowledge Loss" (p. 87ff.). Moreover, the only complete presentation of the Knowledge Inventory data is in the form of graphs which plot the percentage correct as a continuum across the grade levels. The source of these mistaken judgments is the failure to realize that the study was cross-sectional, i.e., the Inventory was administered to the entire student sample at one point in time. Therefore, comparisons across grade levels are fraught with danger, aside from being technically invalid. A true tracking study, conducted longitudinally, with repeated measures on the same students across time, would provide the basis for explaining what happens to students throughout their supplementary school experience.

Instead, the Study reports judgments like these:

One of the most significant findings of the Jewish knowledge section of the Inventory is the increase in knowledge levels of pupils who continue their Jewish schooling beyond Bar/Bat Mitzvah ... [who] are especially motivated to achieve, even though they did not attain the minimal levels that principals expected pre-Bar/Bat Mitzvah students to reach (p. 89, emphasis added).

Since the study was cross-sectional, the data on post-Bar/Bat Mitzvah performance are drawn from different students than the pre-Bar/Bat Mitzvah results. Therefore, we simply do not know how the students who scored high as "post's" would have scored as "pre's". However,

there is reason to think that these students probably had been high-achievers all along.

Jacoby (1970) found that students who reported learning a lot in Hebrew school enjoyed it more than those who learned less. Furthermore, those who learned a lot (and enjoyed it) continued their Jewish studies after Bar/Bat Mitzvah in much greater proportions than other students. That is the phenomenon probably at work in the Study. The post-Bar/Bat Mitzvah scores are those of the successful students who stayed on. There is a group of them in the pre-Bar/Bat results, too, but their high scores are washed out by the results from the larger number of "underachievers."

Support for this interpretation can be found in the Study's listing of the number of students tested, by grade level (p. 83): *Vav* (7-8th public school grade; 525 students); *Zayin* (8-9th public school grade; 365 students); *Het* (9-10th public school grade; 28 students). In *Zayin*, the first year after Bar/Bat Mitzvah, enrollment has dropped by one-third, and we can assume (based on Jacoby's findings) that it is the underachievers who disproportionately dropped out. That leaves better students, who scored higher, though we have no data on how much more they learned in that one year. The two-year "post" group is only five per cent the size of the *Vav* group, which likely accounts for their dramatically superior performance.

As for curricular areas where little "progress" is made from year to year, the Study is also mistaken. Conversational Hebrew is a prime example.

Conversational Hebrew is the subject in which pupils showed the least knowledge and progress. The graph indicates a slight increase in knowledge between Hebrew grades one and three, a steep decline between Hebrew grades three and four and a steep drop (nine percentage points) between Hebrew grades five and seven. . . Conversational Hebrew is not being taught effectively or learned effectively. (pp. 89, 92)

Even if the data presented were longitudinal, an explanation for "lack of progress" is more readily at hand, in the Study itself. In discussing "Time on Task," the Study indicates that Conservative schools give about one-third of class time to Hebrew in the first two years, but that drops to 15% of class hours in grades four and five. Reform schools essentially teach no Hebrew at all. The reason that so little Hebrew is learned is not that it is taught poorly (a point for which no evidence is adduced), but that it is not taught at all. What is taught in the first two

years may be learned, but is simply forgotten two or three years later, when it has dropped from the curriculum.

In sum, because the Study mistakenly treats its cross-sectional data as if it were longitudinal data, it asserts invalid conclusions about how the supplementary system operates over time. This is not to suggest that the system works superbly, but only that these data cannot support any of the Study's negative conclusions on that score. Indeed, because of this explanatory failure, the Study reaches the wrong conclusions in two other of its hypotheses, those which deal with the "decline" in knowledge in *Vav* (#2), and the "dramatic increases" in post-Bar/Bat Mitzvah scores (#7).

**Hypothesis #6: The attitudes of parents to the Jewish education of their children and parental involvement in the school program are crucial to the learning behavior and attitudes of pupils.**

There were 127 interviews with parents, using a questionnaire. The Study does not indicate what questions were asked, nor are any quantitative results of the interviews presented. Instead, the Study presents summary descriptions of parental attitudes toward, and involvement with, the supplementary school. These include (p. 63): "parental involvement in the school is virtually non-existent"; "many parents do not want 'too much Jewish schooling' for their children"; yet they do not have a clear idea of what kind of Jewish education they do want.

The conclusions section for this hypothesis states that "the findings show" that the hypothesis is confirmed (p. 117). Yet, there are no quantitative findings to that effect. There is no crosstabulation or other analysis linking parental attitude to student achievement or attitude. Such an analysis could have been generated from the items in the Involvement Inventory, since many of them reflect parental involvement (e.g., their synagogue attendance, sabbath candle lighting behavior). While intuitively it makes sense that strong parental support enhances school results, the Study presents only impressionistic data to that effect, and does so in a way which soft-pedals high student achievement on the Inventories. Thus, the Study reports:

After Inventory scores were analyzed, the principals . . . were interviewed regarding the pupils who had the highest [Inventory] scores. . . the overwhelming majority of parents of pupils with high Inventory scores are most actively involved in the school program and in synagogue/temple activity. (pp. 95-96)



This is the only mention in the Study that there were high scorers, though we are not told how many there were, what their scores were, or what other variables might account for their achievement (e.g., a successful class, curriculum, or principal).

Unfortunately, the unwarranted conclusion about the impact of parental attitude and involvement on student achievement is central to the Study's key recommendation, which deemphasizes schooling in favor of family education programs.<sup>6</sup> That proposal had been advanced publicly by the study director even before the Study was begun (Schiff 1983). While such programs may be worthwhile in their own right, or in conjunction with regular instruction, the Study mistakenly asserts that this major reorientation of Jewish education has a firm research basis (p. 121). Moreover, it is puzzling that while the study's recommendations are heavily dependent on parental involvement, parents evidently had no role in the design and conduct of the study, or in shaping its recommendations. This point will be addressed more fully in Section III.

**Hypothesis #10: The professional personnel employed by the synagogue are inadequately prepared for their respective instructional, guidance and supervisory functions, given the changing needs of pupils and their parents.**

The Study reports that this hypothesis was confirmed by the research findings. Yet, few actual findings are reported on this topic, and what findings there are are not irredeemably negative. It is true that most teachers did not have a Jewish education beyond high school level. Perhaps no more than one-third have had any professional teacher training. Most are young and have been in their current position for three years or less. "Most teachers admit they need to improve and are willing to find ways to do so" (p. 69). So the portrait is of an untrained teacher corp, yet one willing and even able to improve.

There were 117 classroom observations, using an instrument which included items on the instructional process, classroom management, and pupil behavior, though the Study supplies no specifics on these items, nor are any data presented. There is a very brief report summarizing what the classes were like, which gives a mixed impression, rather than a wholly negative one.

In a narrow sense, the hypothesis was confirmed since the teachers are inadequately prepared. Yet, the data suggest that despite the lack of preparation, they seem to be doing a nearly adequate job. Even more important, it seems that if trained, they might actually do a good job. However, the Study avoids that conclusion, preferring to

abandon the school paradigm in favor of retraining for family education. There is no basis in the data to prefer training for family education over training for a school setting, nor to assume that proper training would be insufficient to produce substantial improvement in their school functions.

At this point it is worth noting an unexplained change in the Study's research paradigm. The Study dedicates an entire introductory chapter to "Effective Schools Research: What Research in General Education Tells Us About Good Schools" and bases its assessment of supplementary school effectiveness on that model. Moreover, a substantial portion of the Study process was dedicated to identifying and profiling ten effective supplementary schools. Unfortunately, none of those profiles are presented in the Study nor are the Inventory results for the ten schools reported.<sup>7</sup> Instead this entire line of work was abandoned, with only the terse comment that "It was determined that the Study should focus on obtaining an overall portrait of supplementary schooling rather than identifying effective schools" (p. 50). Nonetheless, a central section of the discussion of the Study results is undertaken "In Light of Effective Schools Research" (p. 118). Since the Study endorses the effective schools approach and did succeed in identifying effective supplementary schools, it is puzzling why that model for improvement was rejected, in favor of family education. Moreover, the failure to report on effective supplementary schools, or individual classes, or specific approaches -- some of which, as noted by Reimer (1990), do exist -- creates the misconception that the supplementary framework is beyond repair. In the end, the Study tries to have it both ways: major retooling for family education together with retraining for effective classroom instruction. While either goal, if achieved, would be laudable, the Study seems to lack the clarity of vision to which it aspires.

Having examined some of the key weaknesses of the Study, I now want to show that those weaknesses are typical of research in Jewish education, rather than being exceptional. I will then sketch some of the implications for future research in Jewish education in general, and for the supplementary school in particular.

## II

In an earlier work (Resnick 1982), I identified five weaknesses of the Jewish education research enterprise: 1) in the absence of clear goals developed by educators, researchers generate their own research constructs; 2) there is no corp of well-trained researchers in Jewish

education, nor an acknowledged body of research traditions; 3) results are reported in journals published under Jewish auspices, usually out of touch with current canons of research; 4) the politics of educational research usually dictate a top-down approach, "initiated by outside funding agencies or by those with a stake in education but no direct power" (Resnick 1982, p. 127); and 5) the relationship between research in general and Jewish education is complex. Here is a brief look at how the Study fares on each of these points.

*Researcher-generated educational goals.* The Study reports that most schools did not have a written curriculum, nor explicit goals statements. In their absence, the study team developed the Inventory based primarily on principals' expectations. While such a strategy is probably better than the researchers' developing items in total isolation from the school community, it was a strategy with its own problems, as we have already shown. A research design more closely coupled to the activities of each school, which also takes into account the goals of the many interested parties other than principals (as discussed below in Section III), would have been more appropriate.

*Absence of full-time research team.* The Study was conceived and executed largely by the BJE staff itself. While the staff are undoubtedly highly qualified in their own fields (school consultation, media, etc.), there is no reason they should have research qualifications. Perhaps some of the weaknesses of the Study are due to its having been conducted primarily by non-researchers.

*Results reported in Jewish journals.* The drawbacks of reporting results in journals published under Jewish auspices were detailed long ago by Fishman (1957/8, p. 51):

Social research on American Jewry is, by and large, outside of the mainstream of American social science research. It is published in journals not commonly accessible to (or scanned by) the majority of American social scientists. It is not subjected to the critical reading, to the methodological standards, or to the theoretical insemination that more frequently marks social research under general auspices.

In this case, the Study was published by the BJE itself. Perhaps that explains why the Study does not perceive itself as educational research to be reported in social science terms. For example, despite a discussion of statistical terms and measures used, the Study states that "in order to facilitate the reading and understanding of the analysis by the average lay and professional reader, statistical measures and terms are not used in the discussions [or reported]. Rather, they are the basis for

them" (p. 83).<sup>8</sup> Since a substantial Executive Summary was issued as a separate volume, the full Study report could have been more statistically explicit, at least in its Appendices. As it now stands, readers do not have access to the basic data needed to assess the Study's findings or conclusions.

*Politics of educational research.* This is a topic worthy of extensive treatment, to be partially addressed in the next section. Nevertheless, two aspects are worth mentioning briefly here. First, the Study was an attempt to effect change in a system over which the BJE has no direct control. Hence, the Study had to grapple with many of the challenges endemic to all outside, "top-down" research efforts (Adelman 1984). It succeeded with some (involving school principals), but did less well with others (clear, relevant evaluation criteria). Second, the realpolitik of central agency life precluded reporting results by denominations. For example, the low Conversational Hebrew results might have been "improved" had the scores for schools which emphasize Hebrew (mostly Conservative) been reported separately from those which do not (mostly Reform), instead of lumping all the schools in together, "washing out" the results of the better schools.<sup>9</sup> Indeed, the scores could have been reported with curricular, rather than denominational, labels.

*Relating to research in general education.* On this point, the Study is particularly strong, since it draws generously from findings in general education. Yet, at critical junctures the Study abandons the general education research, as in the case of the effective schools paradigm. Moreover, the Study may not be sensitive enough to the substantial differences between the contexts of general and Jewish education, a point to which we now turn, in greater detail.

### III

This is not the place to offer a manifesto for research and evaluation in Jewish education. There has been some preliminary work in this area (Bank 1985a, 1985b), but the discipline is still in its infancy. Nonetheless, the Study is a good basis for reflecting on which evaluation strategies may be particularly appropriate and worthwhile for Jewish education, in its current configuration. My hunch is that such an assessment has to begin with a deeper appreciation of the unique nature of Jewish supplementary schooling, from which broad guidelines for appropriate evaluation might emerge. At the same time, that assessment can bear in mind those aspects of evaluation which are generally problematic, and especially so in Jewish education, e.g., the

object of evaluation, evaluation criteria, and who is served by the evaluation (following Nevo 1983).

*Object of evaluation.* The assertion that what is to be evaluated is "the supplementary school" seems to be straightforward enough. Yet, the study itself documented what a motley phenomenon that is, in almost every way: weak involvement and low expectations by both parents and students<sup>10</sup>; part-time, untrained staff; rudimentary or nonexistent curriculum; unsupportive or hostile organizational environment. Therefore, earlier researchers have concluded that supplementary schools are not schools at all, but settings for Jewish socializing (Heilman 1983), and there is respectable evidence that they may succeed in this regard (Bock 1976; Himmelfarb 1984). If the "object to be evaluated" is not a system of formal schooling, then the entire evaluative framework would be different. Knowledge inventories for the kids, assessment of teachers' level of Jewish knowledge (as opposed to commitment, for example), examination of the school curricula -- all of these would be largely irrelevant. Supplementary education -- perhaps all of Jewish education -- is **informal** education, and the evaluative paradigm must be adjusted accordingly.<sup>11</sup>

*Evaluation criteria.* This is a bugaboo in the best of circumstances. The most straightforward position is to assess the extent to which the school's goals have been achieved (Tyler 1950). Yet, the Study documents the fact that most of the schools had no explicit goals. So the Study established principals' expectations as the criteria of judgment for the Knowledge Inventory, and invented a scoring system and set of labels for the Involvement and Attitude Inventories. Neither strategy is satisfactory. In some of the areas, like the school visits and parent interviews, no criteria were stated. The Study's approach does not address any of the five evaluative domains which Pateman (cited in Thomas 1985) lists: parental preferences; efficient use of public resources; allowing for teachers professional freedom; meeting society's requirements; and satisfying children's needs. On some of these, supplementary schools actually do quite well (e.g. teacher freedom), and other domains might have yielded results and recommendations different from those of the Study (e.g. how many **parents** are willing to commit themselves to substantial family education programs? What is to be done with the children of those who won't?).

*Who is served by the evaluation.* Presumably, the Study was meant to serve the needs of those audiences which would implement its recommendations. On this point, it seems that the Study missed the mark. The Study was, from start to finish, a BJE process. It was an "outgrowth of BJE's Fall 1983 Board seminar" (p. 47), not an effort

invited by the schools themselves. It was a BJE Board Task Force which ran the study, evidently not including any teachers, principals, congregational rabbis or parents. Yet the bulk of the recommendations could only be implemented by individual congregations, together with their schools. The Study does not evolve some new way of overcoming the gap which separates central agencies from the schools they serve. Instead, it seems to deepen the divide. How else is synagogue leadership likely to interpret recommendations like:

Change the education focus of the synagogue from schooling of the young to education of all members of the family. Change the structure of the synagogue to accomplish the above (p. 133).

Indeed, the Study acknowledges that "individual rabbis, rabbinic groups and lay leaders may not take easily to what amounts to a structural and programmatic revolution in the synagogue" (p. 125).

The Study seems oblivious to the most salient factor in supplementary education: a level of autonomy for the individual school unheard of in general education, even in private schools. The schools voluntarily participated in the Study, which was conducted by external personnel, and the results were intended for the BJE Board, not the individual schools. The results of the Study were shared with school leadership, but the only body committed in advance to the Study results was the BJE itself. Therefore, the prospects for impact -- as in all pure research projects -- are slim. While school autonomy appears to be the impediment to improving supplementary schools, the reverse may well be the case: it is the best (and coincidentally the only) potential source of innovation and commitment to change.

As the most comprehensive research on supplementary schools published in recent years, the Study could have made a substantial contribution. Hopefully, it may still give impetus to further research: "All good science is accumulative; no one can get everything right the first time" (Gould 1991, p. 10).

## NOTES

<sup>1</sup> Thanks to Dr. Adrienne Bank for her helpful comments on an earlier draft of this paper.

<sup>2</sup> All page references, unless otherwise noted, are to the Study. This article is based on the Study as published, since additional data and information were unavailable.

<sup>3</sup> Other items have their own difficulties. The Israel subtest, for example, contains the following item: "The[!] leader of the Zionist movement during the 1900's was: (1) Golda

Meir (2) Theodore Herzl (3) Menachem Begin (4) Albert Einstein". Can Theodore Herzl be the right answer, since he died in 1904? The item "Israel's Independence Day is called: (1) Yom Yerushalayim (2) Yom Kippur (3) Yom ha-Atzmaut (4) Yom ha-Shoah" is more a Hebrew language, than a content item. Finally, the "Conversational Hebrew" subtest is misnamed; it is primarily a vocabulary test, largely unrelated to language comprehension or functioning. (Thanks to Professor Ilana Shohamy for this last point.)

<sup>4</sup> Here, too, the item itself is problematic. Why the sins of the parents should be visited on an inventory of the child's involvement, is utterly unclear. Moreover, how were the children to respond if their parents differ in their synagogue-going behavior? Other items are also fraught with difficulties.

<sup>5</sup> The Study is aware of the absence of comparison groups. The conclusions section lists additional "information [which] would be helpful in guiding the improvement of Jewish supplementary schooling", including "how Jewish day school pupils and children without formal Jewish educational experience compare to pupils in our sample" (p. 114). While such comparisons might be helpful in guiding changes, they are fundamental to interpreting these data.

<sup>6</sup> In this regard, the Study also falls prey to the correlational/causal fallacy. Without even presenting correlational evidence that parental attitude and student achievement are related, the Study leads us to believe that enhancing the former will facilitate the latter. Yet, proving such an assertion would require results from some kind of intervention study, showing that a specific program which raised parental involvement actually resulted in improved student performance. Nor does the Study disprove the contrary assertion, that some educational programs can succeed without a high level of parental involvement.

<sup>7</sup> Failure to report the Inventory results for the effective schools is particularly strange, since what started the effective schools movement in public education was the attempt to explain the high achievement test scores of schools which, on objective criteria (inner city location, low family incomes, etc.), should have been low scorers.

<sup>8</sup> The absence of a full-time research team and the publication of the results without benefit of peer review resulted in errors of interpretation. For example, in discussing the reliability of the Knowledge Inventory, the Study states that "Since the Inventory of Jewish Knowledge consists of ten parts, each measuring a different aspect of Jewish knowledge, it is a heterogeneous test. Therefore the obtained reliability of .913 is all the more remarkable" (p. 97). The high reliability score probably points to the opposite conclusion: had the subtests actually been assessing different domains, the reliability score should have been low. What the high score indicates is that the Knowledge Inventory probably tapped a single, global Jewish knowledge base, rather than discrete areas of knowledge. (Thanks to Professor Ephraim Darom for drawing my attention to this point.)

<sup>9</sup> The Study asserts that there were no significant differences among schools, despite the differences in time allocation to Hebrew instruction (p. 106). This point would be more convincing had the actual data been presented. As things stand now, the absence of a difference among schools may as likely be due to the irrelevance of the Inventory, as the presumed ineffectiveness of the schools.

<sup>10</sup> One of the Study's noteworthy, though incidental, findings was that the student absentee rate during the seven-week testing period was a whopping 30%.

<sup>11</sup> Two key criteria of informal education, in the supplementary school context, are its voluntary nature and its not being subject to governmental supervision. While the Study recommends that supplementary schools switch to informal education, I maintain that the schools are already there, they just do not know it. Readjusting to their "true" paradigm is likely to engender all the trauma of major cultural change, but that is a discussion for another time and place. Suffice it to say that, in this regard, the Study's recommending informal education is either belated or the right conclusion for unsubstantiated reasons.

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