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ABSTRACT

This study used data from the National Longitudinal Survey of Youth to investigate the effects of participation in Head Start preschool programs on a range of child outcomes. In order to control for selection into the Head Start program, comparisons are drawn between siblings and also between the relative benefits of Head Start versus other preschool programs. The study found large and significant gains associated with attending Head Start, as measured by test scores. These gains were evident relative to children who did not attend preschool, as well as to children who attended non-Head Start preschools. The study also found that while whites and blacks experienced initial gains in test scores as a result of participation in Head Start, the gains of blacks were quickly lost while the gains of whites persisted into adulthood. Head Start participation significantly reduced the probability that a white child would repeat a grade, but had no effect on grade repetition among black children. Relative to children who did not attend preschool, both whites and blacks gained greater access to preventive health services in Head Start and non-Head Start programs. A list of related RAND Corporation papers is included. (Contains 68 references.) (MDM)

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Does Head Start Make a Difference?

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Abstract

Data from a national sample of children are used to investigate the effects of participation in Head Start on a range of child outcomes. In order to control for selection into the program, comparisons are drawn between siblings and also between the relative benefits associated with attending Head Start, on one hand, and other preschools, on the other. There are large and significant gains associated with attending Head Start, as measured by test scores. This is true, relative to children who attend no preschool and also relative to those who attend other preschools. There are also important racial differences in these benefits. Both whites and African-Americans experience initial gains in test scores as a result of participation in Head Start. But, among African-Americans, the gains are quickly lost whereas, for whites, the gains persist well into adulthood. As a result, perhaps, Head Start significantly reduces the probability that a white child will repeat a grade, but has no effect on grade repetition among African-American children. In contrast, relative to children who attend no preschool, both whites and African-Americans gain greater access to preventive health services, as measured by immunization rates although children who attend other preschools enjoy similar benefits.

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.[1] Begun in 1964, as part of the "War on Poverty", Head Start has enjoyed great public and bi-partisan support. Presidents Bush and Clinton both pledged to increase federal funding so that all eligible children could be served. Today 622,000 children, roughly 28% of eligible 3 to 5 year olds, are served at a cost of $2.2 billion per year, or approximately $3,500 per child (Stewart, 1992).

Policy makers 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 shows that the evidence in support of lasting effects of Head Start is limited. While Head Start has been shown to have significant immediate effects on IQ, many studies find that these gains appear to decline over time becoming insignificant by the third grade. Head Start is also said to reduce grade repetition, high school dropout rates, and teen pregnancies, and to improve children's medical care and health status (c.f. Children's Defense Fund, 1992). Still, some critics have attacked the "Head Start Scam" (Hood, 1992), arguing that Head Start has little, if any, long-run impact on children.

In this study we use data from the National Longitudinal Survey of Youth (NLSY) and the National Longitudinal Survey's Child-Mother file (NLSCM) to re-examine the impact of Head Start. One of our key findings is that there are significant racial differences in the effects of Head Start. Nationally, 3% of Head Start enrollees are African-American and many previous studies have examined samples of predominantly African-American children. When white children have been included, they have usually been too few in number to allow separate analyses. Lee *et al.* (1990) cite lack of attention to possible racial effects as a serious shortcoming of the previous Head Start literature.

Our study makes several additional contributions. This is the first study to look at a national sample of children who attended regular Head Start programs and to examine a broad range of outcomes. These include measures of the child's cognitive attainment, scholastic success, utilization of medical care,

[1] Federal guidelines require that 90% of the children served be from families with incomes below the federal poverty line; recently, more than 95% of children served have been below the poverty line (U.S. DHHS, 1993).

and nutritional status. Possible long-range effects of improvements in these measures are also considered.

Finally, careful attention is paid to the effects of possible non-random selection into the program. This is dealt with in two ways. First, models that incorporate mother fixed effects are estimated in order to control for unobserved household-level determinants of participation in Head Start. Second, we compare fixed effects estimates of 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 help control for possible biases in the mother fixed effects 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 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.

Participation in either Head Start or preschool is associated with improved utilization of preventive medical care, as proxied by immunization rates, relative to no preschool. This is true for both white and African-American children. In contrast, there is no evidence that Head Start has any effect on a longer-run indicator of health and nutritional status, child height-for-age.

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.

1. 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 (McKey *et al.*, 1985)[2] 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.[3]

The most convincing studies of the IQ effects of Head Start utilize a treatment and control design with random assignment.[4] However, 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. For example, as discussed above, many studies focus exclusively on African-American children.

The most widely cited evidence in support of the longer-term benefits of Head Start actually comes from experimental studies of model preschool programs such as the Perry Preschool Project or the

[2] There have been several other surveys of the Head Start literature. See Westinghouse Learning Corporation and Ohio University (1969), Bronfenbrenner (1975), Datta (1979), Horowitz and Paden (1973), and White (1985-86). Vinovskis (1993) shows that the debate about the efficacy of compensatory education in the U.S. dates back at least to the 1840's when 40% of all three year olds in Massachusetts were attending infant schools.

[3] The handful that have include: McDonald and Monroe (undated), Goodstein, Cawley, and Burrows (1975), Consortium for Longitudinal Studies (1983), Copple, Cline, and Smith (1987), Bee (1981), Hebbeler (1985), and Fuerst and 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.

[4] Other studies make use of a quasi-experimental design in which the comparison children are drawn from waiting lists for the Head Start program. Lee, Brooks-Gunn and Schnur (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.

Tennessee Early Training Project. These programs were funded at higher levels, involved more intensive interventions, and had better-trained staff than the typical Head Start program. For example, the Perry Preschool Project was funded at a rate of about $6,000 per child. Twenty years after the program, researchers found that the "treatments" were more likely to graduate 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 (Berrueta-Clement *et al*, 1984).[5]

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.

2. Methods

The key empirical problem facing us is that, as we will see below, children are not randomly selected into the Head Start program. Given the rules governing the program, we expect that Head Start children will be poorer than other children, and that they may also be disadvantaged in other observable ways. Estimates that do not take account of these differences are likely to under-estimate 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.

However, the economic model of the family (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 over-estimate the beneficial effects of Head Start.

If there are fewer places than child applicants, selection will also reflect the choices made by program administrators. Haskins (1989) cites evidence that local staff tend to select the most

[5] 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.

disadvantaged children to participate in Head Start, in which case estimates of the effects of Head Start that do not take account of variables used by staff to select participants will also be biased downwards.

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 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 that the inclusion of household fixed effects could bias estimated program effects towards 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 it is largely "noise" that 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

[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 participation in Head Start affected her child's outcomes only through the child's own participation in Head Start. Although maternal participation in Head Start is a significant predictor of the child's participation (Mott and 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. Nelson and Startz (1990) report that in these circumstances, IV estimates can be very misleading; see also Bound, Jaeger and Baker (1993) and Staiger and Stock (1993). In view of these results, we do not report IV estimates in this paper.

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/or 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 it there are no unobserved child-specific characteristics that also enter 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 upwards. 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. Second, 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 that suggests 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.

3. 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 25 through 32 of age 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

[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. One test that we employ, the PPVT, has been administered to the same child more than once but, again because of the design of the NLSCM, it is not possible to do a pre/post comparison of scores.

3 years old in the 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 a fifth -- participated in Head Start. In addition, due to over-sampling there are large enough numbers of African-Americans to allow separate examination of this group.[9]

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 whether the child has ever repeated 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). The following chart 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]

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[8] Examining only mothers with at least two age-eligible children reduces the sample by 14%. 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 below.

[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 differences between them and African-Americans.

[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 below 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.

[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 Baker and Mott (1989).

Chart 1: Child Outcome Measures

Measure	Age Group	Comments
PPVT Score	4 years +	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.
Grade Repetition	10 years +	"Has your child repeated any grades for any reason?" Coded 1 if the mother answered yes in either 1988 or 1990, d 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 5th birthday is used.

The relationship between test scores and future wages has received a great deal of attention from economists. In his summary of this literature, Hanushek (1986) 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, Murnane, Willett, and Levy (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, 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 (Barrington and Hendricks, 1989; Cairns, Cairns and Neckerman, 1989; Grissom and Shepard, 1989; Lloyd, 1978; Stroup and Robins, 1972; Ensminger and 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% increase in lifetime wages (See Angrist, 1990 for a recent estimate). High school graduates are also less likely to be unemployed (Markey, 1988). And while economists tend to focus on labor market performance, educational attainment has also been shown to be associated with improvements in health (Grossman, 1973) and job satisfaction (Michael, 1982; Haveman and 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 program provides a broad range of health care services. Specifically, Head Start guidelines require that each child have a physical exam; 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.

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% of eligible children participate in the Early and Periodic Screening, Diagnosis and Treatment (EPSDT) component of the Medicaid program (U.S. DHHS, 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 (The Washington Post, 1993). And in contrast to the results reported below, 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.

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). Poor children are at much greater risk of nutritional deficiencies. For example, 21% of 1 to 2 year old children in low-income households suffer iron anemia compared to 7% of 1 to 2 year olds from higher income households (Devaney, Haines, and Moffitt, 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 captures the effects of longer-term deprivation. It has been profitably used in the economic history and development literatures (see, for example, Fogel, 1986; Martorell and Habicht, 1986; and the review in Strauss and Thomas, 1994). 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) indicates that 15% of poor female children 2 to 5 years old are below the 5th percentile of height-for-age. The corresponding figure for males is 11%.

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

[13] See Currie (1994) 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 (personal communication, Paula Baker, 1993). In the 1988 and 1990 surveys, the heights of about 30% 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 and so, in this paper, we use all reported child heights.

similar ages.[15]

Evidence on the Importance of Selection

Figure 1 illustrates the fact that neither Head Start participants nor enrollees in other preschools are random samples of children. The figure shows the relationship between the type of preschool and the logarithm of household permanent income measured as the average annual household income between 1978 and 1990, in real 1990 dollars.[16]

Figure 1 suggests that the mechanism governing selection into Head Start is quite different from that underlying selection into other preschools, or even into no preschool. The probability of attending Head Start declines with income whereas the probability of attending other preschools rises with permanent income. The probability of attending no preschool is, relatively speaking, constant across the income distribution.

The figure also shows clear evidence of differences between the races. In this sample, 14% of white children attended Head Start compared to 32% of African Americans. In part, this difference reflects the relative poverty of the latter. But that is only *part* of the story. As Figure 1 demonstrates, African-Americans are more likely to attend Head Start than whites at *all* income levels, and the gap is

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

[16] Permanent income is adopted in order to attenuate the influence of random measurement error and to break the link between household income at a point in time and eligibility for the Head Start program. The estimates in Figure 1 are non-parametric and thus place no restrictions on the shape of the relationship between type of pre-school and permanent income. They are estimated by the method of locally-weighted smoothed scatterplots (LOWESS, see Cleveland, 1979; Hardle 1990) which is a nearest neighborhood type estimator. Essentially, each observation is replaced by its predicted value based on a weighted regression of the dependent variables on permanent income. The sample in each weighted regression includes only those children from households whose incomes are in the same "neighborhood" or band. The fraction of children included, the band-width, is arbitrary and we have experimented with a range of values. The estimates in Figure 1 include 20% of the sample and they seem to us to be sufficiently smoothed. (Kernel estimators differ from nearest neighborhood estimators in the definition of this band. If it is constant in width across the income distribution, then it is a kernel estimator; if, as in our case, the band maintains a constant share of the sample, it is a nearest neighborhood estimator). The weights in each of the LOWESS regressions are given by $w_j=(1-d_j^3)^3$, where d_j is a measure of the difference in income of child j and the child of interest, i. In this case, $d_j=(y_j-y_i)/(y_i^*-y_i)$ where $(y_i^*-y_i)$ is the maximum income gap between the child of interest and all other children in this particular band. The weight is positive for every observation within the band and the weighting function is similar to a Gaussian in shape. Since $d=0$ for the child itself, the weight is equal to one; the weights decline as the income gap increases. Children outside the band are given a weight of zero and hence it is a local rather than global estimator. The x-axis of the figure is drawn to a logarithmic scale.

greatest among children living in higher income households.[17] Conditional on income, whites and African-Americans are roughly equally likely to attend other types of preschool, but whites are 10% more likely to attend no preschool.

Further evidence of differences between the races in the mechanisms determining selection into Head Start is given in Figure 2 with reference to two of the four child outcome measures. The upper panel shows non-parametric estimates of the relationship between PPVT scores and household permanent income. The figure shows that whites have higher PPVT scores than African-Americans at all levels of permanent income and that the gap actually increases with income. Figure 2 also shows that among whites, there is a gap between Head Start and other preschool children that tends to persist across the income distribution.[18] In contrast, among African-Americans, there is little difference in PPVT scores between Head Start children and those that attend other preschools, although there is some tendency for those who attend any preschool to have higher scores that those who attend no preschool.

These racial differences in the effects of Head Start suggest either that despite the low mean level of scores, Head Start has a much more positive effect on African-American children than on white children (by bringing them up to the same level as the preschool children), or that there are racial differences in the way children are selected into Head Start and other preschool programs. In any case, it is clear that racial differences in selection mechanisms and/or in child outcomes are not simply a reflection of disparities in family income.

The bottom half of Figure 2 shows non-parametric estimates of the relationship between height-for-age and permanent income. The figure illustrates that height-for-age tends to rise with income,

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[17] Put another way, the negative effect of income on participation is much greater among whites. In a simple regression of the probability of participating in Head Start on the logarithm of permanent income, the (absolute value of the) coefficient on income is twice as large for whites (-0.19) than for African Americans (-0.10); the standard errors are about 0.02 and so this difference is significant. After controlling for income, African-Americans are about 5% more likely to attend Head Start than whites. For a more detailed discussion of correlates of Head Start participation in the NLSCM see Mott and Quinlan (1992).

[18] Conditional on permanent income, white Head Start children tend to score about 5 points below those who attended other preschools (and the difference is significant with a t of 2.7). The difference between Head Start and those who never attended preschool is small (½ a point) and insignificant.

especially among whites, and that in contrast with the results for PPVT scores, even poor African-American children are taller than the average white child. Second, Figure 2 shows that white children who attended other preschools are about 1% of the US median taller than Head Start children *at all levels of permanent income.* In contrast, among African-Americans, Head Start children are taller than those that attended other preschools at all but the lowest levels of income. However, African-American preschool children are taller than no preschool children only at low levels of income.

These results suggest that either Head Start has large positive effects on the height-for-age of African-Americans, but not of whites, or that once again, the mechanism underlying selection into Head Start and preschool varies with race. That is, conditional on income, white Head Start children are shorter and have lower PPVT scores than white children who attended preschool, while African-American children who attend Head Start are taller and have PPVT scores equal to those who attended other preschools. These observations provide a powerful motivation for attempting to control for characteristics associated with selection into the program, so that we can attempt to answer the question of whether the effects of Head Start do, if fact, differ by race.

Thus far, in our discussion of selection, we have focused on income differences between Head Start, preschool, and no preschool children. Table 1 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 mothers and grandmothers who are less educated, and who had lower scores on the Armed Forces Qualification Test (AFQT), a measure of human capital.[19]

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

[19]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 below.

in other preschools is 1.6 years, while the difference is only .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. Head Start children also tend to be disadvantaged relative to children who attended no preschool, though the gaps are smaller.

Finally, Table 1 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 that of whites, a fact that is true throughout the income distribution and suggests that AFQT measures more than native "ability".[20]

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 2 focuses on the within-family income changes that are associated with participation in Head Start and other preschools.

Panel A of Table 2 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 northwest corner of the table indicates that 41% of white children who attended Head Start had a sibling who also attended Head Start and, therefore, 59% had a sibling who did not. In the fixed effects models, only the latter group are used to identify the effects of Head Start.

Of these 59%, 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

[20] A similar racial difference in the PPVT scores of children was noted above.

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 non-overlapping 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 2 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 3 years old is reported in the second. Income at age 3 is relevant since this is the time when most children would enter Head Start or some other preschool. Rows 1 to 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 2, is that there is little within-family difference in income at age 3 between Head Start children and no preschool children. 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.[21] 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

[21] 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 (1994) for a discussion of this literature.

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.

4. Estimation Results

Tables 3 and 4 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" OLS estimates (in columns 1 to 3) do not control for any observable covariates: this baseline shows the sample means. "Adjusted" OLS estimates (in columns 4 to 6) do control for mother and child-specific observables. Fixed effects estimates (in columns 7 to 9) also control for all unobserved time invariant mother-specific effects in addition to child-specific observables.

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).[22]

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 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

[22] Lagrange Multiplier tests for homoskedasticity (Breusch and Pagan, 1979; 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 are heteroskedasticity robust estimators of the variance-covariance matrix (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 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 balance sample, we prefer to report those estimates.

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.[23]

Measures of Academic Performance

The first three columns of Panel A in Table 3 indicate that the PPVT scores of white children are, on average, about twice those of African-American children. 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 of 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 4 suggests that, among whites, the difference between the PPVT scores of Head Start and other children disappears when observables are controlled.

However, column 7 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 4 and 7 indicates that, consistent with Haskins' (1989) observations, it is the most disadvantaged white children in terms of unobservables that are selected into the Head Start program. On the other hand, controlling for

[23] 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 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 foot of each panel.

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 9 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: grade repetition. The first 3 columns of Panel B in Table 3 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.[24] Although white Head Start children are about 20% 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 4 to 6 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 7 to 9, indicate that whites who attended Head Start are 47% 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%) 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

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

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 for. Among whites, this "difference-in-difference" estimate is statistically significant for both PPVT scores and grade repetition. Among African-Americans, however, the tale is more dismal: neither Head Start nor other preschools is associated with enhanced academic performance.

Measures of Health Status

Table 4 presents the estimated effects of participation in Head Start and other 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 4 show 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 7 and 8) we find that Head Start is associated with an 8% to 9% 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 also 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 4. The unadjusted OLS estimates (in columns 1 and 2) 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 2 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 4 and 5, the preschool effect among whites is somewhat weaker, but remains significant. A good part of the difference between columns 1 and 4 is accounted for by the influence of maternal height although other measures of maternal human capital (her education) are also statistically significant. This suggests that height is influenced both by genetic factors and also by parental investments in the health and human capital of their children. The fixed effects estimates for whites, in column 7, 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% level of confidence.

Among African-Americans, the inclusion of observable maternal and child characteristics (in column 5) cuts the positive correlation between Head Start and child height by more than half. It also becomes statistically insignificant. Similarly, column 8 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-American noted in column 2 and in Figure 2, reflects the selection of taller African-American children into the program. This impression was confirmed by estimating regressions of birthweight on participation in the program. Birthweight is highly correlated with future child height-for-age, but 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 those that did not. For whites, however, we did not find any correlation between birthweight 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.

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. Given that there are over 1,300 Head Start programs (Hayes *et al.*, 1990) all administered at a local level, and that the program guidelines are not specific about how the goals of the program are to be attained, there is bound to be a great deal of heterogeneity in program content. A recent report on the Head Start program notes that Head Start regional offices often differ in their interpretation and application of program policies. It also concludes that the quality of programming is uneven across the country. While most programs were in compliance with most standards, slightly over 11% 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% needed improvement in 26 to 50 areas (U.S. DHHS, 1993).

It is possible that programs serving African-Americans place less emphasis on academic achievement than programs serving white children because it may be easier to monitor compliance with the health care guidelines. Unfortunately, we cannot test these hypotheses as we have no information about individual program content.

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 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 5. 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 Appendix 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 1 and 2 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. However, turning to grade repetition, column 4 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%. We do not find any similar effect among African-Americans (column 5). Moreover, the difference between whites and African-Americans in the AFQT-Head Start interaction is significant (at 8%, column 6). We do not find any significant interactions between preschool attendance and AFQT for either race.

Finally, the results shown in columns 7 through 9 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 backgrounds, as measured by maternal AFQT, gain more from Head Start but the interaction is only statistically significant in the regressions for 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 5.[25] Columns 1 and 2 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 3 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 effects among children who attend preschool.

Our results for African-Americans are thus consistent with those of earlier studies. We find no effect of Head Start in a sample of children of various ages because the benefits die out very quickly. However, looking only at young African-American children, we find that there are clear benefits associated with Head Start attendance. White children also benefit from Head Start, but they tend to 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" between 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 result 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.

[25] For the interactions, the age of the child is measured in years older than five (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.

Columns 4 to 6 of Table 6, Panel B indicate that there are no statistically significant interactions between age and type of preschool in the regressions for 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.

Finally, columns 7 to 9 of Table 6, Panel B show that older children who attended Head Start are less likely to have been immunized. This result could reflect 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.[26] Thus, it is likely that the result reflects an increasing emphasis on the health care portion of the Head Start program in recent years.

6. 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 2 indicated 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 into adolescence and adulthood among white children.

We also find that white children over 9 years old who attended Head Start are 47% less likely to have repeated a grade than other white children. Given that 35% of white children who did not attend preschool repeated a grade, this translates into a reduction of 16% points in the probability of repeating

[26] In addition, there is evidence that recall error tends to decline with education (see, for example, Smith, Thomas and Karoly, 1991). If better educated mothers are less likely to forget their children were immunized and recall error is the explanation, then we would expect the interaction between 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.

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 to eventually drop out of school altogether. But 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%, 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%.

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% more likely to drop out of school, even when achievement on standardized tests, socio-economic status, gender, and ethnicity were controlled. They also found that grade repetition was disproportionately concentrated in early grades, and especially grade one, which means that their findings should be relevant to our sample. Hence, the 16% decline in the probability of repeating a grade associated with Head Start could lead to a 5% decline in the probability of high school drop out 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. And 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 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% to 11% 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).

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.[27] 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.

[27] The Head Start program also provides benefits to other family members. For example, more than a third of the employees are parents of current or former Head Start students (Washington and Oyemade, 1987) and expenditures on Head Start comprised 20% of all federal expenditures on child care in 1986 (Kahn and 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 based on the perception that it benefits children rather than on the desire to provide these other benefits.

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TABLE 1: Characteristics of Mothers and their Children: Means and [standard errors]

	WHITES				AFRICAN-AMERICANS			
	ALL (1)	HEAD START (2)	PRE SCHOOL (3)	NEITHER (4)	ALL (1)	HEAD START (2)	PRE SCHOOL (3)	NEITHER (4)
MOTHER CHARACTERISTICS								
Permanent household income	26.12	16.89	32.73	24.08	17.26	15.04	21.29	16.55
(000s 1990 $)	[0.26]	[0.39]	[0.52]	[0.3]	[0.29]	[0.38]	[0.75]	[0.42]
Human Capital								
Education	11.70	10.91	12.48	11.37	11.84	11.64	12.48	11.62
	[0.04]	[0.09]	[0.06]	[0.05]	[0.05]	[0.07]	[0.09]	[0.07]
AFQT Score	0.83	0.58	1.01	0.78	0.43	0.37	0.55	0.42
	[0.01]	[0.02]	[0.02]	[0.01]	[0.01]	[0.02]	[0.02]	[0.02]
Height (inches)	63.85	63.42	64.06	63.83	64.01	64.12	64.18	63.83
	[0.04]	[0.12]	[0.07]	[0.06]	[0.07]	[0.11]	[0.14]	[0.11]
Grandmother's Education	9.81	8.68	10.69	9.51	10.02	9.74	10.81	9.77
	[0.06]	[0.15]	[0.09]	[0.08]	[0.07]	[0.11]	[0.13]	[0.11]
# of Maternal siblings	4.30	4.68	3.74	4.58	5.45	5.68	4.97	5.55
(at age 14)	[0.05]	[0.13]	[0.07]	[0.07]	[0.09]	[0.15]	[0.17]	[0.13]
CHILD CHARACTERISTICS								
Age in months, 1990	99.18	115.04	94.27	98.30	107.74	119.07	98.57	104.72
	[0.68]	[1.78]	[1.01]	[0.99]	[1.09]	[1.81]	[2.0]	[1.73]
(1) if first born	0.47	0.50	0.56	0.41	0.44	0.47	0.47	0.39
	[0.01]	[0.02]	[0.01]	[0.01]	[0.01]	[0.02]	[0.03]	[0.02]
(1) if male	0.49	0.47	0.48	0.49	0.51	0.48	0.55	0.52
	[0.01]	[0.02]	[0.01]	[0.01]	[0.01]	[0.02]	[0.03]	[0.02]
Number of children	3285	450	1149	1686	1502	477	376	649
Sample proportions	100	14	35	51	100	32	25	43

Notes:
Maternal Education is measured as highest grade attained. The AFQT score is age-standardized. The number of maternal siblings is measured when the mother was aged 14.

TABLE 2: Characteristics of children and their siblings by type of preschool attended

A: Percentage of children and siblings by type of preschool attended

	Child in:	WHITES Head Start	WHITES Preschool	WHITES Neither	AFRICAN-AMERICANS Head Start	AFRICAN-AMERICANS Preschool	AFRICAN-AMERICANS Neither
Sibling in:	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	1230	329	259	480

B: Income by type cf preschool attended by child and sibling: Means and [standard errors]

Child attended	Sibling attended	WHITES Permanent Income	WHITES Income at age 3	AFRICAN-AMERICANS Permanent ıcome	AFRICAN-AMERICANS Income at age 3
All children		26.12 [0.3]	23.35 [0.48]	17.50 [0.35]	15.02 [0.66]
1. Head Start	Head Start	17.36 [0.79]	14.17 [1.11]	13.76 [0.57]	11.40 [0.81]
2. Preschool	Preschool	34.32 [0.83]	34.81 [1.54]	24.44 [1.71]	23.27 [4.3]
3. Neither	Neither	23.53 [0.4]	20.32 [0.59]	16.17 [0.53]	13.73 [0.73]
4. Head Start	Neither	16.29 [0.66]	13.18 [0.77]	16.90 [0.99]	14.89 [1.41]
Neither	Head Start		13.11 [1.06]		13.91 [1.85]
5. Preschool	Neither	30.07 [0.78]	28.32 [1.14]	18.26 [1.21]	17.33 [1.84]
Neither	Preschool		21.92 [1.28]		9.77 [1.24]
6. Head Start	Preschool	19.80 [1.46]	14.92 [1.91]	19.51 [1.31]	17.32 [2.03]
Preschool	Head Start		19.65 [2.9]		20.19 [2.62]

TABLE 3: Effect of participation in Head Start and Preschool on PPVT Score and Grade Repetition

	OLS-UNADJUSTED			OLS-ADJUSTED			MOTHER FIXED EFFECTS		
	White	African American	Diff-erence	White	African American	Diff-erence	White	African American	Diff-erence
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A: PPVT score									
(1) if participate in									
Head Start	-5.621	1.037	-6.658	-0.354	0.761	-1.115	5.875	0.247	[illegible]
	[1.570]	[1.223]	[1.990]	[1.452]	[1.136]	[1.843]	[1.520]	[1.358]	[2.[illegible]38]
Other preschool	9.077	2.007	7.070	1.681	-0.746	2.427	1.173	0.615	0.557
	[1.275]	[1.481]	[1.955]	[1.171]	[1.311]	[1.758]	[1.296]	[1.296]	[1.833]
Constant	31.512	13.762	17.749	-107.739	-49.850	-57.889	.	.	.
	[0.783]	[0.823]	[1.136]	[16.356]	[15.958]	[22.851]			
F(Head Start=Preschool)	75.38	0.40	36.22	1.52	1.18	2.70	7.45	0.06	4.81
	[0.00]	[0.53]	[0.00]	[0.22]	[0.28]	[0.10]	[0.01]	[0.81]	[0.03]
F(All covariates)	43.62	0.99	133.49	71.02	14.62	74.28	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	2319	1158	3477	2319	1158	3477	2319	1158	3477
B: Probability REPEATED GRADE									
(1) if participate in									
Head Start	0.035	0.010	0.025	-0.004	-0.008	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	-0.029	0.069	-0.098	0.006	0.088	-0.082	-0.061	-0.163	0.102
	[0.062]	[0.085]	[0.104]	[0.063]	[0.088]	[0.107]	[0.099]	[0.125]	[0.158]
Constant	0.346	0.463	-0.118	0.486	0.049	0.437	.	.	.
	[0.031]	[0.043]	[0.052]	[0.818]	[0.886]	[1.200]			
F(Head Start=Preschool)	0.76	0.47	1.20	0.02	1.20	0.56	8.40	1.22	8.05
	[0.38]	[0.49]	[0.27]	[0.90]	[0.27]	[0.45]	[0.01]	[0.27]	[0.01]
F(All covariates)	0.39	0.34	2.82	2.32	1.17	2.11	1.90	1.75	1.87
	[0.68]	[0.72]	[0.02]	[0.01]	[0.30]	[0.00]	[0.00]	[0.28]	[0.01]
R^2	.01	.01	.01	.07	.05	.08	[illegible]2	.58	.61
Sample size	414	314	728	414	314	728	414	314	728

Notes: Standard errors below coefficients; p-values below F statistics. Variance-covariance matrices estimated by 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, AFQT score, height and number of siblings when she 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.

TABLE 4: Effect of Participation in Head Start and Preschool on Measles Immunization and Height for Age

	OLS-UNADJUSTED			OLS-ADJUSTED			MOTHER FIXED EFFECTS		
	White	African American	Difference	White	African American	Difference	White	African American	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A: Probability of MEASLES IMMUNIZATION									
(1) if participate in									
Head Start	0.152	0.167	-0.015	0.025	0.065	-0.039	0.082	0.094	-0.011
	[0.025]	[0.026]	[0.037]	[0.017]	[0.018]	[0.026]	[0.030]	[0.033]	[0.045]
Other preschool	0.021	-0.018	0.039	0.027	0.002	0.025	0.123	0.050	0.073
	[0.018]	[0.029]	[0.035]	[0.013]	[0.020]	[0.025]	[0.024]	[0.034]	[0.042]
Constant	0.698	0.714	-0.016	-0.153	-0.010	-0.143	.	.	
	[0.011]	[0.017]	[0.021]	[0.189]	[0.252]	[0.324]			
F(Head Start=Preschool)	24.85	35.50	1.67	0.01	8.37	4.67	1.42	1.21	2.52
	[0.00]	[0.00]	[0.20]	[0.94]	[0.00]	[0.03]	[0.23]	[0.27]	[0.11]
F(All covariates)	19.01	25.30	18.53	240.01	125.11	176.17	3.10	3.27	3.16
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0 [illegible]	[0.00]
R^2	0.01	0.03	0.02	0.54	0.57	0.55	0.69	0.68	0.69
Sample size	2829	1336	4165	2829	1336	4165	2829	1336	4165
B: HEIGHT for age (% median)									
(1) if participate in									
Head Start	-0.171	1.024	-1.195	-0.199	0.431	-0.629	0.084	0.549	-0.465
	[0.330]	[0.382]	[0.505]	[0.328]	[0.504]	[0.490]	[0.399]	[0.540]	[0.671]
Other preschool	0.927	0.477	0.450	0.738	0.318	0.420	0.582	0.182	0.399
	[0.265]	[0.485]	[0.553]	[0.264]	[0.475]	[0.544]	[0.318]	[0.509]	[0.600]
Constant	99.627	100.694	-1.067	63.785	55.040	8.745	99.895	97.708	
	[0.166]	[0.278]	[0.324]	[4.145]	[6.012]	[7.303]	[2.570]	[4.139]	
F(Head Start=Preschool)	9.71	1.32	7.72	6.33	0.06	3.06	1.25	0.34	1.26
	[0.00]	[0.25]	[0.01]	[0.01]	[0.81]	[0.08]	[0.26]	[0.56]	[0.26]
F(All covariates)	7.54	3.60	12.57	13.24	10.59	12.89	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	0.10	0.08	0.58	0.56	0.58
Sample size	2789	1303	4092	2789	1303	4092	2789	1303	4092

Notes: Standard errors below coefficients; p-values below F statistics. Variance-covariance matrices estimated by 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, AFQT score, height and number of siblings when she 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.

TABLE 5: 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			Probability Repeated Grade			Probability of Measles Immunization		
	White (1)	African American (2)	Difference (3)	White (4)	African American (5)	Difference (6)	White (7)	African American (8)	Difference (9)
A: Include interactions with AFQT of mother									
(1) if participate in Head Start	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]
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	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]
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 & 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 & 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									
(1) if participate in Head Start	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]
Age of child (years since age 5)	-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	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]
Age of child (years since age 5)	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 & 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 & 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 below coefficients, p-values below F statistics. Variance-covariance matrix for PPVT models calculated by method of infinitesimal jackknife. All models include controls for child age, gender, whether first born and household income at age 3

Figure 1
Type of preschool and permanent income
(Non-parametric estimates)

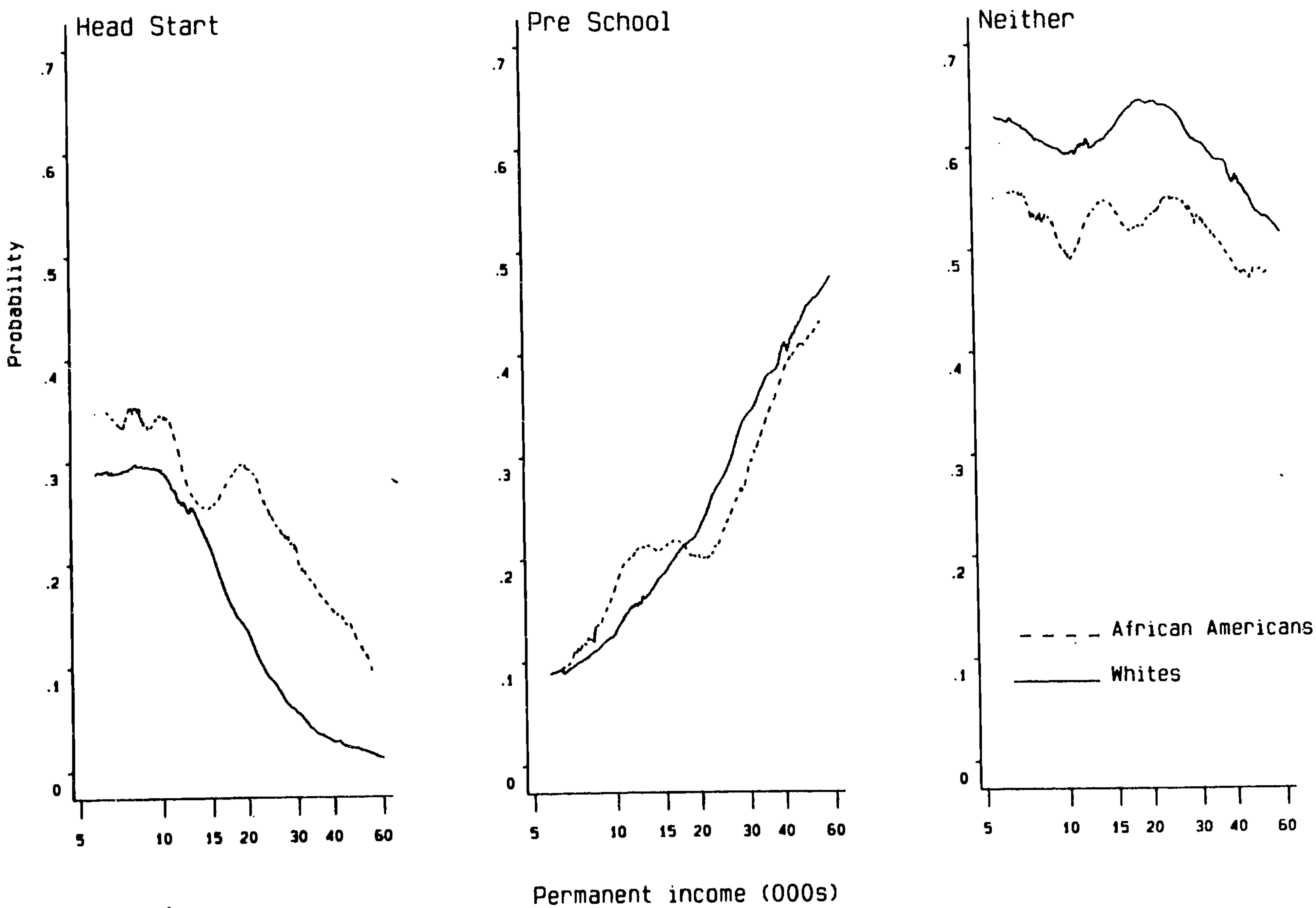
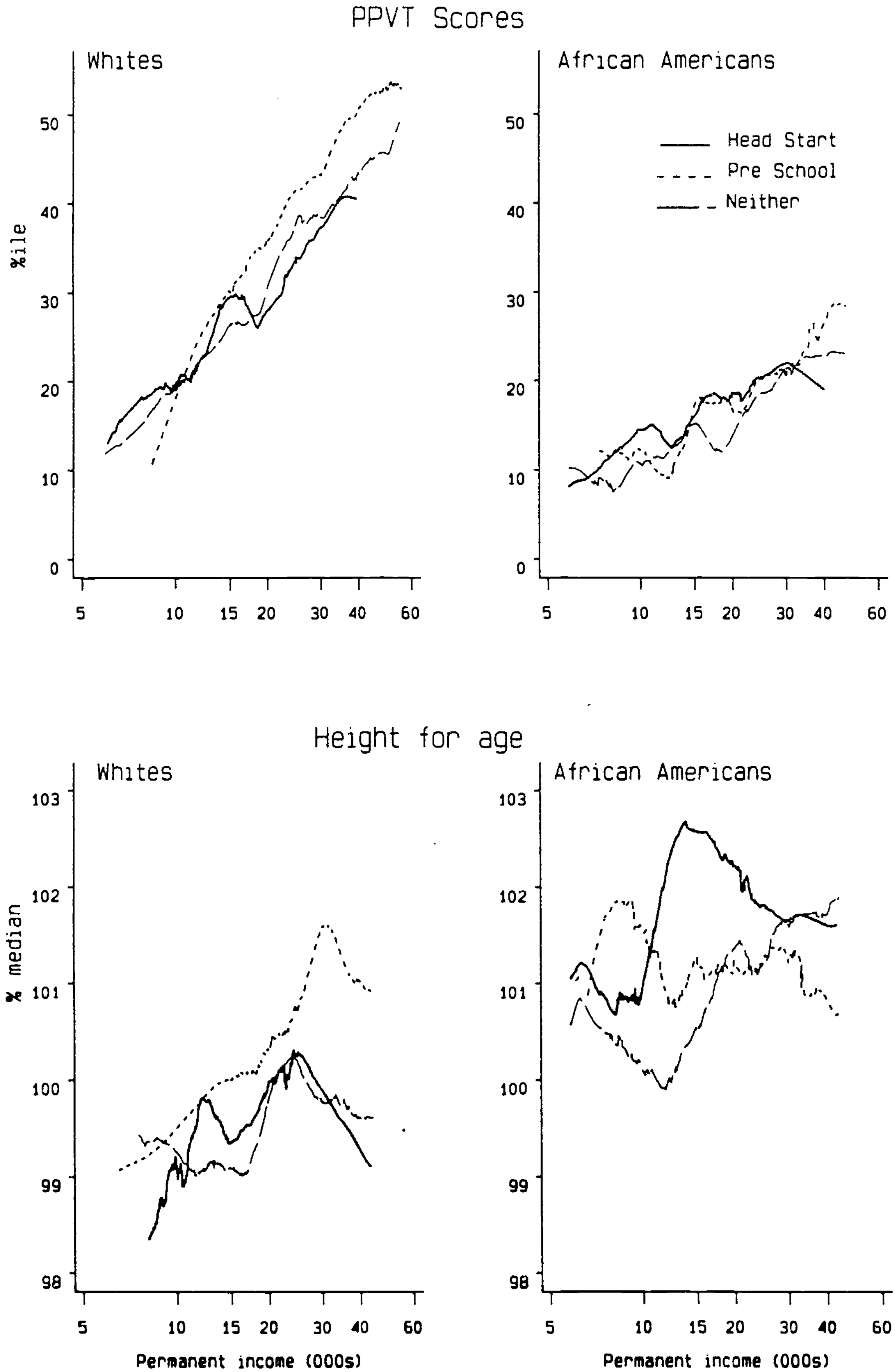
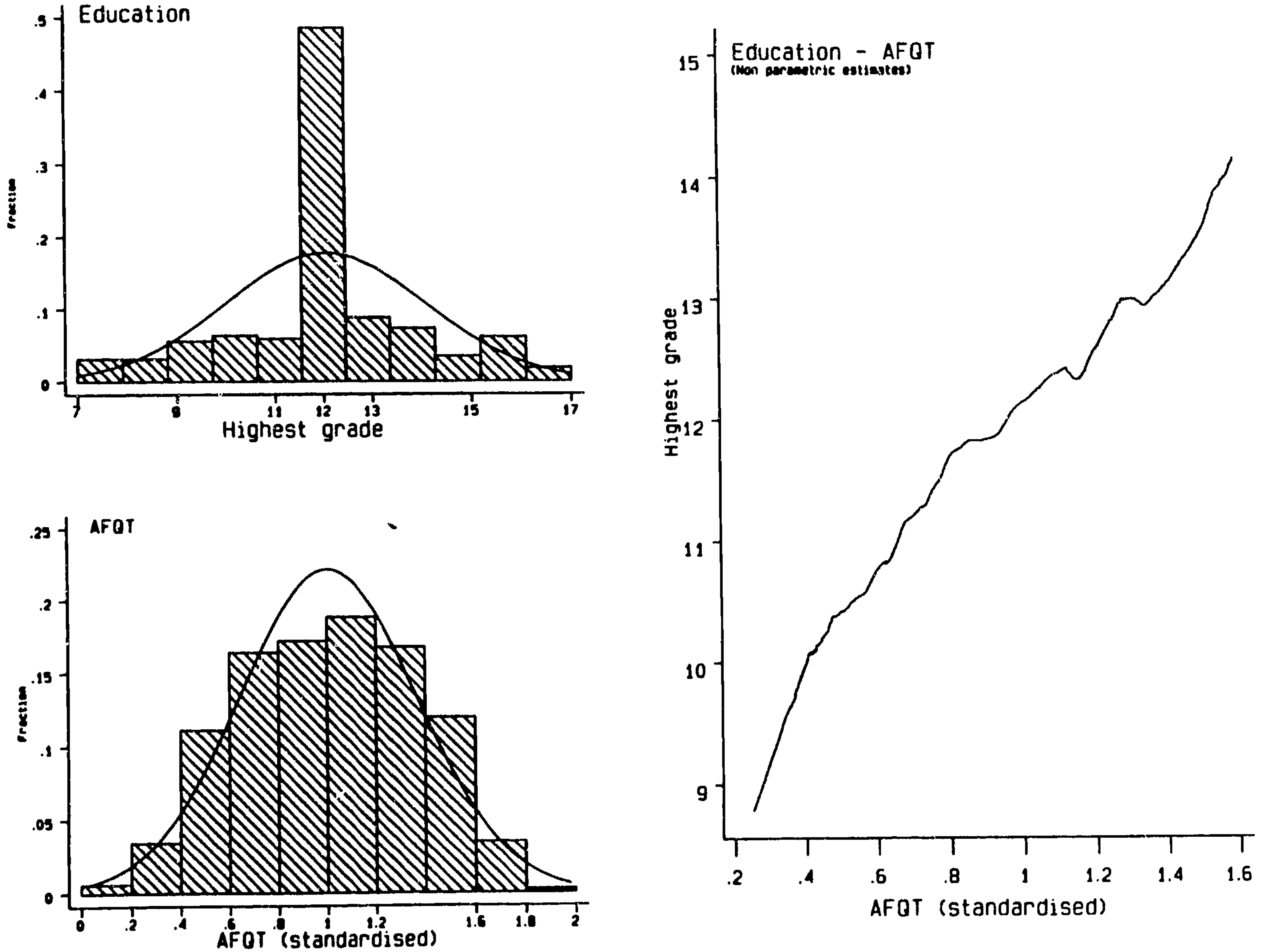


Figure 2
PPVT Scores, height for age and permanent income by type of preschool
(Non-parametric estimates)



Appendix Figure 1
The distribution of maternal education and AFQT and the relationship between them



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