# Benefits of Compensatory Preschool Education

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ABSTRACT

*Although there is widespread agreement that compensatory preschool education can produce short-term gains in test scores, its ability to produce meaningful long-term improvements in educational and economic success has been questioned. This paper reviews the evidence regarding long-term effects, including a classical experiment and benefit-cost analysis. It is concluded that compensatory preschool education can produce long-term gains in school success through contributions to cognitive abilities not adequately measured by Intelligence (IQ) tests. Greater educational success is accompanied by substantial improvements in social and economic outcomes including employment, teen pregnancy, and welfare assistance.*

## I. Introduction

Research on compensatory preschool education programs such as Head Start, as it has appeared in the economics literature, might be summarized by Arthur Jensen's statement: "Compensatory education

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has been tried, and it apparently has failed" (cited by Becker 1981).[1] The present paper introduces to the economics literature a broader range of evidence regarding compensatory preschool education, including a comprehensive cost-benefit analysis based upon the results of a classical experiment. As a whole, the evidence indicates that pessimism about the potential for preschool education to produce long-term gains in human capital accumulation for children from low income families is unwarranted. On the contrary, compensatory preschool education seems to be an economically efficient public investment.

## II. Background

In the early 1960s, researchers investigating the malleability of human development created a variety of preschool education programs that sought to improve the intellectual development of young children living in poverty by improving the educational environments of these children before they entered regular school. The poor test scores and school performance of children from low income families were well known, and the originators of these programs hoped to produce cognitive gains that would contribute to later educational success. It was hoped that increased educational success would lead to better jobs and higher incomes. This type of research on specially created preschool programs has continued, but with the creation of Head Start in 1965 and the development of state and local education agency compensatory preschool programs researchers began to study the effects of large scale public programs as well. All of these studies can be viewed as investigations into the efficiency of extra-familial investment in the human capital of children who would otherwise be expected to accumulate relatively low stocks of human capital because of their family's low human capital.[2] In conceptu-

1. For example, Ritzen and Winkler (1977) reported that "while evaluations of early childhood intervention programs typically find large short-run gains in learning, the long-run gains are statistically insignificant. Wolfe (1982) concluded that "early childhood training does not seem to be a very promising area for the achievement of economic equality," although the next sentence added the qualification "unless training can be shown to have important direct effects on schooling or earnings . . ." (p. 233). Becker (1981, 1985) and Becker and Tomes (1986) have even developed a plausible explanation for the failure of compensatory preschool education within the framework of a theory of the family. The issue of the effectiveness of compensatory preschool education has arisen in a recent debate between Goldberger (1989) and Becker (1989).

2. This paper does not enter the nature-nurture debate. Even the largest estimates of the influence of genetic inheritance on cognitive abilities allow for environmental effects of the size claimed for compensatory preschool education (Goldberger 1979, Scarr and Kidd 1983).

alizing the problem this presents, it may be helpful to view such families as analogous to firms with little capital trying to spin off capital-intensive firms. This may be achieved only by an infusion of capital from outside the parent firm.

## III. Review of Empirical Evidence

Although the effects of schooling on human capital accumulation and the effects of home resources on child development have been of great interest to economists, relatively little research on compensatory preschool education has been reported in the economic literature.[3] Perhaps the best known study is the Westinghouse Learning Corporation (Cicirelli et al. 1969) evaluation of Head Start which is widely cited and has been reanalyzed in the economics literature (Barnow and Cain 1977). To my knowledge, only Wolfe (1982) has published primary research on preschool education in the economics literature.[4] A much larger body of research is available, however.

More than 100 studies provide some information about the effects of compensatory preschool education on disadvantaged children. Most estimate the program's immediate effect on IQ. These data have been efficiently, if crudely, summarized by meta-analyses that average effect sizes (in standard deviation units) across studies. Casto and Mastropieri (1986) have done this for the literature as a whole and McKey and her colleagues (1985) have done this for Head Start studies. Both find an average effect on IQ of about 0.5 standard deviations (eight IQ points). The meta-analyses also find that the effects of IQ fadeout over time in nearly every study. The failure to find permanent effects on IQ is the source of claims that preschool education has failed to help disadvantaged children.

A much smaller number of studies have investigated effects on variables other than IQ and provide evidence that preschool programs can produce persistent effects on academic success despite the erosion of IQ effects. The most commonly researched of these effects have been improvements in: (1) achievement test scores, (2) school progress as indi-

3. Hanushek (1986) reviewed studies of the effects of school. Examples of economic research on the effects of home resources on child development include: Behrman and Wolfe (1987); Datcher-Loury (1989); Kenny (1980); Leibowitz (1974, 1977); Lazear (1980); Murnane, Maynard, and Ohls (1981); Stafford (1987); and Wolfe (1982). The benefit-cost analysis of the Perry Preschool Study (Barnett 1985a) has been cited by Levin (1989) and used by Gramlich (1986) to illustrate the potential of benefit-cost analyses of educational programs.
4. Ritzen and Winkler (1977) investigated the relative efficiency of early and later educational inputs, but their study began at first grade and did not include data on preschool education.

cated by special education placement and grade retention, and (3) educational attainment. The limited number of studies makes it possible to critically assess and compare the information from each study through a detailed traditional review. To ensure a comprehensive review a computer-assisted search of literature indexes and the meta-analysis data bases was conducted and supplemented by searches of the bibliographies of studies and reviews. All studies of the effects of center-based preschool education programs for children of low-income families with data through at least third grade were selected for review; 22 such studies were found. The requirement for third grade data allowed sufficient time for effects to show erosion. Little was lost by this restriction as these 22 studies found the same pattern of early effects as the larger body of short-term studies.

The studies identified for review fall into two categories. One consists of eight studies in which researchers created and implemented compensatory preschool programs specifically for research and used experimental or quasi-experimental designs to estimate program effects. These studies were all initiated in the 1960s, some of them before Head Start was introduced. In the 1970s the researchers conducting seven of these studies joined a consortium for common data collection and analysis. The publications of the Consortium's work were a major source of information for this review (Lazar et al. 1982; Consortium 1983), although findings reported here were updated from individual study reports published after the consortium work.[5] The other category consists of 14 studies that used natural variation in public program participation to estimate the effects of compensatory preschool. These studies cover the late 1960s, 1970s, and 1980s. The researcher initiated programs were small scale, well-funded, carefully scrutinized programs run by experts and focused on cognitive and social development. The large scale public programs were multi-purpose and varied greatly from site to site. Head Start, in particular, had broad goals that included improving health and nutrition and providing employment to parents. Less well funded, the public programs had fewer staff per child, and less highly qualified staff.

### *A. Researcher Initiated Programs*

The eight studies based on researcher initiated programs are identified and briefly described in Tables 1 and 2. Generally, the preschool education programs in these studies should be regarded as of higher quality than typical public programs. Among the reasons for this are the close

5. Partial summaries of the literature on long-term effects also can be found in Berrueta-Clement et al. (1984) and Brown (1978).

supervision and direction of experts and relatively low child-staff ratios and group size. Most of the experimental children entered programs at ages three or four. Most of the comparison children began school at kindergarten (some of the Harlem Training Project children attended preschool programs). Three of the studies used designs that randomly assigned children to experimental and control groups. The other studies obtained comparison subjects from the same communities in a variety of other ways. In two cases there is evidence of significant initial differences among groups. In the two curriculum studies, only children who had not attended another preschool program were chosen for comparison and this eliminated significant numbers of children from the potential comparison pools. In the Harlem Training Project, attrition during the waiting period before entry at age three may have introduced differences favoring that later entry group as it has a higher IQ prior to treatment. Initial sample sizes are modest, and in some cases the comparison groups are extremely small. Table 1 figures for long term follow-up sample size are maximums and sample sizes are even smaller for many analyses.

All of the studies of researcher-initiated programs find initial IQ gains for the experimental group by the end of the program followed by declines in this advantage over time. All of the IQ tests were individually administered by qualified testers who uniformly acclimated and assessed all children. The initial gain of experimentals over the comparisons ranges from five to 13 IQ points, with the largest gains for the programs providing two years. The longest lasting IQ gain is reported by the Philadelphia Project at grade five. No study reports differences in IQ that persist beyond grade school (unfortunately, there are no later measures in the Philadelphia study). The Harlem Training Project reports an experimental group advantage on one of two subscales for IQ at age 12, but this can be explained by an initial IQ advantage of those experimental children who entered the program in the second year.

At first glance, the achievement test results are hardly more encouraging than the IQ data. Only the Perry Preschool Project reports advantages for the experimental group at grade three or beyond. In the Perry Project, the preschool group surpasses the control group on achievement tests in elementary school, and at the two later testing points, ages 15 and 19. Of four other studies with achievement test data at grade three or beyond, however, only the Early Training Project individually administered its own tests. In contrast to their procedures for collection of IQ data, the others relied on school administered achievement tests, which works against the detection of significant effects in three ways. First, school administered tests are likely to have more errors in administration and scoring and to obtain less than a student's best performance. Second, children attending different schools, may be given different tests which

**Table 1**
*Descriptions of Studies of Researcher Initiated Programs*

| Name and Location | Program Description | Age at Entry (in years) | Program Length (in years) | Years of Program | Design | Initial *N*'s Exp, Comp | Followup *N*'s Exp, Comp |
|---|---|---|---|---|---|---|---|
| 1. Perry Preschool Project, Ypsilanti, Michigan | Two and a half hour preschool class, five days/week, and weekly home visits, Fall to Summer | 3 or 4 | 1 or 2 | 1962–67, five waves | Randomized | 58, 65 | 58, 65 |
| 2. Early Training Project, Tennessee | Four hour preschool class, five days per week, 10 weeks of Summer, and weekly home visits, Fall to Summer | 3.8 or 4.8 | 14 or 26 months | 1962–65 | Randomized | 44, 21 | 41, 21 |
| 3. Institute for Developmental Studies (IDS), Harlem | Preschool class, Fall to Summer with home visits and IDS classes up through third grade | 4 | 1 (preschool) 4 (elementary) | 1963–67 | Randomized | 312,191 | 63, 34 |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| 4. Philadelphia Project, Philadelphia | Four hour preschool class, four days/week, and weekly home visits, Fall to Summer | 4 | 1 | 1963–64 | Matched comparison group from same kindergarten classes | 60, 53 | 44, 37 |
| 5. Harlem Training Project, Harlem | One-to-one tutoring or child-directed play, two hours/week, Fall to Summer | 2 or 3 | 1 | 1966–67 | Comparison group recruited from children born 1–2 months later | 244, 68 | 168, 51 |
| 6. Curriculum Comparison Study, Urbana, Illinois | Two plus hours preschool class, five days/week, Fall to Summer, some continued in similar kindergarten classes | 4 | 1 (preschool), some 1 (kindergarten) | 1965–67, two waves | Post hoc comparison group from original pool | 116, 24 | 102, 19 |
| 7. Experimental Variation of Head Start, Louisville | Six and a half hour preschool class, five days/week, Fall to Summer | 4 | 1 | 1968–69 | Post hoc comparison group from same communities | 214, 34 | 134, 22 |
| 8. Howard University Project, Washington, D.C. | Seven hour preschool, five days/week, 10 months per year | 3 | 2 | 1964–66 | Comparison group from neighboring tracts | 38, 69 | 30, 69 |

Note: 1 year of a preschool program refers to an 8 to 9 month school year program.
Primary Sources: 1. Berrueta-Clement, et al. (1984) 2. Gray, Ramsey and Klaus (1982, 1983) 3. Deutsch, Deutsch, Jordan, and Grallo (1983) 4. Beller (1983) 5. Palmer (1983) 6. Karnes, Swedel, and Williams (1983) 7. Miller and Bizzell (1983, 1984) 8. Herzog, Newcomb, and Cisin (1974)

**Table 2**
*Sample Characteristics for Studies of Researcher Initiated Programs*

| Project Name | Mean Pretest IQ | Mean Mother's Education (years) | Percent Black | Percent Female |
|---|---|---|---|---|
| Perry Preschool | 79 | 9.4 | 100 | 42 |
| Early Training | 88 | 9.2 | 100 | 56 |
| IDS | 92 | 10.3 | 100 | 51 |
| Philadelphia | 91 | 10.5 | 90 | 50 |
| Harlem Training | 92 | 11.2 | 100 | 0 |
| Curriculum comparison | 92 | 10.1 | 65 | 51 |
| Experimental variation | | 10.7 | 92 | 51 |
| Howard University | | | 100 | |

Sources: Consortium (1982), Herzog et al. (1974).

cannot be equated. Third, schools test by grade level so that children who have been retained in grade are not tested with their birth cohort, and in many cases children in special education are not tested at all. If grade retention and special education placement differ between groups, the exclusion of these low achievers would bias the estimated effect of preschool on achievement. Only the Early Training Project could provide comparable estimates to the Perry Project, but its small sample size and attrition reduce the number of subjects on long term achievement to 33 experimentals and 15 controls so that finding no significant effect is highly uninformative.

Findings for grade retention, special education placement, and high school graduation are presented in Table 3. These variables are indicators of cumulative school success. The four studies that collected data through high school graduation all find significant effects on at least one of the three variables, and three have remarkably similar estimates for graduation. The studies with shorter follow-up can be used to look at retention and special education, recognizing that they provide only partial data, particularly the Howard University study (in the other studies significant effects were not evident until fifth or sixth grades). Three studies find significant effects on retention, while one has such minimal rates for both groups that it would be difficult to affect retention. Two find significant effects on special education, while two have minimal rates for both groups. The other study with data on special education, Perry, finds a significant effect on number of years of special education as opposed to

**Table 3**
*Researcher Initiated Program Effects on School Success: Experimental versus Comparison Groups*

| Project Name | Percent Retained | Percent in Special Education | Percent High School Graduates | Time of Follow-up |
|---|---|---|---|---|
| Perry Preschool | 15 v. 20 (86) | 37 v. 50 (112) | 67 v. 49* (58, 63) | Post high school |
| Early Training | 58 v. 61 (58) | 5 v. 29* (62) | 68 v. 52 (41, 21) | Post high school |
| IDS | 23 v. 43 (29, 8) | 0 v. 13 (29, 8) | | Seventh grade |
| Philadelphia | 38 v. 53* (78) | 5 v. 6 (80) | 65 v. 62 (43, 37) | Post high school |
| Harlem Training | 30 v. 52* (161, 51) | | | Seventh grade |
| Curriculum comparison | 26 v. 58+ (88) | 32 v. 63* (115) | 67 v. 53 (92, 17) | Post high school |
| Experimental variation | 10 v. 16 (132) | 13 v. 15 (120, 20) | | Seventh grade |
| Howard University | 33 v. 47 (30, 69) | | | Fourth grade |

Note: *N*s in parenthesis. *N*s are smaller for grade retention analyses because children are excluded from retention count after placement in special education.
*indicates significant difference at .10 level (2-tailed test) in both chi-square and regression analysis.
+indicates significant only in chi-square analysis.
Sources: Consortium (1982, 1983), Berrueta-Clement et al. (1984), Herzog et al. (1974).

whether a child had any at all. These findings are much stronger than might have been supposed from the data on IQ and achievement.

### *B. Large Scale Public Programs*

Studies estimating effects by comparing children who attended ordinary public compensatory preschool programs to children who did not are described in Table 4. The first three deal with state and locally funded programs, the others with Head Start programs, although the latter were often part of the public schools. These studies examine the effects of routine programs that can be considered typical of public programs operating at the time. Generally, sample sizes are much larger than in the previous category of studies. In addition to these advantages, there are some disadvantages. Most report little about the programs children attended. Few reports have been peer-reviewed and they often omit important information about procedures or findings. The comparison groups tend to be problematic: some are generated by self-selection; others are drawn from different populations (e.g., different school districts) than the experimental groups. Statistical analyses tend to be limited to simple comparisons of the groups, sometimes using analysis of covariance to adjust for initial differences. More recently developed techniques that attempt to correct for selection bias were not employed. In several studies, preschool is confounded with later program variations. For example, in the Cincinnati study most full-day kindergarten students had attended preschool and most half-day kindergarten students had not. In some cases the preschool program was kindergarten, which was introduced to public schools in the southeast relatively recently.

For IQ and achievement test scores, the findings of the public program studies are similar to those of the studies of researcher-initiated programs. The three studies with IQ data find no persistent effects. The first 10 studies in Table 4 have achievement data for several years over time. Two find no effects at all during the school years. Six of the others find initial effects that disappear during the elementary grades. One study finds significant effects at third and fourth grade, but has no later data. The other finds significant effects at third, fifth, and eighth grade (the last measurement). As with the studies in Table 1, design problems could explain the failure to find persistent effects on achievement. Most relied on school administered tests, and the studies where effects fade all suffer from heavy attrition. None of the studies collected test data on children who were retained in grade, and special education students were frequently lost.

The most common reason for the loss of retained and special education students is the reliance on routine school tests administered by grade

**Table 4**
*Descriptions of Studies of Large Scale Public Programs*

| Study Name and Location | Age at Entry (in years) | Program Length (in Years | Years of Program | Design | Initial *N*'s (Exp, Con) | Followup N's |
|---|---|---|---|---|---|---|
| 1. New York State Experimental Prekindergarten | mostly 4, some 3 | mostly 1 yr some 2 yrs | 1975–76 | Compared attenders with children in same district on waiting list and in other districts with no prekindergarten | 1,800[a] | 1,348; 258 |
| 2. Maryland Extended Elementary Pre-k | 4 | 1 | 1977–80 | Compared attenders to nonattenders, only children born in 1975 and continuously in school district kindergarten to grade 5 with no preschool prior to 4. | Unknown | 356; 306 |
| 3. Cincinnati Title I Preschool | 4 and 5 | 1 prekindergarten and full-day kindergarten) | 1969–70 1970–71 | Compared full-day kindergarten attenders who mostly had preschool to half-day kindergarten attenders who mostly had no preschool | 410, 141 | 205; 70[b] |
| 4. Philadelphia School District Head Start | 4 | 1 (prekindergarten) | 1969–70 1970–71 | Compared children in Follow-Through kindergarten who had and had not had preschool (FT went to 3rd grade) | 1,082; 1,615 | 688; 524 |

**Table 4** (*continued*)

| Study Name and Location | Age at Entry (in years) | Program Length (in Years | Years of Program | Design | Initial *N*'s (Exp, Con) | Followup N's |
|---|---|---|---|---|---|---|
| 5. Westinghouse National Evaluation of Head Start | 4 or 5 | 1 | 1965–66 | Compared children in grades 1, 2, 3 who had attended Head Start to those who had not | | |
| 6. ETS Longitudinal Study of Head Start (Portland, St. Louis, Trenton) | 4 or 5 | 1 | 1969–70<br>1970–71 | Compared children[c] who went to Head Start to children who went to other preschools or with no preschool | 1,875[a] | 852[a] |
| 7. New Haven Head Start | 4 y | 1 | 1968–69 | Compared children who had Head Start with those who did not | 61, 48 | 35, 26 |
| 8. Rome, Georgia Head Start | 5 y | 1 | 1966 | Compared school records for all children in first grade in disadvantaged schools in 1966 | 130, 88 | 94, 60 |
| 9. Montgomery County, Maryland Head Start | 4 | 1 | 1970–71<br>1974–75<br>1978–79 | Compared tests and records for children who had attended eight or nine months to those for children who attended one month or less (three waves) | 1915, 619[d] | 186, 112 |
| 10. Hartford Head Start | 4 | 1 | 1965–66 | Compared children who had Head Start with those who did not | 293[a] | 148, 50 |

| | | | | | | |
|---|---|---|---|---|---|---|
| 11. Detroit Head Start | 4 | 1 | 1969–70 | Compared children who had Head Start with children in Title I elementary programs who had not attended | unknown | unknown |
| 12. Detroit Head Start and Title I Preschool | 4 | 1 | 1972–73 | Compared children who had attended Head Start or Title I preschool with children who were eligible but did not attend | unknown | unknown |
| 13. Kanawha County, West Virginia Head Start | 4 | 1 | 1973–74 | Compared children who had attended Head Start with low income children who had not | unknown | unknown |
| 14. Cincinnati Head Start | 4 | 1 | 1968–69 | Compared third graders who attended Head Start with those who had not | unknown | unknown |

Note: One year of a preschool program refers to eight- or nine-month school year program.
a. Size of experimental and comparison groups separately unknown.
b. Followup Ns estimated from report statement that attribution across the entire sample was 50 percent.
c. ETS study was designed to study child development, not to estimate program effects, but data have been used in this way.
d. Potential comparison group children who *never* entered the school district are not included in the initial *N*.
Primary Sources: 1. State Education Department (1982), Irvine et al. (1982) 2. Eckroade, Salehi, and Carter (1988) 3. Nieman and Gastright (1981) 4. Copple, Cline, and Smith (1987) 5. Cicirelli et al. (1969) 6. Educational Testing Service (1976) 7. Abelson (1974); Abelson, Zigler, and DeBlasi (1974) 8. McDonald and Monroe (1981) 9. Hebberler (1985) 10. Goodstein, Owens, and Cawley (1975) 11. O'Piela (1976) 12. Clark (1979) 13. Kanawha County Board of Education (1978) 14. Pinkleton (1976)

level for achievement data. However, even researchers administering tests did not test children who had been retained in grade or, in some cases, placed in special classes. While space does not permit a description of each study's procedures, four examples illustrate the problems. The New Haven Head Start study individually administered tests, but only to children at expected grade level. The ETS Head Start study tested children only in classes where at least 50 percent of the children were study participants. The WLC Head Start study matched former Head Start children in grades 1, 2, and 3 with other children in their grade levels. This created an additional problem as the comparison groups include children retained from older cohorts and the comparison group is significantly older than the Head Start group in second and third grades. The Montgomery County study compared scores on tests routinely administered at each grade level.

Eight studies examined grade retention, five examined special education placement, and one looked at high school graduation. These studies could find children retained or placed in special education, even though they did not have test scores. The findings are presented in Table 5. Despite short follow-up periods in some studies, only the Cincinnati Head Start study did not find a significant effect. This is the one study with persistent achievement effects. As might be expected with cumulative data, the frequencies of retention and special education placement, and the differences between groups, tend to be lower for the shorter follow-ups (compare Table 3). However, there is substantial variation not explained by length of follow-up.

### *C. Review Summary*

The studies reviewed suggest that compensatory preschool education has an important effect on long term school success, especially as indicated by school attainment, retention, and special education placement, but do not find that there are persistent effects on IQ or, usually, achievement. Unfortunately, most of the studies suffer from serious design limitations and sharply decreasing power over time due to attrition. The IQ test results seem likely to be dependable. The achievement test results are more problematic and may be seriously biased. The potential for deriving insights into how preschool may produce long term effects is limited by the lack of year-to-year data on many of the variables—affective as well as cognitive—that might influence school success. However, one study provides a stronger basis for estimating effects on achievement and investigating the ways in which preschool may produce its effects—the Perry Preschool Project.

**Table 5**
*Public Program Effects on School Success: Experimental Versus Comparison Groups*

| Study Name | Percent Retained | Percent in Special Education | Percent High School Graduates | Time of Followup |
|---|---|---|---|---|
| 1. New York | 16 v. 21* | 2 v. 5* | | Third grade |
| 2. Maryland | 31 v. 45* | 15 v. 22* | | Fourth grade |
| 3. Cincinnati | 9 v. 12 | 5 v. 11 | | Eighth grade |
| 4. Philadelphia | NA* | | | Sixth grade |
| 7. New Haven | 18 v. 35* | | | |
| 8. Rome, Georgia | 51 v. 63 | 11 v. 25* | 50 v. 33* | Post high school |
| 10. Hartford | 10 v. 22* | 5 v. 10 | | Sixth grade |

*Significant difference between groups, $p < .10$. Note: The Philadelphia report does not present exact percentages, but displays results in a graph indicating a significant difference. Sources: 1. State Education Department (1982); Irvine, et al. (1982) 2. Eckroade, Salehi, and Carter (1988) 3. Nieman and Gastright (1981) 4. Copple, Cline, and Smith (1987) 7. Abelson (1974); Abelson, Zigler, and DeBlasi (1974) 8. McDonald and Monroe (1981) 10. Goodstein, Owens, and Cawley (1975)

## IV. A Comprehensive View: Perry Preschool Project

The Perry Preschool Project began in 1962 in Ypsilanti, Michigan.[6] The goal of the project was to learn if a preschool education program could improve the school success of children in a low-income black community (the Perry elementary school cachement area) who historically did not fare well in the school system. The project was federally funded and conducted by the Ypsilanti Public Schools under the direction of David Weikart who later continued the follow-up study through the High/Scope Educational Research Foundation. The project was designed as a classical experiment in which children were to be randomly assigned to an experimental preschool group or a control group. This was done in five waves due to limitations on the number of children who could be served in the preschool each year. The first year one wave of 4-year-olds

6. The preschool program, research design, procedures, and findings are described in a project report (Weikart 1967) and series of monographs (Weikart, Bond, and McNeil 1978; Schweinhart and Weikart 1980; Berrueta-Clement, Schweinhart, Barnett, Epstein, and Weikart 1984).

**Table 6**
*Perry Preschool Study Sample*

| Wave | Year | Age (in years) | Treatment Group | Control Group | Total |
|---|---|---|---|---|---|
| One | 1962 | 4 | 13 | 15 | 28 |
| Two | 1962 | 3 | 8 | 9 | 17 |
| Three | 1963 | 3 | 12 | 14 | 26 |
| Four | 1964 | 3 | 13 | 14 | 27 |
| Five | 1965 | 3 | 12 | 13 | 25 |
| Total | — | — | 58 | 65 | 123 |

and one wave of 3-year-olds were assigned in order to have a mixture of ages. In the next three years, only 3-year-olds entered the study. The wave of 4-year-olds attended the program for one school year. The 3-year-olds attended the program for two school years. A total of 128 children entered the project and 123 completed the preschool years. Five children were lost due to death and moving away. The number of children in each wave is reported in Table 6.

### *A. Sample Selection and Assignment to Groups*

Participants were recruited by contacting parents of children attending the Perry elementary school and seeking information from parents with preschoolers about other families with preschoolers. Children of the appropriate age and living in families with a low rating on a socioeconomic status scale (based on occupation, education, and housing crowdedness) were administered an IQ test if their parents agreed to participate. Undoubtedly some self-selection into the study occurred at this point, but it should be recognized that the most common objection to the project at the time was that it was harmful to take preschoolers away from home. Children scoring one standard deviation or more below the mean and showing no signs of organic causation for mental retardation or physical handicap were eligible for the study. Each wave of children was ranked on IQ and matched pairs were formed by adjacent children. One member of each pair was randomly assigned to form two groups. As each wave was too small to rely on chance to produce comparable groups, pair members were switched to equate the groups on sex and socioeconomic status. One group was then randomly designated the experimental group.

Two exceptions to the randomization procedure occurred. First, sib-

lings of children already participating in the study who later entered the sample were assigned to the same group as their older brother or sister to prevent treatment diffusion. Siblings were included in the study even if their IQs were one or two points above the cutoff. Second, a few working mothers could not participate in the afternoon home visits and their children were shifted to the control group. The exact number of children shifted for this reason is unknown, but program records confirmed that two children were switched. The characteristics of the two groups at entry are compared in Table 7. The groups differed on mother's employment alone. The next time mother's employment was assessed (age 15), there was no significant difference between the groups on this, or any other, family background variable. Analyses conducted to investigate the effects of these exceptions (estimating effects by family, with mother's employment as an independent variable, with suspect cases deleted) indicate that the findings are unaffected.

### *B. Treatment Description*

The experimental group attended a two-and-a-half-hour a day program five days per week from October to May. There were four teachers for the two waves served each year. A teacher made a home visit to each family once a week in the afternoon to provide about one and a half hours of home education for the child. During the visit efforts were made to involve the mothers and to discuss the techniques used by the teacher.

Although well-funded, the program was not insulated from the problems that confront ordinary public preschool programs. Attendance at the center and participation in home visits varied. For example, average morning attendance was 69 percent the first year. Mothers sometimes viewed the home visits as free baby sitting and left. The program began in the auditorium of a community center, and the second year moved to an old gymnasium at the Perry school. Some advantages are that teachers were experienced public school teachers certified in special education and preschool education. There was extensive direction, planning, discussion, inservice training, and other program development activity. However, the curriculum was constantly evolving, there was conflict among the staff over curriculum, and there was substantial turnover. By fall of 1965 only one of the four teachers from 1962–63 remained, and some positions had turned over more than once.

It is improbable that control children attended a program comparable to the Perry Preschool. The first three waves preceded Head Start. While control children in the last two waves could have attended Head Start, it was primarily a summer program at that time and experimental group children would have been eligible as well. Thus, Head Start could have

**Table 7**
*Characteristics of Treatment and Control Groups at Study Entry (standard deviations in parentheses for means)*[a]

| Characteristic | Treatment Group ($N$ = 58) | Control Group ($N$ = 65) |
|---|---|---|
| Age in months[b] | 42.7 (6.2) | 41.9 (5.9) |
| IQ (Stanford-Binet) | 79.6 (5.9) | 78.5 (6.9) |
| Number of children in family | 4.9 (2.4) | 4.9 (2.7) |
| Mother's years of schooling | 9.5 (2.4) | 9.4 (2.0) |
| Father's years of schooling | 8.4 (2.3) | 8.8 (2.5) |
| Percent female | 43% | 40% |
| Percent two-parent families | 55% | 51% |
| Percent receiving welfare | 57% | 45% |
| Percent with working mother | 13% | 33%* |
| Percent with working father | 48% | 45% |

*Significant difference between groups, $p < .10$.
a. Source: Weikart, Bond, and McNeil 1978.
b. Mean age was 4.3 years for Wave 0 and 3.3 years for Waves 1–4.

diluted differences between the groups, but seems unlikely to have had much impact. The absence of significant differences in program effects by wave offers empirical support for this view. No doubt, some of the control group were in nonparental care during the day, as were some of the experimental group. Although data were not collected on this variable, most nonparental care at the time consisted of informal arrangements.

### *C. Assessment and Follow-up Procedures*

Testing and other research contacts with experimental and control subjects followed the same schedules from project entry on. During the preschool years, a large number of tests of cognitive abilities were administered by trained examiners who were blind to group assignment and sensitive to issues of creating a comfortable situation for the child and obtaining an accurate measure of ability. By the time children entered school they had been tested many times. At the end of preschool there was no further contact between teaching staff and program participants, and subsequent contact with the project was the same for both groups.

Project-administered individual intelligence and achievement testing continued through the elementary school years, and were given again at age 15. A test of basic adult competencies was administered at age 19. Teacher ratings of children in the early grades were performed on entire classes so that teachers were not only blind to group membership, but to study participation. Data on children's homes and families were collected at project entry, the end of the preschool period, age 15, and age 19. Ages 15 and 19 were points of major data collection that included participant interviews (children and parents at age 15), school records, and other government agency records. The collection and coding of records and interviews were conducted by staff who were uninformed about group membership and were not otherwise involved in the research.

### *D. Findings*

The depth, breadth, and continuity of measurement from age three to age 19 preclude a complete report of findings here. Table 8 presents findings over the years that are indicative of the major effects of the preschool program. Omitted are essentially redundant measures, variables for which no significant effects of preschool were found, and variables of limited interest unrelated to issues discussed in this paper. Attrition in the study was relatively minor. At age 19, all 123 subjects who completed the preschool period were contacted, and 121 were interviewed. Complete school records were obtained for 112 subjects. Although many had remained close to home, subjects were found throughout the United States and abroad. Across all variables attrition averaged about 5 percent. Some indication of variation in attrition is given by the $N$'s in Table 8. Attrition analyses indicated no differences between groups on missing data.

As can be seen, the Perry Preschool program's estimated effects on IQ are entirely consistent with those of other studies. There was an immediate effect on IQ of 12 points (three-fourths of a standard deviation.)[7] This advantage for the preschool group was reduced by half in kindergarten. It appears that the preschool group's IQ scores fell at the same time that the control group's IQ scores were boosted by kindergarten experiences. As time passed, the preschool group's IQ continued to fall, and by age nine, there was no IQ difference between treatment and control groups. No differences were found on any later IQ tests.

The Perry Study found a different pattern for achievement test scores.

7. The initial rise in IQ of the control group may be due to regression to the mean, as children were selected for the study based on low IQ scores.

**Table 8**
*Selected Findings of the Perry Preschool Study*[a]

| Outcome | Treatment Group (*N*) | Control Group (*N*) | $p$[b] |
|---|---|---|---|
| Intelligence Test Scores | | | |
| At study entry | 79.6 (58) | 78.5 (65) | — |
| After 1 year | 95.5 (58) | 83.3 (65) | .001 |
| Age 6 | 91.3 (56) | 86.3 (64) | .024 |
| Age 7 | 91.7 (58) | 87.1 (61) | .040 |
| Age 8 | 88.1 (55) | 86.9 (62) | — |
| Age 9 | 87.7 (56) | 86.8 (61) | — |
| Age 10 | 85.0 (57) | 84.6 (57) | — |
| Age 14 | 81.0 (54) | 80.7 (56) | — |
| Achievement Test Scores | | | |
| Age 7 | 97.1 (53) | 84.4 (60) | .216 |
| Age 8 | 142.6 (49) | 126.5 (56) | .079 |
| Age 9 | 172.8 (54) | 145.5 (55) | .042 |
| Age 10 | 225.5 (49) | 199.3 (46) | .040 |
| Age 14 | 122.2 (49) | 94.5 (46) | .003 |
| Age 19 | 24.6 (52) | 21.8 (57) | .059 |
| School Success (to age 19) | | | |
| Years spent in special education | 16% (54) | 28% (58) | .004 |
| Classified mentally retarded | 15% (54) | 35% (58) | <.05 |
| Graduated from high school | 67% (58) | 49% (63) | <.05 |
| Received post-secondary education | 38% (58) | 21% (63) | <.05 |
| Economic Success (at age 19) | | | |
| Employed | 50% (58) | 32% (63) | <.05 |
| Median earnings[c] | $3,860 (58) | $1,490 (63) | .061 |
| Self-supporting | 45% (58) | 25% (62) | <.05 |
| Receives welfare | 18% (58) | 32% (63) | <.05 |
| Social Adjustment (to age 19) | | | |
| Arrested | 31% (58) | 51% (63) | .021 |
| Average number of arrests | 1.3 (58) | 2.3 (63) | .001 |
| Average number of teen pregnancies | .7 (25) | 1.2 (24) | .076 |

a. Sources: Statistical appendices in Schweinhart and Weikart (1980) and Berreuta-Clement et al. (1984)
b. Statistical analyses for IQ test scores, achievement test scores, and years in special education were analyses of covariance with gender, family background variables (including mother's employment), and initial IQ as covariates. Comparable probit analyses were performed for dichotomous variables. Differences in number of arrests and pregnancies per group were tested for significance by chi-square. The median test was applied to median earnings. A number of alternative models and statistical techniques (parametric and nonparametric) were used to analyze the data in order to examine the sensitivity to various assumptions. The results are reported in the appendices cited above and indicate that the findings are quite robust with respect to statistical approach.
c. Expressed in 1988 dollars. The earnings difference appears to be primarily the result of differences in employment rather than wages.

At the same time that the preschool group lost its IQ advantage, it appears to have gained an advantage on achievement tests. By age 14, this advantage was quite strong—the preschool group was ahead by 1.2 grade equivalents on language and math.[8] At age 19, the preschool group scored significantly higher on the Adult Performance Level, an achievement test designed to measure the skills and knowledge needed for educational and occupational success in five content areas (community resources, occupational knowlege, consumer economics, health, and government and law) and five skill areas (knowledge of facts and terms, reading, writing, computation, and problem solving).

The finding of persistent advantages on standardized tests of achievement in the Perry Project is especially important because it is the only study with reasonable sample size (the Early Training Project had test data on only 15 controls) and individually administered tests for all children by age, whether or not the children were retained in grade or in special education. This, combined with the fact that the only other study with persistent achievement test effects had minimal grade retention and no difference in retention between groups, makes it plausible that the absence of persistent effects in other studies is an artifact of flawed design for test data collection. Specifically, other studies have higher grade retention, special education placement, or both in the control group, but systematically exclude these low performers in collecting achievement test data. The differential attrition on achievement tests biases the estimated effect of preschool toward zero.

The data on schooling indicate that the preschool had significant cumulative effects on school success more broadly defined. Preschool group members spent less time in special education classes, were less likely to be classified mentally retarded, and were more likely to have graduated from high school. This pattern suggests that preschool education's effects are related to cognitive performance rather than to a general improvement in attitude or in the way teachers perceived students which might be expected to affect special education placements for all types of problems (behavior problems, for example). A higher percentage of the preschool group enrolled in some type of post-secondary education or training program. There were no significant differences in grade retention or in other types of special education placements. More of the experimental group had been in remedial classes, but the number of children involved was quite small (8 percent versus 3 percent).

The Perry Study found that the long-term effects of preschool educa-

8. Table 8 reports achievement test results in terms of raw scores, which are preferred for statistical analysis. For easier interpretation, differences in test scores can be converted to grade equivalents using a table provided by the test developers.

tion were not limited to effects on education, as shown by the findings summarized in Table 8 under "Economic Success" and "Social Adjustment." At age 19, the preschool group reported a higher employment rate and higher earnings. More of the preschool group reported that they supported themselves by their own (and spouse's) earnings and fewer reported that they received welfare. With respect to social adjustment, the treatment group had a lower arrest rate, and treatment group women had a lower pregnancy rate as teenagers. Although effects on employment, earnings, crime, welfare, and fertility might have been predicted from the educational effects (reviewed by Haveman and Wolfe 1984), it is reassuring to have direct estimates.[9] Having established that there were substantive long-term effects, the next step in the Perry Study was to try to weigh the economic benefits of those effects against the costs.

### *E. Benefit-Cost Analysis*

A benefit-cost analysis was conducted for the Perry study based on the data discussed above. Program costs were calculated, and five types of benefits were estimated: (a) the value of child care, (b) reductions in the costs of public education, (c) increases in earnings (and fringe benefits), (d) reductions in welfare payments, and (e) reductions in the costs of crime. Benefit estimates were based on the results through age 19 and forecasts beyond age 19. Some benefits depended entirely or almost entirely on results in hand. For example, the reductions in the costs of public education are due to reductions in special education and grade repetition that occur in elementary and secondary school.[10] Other benefits depended almost entirely on forecasts. For example, ultimate educational attainment was forecast based on attainment at age 19, and lifetime earnings were estimated using U.S. Census data relating earnings to education, ethnicity, gender, and age.

Table 9 presents the results of the benefit-cost analysis using a real discount rate of 5 percent and assuming that children attend two years of preschool education. Net present value for society as a whole is estimated to be positive, indicating that the program demonstrated promise

9. In general, there is strong agreement between official records data and self-report, even on the potentially sensitive topic of crime and arrest (Berrueta-Clement et al. 1984). The one area of weak agreement was welfare. This is also the one area where officials did not cooperate with the researchers to verify that correct matches were made between the research subjects and records data which reduces the confidence that can be placed in the official information.

10. The effect on grade repetition was not statistically significant by itself, but did contribute to estimates of economic benefits that are based on individual educational histories of study participants.

**Table 9**
*Costs and Benefits of 2 Years of Compensatory Preschool Education*[a,b]

| Category | Present Value of Benefits or Costs (1988 $'s) | | |
|---|---|---|---|
| | To Society | To Participants | To Taxpayers |
| Program costs | −12,570 | 0 | −12,570 |
| Benefits to age 19 | | | |
| Child care provided | 773 | 773 | 0 |
| School cost savings | 5,500 | 0 | 5,500 |
| Crime reduction | 1,260 | 0 | 1,260 |
| Earnings increase | 621 | 466 | 155 |
| Welfare reduction[c] | 50 | −499 | 549 |
| Benefits beyond age 19 | | | |
| College costs[d] | −673 | 0 | −673 |
| Crime reduction | 1,500 | 0 | 1,500 |
| Earnings increase | 15,588 | 12,590 | 2,998 |
| Welfare reduction | 1,075 | −10,747 | 11,822 |
| Total net benefits | 13,124 | 2,583 | 10,541 |

a. Source: Barnett, 1985b. Costs for two years, benefits estimated from entire sample.
b. Discounted present value at a 5 percent real rate.
c. Welfare reduction appears as a cost to participants who no longer benefit from the payments.
d. All attended state institutions and most of the cost was borne by the public. Costs to the students could not be estimated, except for "foregone earnings," which are accounted for in the "earnings increase" category.

as an efficient public investment.[11] Note that net gains are produced for both participants and taxpayers considered separately. Estimated net present value remains positive at real discount rates as high as 8 percent.[12] As there is much uncertainty surrounding the benefit estimates,

11. Although a positive economic return is not sufficient reason for a governmental organization with a fixed budget to make an investment, it is presumed that other public expenditures have much less favorable evidence of positive returns.
12. The estimates in Table 9 are based upon the cost of two years of the preschool program and average benefits for the sample as a whole. If one year of the preschool program produced lower benefits than two years, then net present value is underestimated to some extent. Unfortunately, the small number of participants with one year of preschool education ($n$ = 13) precluded meaningful conclusions regarding the relative benefits of one and two years. Obviously, if one year of preschool education produced about the same benefits as two years, it would have a much higher rate of return. Net present value would be positive at real discount rates up to 12 percent.

especially those forecast beyond age 19, extensive sensitivity analyses were conducted. These have been reported elsewhere (Barnett 1985b). Even quite pessimistic departures from basic assumptions do not alter the positive finding. For example, if all estimates beyond age 19 are reduced by 75 percent, net present value discounted at 5 percent remains positive.

### *F. Explaining Compensatory Preschool Education's Long-Term Effects*

The findings of long-term educational, economic, and social effects in the absence of permanent effects on IQ has been called "something of a mystery" (Gramlich 1986).[13] However, there are at least three plausible explanations for this result originating from different perspectives: (1) preschool education influences the parents who in turn increase their children's abilities, motivation, or effort; (2) preschool education increases motivation to succeed in school or some other affective variable related to school success; and (3) preschool education increases cognitive abilities in ways that are not satisfactorily measured by IQ tests. It is argued that the Perry Preschool Project evidence is most consistent with the view that preschool education's effects are primarily cognitive and provides grounds for rejecting the other two hypotheses.

### *G. Parents and Home Environment*

Many compensatory preschool programs work with the parents as well as the child, seeking to benefit the child through improvements in the home environment and parent-child interaction. It can be argued that short-lived programs produce persistent effects on children's school performance because they produce permanent changes in the home environment, which influence the child's development and motivation. In the Perry Study, there is little evidence that the program had any significant direct effects on parents and the home, even though the program staff made weekly visits to children's homes. The study found no differences in home environment or parenting behavior attributable to the program. No differences were found between groups on teacher ratings of parent cooperation and participation in the first 4 years of school, either. Measures of the home environment after the intervention period were more powerful predictors of later achievement and school success for the

13. Professor Gramlich (at the time, chairman of the Department of Economics at the University of Michigan) was kind enough to serve at my request on an advisory board for the benefit-cost analysis of the Perry Preschool Study.

control group, suggesting that preschool education attenuated this relationship.

### *H. Motivation and Self-Esteem*

A second explanation is that preschool education increased children's motivation or self-esteem, leading them to work harder and more confidently in school. In this view, the initial effects on IQ tests and school performance may be due to increased knowledge, but later effects are due to increases in motivation and self-esteem generated by early success. Support for this view is not strong in the Perry data. No significant differences are found in kindergarten teacher ratings of children's motivation, socio-emotional health, or classroom behavior. Significant differences in teacher ratings between the treatment and control groups begin to appear in first grade, but are not consistent from year to year. At ages 14 and 19, the interviews with the children reveal no significant differences between the two groups on measures of self-esteem, either generally or with respect to school. Although these affective characteristics may be important for school success, they do not appear to have provided the route for the preschool program's effects.

### *I. Cognition*

The third explanation is that preschool education has persistent effects on cognitive abilities, but that these effects are not reflected in permanent increases in IQ test scores. The basis for this explanation is that IQ measures only part of cognitive abilities. Many psychologists would argue that there is much more to intelligence than IQ and some hold that there are multiple intelligences and that IQ tests only measure one type of intelligence.[14] What IQ tests measure may be conceptualized as the speed of error-free information processing (Eysenck 1982). Thus, it is possible for compensatory preschool education to increase cognitive abilities by contributing to knowledge, problem-solving skills, and other aspects of intelligence without having any effect on what is measured by IQ tests.

In this context, the pattern of effects on IQ can be explained as follows. Preschool education increased children's knowledge and skills during the program. It may have improved some aspects of their cognitive processes, as well, but it did not affect the part of intelligence that IQ tests try to measure. Nevertheless, these effects on cognitive abilities regis-

14. A recent volume edited by Sternberg and Detterman (1986) offers 24 different theoretical perspectives on intelligence.

tered on IQ tests at ages three and four, because IQ tests for children of this age must be very concrete and so depend heavily on knowledge and a broad range of cognitive skills that can be learned in preschool programs. As children become older, IQ tests rely on more abstract items and become increasingly independent of knowledge and aspects of cognitive ability unrelated to processing speed. Thus, the differences in IQ between treatment and control groups declined with age and eventually disappeared.

The cognitive effects of preschool education had very different long-term consequences for achievement and school performance than for IQ. The treatment group entered school with richer experiences that provide more knowledge of facts and, perhaps, knowing more about how to solve problems and how to learn, and other cognitive advantages. These advantages were building blocks for the accumulation of more knowledge and skills in school. Thus, the treatment group maintained and even increased its advantages over time on measures of school performance and achievement. At each succeeding level of education, the treatment group was better able to take advantage of learning opportunities because it had more to build on. This view of school learning as a cumulative process in which early advantages lead to greater advantages in later achievement is consistent with the findings of research on cognitive processes and learning (e.g., Bransford 1979; Borne, Dominowski, Loftus, and Healy 1986; Prawat 1989; Walberg and Tsai 1983; Whitener 1989). In addition, other evidence (e.g., Natriello and Dornbusch 1983; Peterson 1989) suggests that the school's response to student abilities could have played a role by giving more competent students work of greater difficulty and opportunities to learn higher level material. Support for this cognitive explanation of preschool's effects is found in the limitation of the effect on special education to lower mental retardation placements which are based on school performance as well as IQ. Interestingly, the Consortium (1983) found that significant differences in special education placements did not appear until considerably after IQ differences had disappeared indicating that transitory IQ increases are not the direct source of the special education effect. Also supportive is the Perry data from the test of basic adult abilities administered at age 19, which indicated that the preschool group was advantaged in knowledge, identifying facts and figures, reading, and writing.

## V. Discussion

Research on the long-term effects of compensatory preschool education sheds light on a number of issues of interest to econo-

mists. First, it appears that the benefits of compensatory preschool education as an investment in the human capital of children in poverty may be greater than has been generally believed. A variety of studies, including some of ordinary public programs, find that preschool education can improve the long term educational success of poor children as indicated by grade retention, special education, and educational attainment. The Perry Preschool Project offers evidence that preschool's educational effects lead to improvements in economic well-being and social adjustment. This is consistent with the broader literature on the economic benefits of educational success. A benefit-cost analysis of the Perry results indicates that compensatory preschool education can be an economically efficient and politically palatable public investment. Compensatory preschool education apparently produces its long term benefits without permanently increasing IQ, but it is argued that it does permanently improve cognitive abilities as measured by achievement tests. The failure of most studies to find long term effects on achievement tests is attributed to problems of design, testing procedures, and attrition. As always, there are important caveats.

Even when findings are supported by a number of studies, the samples are predominantly composed of black children, the preschool programs are not a random sample of today's programs, and much has changed in the schools and communities of poor people since most of the studies were conducted. One potentially important change is that the relevant alternative to compensatory preschool education today is more likely to be custodial child care than maternal care. These problems are particularly acute for the Perry Project which had only black participants, a program that was of high quality (small group size, highly qualified teachers, one session a day), and began in 1962. Whether all the differences suggest that the effects of compensatory preschool programs in the 1990s are likely to be higher or lower than the estimates of previous studies is difficult to know. Of course, even looking at the Perry Preschool Project alone, the confidence intervals for most effects are relatively large.

All of the studies discussed in this paper are potentially affected by queuing bias and can provide only partial equilibrium estimates of preschool education's effects (Burtless and Orr 1986). Queuing bias is an issue because the treatment is not available to everyone and may confer advantages on participants at the expense of others. For example, if schools refer a fixed percentage of students to special education, other students replace treatment group members in special education and there are no cost savings. Small-scale experiments produce partial equilibrium estimates, because the general equilibrium effects are so small or dispersed that they cannot be practically measured. For example, if the supply of high school graduates to the labor force is increased, it may

reduce the wages of all high school graduates in the short-run, but information on wage effects is not obtainable from small-scale studies.[15]

Even if public programs were all as successful as the Perry Preschool program, compensatory preschool education would not be a panacea. Preschool programs can have meaningful effects and still not eliminate the long-term disadvantages associated with being born into poverty. For example, the Perry Preschool program boosted achievement by more than a full grade level, but the treatment group continued to score well below the fiftieth percentile on achievement tests and to have a dropout rate significantly above the national average. The benefits of compensatory preschool education are substantial and should not be trivialized. At the same time, the desirability of further improvement in the educational and economic success for children of low-income families should not be overlooked.[16]

Research on compensatory preschool education has at least one important implication for other human capital studies. The finding that IQ test scores do not adequately capture the effects of preschool education on cognitive human capital suggests that economists risk serious errors if they do not account for the complexities of cognitive abilities in research on human capital. It does not appear to be safe to assume that the contributions of schools (or families) to cognitive human capital can be precisely described by the term "intelligence" or "ability" or adequately measured by whatever test scores happen to be available. It should not be assumed that scores from different types of cognitive tests are comparable. Efforts to elaborate the theoretical definition of human capital and greater attention to the correspondence between theoretical constructs and measures might significantly improve human capital studies.

One starting place would be for researchers to specify more precisely the characteristics and behaviors included in their models and measures of human capital. This would impose greater rigor on theorists and increase the possibility of developing valid empirical tests of theoretical models for such phenomena as the intergenerational transmission of earnings and wealth. In addition, although some theorists have recognized that human capital can encompass a range of personal characteristics and

15. The existing research does not support strong conclusions about the magnitudes of long-run elasticities (Freeman 1987, Hammermesh 1986). Other general equilibrium effects might be expected that could have positive, as well as negative, consequences. For example, a general rise in academic performance and decrease in delinquency might sufficiently change the climate of schools that all students learned more, and the intrinsic rewards to teaching rose.

16. Nevertheless, a combination of many small efforts to combat poverty might have a greater impact than the more massive efforts.

behavior beyond IQ and knowledge, there has been a tendency to employ a simple dichotomy between cognition-achievement-knowledge and affective behavior-personality traits (e.g., Blaug 1985, Bowles and Gintis 1976). This dichotomy inadequately distinguishes important aspects of cognition and classifies as noncognitive aspects of behavior that have cognitive components, such as task persistence. In developing clearer concepts of human capital and selecting measures that correspond to those concepts, economists might benefit from increased collaboration with researchers in education, psychology, human development, and cognitive science.

Returning to more central issues, while the similarity of findings in all the studies reviewed offers some confidence that large-scale public compensatory preschool education programs can improve the cognitive development and academic success of children from low income families, the findings are not entirely uniform. A great deal remains to be learned about the effects of program characteristics, population characteristics, and the environment (e.g., peer pressures, later educational quality and practices) on program outcomes. In particular, preschool education's effects on educational attainment, retention, and special education depend on local school policies. It follows that one cannot have much confidence that the precise benefits found in the Perry Preschool or any other study can be attributed to state or city programs or to a national program such as Head Start. Also, remarkably little can be said about important policy questions such as whether preschool should begin at age three or four, whether children should attend for a half-day or full-day (which may vary depending on the child's alternative care arrangements), child-staff ratio, and group size. As public spending on these programs increases, it would seem prudent to initiate large scale longitudinal studies to estimate program effects and to estimate the effects of variation in eligibility requirements, program characteristics, and interactions with school policies, and home and community environments. Economic analyses of the results would not only provide a stronger basis for funding decisions but could contribute to the development of policies for more efficient program design and implementation.

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