Does Head Start Make a Difference?

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The impact of participation in Head Start is investigated using a national sample of children. Comparisons are drawn between siblings to control for selection. Head Start is associated with large and significant gains in test scores among both whites and African-Americans. However, among African-Americans, these gains are quickly lost. Head Start significantly reduces the probability that a white child will repeat a grade, but it has no effect on grade repetition among African-American children. Both whites and African-Americans who attend Head Start, or other preschools, gain greater access to preventive health services. (*JEL* I38, H43)

Head Start is a federal matching grant program that aims to improve the learning skills, social skills, and health status of poor children so that they can begin schooling on an equal footing with their more advantaged peers. Begun in 1964, as part of the "War on Poverty," Head Start has enjoyed great public and bipartisan support. Presidents George Bush and Bill Clinton both pledged to increase federal funding so that all eligible children could be served. Today 622,000 children, roughly 28 percent of eligible 3–5-year-olds, are served at a cost of $2.2 billion per year, or approximately $3,500 per child, per year (Anne Stewart, 1992).

Policymakers and the general public appear to believe that the benefits of Head Start are well known and well documented. However, a careful reading of the literature reveals that credible studies that demonstrate lasting effects of Head Start are limited. The studies that do exist are typically restricted to small geographic regions and specific racial groups.

In this study we use a national sample of data from the National Longitudinal Survey of Youth (NLSY) and the National Longitudinal Survey's Child–Mother file (NLSCM) to reexamine the impact of Head Start on school performance, cognitive attainment, preventive medical care, and health and nutritional status. Although our study is no substitute for a national randomized trial, we do take some novel steps to sort out the effects of the Head Start program from possible nonrandom selection into the program. First, we contrast children who have been enrolled in the Head Start program with their siblings who have not, in order to control for family background effects on cognitive and health outcomes. Second, using the same sibling contrasts, we compare the impact of Head Start relative to "no preschool" with the impact of participation in other preschools relative to "no preschool." These "difference-in-difference" estimates further control for possible biases in the estimates due to child-specific determinants of participation in Head Start.

When selection is controlled in this way, Head Start has positive and persistent effects on the test scores and schooling attainment of white children, relative to participation in either other preschools or no preschool. In contrast, while the test scores

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of African-American children also increase with participation in Head Start, these gains are quickly lost, and there appear to be no positive effects on schooling attainment.

Relative to "no preschool," participation in either Head Start or preschool is associated with improved utilization of preventive medical care, as proxied by immunization rates, among whites and African-Americans. In contrast, there is no evidence that Head Start has any effect on child height-for-age, a longer-run indicator of health and nutritional status.

The rest of the paper is laid out as follows. The first section contains a brief overview of the previous literature. In the second, the methods are discussed. The third section provides a description of the data and our child outcome measures. The estimated effects of Head Start are presented in the fourth section. We conclude with a crude assessment of the possible long-term benefits of the program and weigh these against its cost.

I. A Brief Sketch of the Literature

Most previous studies of Head Start have focused only on assessing gains to IQ, despite the broad goals of the Head Start program. For example, although Head Start provides "a comprehensive health services program which includes a broad range of medical services" (Head Start Bureau, 1992), a recent review of 210 studies conducted by the U.S. Department of Health and Human Services (Ruth McKey et al., 1985)[1] cites only 34 studies that have examined effects on health. These studies provide useful qualitative information about the health effects of the program, but very few of them attempt to quantify the effects in any way. McKey et al. also note that very few studies have examined the impact of Head Start on schooling attainment.[2]

The most convincing studies of the IQ effects of Head Start utilize a treatment and control design with random assignment.[3] These studies typically find that there are initial gains to Head Start which fade over time and become insignificant by the third grade. However, Steven Barnett (1992) notes that experimental evaluations of the longer-term effects on IQ may be biased by attrition because children who move are likely to be lost from the experiment (although the direction of any bias is not obvious). A second limitation is that existing experimental evaluations have not been based on national samples of children in representative Head Start programs. Many studies, for example, focus exclusively on African-American children.

Head Start is also said to be associated with reductions in grade repetition, high-school dropout rates, and teen pregnancies, and with improvements in children's medical care and health status (cf. Children's Defense Fund, 1992). The most widely cited evidence in support of these longer-term benefits of Head Start actually comes from experimental studies of model preschool programs such as the Perry Preschool Project or the Tennessee Early Training Project. These programs were funded at higher levels, involved more intensive interventions, and had better-trained staff than the

[1]There have been several other surveys of the Head Start literature (see Westinghouse Learning Corporation and Ohio University, 1969; Frances D. Horowitz and I. Y. Paden, 1973; Urie Bronfenbrenner, 1975; Louis Datta, 1979; Karl R. White, 1985–1986). Maris Vinovskis (1993) shows that the debate about the efficacy of compensatory education in the United States dates back at least to the 1840's when 40 percent of all three-year-olds in Massachusetts were attending infant schools.

[2]The handful that have include Henry A. Goodstein et al. (1975), Consortium for Longitudinal Studies (1983), Kathleen Hebbeler (1985), Carol E. Copple et al. (1987), and J. S. Fuerst and Dorothy Fuerst (1993). The studies by the Consortium for Longitudinal Studies and the Fuersts actually dealt with programs that were funded at much higher levels than the typical Head Start program.

[3]Other studies make use of a quasi-experimental design in which the comparison children are drawn from waiting lists for the Head Start program. Valerie Lee et al. (1988) reanalyzed data from two of these studies and found that the Head Start children were less likely to have a father present and had less-educated mothers than "controls" who did not participate.

typical Head Start program. For example, the Perry Preschool Project was funded at a rate of about $6,000 per child (almost twice that of the average Head Start program). Twenty years after the program, researchers found that the "treatments" were more likely to graduate from high school, had fewer pregnancies per female child, and had lower crime rates. However, the study involves a very small sample of 58 treatments and 65 controls, and many differences (such as the rate of teen pregnancy and the rate of violent crime) are not statistically significant (John R. Berrueta-Clement et al., 1984).[4]

In summary, despite literally hundreds of studies, the jury is still out on the question of whether participation in Head Start has any lasting beneficial effects.

II. Methods

The key empirical problem facing us is that, as we will see below, children are not randomly selected into the Head Start program. The program guidelines require that 90 percent of participants must be from families living below the federal poverty line although, in practice, 95 percent of children served in 1992 were poor (U.S. Department of Health and Human Services, 1993). In addition to being poor, Head Start children may also be disadvantaged in other observable ways. Estimates that do not take account of these differences are likely to underestimate the beneficial effects of the program. We will, therefore, examine the impact of Head Start on child well-being conditional on an array of *observable* mother and child characteristics.

The economic model of the family (Gary Becker, 1981) suggests that families choose whether or not to make the effort necessary to enroll their children in Head Start or other preschools on the basis of the expected returns from that investment. Families who find this investment worthwhile may make other unobserved investments in the child's human capital. In this case, studies that do not take account of *unobserved* differences between families may overestimate the beneficial effects of Head Start.

At many sites, there are fewer places than child applicants, and so participant selection will also reflect the choices made by program administrators. There are over 1,300 Head Start programs (Cheryl Hayes et al. 1990), all administered at the community level, and there is a good deal of heterogeneity in their management and quality and in the interpretation of the federal guidelines (U.S. Department of Health and Human Services, 1993). Remarkably little is known about the selection practices used by administrators, although Ronald Haskins (1989) cites evidence that local staff tend to select the most disadvantaged children to participate in Head Start.[5] Similar evidence on selection procedures is suggested by Lee et al. (1990). Unlike most adult training programs, evaluation is not based on child performance in the program, and so there is little incentive to cream off the more able applicants. In any case, whatever the mechanism underlying participant selection by administrators, estimates of the effects of Head Start that do not take this process into account may be biased.

In order to control for unobserved characteristics correlated with selection into the program we estimate models with fixed effects for each household. These models control for constant characteristics of households, including permanent income, maternal education, and other measures of (unobserved) family background and tastes. If it is primarily these constant factors that determine participation in Head Start, then

[4] An additional problem is that, in samples of this size, the effects of unobserved heterogeneity can swamp small treatment effects, even when treatments and controls are randomly assigned.

[5] In addition to considering income, program administrators are required to set aside 10 percent of their places to children with disabilities. A recent evaluation of the program indicates that most sites comply with this requirement but suggests that there is variability in the definition of a disability across sites (U.S. Department of Health and Human Services, 1993).

fixed-effects models will provide unbiased estimates of the true program effects.[6]

However, there may also be child-specific factors that affect participation. If, for example, parents wished to maximize the sum of their offspring's lifetime utility, then they might choose to enroll more able children in Head Start. On the other hand, if they seek to equalize outcomes, they might enroll the least able child. In the first case, fixed-effects estimates would provide an overestimate of the impact of Head Start, while in the latter case, they would yield an underestimate.

There are two other reasons why the inclusion of household fixed effects could bias estimated program effects toward zero. First, it is well known that in the presence of measurement error, differencing can result in "throwing the baby out with the bath water," since much of the true "signal" may be discarded while the "noise" remains.

Second, in the fixed-effects models the effects of Head Start are identified using the subset of households in which some children attended Head Start while others did not. If there are any spillover effects of Head Start from one sibling to the other, then the difference between the two siblings will be an underestimate of the true program effect. Spillover effects may be important because a child teaches his or her sibling something learned in Head Start, because the parent gains access to a service that is of benefit to both children, or because the parent makes compensating investments in the non-Head Start child.

In order to gain an understanding of the importance of the potential biases in the fixed-effects estimates due to child-specific factors, and spillover effects, we compare fixed-effects estimates of the effects of participation in Head Start to fixed-effects estimates of the effects of enrollment in other preschools. The decision to enroll a child in some other kind of preschool is also properly treated as a choice. As is the case for Head Start, fixed-effects estimates of the impact of other preschools will be unbiased if there are no unobserved child-specific characteristics that affect this choice, and no spillovers.

If the child-specific factors or spillovers bias the estimated coefficients on Head Start and on preschool in the same way, then the difference between the estimated coefficients will be accurately estimated, even if the individual coefficients are not. For example, suppose that parents send favored children either to Head Start or to preschool, depending on their means, and keep other children at home. In this case the fixed-effects estimates of Head Start and other preschools will both be biased upward. But the estimated difference between the effects of Head Start relative to no preschool and the effects of other preschools relative to no preschool will be subject to less bias.

We show below that, for several of our outcome measures, the fixed-effects estimates of the effects of Head Start exceed those of enrollment in other preschools. Still, there are two possible ways in which these results could be driven by the biases discussed above. First, it could be the case that children who attend either kind of preschool are systematically more favored or more able than their siblings *and* that the gap in ability between Head Start children and their stay-at-home siblings is greater than the gap between other preschool children and their siblings. Sec-

[6]Another way to address the problem of the endogeneity of program participation is to use instrumental-variables (IV) estimators. We have experimented with this approach but have not been successful in identifying convincing instruments, at least from an empirical point of view. We tried, for example, assuming that a mother's own participation in Head Start affected her child's outcomes only through the child's participation in Head Start. Although maternal participation in Head Start is a significant predictor of the child's participation (Frank Mott and Stephen Quinlan, 1992), it does not explain much of the variation in participation, and the second-stage estimates of the impact of Head Start are very imprecise. Similar problems arose in experiments with the proportion of federal funds spent in a state, and state-level Head Start enrollments were not good predictors. Richard Nelson and Richard Startz (1990) report that in these circumstances, IV estimates can be very misleading; see also John Bound et al. (1993), and Douglas Staiger and James Stock (1993). In view of these results, we do not report IV estimates in this paper.

ond, spillover effects could be greater within families in which a subset of children attend other preschools than within families with a subset of children attending Head Start.

It is difficult to rule out the possibility that the degree of parental favoritism is greater in households with some children who attend Head Start than in households in which some children attend preschool. However, we do not find any evidence consistent with the view that Head Start children are favored. For example, relative to their siblings, they are no more likely to be taken to the doctor in the first three months of life, and they score no higher on the "recognition of body parts" test, a test that was administered to sample children before they were age-eligible to attend Head Start.[7] Moreover, we will discuss evidence below which suggests that preschool children may actually be more favored relative to their siblings than Head Start children, in which case the difference between the estimated effects of Head Start and preschool in the fixed-effects models provides a lower bound on the true difference.

Finally, the potential for spillover effects may be greatest in the most disadvantaged households and among children in programs like Head Start that make explicit attempts to improve parenting skills. In this case, Head Start effects will be underestimated relative to the effects of other preschools in the fixed-effects models. Spillovers are also likely to accrue to younger siblings, and we explicitly investigate this issue.

III. Data Description

The National Longitudinal Survey of Youth (NLSY) began in 1979 with 6,283 young women who have been surveyed annually ever since. As of 1990, these women were aged 25–32 and had given birth to over 8,500 children. In 1986, the NLS began a separate survey of the children of the NLSY, the National Longitudinal Survey's Child–Mother file or NLSCM. The second and third waves of the NLSCM were undertaken in 1988 and 1990. In these two waves, mothers were asked whether their children had ever participated in Head Start. For this study, data on children and their mothers from all three waves of the NLSCM have been combined with information about the mother drawn from each wave of the NLSY. Attention is restricted to children aged 3 and older, and since the fixed-effects estimates are based on sibling comparisons, the sample includes only children who have at least one sibling over three years old. These rules result in a sample of nearly 5,000 children.[8]

It is important to note that the original NLSY oversampled the poor, and so a relatively large proportion of the sample children—about one-fifth—participated in Head Start. In addition, due to oversampling there are large enough numbers of African-Americans to allow separate examination of this group.[9]

A. *Child Outcomes*

We focus on four measures of child outcomes. The first pair are indicators of academic performance: the Picture Peabody Vocabulary Test (PPVT) score[10] and

[7] In principle, it may be useful to control for pre-Head Start test scores when examining the effect of the program on post-Head Start scores. However, because of the design of the NLSCM, most tests are age-dependent and thus only taken once by any child.

[8] Examining only mothers with at least two age-eligible children reduces the sample by 14 percent. The excluded children tend to live in higher-income households, their mothers are better educated, and they are better off in terms of the four child outcomes discussed in what follows.

[9] Hispanics have been examined separately in Currie and Thomas (1993). The effects of Head Start are not statistically significantly different from those of non-Hispanic whites for most outcomes. Hispanic and non-Hispanic whites are thus treated as one group in order to place the spotlight on racial differences.

[10] In earlier work, we also reported results using Peabody Individual Achievement Test scores for mathematics, reading recognition, and reading comprehension (Currie and Thomas, 1993). The results for reading recognition and comprehension were similar to, though weaker than, the results reported here for PPVT scores. The only statistically significant result we found for PIAT math scores was that enrollment in other preschools was associated with higher scores among white children.

TABLE 1—CHILD OUTCOME MEASURES

Measure	Age group	Comments
PPVT score	4+	Only measured once per child. Percentile scores based on nationally accepted norms for age and gender are used. Measures taken while a child was in preschool or Head Start are not used.
Absence of grade repetition	10+	"Has your child repeated any grades for any reason?" Coded 1 if the mother answered no in both 1988 or 1990, and zero otherwise. Not asked in 1986.
Measles shot	all	Had child had a shot as of 1990?
Height-for-age	all	Asked in 1986, 1988, and 1990. The measure taken closest to the child's fifth birthday is used.

whether the child has progressed through school without repeating a grade.[11] The second pair of outcomes are related to child health: whether the child has been immunized for measles, and height standardized by age and gender using national norms (height-for-age). Table 1 provides details about the coding of these variables. Each row shows the measure, the age group for whom the measure was recorded, and some additional comments.[12]

The relationship between test scores and future wages has received considerable attention from economists. In his summary of this literature, Eric Hanushek (1986 p. 1152) concludes that, in most studies, "years of schooling and measures of cognitive ability exhibit independent effects on earnings." Unfortunately, the majority of these studies focus on the scores of high-school students rather than on those of young children. However, Richard Murnane et al. (1993) find that a high-school senior's mastery of skills taught no later than the 8th grade (as measured by achievement on standardized tests) is an important determinant of future wages.

While there is some evidence that test scores predict future schooling and labor-market outcomes, the relationship is certainly not one-to-one. For example, developmental psychologists emphasize that a positive self-image and appropriate socialization may also contribute to scholastic success. Thus, the absence of grade repetition is examined as a second, more direct measure of academic performance.

Academic performance in early grades has been shown to be a significant predictor of eventual high-school completion (Atlee L. Stroup and Lee N. Robins, 1972; Dee N. Lloyd, 1978; Byron Barrington and Bryan Hendricks, 1989; Robert Cairns et al., 1989; James Grissom and Lorrie Shepard, 1989; Margaret Ensminger and Anita Slusarcick, 1992). The relationship between high-school completion and wages is well-established: most studies find that an additional year of high school is associated with an 8-percent increase in lifetime wages (see Joshua Angrist [1990] for a recent estimate). High-school graduates are also less likely to be unemployed (James Markey, 1988). Educational attainment has also been shown to be associated with improvements in health (Michael Grossman, 1973) and job satisfaction (Robert Michael, 1982; Robert Haveman and Barbara Wolfe, 1984). These results suggest that by improving performance in early grades, Head Start participation could translate into a significant increase in the probability of graduating from high school with attendant improvements in future wages and employment probabilities.

As discussed above, in addition to early childhood education, the Head Start pro-

[11] If the child repeated a grade, mothers were also asked why the grade was repeated. The possible answers were: academic failure or lack of ability; immature, acts too young; frequently absent; truancy; health reason; moved to a more difficult school; and other. Mothers were allowed to check more than one answer, and we found that virtually all mothers indicated that academic failure was a factor.

[12] Further information about these measures is available in Paula Baker and Mott (1989).

gram provides a broad range of health-care services. Specifically, Head Start guidelines require that each child be given a physical examination; an assessment of immunization status; a growth assessment; vision, hearing, and speech tests; a hemoglobin or hematocrit test (for anemia); and a tuberculin skin test. Head Start centers are also required to screen for sickle-cell anemia, lead poisoning, and parasitic infection, if these problems are common in the community. The NLSCM data only allow us to assess immunization status, and growth (as discussed below), but given the guidelines, it is not unreasonable to suppose that children who gain access to immunization services are also more likely to gain access to at least some of the other required health services. In this case, immunization can be viewed as a marker for access to a bundle of important health services.

Head Start program performance standards also state that "every child in a part-day program will receive a quantity of food in meals...and snacks which provides at least 1/3 of daily nutritional needs..." (Head Start Bureau, 1992 p. 40). Poor children are at much greater risk of nutritional deficiencies than other children. For example, 21 percent of 1–2-year-old children in low-income households suffer iron anemia compared to 7 percent of 1–2-year-olds from higher-income households (Barbara Devancy et al., 1989). These deficiencies have been linked to short attention spans, lethargy, impaired immune status, and growth retardation.[13]

With our second measure of child health, we place the spotlight on nutrition. Height-for-age is an indicator of both nutritional status and health, and it captures the effects of longer-term deprivation. It has been profitably used in the economic history and development literatures (see, for example, Robert Fogel [1986], Reynaldo Martorell and Jean-Pierre Habicht [1986], and the review in John Strauss and Thomas [1995]). Many readers may be surprised to find that even in as rich a society as the contemporary United States, poor children are at risk of stunting, defined as low height-for-age. Data from the second National Health and Nutrition Survey (National Center for Health Statistics, 1981) indicate that 15 percent of poor female children 2–5 years old are below the fifth percentile of height-for-age. The corresponding figure for males is 11 percent.

Since child growth varies systematically with age and gender, height is standardized following guidelines from the National Center for Health Statistics (1976). Each child in the sample is compared with the median child in a population of well-nourished white children of the same age and gender in the United States, and the sample height-for-age is expressed as a percentage of this median.[14] However, given evidence of systematic deviations from the standards in populations of poor children, we use the measure of height taken closest to the child's fifth birthday in order to compare siblings of approximately similar ages.[15]

B. *Characteristics of Head Start and Other Children*

The characteristics of Head Start children, other preschoolers, and all other chil-

[13]See Currie (1995) for a discussion.

[14]In the NLSCM, child height is either measured (by the enumerator or mother) or recalled by the mother. In the 1986 survey, it is not possible to identify those children who were actually measured, although reported height was apparently based on recall for very few children (Paula Baker, pers. comm.). In the 1988 and 1990 surveys, the heights of about 30 percent of children were reported by their mothers, and the probability of being measured rises with age. There is very little evidence of stacking in the recall data, and the variances are similar for both recall and measured data. Therefore, in this paper, we use all child heights as reported in the surveys.

[15]In Currie and Thomas (1993), we show that, relative to the NCHS norms, there is a dip in height-for-age in our sample soon after birth; on average, this is made up by the time the child reaches age 5.

TABLE 2—CHARACTERISTICS OF MOTHERS AND THEIR CHILDREN: MEANS AND STANDARD ERRORS

	Whites				African-Americans			
Characteristics	All	Head Start	Preschool	Neither	All	Head Start	Preschool	Neither
Mother:								
Permanent household income (1990 $1,000's)	26.12 (0.26)	16.89 (0.39)	32.73 (0.52)	24.08 (0.30)	17.26 (0.29)	15.04 (0.38)	21.29 (0.75)	16.55 (0.42)
Human capital								
Education	11.70 (0.04)	10.91 (0.09)	12.48 (0.06)	11.37 (0.05)	11.84 (0.05)	11.64 (0.07)	12.48 (0.09)	11.62 (0.07)
AFQT score	0.83 (0.01)	0.58 (0.02)	1.01 (0.02)	0.78 (0.01)	0.43 (0.01)	0.37 (0.02)	0.55 (0.02)	0.42 (0.02)
Height (inches)	63.85 (0.04)	63.42 (0.12)	64.06 (0.07)	63.83 (0.06)	64.01 (0.07)	64.12 (0.11)	64.18 (0.14)	63.83 (0.11)
Grandmother's education	9.81 (0.06)	8.68 (0.15)	10.69 (0.09)	9.51 (0.08)	10.02 (0.07)	9.74 (0.11)	10.81 (0.13)	9.77 (0.11)
Number of maternal siblings (at age 14)	4.30 (0.05)	4.68 (0.13)	3.74 (0.07)	4.58 (0.07)	5.45 (0.09)	5.68 (0.15)	4.97 (0.17)	5.55 (0.13)
Child:								
Age in months, 1990	99.18 (0.68)	115.04 (1.78)	94.27 (1.01)	98.30 (0.99)	107.74 (1.09)	119.07 (1.81)	98.57 (2.00)	104.72 (1.73)
First born[a]	0.47 (0.01)	0.50 (0.02)	0.56 (0.01)	0.41 (0.01)	0.44 (0.01)	0.47 (0.02)	0.47 (0.03)	0.39 (0.02)
Male[b]	0.49 (0.01)	0.47 (0.02)	0.48 (0.01)	0.49 (0.01)	0.51 (0.01)	0.48 (0.02)	0.55 (0.03)	0.52 (0.02)
Number of children:	3,285	450	1,149	1,686	1,502	477	376	649
Sample proportions:	100	14	35	51	100	32	25	43

Notes: Standard errors are given in parentheses. Maternal education is measured as highest grade attained. The AFQT score is age-standardized. The number of maternal siblings is the number when the mother was age 14.
[a]Dummy variable = 1 if first born.
[b]Dummy variable = 1 if male.

dren are presented in Table 2, distinguishing whites from African-Americans. Neither Head Start participants nor enrollees in other preschools are random samples of children: the probability of attending Head Start declines with income, whereas the probability of attending other preschools rises with permanent income.[16] For example, among all children living in households in the bottom quartile of the permanent-income distribution, nearly 30 percent have attended Head Start, whereas only 15 percent attended other preschools. In the top quartile, 40 percent of children attend other preschools and 4 percent attend Head Start. Slightly over half the children in the sample never attend any preschool, and that fraction is essentially constant across the income distribution. This suggests that the mechanism governing selection into Head Start is quite different from that underlying selection into other preschools, or even into no preschool.

Table 2 shows that, in addition to lower average levels of permanent income, Head Start children are disadvantaged in most other observable respects. Relative to children who attended other preschools, children who attended Head Start have moth-

[16]As our measure of income we use "household permanent income," the average of annual household income between 1978 and 1990, in real 1990 dollars, in order to attenuate the influence of random measurement error.

ers and grandmothers who are less educated, and who had lower scores on the Armed Forces Qualification Test (AFQT), a measure of human capital.[17] These differences between Head Start and other preschool children are all statistically significant for both whites and African-Americans, although the gaps are substantially larger among whites. For example, the difference in maternal education between white children in Head Start and white children in other preschools is 1.6 years, while the difference is only 0.8 years among African-Americans. The major exception to this generalization is that the mothers of African-American Head Start children are as tall as the mothers of other African-American children, while white mothers of Head Start children are shorter than other white mothers.

White Head Start children also tend to be disadvantaged relative to children who attended no preschool, though the gaps are smaller than those between the Head Start and preschool groups. Among African-Americans, however, the only significant difference is in income: in all other observable respects, Head Start children are no worse off than their peers who attended no preschool.

Finally, Table 2 shows that, relative to whites, and controlling for preschool status, African-American mothers of Head Start children are actually better educated than comparable white mothers, although they tend to live in lower-income households. However, the AFQT scores of African-American women are much lower than those of whites, a fact that is true throughout the income distribution and suggests that AFQT measures more than native "ability."

[17]Since the NLSY respondents were of different ages when the test was administered, the scores are standardized using the mean score for each year of age. Head Start participants are also less likely to have had a father figure present at age 3, and their mothers were less likely to be employed at that time. Including these potentially endogenous variables in our models did not change the results reported here.

C. *Parental Favoritism? Evidence from Within-Family Income Differences*

As discussed above, the fixed-effects models estimated below are identified using the subset of families with at least one child who attended Head Start and at least one who did not. Similarly the effects of preschool attendance are identified using the subset of children in which at least one child attended preschool and at least one did not. Table 3 focuses on the within-family income changes that are associated with participation in Head Start and other preschools.

Panel A of Table 3 reports, for children who attended Head Start, other preschools, or no preschool (in the columns), the percentage with siblings who attended Head Start, other preschools, or no preschool (in the rows). For example, the entry in the upper left corner of the table indicates that 41 percent of white children who attended Head Start had a sibling who also attended Head Start, and therefore, 59 percent had a sibling who did not. In the fixed-effects models, only the latter group is used to identify the effects of Head Start.

Of these 59 percent, the vast majority (about three-quarters) did not attend any preschool. Thus, fixed-effects estimates of the impact of Head Start will be based largely on within-family comparisons of children in Head Start with siblings who did not attend any preschool. The converse is also true: families with at least one child in preschool and at least one child not in preschool were unlikely ever to have had a child in Head Start. Estimates of the effects of Head Start and other preschools are therefore based on largely nonoverlapping samples of families. This result is important because it facilitates the comparison of Head Start effects to the estimated effects of attending other preschools.

Panel B of Table 3 presents the means and standard errors of two measures of income for each type of sibling pair. Permanent income (which is family-specific) is reported in the first column, while income at the time the child was three years old is reported in the second. Income at age 3 is

TABLE 3—CHARACTERISTICS OF CHILDREN AND THEIR SIBLINGS BY TYPE OF PRESCHOOL ATTENDED

A. *Percentage of Children and Siblings by Type of Preschool Attended*

	White child attended:			African-American child attended:		
Sibling attended	Head Start	Preschool	Neither	Head Start	Preschool	Neither
Head Start	41.3	5.7	10.9	57.1	18.2	19.6
Other preschool	15.5	61.8	22.4	14.2	50.2	17.1
Neither	43.2	32.6	66.7	28.6	31.7	63.3
Total:	100	100	100	100	100	100
Sample size:	310	848	1,230	329	259	480

B. *Income by Type of Preschool Attended by Child and Sibling: Means and Standard Errors*

			Whites		African-Americans	
Row	Child attended	Sibling attended	Permanent income	Income at age 3	Permanent income	Income at age 3
1	Head Start	Head Start	17.36	14.17	13.76	11.40
			(0.79)	(1.11)	(0.57)	(0.81)
2	preschool	preschool	34.32	34.81	24.44	23.27
			(0.83)	(1.54)	(1.71)	(4.30)
3	neither	neither	23.53	20.32	16.17	13.73
			(0.40)	(0.59)	(0.53)	(0.73)
4	Head Start	neither	16.29	13.18	16.90	14.89
			(0.66)	(0.77)	(0.99)	(1.41)
	neither	Head Start		13.11		13.91
				(1.06)		(1.85)
5	preschool	neither	30.07	28.32	18.26	17.33
			(0.78)	(1.14)	(1.21)	(1.84)
	neither	preschool		21.92		9.77
				(1.28)		(1.24)
6	Head Start	preschool	19.80	14.92	19.51	17.32
			(1.46)	(1.91)	(1.31)	(2.03)
	preschool	Head Start		19.65		20.19
				(2.90)		(2.62)
	All children:		26.12	23.35	17.50	15.02
			(0.30)	(0.48)	(0.35)	(0.66)

Note: Standard errors are reported in parentheses.

relevant since this is the time when most children would enter Head Start or some other preschool. Rows 1–3 confirm that, relative to children who attended other preschools or no preschool, Head Start children are disadvantaged both in terms of permanent income and income at a point in time.

A second fact, which is apparent from row 4 of Table 3, is that there is little within-family difference in household income at the time the child was age 3 between Head Start children and those who never went to preschool. In contrast, rows 5 and 6 indicate that transitory income is associated with within-family movements between other preschool and no preschool, and also between Head Start and other preschool. The within-family gap between preschool and no-preschool children is about \$6,000 among whites and \$8,000 among African-Americans. Similarly, the within-family gaps between other-preschool and Head Start children are \$5,000 and

$3,000 for whites and African-Americans, respectively.

These results show that, when family incomes rises, parents are more likely to send age-eligible children to preschool. Assuming that parents want to do what is best for their children, but are constrained by income, this finding suggests that a favored child would be more likely to be sent to preschool, other things being equal.[18] We do not find any similar pattern for Head Start. Hence, there is some evidence consistent with the view that preschool children are actually more favored relative to their stay-at-home siblings than Head Start children, which implies that the difference between the estimated effects of Head Start and of preschool in the fixed-effects models discussed below may be an underestimate of the true Head Start premium.

IV. Estimation Results

Tables 4 and 5 present regression estimates of the effects of participation in Head Start and other preschools on the four child outcomes. In order to highlight the importance of controlling for observed and unobserved family-specific effects, three sets of estimates are presented in each case. "Unadjusted" ordinary least-squares (OLS) estimates [in columns (i)–(iii)] do not control for any observable covariates: this baseline shows the sample means. "Adjusted" OLS estimates [in columns (iv)–(vi)] do control for mother- and child-specific observables. Fixed-effects estimates [in columns (vii)–(ix)] also control for all unobserved time-invariant mother-specific effects in addition to child-specific observables.[19]

All the regressions are estimated separately for whites and African-Americans; to facilitate comparisons between the two groups, differences between the estimated coefficients are reported in the third column in each panel. In each regression, the excluded category is children who did not attend preschool. The F statistic for the test that the estimated "difference-in-difference" between Head Start and other preschool children is zero is reported just below each panel of estimates (along with the associated p value).[20]

The observables in the "adjusted" OLS regressions include child age, gender, and whether the child was the first born, (log) household permanent income, the mother's

[18]The argument is made somewhat more complicated, but is not reversed, if we consider the effects of maternal employment on preschool enrollment. If the mother's aim is to do what is best for her child, then she will work if and only if the positive effects of gaining more income outweigh any negative effects of spending less time with the child. In fact, there is little evidence that maternal employment harms children. See Currie (1995) for a discussion of this literature.

[19]To facilitate comparisons, the sample is restricted to children with at least one sibling for whom the outcome is reported in all the regressions. The importance of this sample selection can be assessed by comparing the OLS results with OLS regression estimates based on the full sample of children. In both the unadjusted and adjusted cases, the impact of Head Start and preschool changes by less than a standard error, and inference is unchanged for all four child outcomes. For example, when the full sample of white children is used, the adjusted Head Start effect on PPVT is one-tenth of a standard error bigger and the preschool effect is one-quarter of a standard error bigger than the estimates reported here.

[20]Lagrange-multiplier tests for homoscedasticity (Trevor Breusch and Adrian Pagan, 1979; Halbert White, 1980) are rejected for PPVT and height-for-age; for these two outcomes, standard errors and test statistics are based on the infinitesimal jackknife, which is a heteroscedasticity-robust estimator of the variance-covariance matrix (Louis Jaeckel, 1972; White, 1980). The OLS models have been estimated using logits and probits for the two discrete outcomes (grade repetition and measles immunization); inferences are identical in all cases. We have also estimated the effect of Head Start and preschool on these two outcomes using Chamberlain conditional logits which allow for mother fixed effects (but which require randomly dropping one child from all families in which there is an odd number of children). Inferences drawn from these estimates are the same as those reported in Tables 4 and 5. Since the OLS fixed-effects coefficient estimates have a direct interpretation and do not require a balanced sample, we prefer to report OLS estimates.

TABLE 4—EFFECT OF PARTICIPATION IN HEAD START AND PRESCHOOL ON PPVT SCORE AND ABSENCE OF GRADE REPETITION

	OLS-unadjusted			OLS-adjusted			Mother fixed effects		
Variable	White (i)	African-American (ii)	Difference (iii)	White (iv)	African-American (v)	Difference (vi)	White (vii)	African-American (viii)	Difference (ix)
A. Dependent Variable: PPVT Score									
Head Start[a]	−5.621	1.037	−6.658	−0.383	0.739	−1.122	5.875	0.247	5.628
	(1.570)	(1.223)	(1.990)	(1.453)	(1.135)	(1.844)	(1.520)	(1.358)	(2.038)
Other preschool[b]	9.077	2.007	7.070	1.679	−0.790	2.469	1.173	0.615	0.557
	(1.275)	(1.481)	(1.955)	(1.171)	(1.311)	(1.759)	(1.296)	(1.296)	(1.833)
Constant	31.512	13.762	17.749	−106.706	−49.201	−57.505	·	·	·
	(0.783)	(0.823)	(1.136)	(16.306)	(15.846)	(22.737)			
F (Head Start = preschool)	75.38	0.40	36.22	1.56	1.21	2.77	7.45	0.06	4.81
	[0.00]	[0.53]	[0.00]	[0.21]	[0.27]	[0.10]	[0.01]	[0.81]	[0.03]
F (all covariates)	43.62	0.99	133.49	71.51	15.70	79.78	3.75	3.13	4.31
	[0.00]	[0.37]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
R^2	0.03	0.01	0.14	0.27	0.19	0.34	0.73	0.68	0.75
Sample size	2,319	1,158	3,477	2,319	1,158	3,477	2,319	1,158	3,477
B. Dependent Variable: Probability Never Repeated Grade									
Head Start[a]	−0.035	−0.010	−0.025	0.004	0.000	−0.004	0.473	0.008	0.465
	(0.058)	(0.061)	(0.084)	(0.061)	(0.064)	(0.088)	(0.122)	(0.098)	(0.158)
Other preschool[b]	0.029	−0.069	0.098	−0.005	0.100	0.095	0.061	0.163	−0.102
	(0.062)	(0.085)	(0.104)	(0.063)	(0.088)	(0.106)	(0.099)	(0.125)	(0.158)
Constant	0.654	0.537	0.118	0.487	0.049	0.572	·	·	·
	(0.031)	(0.043)	(0.052)	(0.810)	(0.882)	(1.191)			
F (Head Start = preschool)	0.76	0.47	1.20	0.02	1.30	0.61	8.40	1.22	8.05
	[0.38]	[0.49]	[0.27]	[0.90]	[0.26]	[0.44]	[0.01]	[0.27]	[0.01]
F (all covariates)	0.39	0.34	2.82	2.50	1.15	2.21	3.57	1.26	2.35
	[0.68]	[0.72]	[0.02]	[0.00]	[0.32]	[0.00]	[0.00]	[0.28]	[0.01]
R^2	0.01	0.01	0.01	0.08	0.05	0.08	0.62	0.59	0.61
Sample size	414	314	728	414	314	728	414	314	728

Notes: Standard errors are reported in parentheses below the coefficients; *p* values are given in brackets below the *F* statistics. Variance-covariance matrices were estimated by the method of infinitesimal jackknife for PPVT scores. OLS-adjusted regressions include controls for child age, gender, and whether first born, (log) household permanent income, mother's education, mother's AFQT score, mother's height, number of siblings when the mother was age 14, and grandmother's education. Fixed-effect models include controls for child age, gender, whether first born, and household income at age 3.

[a]Dummy variable = 1 if participated in Head Start.

[b]Dummy variable = 1 if participated in other preschool.

education, her AFQT score, her height, the number of siblings in the mother's household when she was age 14, and the education of the maternal grandmother. The fixed-effects models include child age, gender, and whether the child is the first born, as well as household income at the time the child was age 3.[21]

[21]It turns out that while these controls do affect the outcomes, their inclusion has only a small (depressing) impact on the estimated effects of Head Start and preschool. Inferences are not changed in any cases, and so only the controlled fixed-effects estimates are reported in the tables. We have also experimented with OLS models that include such potentially endogenous variables as number of children under age 18 in the household, mother's age at first birth, employment, and marital status (when the child was 3). The latter two covariates have also been included in fixed-effects models. In all cases, the results are qualitatively similar to those discussed below. All regressions also include controls to identify cases in which covariates are missing. Since not all children are eligible for all questions and some were not tested, sample sizes vary across the outcomes. They are reported at the bottom of each panel.

TABLE 5—EFFECT OF PARTICIPATION IN HEAD START AND PRESCHOOL ON MEASLES IMMUNIZATION AND HEIGHT FOR AGE

	OLS-unadjusted			OLS-adjusted			Mother fixed effects		
Variable	White (i)	African-American (ii)	Difference (iii)	White (iv)	African-American (v)	Difference (vi)	White (vii)	African-American (viii)	Difference (ix)
	A. *Dependent Variable: Probability of Measles Immunization*								
Head Start[a]	0.152	0.167	−0.015	0.030	0.072	−0.043	0.082	0.094	−0.011
	(0.025)	(0.026)	(0.037)	(0.019)	(0.020)	(0.028)	(0.030)	(0.033)	(0.045)
Other preschool[b]	0.021	−0.018	0.039	0.044	0.003	0.041	0.123	0.050	0.073
	(0.018)	(0.029)	(0.035)	(0.015)	(0.022)	(0.027)	(0.024)	(0.034)	(0.042)
Constant	0.698	0.714	−0.016	0.256	0.268	0.012	·	·	
	(0.011)	(0.017)	(0.021)	(0.207)	(0.280)	(0.356)			
F (Head Start = preschool)	24.85	35.50	1.67	0.48	8.23	6.58	1.42	1.21	2.52
	[0.00]	[0.00]	[0.20]	[0.49]	[0.00]	[0.01]	[0.23]	[0.27]	[0.11]
F (all covariates)	19.01	25.30	18.53	240.01	89.48	129.37	3.10	3.27	3.16
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
R^2	0.01	0.03	0.02	0.45	0.47	0.46	0.69	0.68	0.69
Sample size	2,829	1,336	4,165	2,829	1,336	4,165	2,829	1,336	4,165
	B. *Dependent Variable: Height for Age (Percentage of Median)*								
Head Start[a]	−0.171	1.024	−1.195	−0.207	0.452	−0.660	0.084	0.549	−0.465
	(0.330)	(0.382)	(0.505)	(0.328)	(0.364)	(0.490)	(0.399)	(0.540)	(0.671)
Other preschool[b]	0.927	0.477	0.450	0.719	0.320	0.393	0.582	0.182	0.399
	(0.265)	(0.485)	(0.553)	(0.264)	(0.475)	(0.543)	(0.318)	(0.509)	(0.600)
Constant	99.627	100.694	−1.067	63.214	55.666	7.548	99.895	97.708	
	(0.166)	(0.278)	(0.324)	(4.144)	(6.030)	(7.318)	(2.570)	(4.139)	
F (Head Start = preschool)	9.71	1.32	7.72	6.10	0.08	3.08	1.25	0.34	1.26
	[0.00]	[0.25]	[0.01]	[0.01]	[0.78]	[0.08]	[0.26]	[0.56]	[0.26]
F (all covariates)	7.54	3.60	12.57	14.03	11.15	13.61	1.95	1.89	1.96
	[0.00]	[0.03]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
R^2	0.01	0.01	0.01	0.06	0.09	0.08	0.58	0.56	0.58
Sample size	2,789	1,303	4,092	2,789	1,303	4,092	2,789	1,303	4,092

Notes: Standard errors are reported in parentheses below the coefficients; p values are given in brackets below the F statistics. Variance-covariance matrices were estimated by the method of infinitesimal jackknife for height-for-age. OLS-adjusted regressions include controls for child age, gender, and whether first born, (log) household permanent income, mother's education, mother's AFQT score, mother's height, number of siblings when the mother was age 14, and grandmother's education. Fixed-effect models include controls for child age, gender, whether first born, and household income at age 3.

[a]Dummy variable = 1 if participated in Head Start.

[b]Dummy variable = 1 if participated in other preschool.

A. *Measures of Academic Performance*

The first three columns of panel A in Table 4 indicate that the PPVT scores of white children are, on average, about twice those of African-American children. In part, this is a reflection of the fact that whites live in higher-income households than African-Americans. But that is only part of the story since nonparametric estimates indicate that white children have higher PPVT scores at all income levels (Currie and Thomas, 1993).

Within racial groups, white children who attended other preschools or no preschool tend to score better, on average, than Head Start children. For example, white Head Start children score an average of 5 percentile points lower on the PPVT than white children who did not attend preschool and 15 percentile points lower than whites who attended other preschools. Both of these differences are statistically significant. In contrast, there are no statistically significant differences among African-Americans.

Moving across the columns in panel A in Table 4 shows the importance of controlling adequately for all observed and unobserved family characteristics associated with selection into Head Start. Column (iv) suggests that, among whites, the difference between the PPVT scores of Head Start and other children disappears when observables are controlled.

However, column (vii) demonstrates that when *unobserved* differences between families are controlled, using mother fixed effects, participation in Head Start is actually associated with a significant 6-percentile-point *increase* in the PPVT score relative to no preschool, while participation in other preschools has no statistically significant effect on test scores. The gap between the effects of Head Start and other preschools is statistically significant. The difference between columns (iv) and (vii) indicates that, consistent with Haskins's (1989) observations, it is the most disadvantaged white children in terms of unobservables who are selected into the Head Start program. On the other hand, controlling for unobservables has little effect on the estimated coefficient for other preschools, once observable characteristics are included in the model.

The results for African-Americans indicate that selection may be less important for them: there are no statistically significant effects of Head Start or preschool in any of the three specifications. Column (ix) shows that the difference between the Head Start effects for whites and African-Americans is large—nearly 6 points—and statistically significant.

We turn next to our second measure of academic performance: absence of grade repetition. The first three columns of panel B in Table 4 show that about one-third of white and nearly half of African-American sample children age 10 or older are reported to have repeated a grade.[22] Although white Head Start children are about 20-percent more likely to have repeated a grade than white children who attended other preschools, this difference is not statistically significant. Among African-Americans, the gaps between the different groups of children are even smaller. The OLS estimates in columns (iv)–(vi) also indicate that there are no statistically significant effects of type of preschool on the probability of grade repetition.

However, the fixed-effects estimates, shown in columns (vii)–(ix) indicate that whites who attended Head Start are 47-percent less likely to repeat a grade, relative to their siblings who did not attend preschool. Those who attended another type of preschool are no less likely to have repeated a grade than their siblings who stayed at home. The "difference in differences," that is, the gap between the effect of Head Start and the effect of preschool, is also large (40 percent) and statistically significant (p value = 0.01).

In contrast, attendance at either type of preschool has no statistically significant effect on the probability of grade repetition among African-Americans (although the point estimate of the coefficient on other preschools is large). Once again, the racial difference in the impact of Head Start is statistically significant.

In sum, after controlling for mother-specific observables and unobservables we find that, for whites, the academic performance of Head Start children is significantly better than that of siblings who stayed at home. In addition, the estimated effects of Head Start are much greater than those of attending other preschools once both observable and unobservable characteristics of families are controlled. Among whites, this difference-in-difference estimate is statistically significant both for PPVT scores and for grade repetition. Among African-Americans, however, the tale is more dismal: neither Head Start nor other preschools is associated with enhanced academic performance.

B. *Measures of Health Status*

Table 5 presents the estimated effects of participation in Head Start and other

[22]The rates of grade repetition reported in the NLSCM are in line with those cited in other sources. For example, Shepard and Mary Smith (1990) report that 6 percent of all public-school students are retained in grade annually. Hence, by the 9th grade, approximately half of public-school students have been retained in grade.

preschools on two measures of health status: immunization probabilities and height-for-age. The first three columns of panel A suggest that both whites and African-Americans are about 15-percent more likely to have had a measles shot if they attended Head Start rather than another preschool. These gaps are statistically significant. There is little difference in these means between the other-preschool and no-preschool children, which is surprising in light of the differences in family background between these two groups. For both racial groups, the difference in differences between Head Start and other preschool children is statistically significant.

Column (iv) shows that, among whites, controlling for observables reduces the effects of Head Start to zero, while the effect of attending other preschools increases slightly and becomes statistically significant. Among African-Americans, the inclusion of observables reduces the Head Start advantage by over half, but it remains significant.

When fixed effects are included [in columns (vii) and (viii)], we find that Head Start is associated with an 8–9-percent higher probability of being immunized among both white and African-American children. Attendance at other preschools is also associated with a higher probability of being immunized. While the estimated coefficient on preschools is greater than the estimated effect of Head Start among whites, the difference is not statistically significant. Among African-Americans, the effect of other preschools is not significantly different from zero, but it is not significantly different from the coefficient on Head Start either. Relative to other preschools then, there is no health-care "premium" associated with Head Start.

The relationship between type of preschool and child height-for-age is presented in panel B of Table 5. The unadjusted OLS estimates [in columns (i) and (ii)] show that white children who attend preschools are significantly taller than other white children, but that African-American children who attend Head Start are taller still. The coefficient on preschool in column (ii) is not statistically significant. However, the hypothesis that Head Start and preschool have the same effect on the height-for-age of African-Americans cannot be rejected with any confidence.

When observables are controlled in columns (iv) and (v), the preschool effect among whites is somewhat weaker, but it remains significant. A good part of the difference between columns (i) and (iv) is accounted for by the influence of maternal height, although other measures of maternal human capital (her education) are also statistically significant. This result suggests that height is influenced both by genetic factors and by parental investments in the health and human capital of children. The fixed-effects estimates for whites, in column (vii), eliminate the influence of all shared genetic characteristics as well as all other fixed maternal characteristics; this results in a further weakening of the relationship between preschool and child height, although it remains positive and significant, albeit at a 7-percent level.

Among African-Americans, the inclusion of observable maternal and child characteristics [in column (v)] cuts the positive correlation between Head Start and child height by more than half. It also becomes statistically insignificant. Similarly, column (viii) shows that we do not find any statistically significant effect of either Head Start or preschool when fixed effects are included in the model.

These results suggest that the positive correlation between Head Start and height-for-age among African-Americans that is noted in column (ii) reflects the selection of taller African-American children into the program. This impression was confirmed by estimating regressions of birth weight on participation in the program. Birth weight is highly correlated with future child height-for-age, but it could not possibly be influenced by future participation in Head Start. We found that African-American children who attended Head Start were heavier at birth than African-American children who did not. For whites, however, we did not find any correlation between birth weight and enrollment in Head Start or preschool, so the positive effect of preschool on height-for-age appears to be a genuine program effect.

Thus, in spite of positive effects of attendance at Head Start or other preschools on the utilization of preventive health care, the large nutritional component of the Head Start program, and the fact that other preschools appear to have positive effects on the growth of some children, we find no evidence that participation in Head Start has an effect on nutritional and health status as measured by height-for-age.

C. *Differences in the Effect of Head Start Among Whites and African-Americans*

The cognitive effects of Head Start appear to vary dramatically by race, even when selection into the programs is taken into account: Head Start has a smaller effect on the test scores and schooling attainment of African-Americans than on the test scores and academic achievement of whites. Why does race matter?

One hypothesis is that there is heterogeneity in the Head Start programs that serve children of different races. While most programs are in compliance with most standards, slightly over 11 percent of Head Start operators monitored in 1993 were found to be out of compliance with 50 or more of 222 items reviewed, while another 18 percent needed improvement in 26–50 areas (U.S. Department of Health and Human Services, 1993). It is possible that African-American children are more likely to be served by inferior programs. Unfortunately, this hypothesis cannot be tested directly, as we have no information about individual programs.

An alternative hypothesis is that the benefits of compensatory education depend both on the program itself and on the child's home background, including, for example, the level of resources at home, as well as the type and quality of school attended after Head Start. To the extent that African-American children come disproportionately from more disadvantaged homes, located in poorer communities, and attend troubled schools, one might expect Head Start to have either smaller initial effects or effects that dissipate more quickly over time.

We begin to address these issues by estimating models that allow the effects of Head Start and other preschool attendance to vary with maternal AFQT and child age. These results are shown in Table 6. All of the models include fixed effects. We do not show results for height-for-age, since there were no significant effects of Head Start (or significant racial differences) to be explained.

Maternal AFQT can be regarded as an index of maternal background or of human capital. It is highly correlated with years of education, as shown in Figure 1, but has the advantage of being a continuous rather than discrete variable. If children from better backgrounds gain more from Head Start or preschool, then the interactions between AFQT and Head Start or preschool will be positive.

The results in columns (i) and (ii) of panel A indicate that the positive effects of Head Start on PPVT increase with AFQT among both whites and African-Americans. However, neither interaction is statistically significant. The interactions between AFQT and preschool are also insignificant. Turning to the absence of grade repetition, column (iv) shows that, among whites, there is a large and statistically significant interaction between Head Start and AFQT: a 10-point increase in the normalized maternal AFQT score reduces the probability of failure among Head Start children by 8 percent. We do not find any similar effect among African-Americans [column (v)]. Moreover, the difference between whites and African-Americans in the AFQT× Head Start interaction is significant (at the 8-percent level) [column (vi)]. We do not find any significant interactions between preschool attendance and AFQT for either race.

Finally, the results shown in columns (vii)–(ix) indicate that, in the regressions for immunization probabilities, interactions between Head Start and AFQT and between other preschools and AFQT are all positive but not statistically significant. In sum, there is weak evidence that children from better

TABLE 6—FIXED-EFFECTS ESTIMATES OF IMPACT OF HEAD START AND PRESCHOOL ON CHILD WELL-BEING, INCLUDING INTERACTIONS WITH MATERNAL HUMAN CAPITAL AND CHILD AGE

	Dependent variable: PPVT score			Dependent variable: probability never repeated grade			Dependent variable: probability of measles immunization		
Variable	White (i)	African-American (ii)	Difference (iii)	White (iv)	African-American (v)	Difference (vi)	White (vii)	African-American (viii)	Difference (ix)
A. Include interactions with AFQT of mother:									
Head Start[a]	4.826	−0.462	5.288	0.123	−0.006	0.130	0.046	0.083	−0.036
	(2.136)	(1.821)	(2.807)	(0.186)	(0.146)	(0.239)	(0.047)	(0.050)	(0.069
Head Start ×AFQT of mother	2.032	2.103	−0.072	0.831	0.040	0.791	0.060	0.030	0.029
	(3.352)	(4.810)	(5.863)	(0.323)	(0.316)	(0.452)	(0.062)	(0.099)	(0.119)
Other preschool[b]	2.278	−1.300	3.578	0.217	0.210	0.007	0.086	0.048	0.038
	(2.170)	(1.483)	(2.628)	(0.204)	(0.192)	(0.281)	(0.044)	(0.049)	(0.067)
Other preschool ×AFQT of mother	−1.396	4.545	−5.941	−0.203	−0.135	−0.068	0.045	0.007	0.038
	(2.724)	(3.764)	(4.647)	(0.246)	(0.419)	(0.473)	(0.044)	(0.082)	(0.095)
F (Head Start and interaction)	7.72	0.10	3.39	11.48	0.01	5.39	4.04	4.00	0.16
	[0.00]	[0.91]	[0.03]	[0.00]	[0.99]	[0.01]	[0.02]	[0.02]	[0.85]
F (Preschool and interaction)	0.74	0.74	1.04	0.59	0.89	0.02	14.14	1.12	0.87
	[0.48]	[0.48]	[0.35]	[0.56]	[0.41]	[0.98]	[0.00]	[0.33]	[0.42]
F (all covariates)	3.74	3.12	4.29	3.79	0.95	2.26	154.10	80.26	117.00
	[0.00]	[0.00]	[0.00]	[0.00]	[0.48]	[0.00]	[0.00]	[0.00]	[0.00]
R^2	0.73	0.68	0.75	0.63	0.59	0.62	0.69	0.68	0.69
B. Include Interactions with Age of Child:									
Head Start[a]	6.878	6.845	0.033	0.266	0.218	0.048	0.266	0.258	0.008
	(2.397)	(1.933)	(3.080)	(0.311)	(0.295)	(0.429)	(0.045)	(0.048)	(0.067)
Head Start ×age of child[c]	−0.192	−1.278	1.086	0.025	−0.025	0.050	−0.043	−0.035	−0.008
	(0.410)	(0.309)	(0.513)	(0.036)	(0.033)	(0.049)	(0.008)	(0.007)	(0.011)
Other preschool[b]	0.165	2.970	−2.805	0.173	0.726	−0.553	0.128	0.045	0.083
	(1.832)	(1.863)	(2.613)	(0.350)	(0.461)	(0.572)	(0.031)	(0.046)	(0.057)
Other preschool ×age of child[c]	0.264	−0.467	0.731	−0.014	−0.074	0.061	−0.002	0.002	−0.004
	(0.362)	(0.386)	(0.529)	(0.041)	(0.059)	(0.071)	(0.006)	(0.009)	(0.011)
F (Head Start and interaction)	7.89	8.86	5.26	7.68	0.29	4.78	18.53	15.00	0.48
	[0.00]	[0.00]	[0.01]	[0.00]	[0.75]	[0.01]	[0.00]	[0.00]	[0.617]
F (Preschool and interaction)	0.64	1.27	0.96	0.25	1.69	0.50	13.73	1.21	1.46
	[0.53]	[0.28]	[0.38]	[0.78]	[0.19]	[0.61]	[0.00]	[0.30]	[0.23]
F (all covariates)	3.74	3.19	4.31	2.76	1.17	1.92	160.23	85.57	122.61
	[0.00]	[0.00]	[0.00]	[0.01]	[0.32]	[0.02]	[0.00]	[0.00]	[0.00]
R^2	0.73	0.68	0.75	0.62	0.59	0.61	0.69	0.69	0.69

Notes: Standard errors are reported in parentheses below the coefficients; p values are given in brackets below the F statistics. The variance-covariance matrix for PPVT models was calculated by the method of infinitesimal jackknife. All models include controls for child age, gender, whether first born, and household income at age 3.

[a] Dummy variable = 1 if participated in Head Start.

[b] Dummy variable = 1 if participated in other preschool.

[c] Age of child is expressed as years since age 5.

backgrounds, as measured by maternal AFQT, gain more from Head Start, but the interaction is only statistically significant in the regressions for absence of grade repetition among whites.

Interactions between the type of preschool and child age allow us to address the question of whether the effects of Head Start and other preschools persist as the child grows older. These estimates are reported in panel B of Table 6.[23] Columns (i)

[23] In these regressions with age interactions, the age of the child is measured in years older than age 5 (which is when the child will have completed Head Start or preschool). The interactive effect can thus be interpreted as a measure of the depreciation of the benefit of preschool for each year since completion.

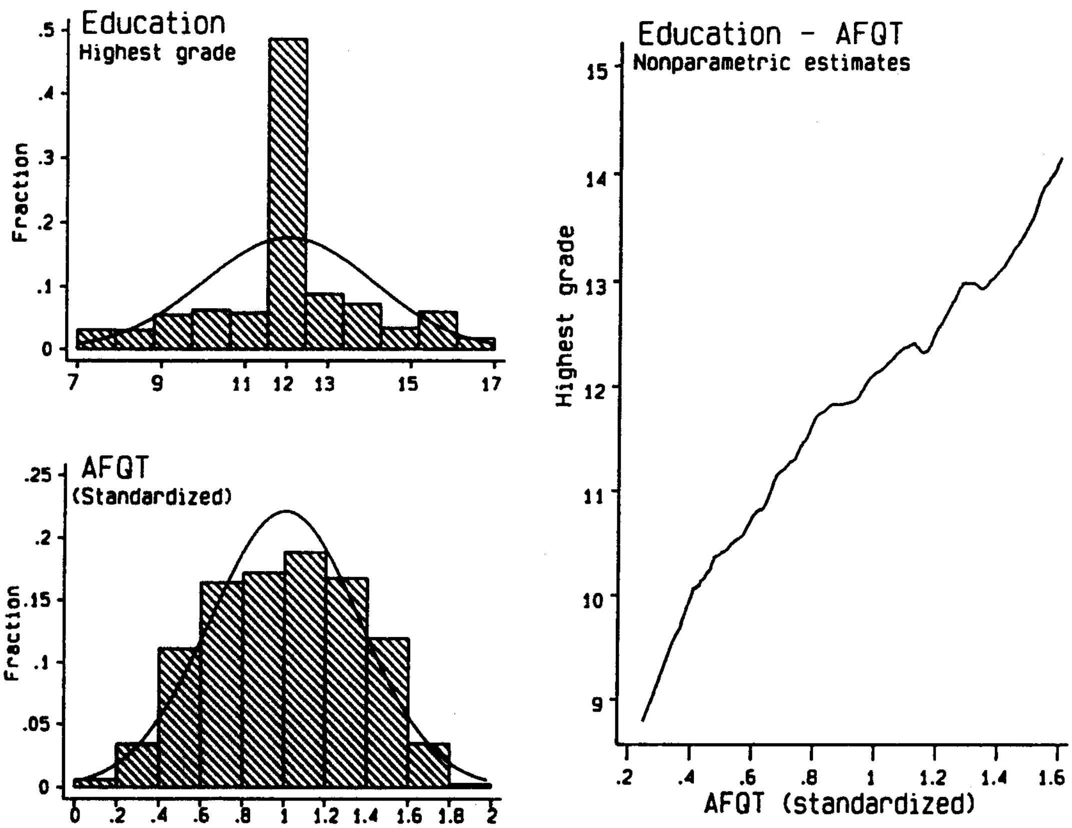

FIGURE 1. RELATIONSHIP BETWEEN MATERNAL AFQT AND HIGHEST GRADE COMPLETED: NONPARAMETRIC ESTIMATES

and (ii) contain one of our most interesting results. Not only is the direct effect of Head Start large, positive, and significant for *both* whites *and* African-Americans, but the effect (of nearly 7 percentile points) is essentially identical for both racial groups.

This finding stands in sharp contrast with the results discussed above. In Table 4 we found that Head Start was associated with higher PPVT scores among whites but that African-American children did not enjoy similar benefits. The difference lies in the age interactions. While the interactions are always negative, for whites they are small and statistically insignificant, while for African-Americans they are large and significant. Thus, for example, by age 10 African-American children have lost any benefits they gained from Head Start, while 10-year-old white children retain a gain of 5 percentile points. There is no evidence of a similar interaction effect among children who attend preschool.

Our results for African-Americans are thus consistent with those of earlier studies (which tended to be dominated by African-American subjects). When we focus on only young African-American children, we find clear benefits of Head Start. However, in a sample of African-American children of all ages, there is no effect of Head Start. This is because the benefits die out very quickly. In contrast, white children experience the same initial gains from Head Start, but they retain these benefits for a much longer period.

It is also possible to ask whether the rate at which the benefits of Head Start dissipate among African-Americans depends on the environment at home. To do this, we have estimated models (not shown) that include "triple interactions" among age, Head Start, and maternal AFQT. If children from better backgrounds retain the gains from Head Start longer, then this triple interaction will be positive (offsetting the fact that the beneficial effect declines with age). We found no evidence for this hypothesis: the coefficient on the triple interaction was -0.04 with a t statistic of 0.09. To the extent that the maternal AFQT score does capture home background, this suggests that at least part of the racial difference in the benefits of Head Start reflects heterogeneity in program delivery or in the types of schools that whites and African-Americans attend once they leave the program.

Columns (iv)–(vi) of panel B in Table 6 indicate that there are no statistically significant interactions between age and type of preschool in the regressions for absence of grade repetition. In part, this reflects the fact that the question was only asked of children over 10 years old, so there is relatively little variation in the age ranges of the respondents.

Older children who attended Head Start are less likely to have been immunized, as shown in columns (vii)–(ix) of panel B in Table 6. This could be due to recall error, if parents of older Head Start children tend to forget that a child has been immunized. However, if the result reflects recall error, then one might expect the same pattern among children who went to preschool, and there is no evidence in support of this "forgetting hypothesis" among these children.[24] Thus, it is likely that the result reflects an increasing emphasis on the health-care portion of the Head Start program in recent years.

[24] In addition, there is evidence that recall error tends to decline with education (see e.g., James P. Smith et al., 1991). If better-educated mothers are likely to forget that their children were immunized and recall error is the explanation, then we would expect the interaction among maternal AFQT, age, and Head Start to be positive. For whites, it is positive but small and insignificant; for African-Americans, the triple interaction is negative, small, and also insignificant.

Since, within families, the firstborn must be the oldest, it may be that differences in the impact of Head Start among children of different ages is picking up a birth-order effect. Adding interactions between type of preschool and whether the child is the firstborn does not affect the inferences discussed above. However, these interactions do provide some information about the extent of spillover to other siblings.

If the benefits of Head Start spill over from older to younger siblings, then in the fixed-effects estimates, the firstborn will appear to have gained the least from the program, and an interaction between Head Start and firstborn will be negative. The point estimates on these interactions are indeed negative for all four outcome measures, and for both races. The interactions are statistically significant in the case of measles shots, an outcome for which information externalities are likely to be very important. These might reflect parental learning about the importance of immunizations or learning about health resources available in the community. Among African-Americans, the Head Start × firstborn interaction is also significantly negative for PPVT scores. In contrast, the evidence for spillovers from older siblings who attended other preschools is weaker. This suggests that, if anything, the difference-in-difference estimates of the effects of Head Start relative to preschool tend to understate the positive impact of Head Start.

V. Discussion and Conclusions

In closing, we offer some observations about the likely importance of the effects we have identified. Participation in Head Start is associated with an increase in the PPVT scores of white children of 5.6 percentile points. Table 4 indicates that the gap in PPVT scores between Head Start children and those who attended other preschools is 15 points. Hence, our results suggest that Head Start closes over one-third of the gap between children attending the program and their more advantaged peers. Moreover, contrary to many previous studies, we find that this beneficial effect persists at least into adolescence among white children.

We also find that white children over nine years old who attended Head Start are 47-percent less likely to have repeated a grade than other white children. Given that 35 percent of white children who did not attend preschool repeated a grade, this translates into a reduction of 16 percentage points in the probability of repeating a grade. A gain of this size more than closes the gap between white Head Start children and their peers who attended other preschools.

It is difficult to evaluate the long-run impacts of the gains in test scores. As discussed above, previous research indicates that children who perform poorly in early grades are more likely than other children eventually to drop out of school altogether. However, it is not clear to what extent this relationship is causal. Nevertheless, we can take some representative estimates from the education literature and extrapolate using our data. Ensminger and Slusarcick (1992) find that children who received C's and D's in Grade 1 are twice as likely to drop out of school as children who received A's and B's. Assuming that the wage gain to an additional year of high school is 8 percent, that most children would drop out in grade 11, and that the increase in test scores we find would be enough to move a child from a C to a B average, enrolling a white child in Head Start could increase his or her expected future wage by 4 percent.[25]

We are on somewhat firmer ground evaluating the likely effects of reductions in the probability of grade repetition. In a study of more than 140,000 students from three different school districts, Grissom and Shepard (1989) found that students who were retained in grade were 30-percent more likely to drop out of school, even when achievement on standardized tests, socioeconomic status, gender, and ethnicity were controlled. They also found that grade repetition was disproportionately concentrated in early grades, and especially first grade, which means that their findings should be relevant to our sample. Hence, the 16-percentage-point decline in the probability of repeating a grade associated with Head Start could lead to a 5-percent decline in the probability of dropping out of high school among white children.

It is notable that enrollment in other preschools has no significant effects (positive or negative) on test scores or on the probability of grade repetition among white or African-American children. For whites, the differences between the effects of Head Start and those of preschool are statistically significant. Given that children in Head Start are disadvantaged relative to even their own siblings, the fact that Head Start has bigger effects than preschool strongly suggests that our estimates are capturing a genuine effect of the program rather than selection bias.

Turning to the effects on the utilization of health care, and on health status, we find that both white and African-American children are 8–11-percent more likely to be immunized if they attended either Head Start or another preschool than if they attended no preschool. These results are consistent with those surveyed in McKey et al. (1985) because they suggest that children in Head Start are gaining access to preventive health care. Once again, it is difficult to place a value on these services. An upper bound is provided by the average cost of providing outpatient services to an AFDC (Aid for Families with Dependent Children) child covered by Medicaid, or $468 per year in 1990 (U.S. House of Representatives, 1992).

It may be objected that the provision of preventive services under the auspices of Head Start duplicates coverage available to many poor children under the Medicaid program and that, therefore, these additional services have little value. However, only 39 percent of eligible children participate in the Early and Periodic Screening, Diagnosis, and Treatment (EPSDT) component of the Medicaid program (U.S. Department of Health and Human Services,

[25] In order to do a full cost/benefit analysis, one would have to take account of the fact that benefits in the form of increases in wages will be deferred for at least 15 years and hence should be discounted.

July 1990), and in the District of Columbia less than half of Medicaid-eligible children receive all their immunizations despite the fact that new mothers receive written reminders (*Washington Post*, 1993). Furthermore, in contrast to the results reported here, we found no evidence that Medicaid coverage increased immunization rates in the NLSCM. Hence, we suggest that the possibility that the Head Start program plays an important role in the provision of preventive services cannot be dismissed out of hand.

Finally, we turn to the $2.2 billion question—is the money spent on Head Start a worthwhile investment, or are there less expensive ways of providing similar benefits? The results for African-American children suggest that the primary long-term benefits of Head Start are in terms of access to health care. Hence, it is appropriate to compare Head Start's price tag of $3,500 per child to the $468 estimate for health services cited above. This comparison suggests that when viewed strictly in terms of lasting benefits provided to children, Head Start programs serving African-American children are not cost-effective.[26] Whether this result reflects inadequacies in these programs, or the limited opportunities available to African-American children after they leave the program, is sure to be a hotly debated question.

In contrast, the results for white children suggest that the potential gains are much larger than the costs, since even a small decline in the high-school dropout rate has the potential to pay for itself in terms of future wage gains. If the factors preventing African-American children from maintaining the gains they achieve in Head Start could be removed, the program could probably be judged an incontrovertible success.

[26]The Head Start program also provides benefits to other family members. For example, more than one-third of the employees are parents of current or former Head Start students (Valora Washington and Ura Jean Oyemade, 1987), and expenditures on Head Start comprised 20 percent of all federal expenditures on child care in 1986 (Alfred Kahn and Sheila Kamerman, 1987). A complete cost/benefit analysis would have to take account of these factors. On the other hand, public support for the program seems to be based on the perception that it benefits children, rather than on the desire to provide these other benefits.

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94-10	RP-319	*The Work-Employment Distinction Among New Mothers* [The Journal of Human Resources, Vol. 29, No. 2, pp. 277-303]	Jacob Alex Klerman Arleen Leibowitz
94-09	RP-322	*The Demographic Transition in Southern Africa: Reviewing the Evidence from Botswana and Zimbabwe* [Demography, Vol. 31, No. 2, May 1994, pp. 217-227]	Duncan Thomas Ityai Muvandi
94-08	RP-321	*The Demographic Transition in Southern Africa: Another Look at the Evidence from Botswana and Zimbabwe* [Demography, Vol. 31, No. 2, May 1994, pp. 185-207]	Duncan Thomas Ityai Muvandi
94-07	RP-301	*Gender and Life Cycle Differentials in the Patterns and Determinants of Adult Health* [Journal of Human Resources, Vol. 28, No. 4, Fall 1993, pp. 791-837]	John Strauss Paul Gertler Omar Rahman Kristin Fox
94-06	RP-300	*American Families: Policy Issues* [Population Index, Vol. 59, No. 4, Winter 1993, pp. 547-566]	Julie DaVanzo M. Omar Rahman Kul T. Wadhwa
94-05	RP-299	*Converging Employment Patterns of Black, White, and Hispanic Women: Return to Work After First Birth* [Journal of Marriage and the Family, Vol. 56, February 1994, pp. 209-217]	Young-Hee Yoon Linda J. Waite
94-04	RP-284	*Living Arrangements of Older Malaysians: Who Coresides with Their Adult Children?* [Demography, Vol. 31, No. 1, February 1994, pp. 95-113]	Julie DaVanzo Angelique Chan
94-03	RP-283	*American Families: Trends and Correlates* [Population Index, Vol. 59, No. 3, Fall 1993, pp. 350-386]	Julie DaVanzo M. Omar Rahman
94-02	RP-287	*How Economic Development and Family Planning Programs Combined to Reduce Indonesian Fertility* [Demography, Vol. 31, No. 1, February 1994, pp. 33-63]	Paul Gertler Jack Molyneaux

94-01	RP-277	*Using Regional Data to Reexamine the Contribution of Demographic and Sectoral Changes to Increasing U.S. Wage Inequality* [In J. H. Bergstrand, T. F. Cosimano, J. W. Houck, and R. G. Sheehan (eds.), The Changing Distribution of Income in an Open U.S. Economy, New York: NY: North Holland, 1994, pp. 183-216]	Lynn A. Karoly Jacob Alex Klerman