Preventing Behavior Problems in Childhood and Adolescence: Evidence from Head Start*

Pedro Carneiro
University College London,
Centre for Microdata Methods and Practice,
and Institute for Fiscal Studies

Rita Ginja†
University College London

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Abstract

This paper shows that participation in Head Start reduces the incidence of behavioral problems, grade repetition, and obesity of children at ages 12 and 13, and depression, criminal behavior, and obesity at ages 16 and 17. Head Start's eligibility rules induce discontinuities in program participation as a function of income, which we use to identify program impacts. Since there is a range of discontinuities (they vary with family size, state and year), we identify the effect of Head Start for the large set of individuals in the neighborhood of each of several discontinuities, as opposed to a smaller set of individuals around a single discontinuity.

JEL Codes: C21, I28, I38.

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†Address: Department of Economics, University College London, Gower Street, London WC1E 6BT, United Kingdom. Tel.: +44 020 7679 5888, Fax: +44 020 7916 2775. E-mail: p.carneiro@ucl.ac.uk, r.ginja@ucl.ac.uk.

Where there's a need for early intervention, we will work very intensively with those families so that young people are deterred from going into gangs and guns and knife crime. Gordon Brown, August 23, 2007, BBC News

To prevent: parents held accountable - fined if they fail to supervise. And so that these young people are not left to hang around street corners, councils and authorities obligated to maintain their education and supervision. Gordon Brown, September 24, 2007, Speech to Labour Conference

1 Introduction

Problem behaviors among adolescents are at the center of the social agenda in most developed countries. Faced with increasingly visible gang violence in the UK, prime minister Gordon Brown launched a call for better parenting. While he is right in preferring prevention to remediation, and in asserting that home environments are key for prevention, he will soon find out that they are incredibly hard to change.

Early childhood programs for poor children have gained prominence as an alternative (e.g., Currie, 2001, Carneiro and Heckman, 2003). Model interventions such as Perry Preschool and Abecedarian have proven to be effective in preventing behavioral problems (e.g., Barnett, 2004). The central question is whether more universal (and less well funded) programs like Head Start in the US, or Sure Start in the UK, can be equally successful.

In this paper, we study the impact of Head Start on behavioral problems of recent cohorts of children and adolescents. We find strong program impacts on grade repetition, social behaviors (measured by a battery of behavioral questions), and obesity[1] at ages 12-13; and on depression (measured by a depression scale), crime, and obesity at ages 16-17. We account for self-selection into Head Start using a (fuzzy) regression discontinuity design which explores program eligibility rules. We determine eligibility to the program for each child by examining whether her family income is above or below the income eligibility cutoff, which varies with year, state, family size, and family structure. Then we check whether the relationship between family income and Head Start participation, outcomes at

[1]Obesity is usually seen as a health problem. While it is true that most of the effect of Head Start on this outcome is probably due to nutrition education for children and parents, as well as other exercise and nutrition components of the program, there may also be a behavioral problem component to it. Frisvold (2007) and Frisvold and Lumeng (2008) show substantial effects of this program on obesity.

12-13, and outcomes at 16-17, is discontinuous at the income eligibility cutoff for each child.

The focus on behavioral problems is especially important given the controversy about the fade-out of the effects of Head Start (and other early childhood programs) on the cognitive development of children, especially that of blacks (see, e.g., Currie and Thomas, 1995, 1999). Recent research argues that behavioral skills are more malleable than cognitive skills (Cameron, Heckman, Knudsen, Schonkoff, 2006, Cunha and Heckman, 2006), and therefore more amenable to being affected by policy (see, e.g., Carneiro and Heckman, 2003). Furthermore, behavioral problems in childhood and adolescence are strong predictors of adult outcomes (e.g., Bowles, Gintis and Osborne, 2003, Heckman, Sixtrud, and Urzua, 2006, Carneiro, Crawford and Goodman, 2007).

Our empirical strategy is novel in the study of Head Start. We implement it using the Children of the National Longitudinal Survey of Youth of 1979 (CNLSY79), a survey with rich information on children outcomes at different ages. In contrast with the standard regression discontinuity setup, there are multiple discontinuity points, which vary across families because they depend on year, state, family size and family structure. Therefore our estimates are not limited to individuals located around a single discontinuity, but they are applicable to a more general population. The use of this dataset also allows us to focus on recent program participants and answer questions about the impact of the program in its present format.

Some recent evaluations of Head Start also address endogenous program participation (see Ludwig and Phillips, 2007, for survey of the recent literature on Head Start.). Currie and Thomas (1995, 1999, 2000) use data from the CNLSY79 (the same dataset we use), and rely on sibling comparisons. They find strong impacts of the program on a cognitive test (which fade-out for blacks, but not whites) and grade repetition. Currie, Garces and Thomas (2002) use a similar strategy in the Panel Study of Income Dynamics (PSID), and show that the program has long lasting impacts on adult schooling, earnings, and crime. Ludwig and Miller (2007) explore a discontinuity in Head Start funding across US counties induced by a federal assistance program in 1965. They show that Head Start positively impacts children's health and schooling. The latter two papers measure impacts of Head Start for those who participated in the program in the 1960s and 1970s. More recently, Currie and Neidell (2007) use CNLSY79 to study the quality of Head Start centers and find a positive association between scores in cognitive tests and county spending in the program. They also find that children in programs that devote higher shares of the budget to education and health have fewer behavioral problems and are less likely to have repeated a grade. Frisvold and Lumeng (2007) explore an unexpected reduction in Head Start funding in Michigan to show strong effects of the program on obesity. A recent randomized control trial of Head Start has been commissioned by the US Congress. Only short run results are available, but they show that the program improves cognitive and behavioral outcomes of 3 and 4 year old children. Finally, Neidell and Waldfogel (2006) argue that ignoring spillover effects resulting from interactions between Head Start and non-Head Start children and/or parents underestimates the effects of the program in cognitive scores and grade repetition.

Our paper adds to this literature in at least two important ways. First, we provide a systematic study of the (medium to long term) impacts of Head Start on behavioral outcomes across different ages, for those children participating in the program in the 1980s and 1990s. Second, we adopt a new empirical strategy which explores detailed information on program eligibility rules. It differs from the mother fixed-effects strategy used by Currie and Thomas (1995, 1999, 2000) and Currie, Garces and Thomas (2002) by not requiring differences in Head Start participation across siblings to be random. Instead, it assumes that households are unable to locate strategically just above or below the income cutoffs that determine eligibility. This is a sensible assumption given the complexity of the eligibility rules and the fact that they change over time. We also test and find no evidence of the existence of any strategic behavior of this type. Our method is non-experimental, as opposed to US Congress (2005), but allows us to follow up children until much later ages. Relatively to Ludwig and Miller (2007), central differences are our focus on more recent participants into the program, our emphasis on behavioral outcomes, and the fact that we explore more than one discontinuity.

This paper proceeds as follows. In the next section we describe the data we use. Then we discuss the identification strategy in detail, and discuss several checks to the validity of the procedure. We follow by presenting our empirical results. The last section summarizes and concludes.

2 Data

We use data on females from the National Longitudinal Survey of the Youth of 1979 (NLSY79) combined with a panel of their children, the Children of the National Longitudinal Survey of Youth of 1979 (CNLSY79). The NLSY79 is a panel of individuals whose age was between 14 and 21 by December 31, 1978 (of whom approximately 50 percent are women). The survey has been carried out annually since 1979 (interviews have become biennial after 1994). The CNLSY79 is a biennial survey which began in 1986 and contains information about cognitive, social and behavioral development of individuals (assembled through a battery of age specific instruments), from birth to early adulthood.

We focus on the impact of the program in two age groups.[2] We study behavioral problems of children 12 to 13 years of age using the Behavioral Problems Index scale, and an indicator for smoking habits. We also examine behaviorally related measures of school success and health by looking at an indicator of grade repetition, an indicator of special education attendance, and an indicator of obesity. For adolescents 16 to 17 years of age we study mental health and motivational outcomes using measures of depressive symptoms (the CESD), criminal behavior, smoking habits and obesity (and in Appendix A we also present results for alcohol and marijuana use, high school enrollment, and scores on cognitive tests.). A detailed description of the variables can be found in table A1 in the Appendix A.

Since the CNLSY79 is a biennial survey there is only one observation per child in each two consecutive years. Therefore, we group children in intervals of two consecutive ages in order to maintain a reasonable sample size. The reason to focus on these age groups (and not earlier ones) is that it is likely that behavioral problems become more obvious from early adolescence onwards, and not so much before. We have checked earlier ages and results are indeed weaker.

Head Start is a preschool program that targets disadvantaged children and eligibility is means-tested. Children 3 to 5 years of age are eligible to participate in the program if their family income is below an income threshold, which varies with household characteristics, state of residence, and year. Among the variables available in CNLSY79 there are those that determine income eligibility (total family income, family size, state of residence, Head Start cohort and an indicator of the presence of a father-figure in the child's household[3]) along with outcomes at different ages of each child. All monetary variables are measured in 2000 values using the CPI-U from the Economic Report of the

[2]We have also analyzed individuals ages 20-21. However, because sample sizes are small, results were too imprecise to be conclusive. These are available on request from the authors.

[3]Although father's (or stepfather) employment is also a condition that determines Head Start eligibility, we did not consider it, because the variable "number of weeks mother's spouse worked" has missing values in half of the observations. Inclusion of this variable and an indicator for missing values does not change the results.

President (2006). The earliest year in which we can construct eligibility at age four is 1979 (for children born in 1975), since this is the first year in which income is measured in the survey. Similarly, since we take outcomes measured at ages 12 and older, and the last year of data is 2004, the youngest child in the sample is born in 1992. Therefore, we study the effects of participating in Head Start throughout the 1980s and early 1990s. In section 3 we describe our procedure in detail.

Empirically, we distinguish three possible preschool arrangements: Head Start, other preschool programs, or neither of the previous two (informal care at home or elsewhere). About 82 percent of those mothers who report that their child was enrolled in Head Start, also report that their child was enrolled in preschool, possibly confusing the two child care arrangements. Therefore, as in Currie and Thomas (1995, 2000), we recode the preschool variable so that whenever a mother reports both Head Start and preschool participation, we assume enrollment in Head Start alone (a detailed definition of the alternative arrangements is given in section (4.2)). After recoding this variable, almost 21 percent of the children in the sample ever enrolled in Head Start, 44 percent attended other types of preschool, and the remaining attended neither.[4] In our data, about 70% Head Start participants are in the program for one year only (or less).

It is well known that, as a consequence of the sample design, the children in CNLSY79 are more deprived than the average American child. Given that not all mothers have yet completed their fertility cycle, there is an oversampling of children from young mothers (because they are born earlier). Additionally, roughly half of the original NLSY79 consists of an oversample of African-Americans, Hispanics, and economically disadvantaged whites (and also a subsample of members of the military which we exclude from our work).

As we explain in the next section, it is good practice to restrict the sample to children whose family income at age four was near (in our case, between 5 and 195 percent of) the income eligibility cutoff for the program since points away from the discontinuity should have no weight in the estimation of program impacts (see Black, Galdo, and Smith, 2005, Imbens and Lemieux, 2007). Finally, in

[4]Based on official numbers we would expect the Head Start participation rate to be around 5% (20-25% of children in the US are poor, and 20-25% of poor children enrol in Head Start). One reason for having a larger estimate in our data may be the fact that we are using oversamples of minorities and poor whites, and more importantly, the fact that we overestimate children from young mothers. In fact, our number is comparable to the 19.4% figure in Currie and Thomas (1995). Currie, Garces and Thomas (2000) estimate Head Start participation at 10% in the PSID, and Ludwig and Miller (2007) have participation rates of 20 to 40% in the counties close to their relevant discontinuity (based on data from the National Educational Longitudinal Study).

this paper, we focus on male children only, for whom early behavioral problems are probably more prevalent than for females.[5] Table 1 summarizes the data. The full sample consists of 3029 males for whom at least one of the measured outcomes is available and all the control variables used in the regressions are not missing (child care arrangement at ages 3 to 5, eligibility to Head Start at age 4, family log income and family size at age 4 and at ages 0 to 2, presence of a father or stepfather in the household, state of residence at age 4, and birth weight).[6].

Columns (1) and (2) of table 1 present means and standard deviations for the full sample and in columns (3) to (10) we describe the restricted sample used in the regressions (household income between 5 and 195% of the eligibility cutoff). Average family income is lower for individuals in the restricted sample, and they also perform worse in all but one (probability of being overweight at age 12 or 13) of the outcomes analyzed. Focusing on the relevant sample for our study, we have 1766 individuals. At ages 12 and 13, Head Start participants engage in more problem behaviors as measured by the Behavioral Problems Index (BPI) than non-Head Start children; they are more likely to have repeated a grade than non-participants by ages 12 and 13 (36% versus 32%), but they do not show a strong propensity to be in special education, to be overweight, or to smoke, than children who never enrolled in the program. There is a higher proportion of adolescents that have already been sentenced of any charges or arrested among former participants relatively to non-participants (19% versus 13%), but again not much of a difference in terms of depression (CESD), obesity, or smoking.[7] As expected, participants come from families with lower income and who are more likely to be eligible than non-participants. They belong to families where the father's presence is infrequent, and who are more likely to be below the poverty line than non-participants. Participants' mothers have lower cognitive ability measured by the Armed Forces Qualifying Test (AFQT), and the BPI is higher for African-American children when compared to the rest of the sample.

[5]Unfortunately our results for females are very imprecise (available on request). The main reason is that, although eligibility is a strong predictor of Head Start participation for males, it is much weaker for females. This is puzzling since males and females in our sample look exactly equal in all dimensions. We examined this carefully, but our results were inconclusive so we opted to leave a deeper study of this problem for future work.

[6]We exclude from the sample 22 children whose family size at age 4 is one since the children eligible for interview in the survey are living at least part-time with their mothers.

[7]BPI is the Behavior Problems Index and it measures the frequency, range, and type of childhood behavior problems for children age four and over (Peterson and Zill, 1986). The Behavior Problems total score is based on responses from the mothers to 28 questions that intent to measure (1) antisocial behavior, (2) anxiety and depression, (3) headstrongness, (4) hyperactivity, (5) immaturity, (6) dependency, and (7) peer conflict/social withdrawal. The CESD (Center for Epidemiological Studies Depression) Scale measures symptoms of depression and it discriminates between clinically depressed individuals and others.

3 Empirical Strategy

Our goal is to estimate β from the following equation:

$$Y_i = \alpha + \beta HS_i + f(X_i) + \varepsilon_i \tag{1}$$

where Y_i is the outcome of interest for child i, which in our paper is measured at ages 12 to 13, or 16 to 17, HS_i is an indicator of whether the child ever participated in Head Start, X_i is a vector of controls (entering through function $f(X)$), and ε_i is an unobservable. β is the impact of Head Start on Y which, in principle, can vary across individuals. Even if β is a common coefficient, estimation by ordinary least squares (OLS) is problematic. Since Head Start participants are poor, they are likely to have low levels of ε_i, inducing a negative correlation between HS_i and ε_i. On the other end, not all poor children participate in the program, and perhaps only the most motivated mothers enrol their children, which would create a positive correlation between HS_i and ε_i.

In order to address these problems we explore discontinuities in program participation (as a function of income) that result from its eligibility rules. Children ages 3 to 5 are eligible if either their family income is below the federal poverty guidelines, or if their family is eligible for public assistance: Aid to Families with Dependent Children (or AFDC, which became Temporary Assistance for Needy Families, or TANF, after 1996) and Supplemental Security Income (or SSI; see D.H.H.S., 2007). We construct poverty status by comparing family income with the relevant federal poverty line, which varies with family size and year (Social Security Administration, 2006). Eligibility for AFDC requires satisfying two income tests, and additional categorical requirements, all of which are state specific. In particular, the *gross income test* requires that total family income must be below a multiple of the state specific threshold, that is set annually and by family size at the state level.[8] The second income test that must be verified by applicants (but not by current recipients) is the *countable income test*, that requires total family income minus some income disregards to be below the state threshold for eligibility (U.S. Congress, 1994). In addition, AFDC families must obey a particular structure: either they are female-headed families or families where the main earner is unemployed.[9] We do not

[8]When this test was established in 1981 the multiple was set to 1.5. The Deficit Reduction Act of 1984 raised this limit to 1.85 of the state need standard.

[9]Children in two-parents households may still be eligible to AFDC under the AFDC-Unemployed Parent program. Eligibility for AFDC-UP is limited to those families in which the principal wage earner is unemployed but has a history

impute SSI eligibility because this would require the imputation of categorical requirements which are complex to determine (e.g., Daly and Burkhauser, 2002), and we are unable to observe some of the requirements in the data.[10] Additionally, the literature has showed that classification errors are likely to happen (see Benitez-Silva, Buchinsky and Rust, 2003). Since SSI thresholds are below Poverty Guidelines we opted to ignore this problem, and we show in the rest of the paper that our constructed eligibility variable is a good predictor of program participation (a more detailed description can be found in Appendix B). In our sample, 81.7% of the children have eligibility determined by the federal poverty line criterion (as opposed to the AFDC criterion).

It is important to note that eligibility rules for social programs are not perfectly enforced, and take up rates among those eligible are far below 100%. There are several factors that influence the take up of social programs, such as shortage of funding to serve all eligible, barriers to enrollment and social stigma associated with participation (e.g., Currie, 2006, Moffitt, 1983). Beyond this problem, the number of eligible individuals is also different from the number of actual participants because of lack of perfect enforcement of eligibility rules, and because of other factors affecting participation. Furthermore, Head Start centers are allowed to enrol up to 10 percent of children from families whose income is above the threshold, and 10 percent of the slots must be reserved for children with disabilities (in our sample, 13% of Head Start participants and 16% of nonparticipants to have suffered of some limitation, but the difference is not statistically significant). Thus, the discontinuity in the probability of take-up of social programs around the income eligibility threshold is not sharp, but "fuzzy" (see Hahn, Todd and van der Klauww, 2001, Battistin and Rettore, 2007, and Imbens and Lemieux, 2007).

Due to limited funding, Head Start enrolls less than 60 percent of 3 and 4 years old children in poverty. Since many poor children do not participate and some of those who participate are not poor (although they may be near poor), Head Start may serve an even smaller proportion of the total eligible population (NIEER, 2005). For example, using data from the 2002 March CPS, Butler and Gish (2003) estimate that only 54 percent of 3 and 4 years old economically eligible to Head Start

of work. As in Currie and Gruber (1996), we consider eligible those whose father (or step-father) was employed for less than forty weeks in the previous calendar year.

[10]There are five stages to assess the categorical requirements to receive SSI through disability. For instance, in the third stage, it is required that the applicant has any impairment that meets the medical listings, conditional on the fact that he/she is not engaging in a substantial gainful activity and has an impairment expected to last for more than 12 months. We do not have accurate information to impute this using NLSY79 (there are variables on whether health limits amount and kind of work an individual can perform, but not to which extent they fulfill medical listings.)

in 2001 were served by the program. Additionally, families' characteristics change over time, making it difficult to estimate the size of the targeted population in each year and to identify all eligible children. Imperfect compliance is not unique to Head Start, but common across social programs.[11]

A child can enrol in Head Start at ages 3, 4, or 5 and it is possible to construct eligibility at each of these ages. However, for implementing the estimator it is convenient to pick an age. In our data eligibility at age 4 is a better predictor of program participation than either eligibility at 3 or at 5, and most children enrol in Head Start when they are 4 (U.S. Congress, 2004). Therefore we focus on eligibility at age 4 in our main specification, but we also present results with eligibility at other ages.

Unfortunately, nonparametric estimation (as proposed in Hahn, Todd and van der Klauww, 2001, Porter, 2003, and Imbens and Lemieux, 2007), is not practical in our setting because of multiple discontinuities and small sample, which makes it difficult to implement a nonparametric estimator for each discontinuity.[12] Instead, we rely on series estimation, as in Angrist and Lavy (1999), Lee and DiNardo (2004), and Chay, McEwan and Urquiola (2005), restricting the sample to values of the forcing variable that are not far off the highest and the lowest cutoff points.

For simplicity, we start by estimating the following reduced form model:

$$Y_i = \phi + \gamma E_i + f\left(Z_i, X_i\right) + u_i \tag{2}$$

where E_i is an indicator of eligibility for Head Start, X_i is a set of determinants of eligibility for each child (year, state, family size, family structure, measured at age 4), Z_i is family income (at age 4), and u_i is the unobservable. The equation for E_i is:

$$E_i = 1\left[Z_i \leq \bar{Z}\left(X_i\right)\right], \tag{3}$$

where $1\left[.\right]$ denotes the indicator function. $f\left(Z_i, X_i\right)$ is specified as a parametric but flexible function, and $\bar{Z}\left(X_i\right)$ is a deterministic (and known) function that returns the income eligibility cutoff for a

[11] Only 2/3 of eligible single mothers used AFDC (Blank and Ruggles, 1996); 69 percent of eligible households for the Food Stamps program participated in 1994 (Currie, 2006); of the 31 percent of all American children eligible for Medicaid in 1996, only 22.6 percent were enrolled (Gruber, 2003); EITC has an exceptionally high take-up rate of over 80 percent among eligible taxpayers (Scholz, 1994); in 1998, participation in WIC (the Special Supplemental Nutrition Program for Women, Infants and Children) among those eligible was 73 percent for infants, 2/3 among pregnant women and 38 percent for children (Bitler, Currie and Scholz, 2003).

[12] One could also think of recentering all the data relatively to the relevant cutoff. Some experiments with this strategy show very similar results to the ones we present here (see the discussion in section 4.2).

family with characteristics X_i (constructed from the eligibility rules). At the end of the next section we study the sensitivity of our results to the choice of different functional forms for $f(Z_i, X_i)$. We use probit models whenever the outcome of interest is binary (the linear probability model is especially inadequate when mean outcomes are far from 50 percent; see Table 1).

Three conditions need to hold for γ to be informative about the effects of Head Start on children outcomes. First, after controlling flexibly for all the determinants of eligibility, E_i must predict participation in the program, which we show to be true. One problem is that, at first sight, the control group is not clearly defined, since we consider two alternatives to Head Start: preschool, and home (or informal) care. Below we show that individuals induced to enter into Head Start because of a shift in eligibility status come almost exclusively out of home (or other informal) care, giving us a clear control group.

Second, families are not able to manipulate household income around the eligibility cutoff. This is the main assumption behind any regression discontinuity design. It is likely to hold in our case because the formulas for determining eligibility cutoffs are complex, and depend on family size, family structure, state and year, making it difficult for a family to position itself just above or just below the cutoff.[13] Still, in order to guard against the possibility of income manipulation, there are standard ways to test for violations of this assumption (e.g., Imbens and Lemieux, 2007), and below we discuss them in detail.

Third, eligibility to Head Start should not be correlated with eligibility to other programs that also affect child outcomes. This assumption is less likely to hold than the first two, because there are other means tested programs which have eligibility criteria similar to those of Head Start (e.g., AFDC, SSI, or Food Stamps). Below we show that these other programs are unlikely to be important determinants of children's behavioral problems. We implement the following test. While most welfare programs exist throughout the child's life, Head Start only exists when the child is between the ages of 3 and 5. If other programs affect behavioral problems of children, then eligibility to those programs in ages other than 3 to 5 should also affect children's outcomes. In contrast, if eligibility is correlated with children's outcomes only when measured between ages 3 and 5, then it probably reflects the

[13]For example, if we focus solely on the federal poverty line for a family of 4, between 1990 and 2000 it took the following values: 12700, 13400, 13950, 14350, 14800, 15150, 15600, 16050, 16450, 16700, 17050. The AFDC cutoffs are state specific and also vary over time.

effect of Head Start alone.

Below we implement these tests and we find no evidence that: i) families strategically manipulate their incomes; and ii) other programs are confounding the impact of Head Start.

In practice, γ does not correspond to the impact of Head Start on the outcome of interest, because eligibility does not fully predict participation (imperfect compliance). In order to determine the program impact, we estimate the following system for continuous Y_i:

$$Y_i = \alpha + \beta HS_i + g\left(Z_i, X_i\right) + \varepsilon_i \tag{4}$$

$$HS_i = 1\left[\eta + \tau E_i + h\left(Z_i, X_i\right) + v_i > 0\right], \tag{5}$$

where equation (5) is estimated using a probit model (van der Klauww, 2002). In practice, $P_i = \Pr\left(HS_i = 1|E_i, Z_i, X_i\right)$ is estimated in a first stage regression, and used to instrument for HS_i in a second stage instrumental variable regression (van der Klauww, 2002, Hahn, Todd and van der Klauww, 2001). If Y_i is binary we use a bivariate probit. $g\left(.\right)$ and $h\left(.\right)$ are flexible functions of (Z_i, X_i).

Relatively to the standard case, the variability in the eligibility cutoff shown in Figure 1 provides additional variation. Figure 1 displays the density of discontinuities in our data. This "continuum of discontinuities" allows us to go beyond the traditional regression discontinuity design and identify treatment effects for individuals over a wide range of values for income and family size (the two main running variables). Black, Galdo and Smith (2005) also recognize the potential of multiple discontinuities to identify heterogeneous effects of the program.

Therefore, we can consider models where β varies explicitly across individuals:

$$Y_i = \alpha + \beta_i HS_i + g\left(Z_{\ i}, X_i\right) + \varepsilon_i. \tag{6}$$

If there is perfect compliance, in the sense that $HS_i = E_i$, then Hahn, Todd and van der Klauww (2001) show that using regression discontinuity we can estimate $E\left(\beta_i|Z_i = \bar{Z}_i, X_i\right)$ (the average effect of the program conditional on Z) over the support of $\bar{Z}$ in the data. Under the weaker condition that $HS_i = E_i$ only when $E_i = 0$, we estimate $E\left(\beta_i|Z_i = \bar{Z}_i, X_i, HS = 1\right)$ (Battistin and Rettore, 2007). More generally, one can have non-compliance on both sides of the discontinuity, in which case we obtain an estimate of a Local Average Treatment Effect (LATE; Imbens and Angrist, 1994) at $Z_i = \bar{Z}_i$, over the support of $\bar{Z}$ (the set of income eligibility cutoffs), or $E\left(\beta_i | HS\left(\bar{Z}_i - \delta_i\right) - HS\left(\bar{Z}_i + \delta_i\right) = 1, X_i, Z_i = \bar{Z}_i\right)$, for $\delta_i > 0$. The latter is the case in our data. There are two reasons why this parameter may vary with $\bar{Z}$: i) β_i is a function of Z_i (income at the time eligibility is measured); or ii) even if there is independence between β_i and Z_i in the sample, independence may not hold conditional on program participation if HS_i depends on β_i.[14] In our setting, it is impossible to distinguish the two.

We implement this estimator as follows. Say $\beta_i = h\left(Z_i, X_i\right) + u_i$ (where h is a flexible function of X and Z, and u_i is independent of (X_i, Z_i)). Then, for each value of Z in the support of $\bar{Z}$:

$$\begin{aligned} & E\left(\beta_i | HS\left(\bar{Z}_i - \delta_i\right) - HS\left(\bar{Z}_i + \delta_i\right) = 1, X_i, Z_i = \bar{Z}_i\right) \\ = \; & h\left(\bar{Z}_i, X_i\right) + E\left(u_i | -\eta - \tau - h\left(\bar{Z}_i, X_i\right) < v_i \leq -\eta - h\left(\bar{Z}_i, X_i\right)\right) \\ = \; & h^*\left(\bar{Z}_i, X_i\right). \end{aligned}$$

We recover this object by estimating the following system:

$$Y_i = \alpha + h^*\left(Z_i, X_i\right) \times HS_i + g\left(Z_i, X_i\right) + \varepsilon_i \tag{7}$$

$$HS_i = 1\left[\eta + \tau E_i + h\left(Z_i, X_i\right) + v_i > 0\right]. \tag{8}$$

For simplicity, we model $h^*\left(Z_i, X_i\right)$ as:

$$\begin{gathered} h^*\left(Z_i, X_i\right) = \beta_0 + \beta_1 \times (family\ (log)\ income\ at\ age\ 4)_i \\ +\beta_2 \times (family\ (log)\ income\ at\ age\ 4)_i^2 + \beta_3 \times (family\ size\ at\ age\ 4)_i + \beta_4 \times (family\ size\ at\ age\ 4)_i^2 \\ +\beta_5 \times (family\ (log)\ income\ at\ age\ 4)_i \times (family\ size\ at\ age\ 4)_i. \end{gathered}$$

Potentially we would like to estimate $h^*\left(Z_i, X_i\right)$ using a more flexible specification, but our sample size forces us to be parsimonious. We report estimates of β_0, β_1, β_2, β_3, β_4 and β_5, as well as estimates

[14] Say program participation is determined by equation (5), and β_i is correlated with v_i. Even if β_i is independent of Z_i,

$$E\left[\beta_i | HS\left(\bar{Z}_i - \delta_i\right) - HS\left(\bar{Z}_i + \delta_i\right) = 1, X_i\right] = E\left[\beta_i | -\eta - \tau - h\left(\bar{Z}_i, X_i\right) < v_i \leq -\eta - h\left(\bar{Z}_i, X_i\right), X_i\right],$$

which is a function of $\bar{Z}_i$. Intuitively, the set of v_i for individuals at the margin varies with the level of $\bar{Z}_i$.

of the average partial effect of Head Start. We also display graphical representations of $h^*(Z_i, X_i)$. As before, equation (8) is estimated assuming v_i has a normal distribution (probit). Whenever Y_i is a discrete outcome we assume that ε_i is also normal, and estimate the system using a bivariate probit. Even with such parsimonious specification our estimates of this function in the next section are quite imprecise, and results should be seen as suggestive and illustrative of the potential of this approach.

4 Results

4.1 Validity of the Procedure

Our identifying assumption in this setting is that children *just* above the income eligibility cutoff are equal to those *just* below it in all dimensions except program participation. A priori this is a plausible assumption, since it is very difficult for any family to purposely locate *just* below (or *just* above) the eligibility cutoff in order to gain access to the program. Still, there are strong incentives for a family to behave this way. For example, a family just above the income cutoff could try to underreport income in order to become just eligible. Similarly, Head Start providers who know the eligibility rules well, and who have a desire to serve children who are easy to care for, may try to game the system in order to accept a large proportion of those children who are just ineligible. Fortunately, there are several sources of information on which we can draw on to understand the importance of these concerns.

We start this section with a standard test of the validity of our identification assumptions. We take a set of pre-program variables that should not be affected by participation in the program, and we use them as dependent variables in equation (2). If our procedure is valid then the estimate of γ should be equal to zero. These variables are: birth weight, whether the child was breastfed, mother's age at child's birth, mother's AFQT score, mother's education, and average log family income and family size between the ages of 0 and 2. Eligibility is measured at age 4, as explained above. $f(Z_i, X_i)$ consists of fourth order polynomials in log family income and family size at age 4, an interaction between these two variables, a dummy indicating the presence of a father figure (father or step-father) in the household at age 4, gender, race and age dummies, and dummies for year and state of residence at age 4. Panel A of table 2 presents the results for the whole sample, while panel B focuses on blacks only. Results for the older group (16-17) are similar (and available from the authors). Unless mentioned otherwise, all standard errors in the paper are clustered at the level of the mother, since each mother may have more than one child in the sample.[15]

Table 2 shows that our procedure is valid. Most estimates of γ are small (compared with the mean and standard deviation of each variable in table 1), and all of them are statistically insignificant. In order to better understand the magnitude of these estimates we conducted the following exercise. Take a few of our main outcomes of interest, such as BPI and grade repetition at ages 12-13, and ever sentenced by ages 16-17. Then regress each outcome on each of the variables in table 2 (one regression per column), and compute predicted values for each regression. We can now rerun the regressions on table 2 using these predicted values instead of the variables that generated them, allowing us to translate the coefficients in table 2 into magnitudes of the outcomes of interest. We do not report this in a table, but describe the results briefly: in terms of BPI, all the coefficients in table 2 are between -0.017 and 0.011 (expressed as a fraction of a standard deviation), for grade repetition they are between -1.4 and 1.8 percentage points (grade repetition has a mean of about 35%), and for ever sentenced up to ages 16 to 17 they are between -0.6 and 0.5 percentage points (ever sentenced has a mean of about 15%). All these figures are very small. Throughout the rest of the paper we augment our basic specification of $f(Z_i, X_i)$ with some of the variables in table 2 as additional covariates, since they are useful to reduce sampling error and small sample bias (e.g., Imbens and Lemieux, 2007). In particular, we add fourth order polynomials in average log family income and average family size between ages 0 and 2, an interaction between the two, and a fourth order polynomial in birth weight.[16]

[15]Column "All" of Tables A2.1 and A2.2 in Appendix presents estimates when our specification includes an extended set of controls. In particular, besides the controls included in our "Basic" specification (fourth order polynomials in log family income and family size at age 4, an interaction between these two variables, a dummy indicating the presence of a father figure in the household at age 4, fourth order polynomials in average log family income and average family size between ages 0 and 2, an interaction between the two, and a fourth order polynomial in birth weight, race and age dummies and dummies for year and state of residence at age 4), we also add polynomials up to the fourth order on mother's AFQT, on mother's age at child's birth, on mother's highest grade completed when child was three years old and an indicator for whether the child was breastfed. We do not use the full set of controls in table 2 because in such an extended specification our results are slightly more imprecise, although they have similar magnitude and sign.

[16]An alternative and more direct test, developed by McCrary (2007), checks whether there is bunching of individuals just before the discontinuity. This test is not practical with multiple discontinuities unless we have a large sample size. However, when we implemented it using a single discontinuity (using percentage distance to the eligibility cutoff as the running variable) we found no evidence of income manipulation. Moreover, since we have panel data on maternal income we can check whether there is direct evidence of income manipulation. Our assumption is that if mothers are underreporting income for the purposes of becoming eligible for Head Start, then they are also likely to be underreporting income in the survey. In addition, if they are far away from the discontinuity, they have no incentive to missreport income. Under these conditions, if manipulation of income close to the income eligibility cutoffs was an empirically important phenomenon then we would expect income to be unexpectedly low whenever the mother is just below the cutoff. In order to test this formally we run a regression of family income on child fixed effects, dummies for year and age of the mother, and a dummy indicating whether the child is just below the income eligibility cutoff (more precisely,

4.2 Estimates from the Reduced Form Equations

We proceed by checking whether the discontinuity in eligibility status also induces a discontinuity in Head Start participation, by estimating equation (5) (participation equation). We present estimates for the main sample described above, and for a sample of black children, for whom behavior problems are likely to be especially serious. Table 3 shows estimates of τ in equation (5), and for the average marginal change in participation as the eligibility status varies, which is defined by:

$$\frac{1}{N}\sum_{i=1}^{N}\left\{\Pr\left(HS_i = 1|E_i = 1, Z_i, X_i\right) - \Pr\left(HS_i = 1|E_i = 0, Z_i, X_i\right)\right\} =$$

$$\frac{1}{N}\sum_{i=1}^{N}\left[\Phi(\eta + \tau + h\left(\bar{Z}_i - \delta_i \leq Z_i \leq \bar{Z}_i + \delta_i, X_i\right)) - \Phi(\eta + h\left(\bar{Z}_i - \delta_i \leq Z_i \leq \bar{Z}_i + \delta_i, X_i\right))\right]$$

which we call the "mean change in marginal take-up probability" (where N is the number of children in the sample, and Φ is the standard normal c.d.f.).[17]

Each set of columns in table 3 corresponds to a different age group. The top panel refers to the whole sample, while the bottom panel refers only to blacks. For each outcome we present estimates of τ in a model without controls (columns 1 and 3), and in a model with all the control variables (columns 2 and 4). Across ages and samples, eligibility is a strong predictor of program participation, although the estimated effect is well below 100%. This is an indication of weak take-up of the program at the margin of eligibility (common to many social programs), which could be a result of several factors, such as lack of available funds to cover all eligible children (since Head Start was never fully funded), stigma associated with program participation (Moffitt, 1983), or the fact that most of the centers are only part-day programs, and thus unable to satisfy the needs of working families (Currie, 2006).[18]

her family income corresponds to 90 to 99% of the income eligibility cutoff). In results available on request we find no evidence that there is underreporting just below the cutoff. Oddly, if anything, the opposite is true. This conclusion holds even if we add to this regression a dummy indicating whether the child is between the ages of 3 and 5, and we interact it with the dummy indicating whether the child is just below the cutoff. These tests are available from the authors.

[17] In results not presented in the paper, we also estimate this effect at the median of the distribution of effects, as well as the effect averaged over the set of individuals whose family income was between 75 and 125 percent of the income discontinuity. All the estimates produced the same results.

[18] Our paper is novel in obtaining estimates of Head Start take-up for individuals near the eligibility threshold as the eligibility status change (although we consider a continuum of eligibility thresholds). Most of the evidence of how newly eligible to social programs respond in terms of participation comes from Medicaid expansions throughout the 1980s and early 1990s. Cutler and Gruber (1996) and Currie and Gruber (1996) estimate that only 23 and 34 percent of newly eligible children and women of childbearing age take-up Medicaid coverage, as many were already covered by

Table 3 shows how Head Start participation responds to eligibility, but it also raises the following question: in which type of child care would children enrol in the absence of the program? The answer is crucial for interpreting the results, since it defines the "control group" in our study. While it is possible to reconstruct the child care experiences during the first three years of life for all children from mothers' reports, for children aged 3 to 5 (when Head Start is available) the information about child care arrangements is less detailed. The information we use is the following. Since 1988 child surveys include questions, posed to the mother for children three years of age or older on whether the children attend nursery school or a preschool program or had ever been enrolled in preschool, day care, or Head Start. Using the age when first attended Head Start and the length of time attending we construct an indicator of Head Start attendance between ages 3 to 5. This is our treatment variable.[19] We recover information about preschool attendance from the question "Ever enrolled in preschool?". The alternative child care arrangements between ages 3 to 5 we consider are:

$$HS_i = 1[Ever\ in\ Head\ Start\ between\ ages\ 3\ to\ 5]$$

$$OP_i = 1[Ever\ enrolled\ in\ preschool\ but\ not\ in\ Head\ Start]$$

$$Home_i = 1[Never\ in\ Head\ Start\ or\ in\ any\ other\ preschool]$$

where 1[.] is the indicator function ($Home_i$ denotes any other child care arrangement). Table 4 shows how participation in the three alternative child care arrangements respond to eligibility. We regress the dummy variables indicating participation in each type of child care on eligibility and the remaining control variables, as in Table 3. Across age and race groups, when an individual becomes Head Start eligible there is only a statistically significant movement out of Home Care and into Head Start. Therefore we interpret our estimate of β (from equation (4)) as the effect of Head Start relatively to Home care.[20]

other insurance. In our sample, 40.4% percent of those eligible at age four who did not attend Head Start were enrolled in another preschool program. Card and Shore-Sheppard (2002) find that expansion of Medicaid eligibility to children whose family income was below 133 percent of the poverty line had no effects on the decision of take-up, whereas the expansion of eligibility to all poor children led to an increase of nearly 10 percent in Medicaid coverage. LoSasso and Buchmueller (2002) estimate that take-up rates among newly eligible children for SCHIP (State Children's Health Insurance Program) ranged between 8 and 14 percent.

[19] The questions used to construct the indicator of Head Start attendance are: "Child ever enrolled in Head Start program?", "Child's age when first attended Head Start?" and "How long was child in Head Start?".

[20] The estimates for the marginal change in the take-up of the three child care alternatives do not change if a multinomial logit model is estimated instead of separate probit models for each choice. For the sample of all children 12 and 13 years old the mean of individuals' marginal effect of eligibility at age 4 on "Head Start" is 0.136, the marginal

Tables 5 and 6 are the central tables of our paper. They present estimates of equation (2) (a regression of outcome on eligibility) for the main set of outcomes. Table 5 refers to ages 12-13, and table 6 refers to ages 16-17. Again, we consider two specifications for each outcome: one without control variables (not controlling for selection), and one with all the controls (where the reported coefficient corresponds to an "intent to treat" estimate). In the first column we expect to see eligible children having worse outcomes than ineligible children, because they are in poorer households. In the second column we will have an estimate of the impact of the program. Recall that we restrict the sample to children whose family income is between 5% and 195% of the income eligibility cutoff (this excludes middle and high income children).

The first column of table 5 shows that eligible children have a Behavior Problems Index which is 0.15 of a standard deviation worse than ineligible children. Columns 3, 5, 7 and 9 also show that they have much higher rates of grade repetition (7.4 percent higher among eligible children), and enrolment in special education (4.8 percent higher among eligible), while there is no apparent difference in obesity. In contrast, the estimates in the second column for each outcome document that, as a result of Head Start eligibility, problem behaviors improve by ages 12-13, the probability of grade repetition decreases on average by 11.6%, and the incidence of obesity is reduced by 8%. Among blacks, the program only appears to have a strong effect on obesity, and it has an unexpected negative impact on enrolment in special education. The latter may be just the result of sampling error, given that both at ages 10-11, and 14-15, the coefficient is negative, as expected (available from the authors).

Table 6 shows that, if we do not account for selection into Head Start, eligible adolescents are more likely to have been sentenced for a crime by ages 16 to 17 (either in the entire sample and for Blacks) and more likely to ever have smoked. Once selection is appropriately accounted for, Head Start improves the incidence of depression in late adolescence (measured by CESD) as well as obesity. When we focus on blacks, the strongest effect is on the probability of ever being sentenced up to the age of 16-17 (a decrease of 18%). Tables A2.1 and A2.2 in Appendix A show (for a selected set of outcomes) that these results are robust to the degree of the polynomial we choose to specify

effect on "Other Preschool" is 0.13, and for "Home" the marginal effect is -0.266. Among Black children the marginal effects of eligibility on each alternative are 0.18, 0.106 and -0.295 for "Head Start", "Other Preschool" and "Home", respectively. Similar results were found for the sample of 16-17 years individuals and are available from the authors.

$g(Z_i, X_i)$, whereas table A3 shows robustness to the size of the window of data chosen around the discontinuity, and table A4 reports the partial effects (and standard errors) when we allow $f(Z_i, X_i)$ in equation (2) to be a different function in either side of the discontinuity. Notice that the latter case allows for heterogenous effects of the program as the "Effect at Mean" is a function of the child's income threshold, which in turn depends on a bundle of observable family characteristics (see section (3)).[21] When we allow for different specifications on either side of the threshold estimates become quite imprecise, although they have the same sign and roughly similar magnitudes to the ones we report here.[22] Therefore we proceed with the simpler and more robust specification.

In appendix table A5 we analyze the following additional outcomes at ages 16 and 17: alcohol and marijuana use and enrolment in school. We were not able to reject the hypothesis that Head Start had no impact in each of these outcomes, although in some cases standard errors were too large for our estimates to be informative. In the appendix we also present estimates of equations (5) and (2) for the sample of non-black males (tables A6 to A8), which are similar to the ones we report in the main text, but with a weaker "first stage" relationship.

It is standard practice to also present a graphical analysis of the problem. However, the standard setting has a single discontinuity and, since our setup makes use of a range of discontinuities, this is not practical. One alternative that does not correspond exactly to the specification of our model is, as mentioned above, to measure every household's income relatively to their income eligibility cutoff, and define the variable distance to the eligibility cutoff. In the appendix we plot Head Start participation (figures A1 and A2, for ages 12-13 and 16-17, respectively) and some selected outcome variables (figures A3, A4, A5, A6, A7, A8, A9) against distance to the eligibility cutoff as a percentage of the income cutoff for the samples we use in our main specifications. We divide the sample into bins of this variable (size equal to 9.5%) and compute cell means for the variable of interest, we draw a vertical line at zero (point of discontinuity), and we run local linear regressions of each variable on distance to cutoff on either side of the discontinuity (bandwidth = 0.3).

The figures suggest that there are large discontinuities in program participation at the eligibility

[21] The "Effect at Mean" is computed obtained by: $E\left[Y_i|Z_i = \bar{Z}(X_i) - \delta\right] - E\left[Y_i|Z_i = \bar{Z}(X_i) + \delta\right] = \gamma + h(\bar{Z}(X_i))$. See note of table A4 for details on the model estimated.

[22] Standard errors for the estimated "Effect at Mean" are also presented in table A4. These were obtained by 249 bootstrap replications. We use the non overlapping block bootstrap procedure described in Lahiri (1999). Blocks are defined by mother.

cutoff. There are also discontinuities in the level of most outcomes, generally with the same sign as in the tables above. The most important difference is in the variable ever sentenced for blacks, which shows a strong impact of the program in the table (which is quite robust, as seen above) but not in the figure. It is possible that the differences can be attributed to different specifications of the model and small sample: use of household income vs. distance to the eligibility cutoff as a percentage of the cutoff. In this setting we prefer the former specification because it corresponds more closely to a simple economic relationship (between inputs and outputs).

As mentioned in section 3, eligibility to Head Start is correlated with eligibility to other programs, such as AFDC, Medicaid, or SSI. It is therefore possible that the estimates in tables 5 and 6 confound the effects of Head Start with those of other programs.[23] However, while most of these programs exist during several years of the child's life, Head Start is only available when the child is between ages 3 and 5. This fact allows us to assess whether confounding effects from other programs are important. Our reasoning is as follows. Suppose that we estimate equation (2) using eligibility (as well as the covariates) measured at different ages of the child. If participation in other programs is driving our results, E_i should have a strong coefficient even when measured at ages other than 3 to 5. Otherwise, we can be confident that our estimates reflect the impact of Head Start, since it is unlikely that other programs affect child development only if the child enrols at ages 3 to 5, but have no effect if she enrols either at ages 0, 1 and 2 or at ages 6 and 7.

This reasoning will work if the set of individuals who are at the margin of eligibility at ages 3 to 5, are different from those who are at the margin of eligibility at ages 0, 1, 2, 6 and 7. If they were all the same individuals it would be impossible to distinguish eligibility to Head Start (only at ages 3 to 5) from eligibility to other programs (at all ages). Table 7 presents estimates for a representative set of outcomes, one for each panel (the remaining outcomes show the same patterns, and are available from the authors). Each column represents a different regression, where the age of eligibility (and the corresponding controls) goes from 0 to 7. Across panels, the largest and strongest estimates occur consistently at age 4, and sometimes 5 (grade repetition in panel A1, probability of being overweight among children in panels A2 and B2 and probability of being overweight among

[23]Almond, Hoynes and Schanzenbach, 2007, study the impact of Food Stamp program on infant health and also address the possibility of confounding the effect of Food Stamp with the effect of Head Start, AFDC or Medicaid, as these were also expanded or introduced during the period of introduction of FSP. See Keane and Moffitt, 1998, for effects of multi-program participation on women' labor supply.

adolescents in panel A5) while for all other ages the coefficients are generally small and insignificant (with a few exceptions). We take this as evidence that (in our main specifications) we are capturing the effect of Head Start and not of other programs.

In appendix table A9 we show that eligibility at ages 0-2 does not predict program participation, eligibility at ages 3-5 strongly predicts program participation, and for later ages there is some predictive power but it is slightly weaker than at ages 3 to 5. Therefore, the population of children for whom we are able to estimate the impact of Head Start (those at the margin of eligibility at that age) is likely to consist of children who suffer income shocks between the ages of 3 and 5 (we account fully for these shocks through our set of controls). We are not able to estimate the impact of Head Start on those who are permanently and substantially below the poverty line. Our results are most useful to think about marginal expansions of the program, not for evaluating the effectiveness of the program on the whole population that it currently serves.

4.2.1 Testing for No Program Impact with Multiple Outcomes

Since we are examining the impact of a program on multiple outcomes there is a danger that some of our results are spuriously statistically significant. If we are doing hypothesis testing with a significance level of 5% (10%), even if the program has no effect, it will show statistically strong results for 5% (10%) of the outcomes we examine. Several procedures can be used to account for this, but the most recent one is developed in Romano and Wolf (2005) (which accounts for non-independence across outcomes, and has significantly large power than most of its predecessors, in particular than Westfall and Young, 1993 algorithm 2.8). We apply their procedure (see Appendix C for a detailed description) to tables 5 and 6 (separately), and the results are as follows. For the whole sample at ages 12 and 13 (first panel of table 5), we can reject that the program has no effect on BPI, grade repetition, and probability of being overweight, using a two tailed test with a 10% significance level controlling for family wise error rate; for blacks, we find strong effects for overweight status and special education (the latter with the opposite sign from the one we expected). When we reexamine table 6, we reject that the program has no effect on CESD and probability of ever being sentenced in the whole sample, with a 10% level of significance; for blacks, we reject that the program has no effect on the probability of ever being sentenced at a 10% level. In doing this exercise, we also include the three of the four cognitive tests of table A10 (PPVT is excluded because the small number of observations), and ever tried marijuana, ever tried alcohol, and still enrolled in school by ages 16 and 17. In sum, our conclusions regarding the statistical significance of the parameters in tables 5 and 6 are essentially the same whether we perform individual tests on the coefficients, or we apply the more robust procedure of Romano and Wolf (2005).

4.3 Estimates from the Structural Equations

The reduced form analysis of table 5 tells us that there are strong effects of Head Start on behavior problems, the risk of being overweight, and grade repetition at ages 12 and 13, while table 6 shows strong effects on depression, risk of being overweight, and crime. These two tables summarize our main results, but the estimates in these tables do not correspond to the quantitative impact of the program on individuals because compliance with the program is imperfect, and eligibility does not equal participation. These estimates need to be scaled up by the estimated effect of eligibility on participation, and the best way of doing this is to estimate equation (4) jointly with (5) (Van der Klauww, 2002). In doing so, we encountered two problems, which reflect some instability in the procedure. First, some of the estimated effects became quite imprecise. Second, some of the estimated effects turned out to be larger than we expected based on our estimates of equations (5) and (2). In spite of this, the main patterns of tables 5 and 6 remain roughly unchanged. Therefore, we use them to guide our reading of the remaining estimates of this section.

Behind the problem may be the fact that either one or both equations in this system are non-linear. This is particularly true when we estimate bivariate probits, which involve maximizing non-concave likelihood functions with more than one local maximum. For each outcome we started the optimization routine at different initial values, and the results we report correspond to the maximum values of the likelihood that we found. We experimented extensively with different initial values and different optimization algorithms, and we report our most robust results.[24]

Table 8 shows results at ages 12-13. For each outcome we present 2 columns: 1) estimates of β from equation (4) without accounting for endogenous program participation; 2) estimates of β coming

[24]We started each algorithm by using as initial values the estimates of the coefficients when the equations were estimated separately. We then let the model run until a local maximum was reached. We recorded the estimated coefficients, constructed new initial values by multiplying them by a constant between 0.5 and 2 (e.g., $\lambda = 1.1$, or $\lambda = 0.9$), reran the optimization algorithm, and compare the across different local maxima to pick the highest one.

from the system consisting of (4) and (5), which accounts for selection into the program. We expect the estimates in the first set of columns to be biased towards a negative effect (or no effect) of the program, since Head Start targets poor children, who have worse outcomes than less poor children (the bias could be in the opposite direction if more motivated mothers were more likely to enrol their children in Head Start). The table reports estimates of β, as well as average marginal effects of Head Start on outcomes (labeled *effect at mean*). For discrete outcomes, the latter is:

$$\frac{1}{N}\sum_{i=1}^{N}\{\Pr[Y_i=1|HS_i=1,Z_i,X_i]-\Pr[Y_i=1|HS_i=0,Z_i,X_i]\}=$$
$$\frac{1}{N}\sum_{i=1}^{N}\{\Phi[\alpha+\beta+g(Z_i,X_i)]-\Phi[\alpha+g(Z_i,X_i)]\}$$

At ages 12-13, we estimate that participation in Head Start leads to a 0.17 standard deviation decrease in the behavior problems index for the whole sample, a close to 18 percentage point reduction in the risk of obesity both for the whole sample and among blacks, and a 35% reduction in grade repetition for the whole sample (Currie and Thomas, 1995, report a similar figure of 47% among Whites). Among these, only the impact on grade repetition is statistically significant both in this table and in the reduced form results. The other ones we mention are only statistically significant in the reduced form analysis, and the remaining ones are not statistically significant neither in the reduced form analysis nor in the structural analysis.

Table 9 shows that, at ages 16-17, we estimate that the program leads to a 18 percentile points decrease in the depression score for the whole sample, a 31% decrease in the probability of being sentenced for a crime among blacks, and a 34% decrease in the risk of being obese for the whole sample. Again, the impact on depression is not statistically significant here in spite of being statistically strong in the reduced form analysis. Oddly, the estimated impacts of the program on crime for the whole sample and on obesity for blacks are statistically significant, in spite of this not being true in the reduced form analysis. Perhaps the additional structure imposed by normality improves the precision the estimates, but we cannot also rule out misspecification error.[25]

[25]The variance matrix of the Likelihood Estimator for discrete outcomes in tables (8) and (9) is obtained by the outer-product of the gradient.

Tables 8 and 9, and especially tables 5 and 6 (and the subsequent sensitivity analysis), present a picture of strong effects of Head Start on behavioral outcomes of children, which are sustained at least until adolescence. We should mention that, using the same methodology, we were unable to find significant effects of Head Start participation on cognitive test scores, namely the Peabody Individual Achievement Tests for Math, Reading Recognition, and Reading Comprehension, and the Peabody Picture Vocabulary Test. The reason we do not report these results in the main text is because the standard errors are too wide for the analysis to be informative (they are, however, shown in the appendix table A10). However, it is interesting that in the case of behavioral outcomes we were able to find a consistent set of large and statistically significant results. As stressed by Cameron, Heckman, Knudsen and Schonkoff (2007), this may be due to the fact that non-cognitive skills are more plastic than cognitive skills, and early childhood interventions are more likely to have sustained effects on the former than on the latter. Another possible explanation for this difference may be that test scores measure ability with error, while direct measures of behavior are less prone to measurement error.

It is important to notice that because of multiple discontinuities we estimate the impact of Head Start averaged over a large set of different children. In figure 1 we displayed the range of household income values over which there is variation in the eligibility cutoff in our data. However, there is also variation across different family sizes. In Figure 2 we plot the joint support of household income and family size over which we are able to estimate the relevant treatment effect. It shows that the values of income over which we can identify treatment effects strongly depend on family size.

When we estimated the model in equations (9) and (10) results were fairly imprecise for several outcomes (even when we used with simpler specifications). Therefore, we chose to focus on special education alone, the outcome in which we are more confident of the estimates. We report the remaining ones in appendix table A12. Table 10 presents estimates of the impact of Head Start on enrollment in special education when this impact it is allowed to vary across family income and family size. It shows estimates for β_0, β_1, β_2, β_3, β_4 and β_5 in equation (9), as well as for partial effects of Head Start on outcomes and the likelihood ratio test (Wald test for continuous outcomes displayed in table A11) for the joint significance of (β_1, β_2, β_3, β_4,β_5) (test of the importance of heterogeneity).[26]

[26] We do not present standard errors for partial effects of Head Start in tables 10 and A11 due to sparseness and instability of covariance matrix for the estimated coefficients. Standard errors for a simpler model estimated without year and state dummies are available from the authors.

The estimates of the impact of Head Start on participation in special education classes are not statistically significant in tables 5 and 8, but they become significant for the whole sample once we account for heterogeneity. It is interesting that the strongest effects of participating in the program are for children in small and relatively richer families in the sample. Notice also that the amount of impact heterogeneity is very large, and can be as large as -0.3, or as small as zero (see Figure 3).

5 Summary and Conclusions

In this paper we study the impact of Head Start (a preschool program for poor children) on the behavioral problems of children, and on risky behaviors of adolescents. Our identification is based on the fact that the probability of program participation is a discontinuous function of household income (and family size) because of the program's eligibility rules, enabling us to use a "fuzzy" regression discontinuity design. An unusual feature of our problem is that there is a continuous range of discontinuity cutoffs, which vary with family size, family structure, year and state. Therefore we are able to identify the effect of the program over a large range of individuals, and are also able to estimate how it varies in the population.

Unfortunately, we are agnostic about the mechanisms by which the program causes changes in children. It may be the program itself, and its curricula. Or it may also happen that the program has some effect through its parental component. Or it may be that Head Start participation enables parents to enter employment, leading to changes in family environments. Understanding the mechanisms through which the program works is specially relevant given the mixed evidence from the effects of U.S. Welfare Reform in the 1990s in children's outcomes (Grogger and Karoly, 2005). This is a question we leave for future research.

We find that Head Start decreases behavioral problems, probability of grade retention, and obesity at ages 12 to 13, and depression, criminal behavior, and obesity at ages 16 and 17. These effects are large and sustained. They show the potential for preschool programs to improve outcomes of poor children, even when they are universal programs such as Head Start.

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Tables

Table 1: Summary statistics of data

	Full sample		Income at age between 5% and 195% of cutoff							
	All children		All children		African-American		HS Children		Non HS children	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Outcomes										
Ages 12-13										
BPI (standard score)	0.41	0.97	0.55	0.99	0.58	1.02	0.63	1.03	0.52	0.98
Ever repeated a grade (%)	0.27	0.45	0.33	0.47	0.35	0.48	0.36	0.48	0.32	0.47
Attending special education (%)	0.20	0.40	0.25	0.43	0.24	0.43	0.25	0.43	0.25	0.43
Overweight (%)	0.18	0.39	0.19	0.39	0.18	0.38	0.19	0.40	0.18	0.39
Ever Smoke (%)	0.30	0.46	0.35	0.48	0.31	0.46	0.37	0.48	0.35	0.48
Ages 16-17										
CESD (percentile score)	42.53	28.16	43.83	28.55	43.67	27.82	43.24	28.25	44.10	28.70
Ever Sentenced (%)	0.12	0.32	0.15	0.35	0.14	0.35	0.19	0.39	0.13	0.33
Overweight (%)	0.16	0.37	0.17	0.38	0.17	0.37	0.17	0.38	0.17	0.37
Ever Smoke (%)	0.56	0.50	0.61	0.49	0.56	0.50	0.63	0.48	0.60	0.49
Child care - proportion by option										
Head Start	0.21	0.41	0.30	0.46	0.41	0.49	1.00	0.00	0.00	0.00
Preschool	0.44	0.50	0.32	0.47	0.26	0.44	0.00	0.00	0.45	0.50
Neither	0.34	0.48	0.38	0.49	0.33	0.47	0.00	0.00	0.55	0.50
Household characteristics at age 4										
Proportion of eligible children	0.35	0.48	0.59	0.49	0.72	0.45	0.77	0.42	0.51	0.50
Annual Family Income (2000 dollars)	38477.81	55357.96	18493.80	10613.08	16101.32	10266.76	15699.19	10338.33	19695.37	10506.92
Family Size	4.38	1.62	4.54	1.90	4.78	2.19	4.58	1.91	4.52	1.89
Father figure present	0.71	0.45	0.55	0.50	0.32	0.47	0.46	0.50	0.60	0.49
Proportion of poor families	0.35	0.48	0.58	0.49	0.71	0.46	0.75	0.43	0.50	0.50
Average household characteristics before age 3										
Annual Family Income (dollars of 2000)	37213.69	56427.70	21848.08	19415.59	18120.76	23241.92	17712.20	11619.25	23626.35	21694.82
Family Size	4.10	1.63	4.40	1.85	4.87	2.12	4.54	1.82	4.34	1.87
Proportion of poor families	0.36	0.41	0.54	0.41	0.68	0.38	0.67	0.37	0.49	0.42
Child's characteristics										
Birth weight (ounces)	118.60	21.95	116.54	22.26	112.52	22.81	117.00	21.31	116.35	22.66
Breastfed	0.42	0.49	0.35	0.48	0.17	0.37	0.23	0.42	0.40	0.49
Mother's Characteristics										
Mother's age at child birth	23.41	4.06	22.43	3.95	22.21	4.05	22.28	3.96	22.49	3.94
Years of schooling	12.08	2.24	11.41	2.07	11.75	1.78	11.23	1.95	11.49	2.12
Proportion with high school degree or above	0.75	0.43	0.64	0.48	0.67	0.47	0.62	0.49	0.65	0.48
Proportion without high school degree	0.25	0.43	0.36	0.48	0.33	0.47	0.38	0.49	0.35	0.48
AFQT percentile score	34.58	26.66	25.69	22.87	16.55	16.16	20.13	18.42	28.03	24.13
Number of Individuals	3029		1766		680		531		1235	

Note: This table reports means and standard deviations for outcomes and control variables in our sample. Outcomes' statistics at each age group are reported for those (males) individuals whose controls are all not missing in the entire sample in columns (1) and (2) and whose family income at age four is between 5% and 195% of the maximum level of income that would allow participation in Head Start (columns (3) to (10)). The minimum income at age 4 in the sample we use in regressions is $723.06 and the maximum is $84,939.59 (as opposed to a minimum of $3.68 and a maximum of $1,539,248 for the entire sample). As variables for child care arrangement and control variables (characteristics at age 4 and before age 3 and mother characteristics) are constant across ages, we report means and standard deviation using only one observation per individual.

Table 2: Falsification exercise. Pre-Head Start age outcomes.

	Birth Weight	Breastfed	Mother's age at child birth	Mother's AFQT	Mother's Education	Family Size (0-2)	Family Income (log 0-2)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: All							
1 if Head Start eligible at age 4	-4.381	0.034	0.063	-1.743	0.338	-0.112	-0.022
	[2.814]	[0.053]	[0.237]	[2.291]	[0.223]	[0.184]	[0.088]
Observations	1349	1334	1349	1311	1349	1349	1349
Panel B: African-American							
1 if Head Start eligible at age 4	-2.814	0.097	-0.319	-3.317	-0.190	0.067	-0.168
	[4.744]	[0.075]	[0.375]	[3.414]	[0.289]	[0.369]	[0.131]
Observations	581	570	581	569	581	581	581

Note: This table reports OLS regressions of outcomes on the indicator of eligibility status at age four and a flexible function of the variables that determine eligibility at age four. Outcome variables are measured before age three (first age at which children may enrol in Head Start) and include child's birth weight, an indicator for whether the child was breast-fed, mother's age at child's birth, mother's AFQT score, mother's highest grade completed when child was three years old, average log family income and average family size between birth and age 2. Controls excluded from the table include fourth order polynomials in log family income and family size at age 4, an interaction between these two variables, a dummy indicating the presence of a father figure (father or step-father) in the household at age 4, race and age dummies, and dummies for year and state of residence at age 4. Regressions are restricted to the sample of individuals whose family income at age four is between 5 and 195 percent of the maximum level of income that would have enabled eligibility. Robust standard errors are reported in brackets and are clustered at mother's level.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: Probit regression of Head Start participation on Head Start income eligibility.

Age groups	12-13		16-17	
	(1) No Controls	(2) Controls	(3) No Controls	(4) Controls
	Panel A: All			
1 if Head Start eligible at age 4	0.660	0.602	0.716	0.634
	[0.080]***	[0.177]***	[0.090]***	[0.202]***
Mean change in take-up probability	0.221	0.174	0.238	0.184
Observations	1349	1349	1104	1104
	Panel B: African-American			
1 if Head Start eligible at age 4	0.368	0.890	0.425	0.850
	[0.124]***	[0.298]***	[0.138]***	[0.367]**
Mean change in take-up probability	0.140	0.256	0.160	0.242
Observations	581	581	476	476

Note: This table reports results of probit regressions of Head Start participation on income eligibility. The mean change in marginal take-up probability is computed by the average marginal change in the probability of Head Start participation across individuals as the eligibility status changes and all other controls are kept constant. For each age group and sample we present two columns: columns (1) and (3) do not include any controls, and columns (2) and (4) control for determinants of income eligibility at age 4 and for some pre-Head Start age variables. Controls excluded from columns (2) and (4) are fourth order polynomials in log family income and family size at age 4, an interaction between these two variables, a dummy indicating the presence of a father figure in the household at age 4, fourth order polynomials in average log family income and average family size between ages 0 and 2, an interaction between the two, and a fourth order polynomial in birth weight, race and age dummies, and dummies for year and state of residence at age 4. Robust standard errors are reported in brackets and are clustered at mother's level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: Alternative child care arrangements between ages 3 and 5.

	Ages 12-13			Ages 16-17		
	Head Start	Other Preschool	Home	Head Start	Other Preschool	Home
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: All					
1 if Head Start eligible at age 4	0.602	0.117	-0.621	0.634	0.158	-0.684
	[0.177]***	[0.163]	[0.161]***	[0.202]***	[0.178]	[0.178]***
Mean change in marginal take-up probability	0.174	0.034	-0.204	0.184	0.046	-0.221
Observations	1349	1349	1349	1104	1104	1104
	Panel B: African-American					
1 if Head Start eligible at age 4	0.890	-0.204	-0.804	0.850	-0.287	-0.721
	[0.298]***	[0.296]	[0.280]***	[0.367]**	[0.327]	[0.333]**
Mean change in marginal take-up probability	0.256	-0.055	-0.232	0.242	-0.075	-0.194
Observations	581	581	581	476	476	476

Note: This table reports results of probit regressions of participation in different types of child care arrangements at preschool age (3 to 5 years old) on Head Start income eligibility. The child care alternatives between ages 3 and 5 are Head Start, Other Preschool or neither of the previous two. The dummy variable "Head Start" takes value 1 if the child was ever enrolled in Head Start between ages 3 and 5 and 0 otherwise; the indicator "Other Preschool" is equal to 1 if the child was ever enrolled in some form of preschool other than Head Start and does not report to have been enrolled in Head Start between ages 3 to 5, and 0 otherwise; "Home" takes value 1 if the child did not attend Head Start or any other form of preschool. See table (3) for description of mean change in marginal take-up probability and controls excluded from the table. Robust standard errors are reported in brackets and are clustered at mother's level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Reduced Form Regressions: Regressions of outcomes at ages 12-13 on Head Start income eligibility at age four.

	BPI		Overweight		Ever Smoke		Grade Repetition		Special Education	
	No Controls	Controls	No Controls	Controls	No Controls	Controls	No Controls	Controls	No Controls	Controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: All children										
1 if Head Start eligible at age 4	0.148	-0.217	0.016	-0.330	0.212	-0.050	0.207	-0.364	0.153	-0.047
	[0.059]**	[0.117]*	[0.084]	[0.176]*	[0.074]***	[0.159]	[0.077]***	[0.160]**	[0.079]*	[0.168]
Effect at Mean	0.148	-0.217	0.004	-0.081	0.078	-0.016	0.074	-0.116	0.048	-0.014
Mean	0.550		0.186		0.354		0.335		0.250	
S.D.	0.994		0.389		0.479		0.472		0.433	
Observations	1261		1324		1360		1347		1308	
Panel B: African-American children										
1 if Head Start eligible at age 4	0.287	0.121	-0.190	-0.690	0.412	0.379	0.407	-0.265	0.199	0.548
	[0.095]***	[0.195]	[0.139]	[0.288]**	[0.130]***	[0.292]	[0.125]***	[0.260]	[0.135]	[0.270]**
Effect at Mean	0.287	0.121	-0.053	-0.171	0.138	0.106	0.144	-0.086	0.062	0.145
Mean	0.578		0.190		0.314		0.353		0.254	
S.D.	1.018		0.393		0.464		0.478		0.435	
Observations	537		553		577		572		548	

Note: This table reports results of regressions of outcomes measured at ages 12-13 on Head Start income eligibility at age 4. A description of the outcomes is provided in table A1 in Appendix A. A probit model is estimated for discrete outcomes (Grade Repetition, Ever Smoke, Overweight and Attending Special Education). Mean and S.D. are the mean and standard deviation, respectively, of the outcome variable. Two columns are presented for each outcome: one regression without any controls ("No controls") and other with all controls ("Controls"): fourth order polynomials in log family income and family size at age 4, an interaction between these two variables, a dummy for the presence of a father figure in the child's household at age 4, fourth order polynomials in average log family income and average family size between ages 0 and 2, an interaction between the two, and fourth order polynomials in birth weight, race and age dummies and dummies for year and state of residence at age 4. Effect at mean is the partial effect of HS_i, which is computed by taking the average of individual effects across our sample. For discrete outcomes the model estimated is

$$P[Y_i = 1|E_i, Z_i, X_i] \quad = \Phi(\phi + \gamma E_i + g\left(Z_i, X_i\right))$$

where Y_i is the outcome of interest for child i at ages 12 to 13, X_i is the vector of controls described above (entering through function $g\left(X\right)$), Z_i is family income at age 4 and E_i is an indicator of income eligibility at age 4. The partial effect of Head Start is estimated by:

$$\frac{1}{N}\sum_{i=1}^{N}[P[Y_i = 1|E_i = 1, Z_i, X_i] - P[Y_i = 1|E_i = 0, Z_i, X_i]] = \frac{1}{N}\sum_{i=1}^{N}\left[\Phi(\phi + \gamma + g\left(Z_i, X_i\right)) - \Phi(\phi + g\left(Z_i, X_i\right)\right].$$

where N is the number of children in the sample, and Φ is the standard normal c.d.f. Robust standard errors are reported in brackets and are clustered at mother's level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Reduced Form Regressions: Regressions of outcomes at ages 16-17 on Head Start income eligibility at age four.

	CESD		Ever Sentenced		Ever Smoke		Overweight	
	No Controls	Controls	No Controls	Controls	No Controls	Controls	No Controls	Controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: All adolescents								
1 if Head Start eligible at age 4	-0.059	-10.307	0.384	-0.146	0.229	-0.002	-0.137	-0.460
	[1.946]	[4.004]**	[0.103]***	[0.199]	[0.080]***	[0.175]	[0.098]	[0.210]**
Effect at Mean	-0.059	-10.307	0.086	-0.030	0.088	-0.001	-0.036	-0.113
Mean	43.798		0.156		0.615		0.179	
S.D.	28.593		0.363		0.487		0.383	
Observations	911		1053		1113		1034	
Panel B: African-American adolescents								
1 if Head Start eligible at age 4	1.021	-4.22	0.37	-0.951	0.346	-0.254	-0.277	-0.136
	[3.252]	[6.719]	[0.190]*	[0.402]**	[0.128]***	[0.318]	[0.158]*	[0.359]
Effect at Mean	1.021	-4.220	0.076	-0.182	0.137	-0.078	-0.076	-0.031
Mean	43.378		0.147		0.557		0.179	
S.D.	28.005		0.355		0.497		0.384	
Observations	402		462		474		446	

Note: This table reports results of regressions of outcomes measured at ages 16-17 on Head Start income eligibility at age 4. A description of the outcomes is provided in table A1 in Appendix A. A probit model is estimated for discrete outcomes (Ever Smoke, Overweight and Ever Sentenced). Mean and S.D. are the mean and standard deviation, respectively, of the outcome variable. Two columns are presented for each outcome: one regression without any controls ("No controls") and other with all controls ("Controls"): fourth order polynomials in log family income and family size at age 4, an interaction between these two variables, a dummy for the presence of a father figure in the child's household at age 4, fourth order polynomials in average log family income and average family size between ages 0 and 2, an interaction between the two, and fourth order polynomials in birth weight, race and age dummies and dummies for year and state of residence at age 4. Effect at mean is the partial effect of HS_i, which is computed by taking the average of individual effects across our sample. For discrete outcomes the model estimated is

$$P[Y_i = 1|E_i, Z_i, X_i] \quad = \Phi(\phi + \gamma E_i + g\,(Z_i, X_i))$$

where Y_i is the outcome of interest for child i at ages 16 to 17, X_i is the vector of controls described above (entering through function $g\,(X)$), Z_i is family income at age 4 and E_i is an indicator of income eligibility at age 4. The partial effect of Head Start is estimated by:

$$\frac{1}{N}\sum_{i=1}^{N}[P[Y_i = 1|E_i = 1, Z_i, X_i] - P[Y_i = 1|E_i = 0, Z_i, X_i]] = \frac{1}{N}\sum_{i=1}^{N}\left[\Phi(\phi + \gamma + g\,(Z_i, X_i)) - \Phi(\phi + g\,(Z_i, X_i)\right].$$

where N is the number of children in the sample, and Φ is the standard normal c.d.f. Robust standard errors are reported in brackets and are clustered at mother's level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: Outcomes at ages 12-13 an 16-17 and income eligibility to Head Start at different ages (0 to 7).

Age of eligibility = X	0	1	2	3	4	5	6	7
				Outcomes at ages 12-13				
				Grade Repetition				
				Panel A1: All				
1 if Head Start eligible at age X	0.020	0.031	0.102	0.040	-0.364	-0.277	0.056	-0.271
	[0.181]	[0.174]	[0.168]	[0.167]	[0.160]**	[0.168]*	[0.166]	[0.178]
Effect at Mean	0.006	0.010	0.033	0.013	-0.116	-0.089	0.018	-0.088
Observations	1065	1265	1313	1283	1347	1196	1210	1104
				Panel B1: African American				
1 if Head Start eligible at age X	0.164	-0.223	-0.122	0.171	-0.265	-0.409	0.668	-0.206
	[0.289]	[0.307]	[0.293]	[0.279]	[0.260]	[0.287]	[0.274]**	[0.280]
Effect at Mean	0.049	-0.070	-0.039	0.055	-0.086	-0.129	0.211	-0.068
Observations	469	522	573	557	572	520	506	486
				Overweight				
				Panel A2: All				
1 if Head Start eligible at age X	-0.045	-0.185	-0.133	-0.106	-0.330	-0.534	-0.233	-0.096
	[0.208]	[0.194]	[0.187]	[0.197]	[0.176]*	[0.184]***	[0.178]	[0.207]
Effect at Mean	-0.011	-0.049	-0.034	-0.026	-0.081	-0.133	-0.056	-0.023
Observations	991	1219	1296	1230	1324	1140	1171	1055
				Panel B2: African American				
1 if Head Start eligible at age X	0.050	-0.640	0.218	0.133	-0.690	-0.913	-0.170	-0.252
	[0.358]	[0.343]*	[0.297]	[0.370]	[0.288]**	[0.307]***	[0.301]	[0.319]
Effect at Mean	0.012	-0.167	0.049	0.030	-0.171	-0.232	-0.040	-0.057
Observations	433	504	548	499	553	502	478	456
				Outcomes at ages 16-17				
				CESD				
				Panel A3: All				
1 if Head Start eligible at age X	-2.873	0.663	3.274	-1.467	-10.307	-4.486	-4.955	1.377
	[4.397]	[4.493]	[4.184]	[4.038]	[4.004]**	[4.050]	[4.350]	[4.635]
Observations	708	844	890	900	911	893	899	812
				Panel B3: African American				
1 if Head Start eligible at age X	-4.865	1.520	-1.668	-4.130	-4.220	-3.807	-11.025	-2.786
	[6.809]	[7.838]	[6.866]	[6.105]	[6.719]	[6.352]	[6.409]*	[8.027]
Observations	307	364	400	409	402	407	387	368
				Ever Sentenced				
				Panel A4: All				
1 if Head Start eligible at age X	0.113	0.169	0.179	0.277	-0.146	-0.241	0.529	-0.182
	[0.228]	[0.224]	[0.199]	[0.232]	[0.199]	[0.211]	[0.205]***	[0.229]
Effect at Mean	0.024	0.036	0.038	0.056	-0.030	-0.053	0.109	-0.039
Observations	824	977	1035	1057	1053	1031	1043	956
				Panel B4: African American				
1 if Head Start eligible at age X	-0.862	-0.044	-0.079	-0.141	-0.951	-0.343	0.516	-0.613
	[0.500]*	[0.512]	[0.376]	[0.428]	[0.402]**	[0.350]	[0.366]	[0.429]
Effect at Mean	-0.153	-0.008	-0.014	-0.025	-0.182	-0.062	0.086	-0.119
Observations	325	363	455	463	462	457	438	415
				Overweight				
				Panel A5: All				
1 if Head Start eligible at age X	-0.330	-0.115	0.037	-0.165	-0.460	-0.425	0.064	-0.102
	[0.245]	[0.224]	[0.215]	[0.216]	[0.210]**	[0.201]**	[0.200]	[0.228]
Effect at Mean	-0.080	-0.028	0.009	-0.039	-0.113	-0.099	0.014	-0.025
Observations	798	941	986	1011	1034	1000	1000	886
				Panel B5: African American				
1 if Head Start eligible at age X	-0.706	-0.657	-0.185	-0.176	-0.136	-0.254	0.479	-0.325
	[0.373]*	[0.413]	[0.344]	[0.359]	[0.359]	[0.352]	[0.335]	[0.352]
Effect at Mean	-0.182	-0.154	-0.041	-0.040	-0.031	-0.058	0.099	-0.077
Observations	352	401	443	442	446	441	399	384

Note: This table reports results of regressions of outcome measured at ages 12-13 and 16-17 on Head Start income eligibility measured at different ages between 0 and 7 (age "X" in each column). A description of the outcomes is provided in table A1 in Appendix A. A probit model is estimated for discrete outcomes (Grade Repetition, Overweight and Ever Sentenced). Controls excluded from the table are fourth order polynomials in log family income and family size at age "X", an interaction between these two variables, a dummy for the presence of a father figure in the child's household at age "X", fourth order polynomials in average log family income and average family size between ages 0 and 2, an interaction between the two, and fourth order polynomials in birth weight, race and age dummies and dummies for year and state of residence at age "X". Effect at mean is computed as in tables (5) and (6). Robust standard errors are reported in brackets and clustered at mother level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8: Effects of Head Start on outcomes at ages 12-13.

	BPI		Overweight		Eversmoke		Grade Repetition		Special Education	
	OLS	TSLS	Probit	Bivariate Probit	Probit	Bivariate Probit	Probit	Bivariate Probit	Probit	Bivariate Probit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: All										
Head Start	0.061	-0.171	0.020	-0.825	0.054	-0.366	-0.004	-1.251	-0.122	-0.443
	[0.071]	[0.393]	[0.099]	[0.579]	[0.088]	[0.607]	[0.088]	[0.262]***	[0.093]	[0.664]
Effect at Mean	0.061	-0.171	0.005	-0.184	0.017	-0.114	-0.001	-0.354	-0.035	-0.122
Observations	1261		1324		1360		1347		1308	
Panel B: African-American										
Head Start	-0.026	0.326	-0.053	-0.956	-0.05	0.499	0.021	0.234	-0.187	0.137
	[0.104]	[0.556]	[0.152]	[0.697]	[0.134]	[0.832]	[0.130]	[0.902]	[0.140]	[0.913]
Effect at Mean	-0.026	0.326	-0.012	-0.206	-0.014	0.141	0.007	0.074	-0.053	0.038
Observations	537		553		577		572		548	

Note: This table reports effects of Head Start participation for outcomes measured at ages 12-13. A description of the outcomes is provided in table A1 in Appendix A. The model estimated is:

$$\begin{aligned} Y_i &= \alpha + \beta HS_i + g\left(Z_i, X_i\right) + \varepsilon_i \\ HS_i &= 1\left[\eta + \tau E_i + h\left(Z_i, X_i\right) + v_i > 0\right], \end{aligned}$$

which is estimated by a bivariate probit for discrete outcomes. For discrete outcomes, effect at mean is the partial effect of HS_i obtained by:

$$\begin{aligned} & \sum_{i=1}^{N} \{P[Y_i = 1|HS_i = 1, Z_i, X_i] - P[Y_i = 1|HS_i = 0, Z_i, X_i]\} \\ = \; & \frac{1}{N} \sum_{i=1}^{N} \{\Phi[\alpha + \beta + g(Z_i, X_i)] - \Phi[\alpha + g(Z_i, X_i)]\} \end{aligned}$$

where N is the number of children in the sample, Φ is the standard normal c.d.f, Y_i is the outcome of interest for child i at ages 12 to 13, HS_i is a dummy variable indicating whether the child ever participated in Head Start, X_i is a vector of controls, Z_i is family income at age 4 and E_i is an indicator of income eligibility at age 4. Controls excluded from the table are described in table (5). Robust standard errors are reported in brackets for continuous outcomes. For discrete outcomes the variance matrix of the Maximum Likelihood Estimator was obtained by the outer product of the gradient. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 9: Effects of Head Start on outcomes at ages 16-17.

	CESD		Ever Sentenced		Ever Smoke		Overweight	
	OLS	TSLS	Probit	Bivariate Probit	Probit	Bivariate Probit	Probit	Bivariate Probit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: All								
Head Start	-3.731	-18.827	0.128	-1.506	0.091	0.212	-0.043	-1.557
	[2.219]*	[14.636]	[0.113]	[0.085]***	[0.098]	[0.705]	[0.111]	[0.085]***
Effect at Mean	-3.731	-18.827	0.027	-0.312	0.030	0.068	-0.010	-0.340
Observations	911		1053		1113		1034	
Panel B: African-American								
Head Start	0.488	13.753	0.185	-1.581	-0.064	-0.158	-0.133	-1.693
	[3.203]	[26.774]	[0.179]	[0.188]***	[0.145]	[1.002]	[0.171]	[0.150]***
Effect at Mean	0.488	13.753	0.032	-0.307	-0.020	-0.049	-0.029	-0.376
Observations	402		462		474		446	

Note: This table reports effects of Head Start participation for outcomes measured at ages 16-17. A description of the outcomes is provided in table A1 in Appendix A. The model estimated is:

$$
\begin{aligned}
Y_i &= \alpha + \beta HS_i + g\left(Z_i, X_i\right) + \varepsilon_i \\
HS_i &= 1\left[\eta + \tau E_i + h\left(Z_i, X_i\right) + v_i > 0\right],
\end{aligned}
$$

which is estimated by a bivariate probit for discrete outcomes. For discrete outcomes, effect at mean is the partial effect of HS_i, which is obtained by taking the average value of individual effects across our sample:

$$
\begin{aligned}
& \sum_{i=1}^{N} \{P[Y_i = 1|HS_i = 1, Z_i, X_i] - P[Y_i = 1|HS_i = 0, Z_i, X_i]\} \\
= & \frac{1}{N} \sum_{i=1}^{N} \{\Phi[\alpha + \beta + g(Z_i, X_i)] - \Phi[\alpha + g(Z_i, X_i)]\}
\end{aligned}
$$

where N is the number of children in the sample, and Φ is the standard normal c.d.f. Controls excluded from the table are described in table (5). Robust standard errors are reported in brackets for continuous outcomes. For discrete outcomes the variance matrix of the Maximum Likelihood Estimator was obtained by the outer product of the gradient. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 10: Heterogeneity in the effects of Head Start. Special Education - All children 12-13 years old.

Coefficients	
Head Start	0.00047
	[11.86286]
HSXln(Income at age 4)	0.04462
	[2.63476]
HSXln(Income at age 4)^2	-0.02206
	[0.14997]
HSX(Family Size at age 4)	0.21896
	[0.87230]
HSX(Family Size at age 4)^2	-0.01524
	[0.02050]
HSXln(Income at age 4)X(Family Size at age 4)	0.01076
	[0.08951]
Head Start Partial Effects	-0.12895
Likelihood ratio test: Model of table 8 vs unrestricted model	11.908
P-value	0.036
Likelihood ratio test: Model of without HS vs unrestricted model	12.445
P-value	0.053
Observations	1308

Note: This table reports effects of Head Start participation on special education enrolment when the effects vary across income levels and family size. Controls excluded from the table are described in table (5). The partial effect of Head Start is obtained by:

$$\sum_{i=1}^{N} \{P[Y_i = 1|HS_i = 1, Z_i, X_i] - P[Y_i = 1|HS_i = 0, Z_i, X_i]\} =$$

$$\frac{1}{N}\sum_{i=1}^{N}\left\{\begin{array}{c} \Phi[\alpha + \beta_0 + \beta_1 \times (family\ \log income)_i + \beta_2 \times (family\ \log income)_i^2 + \beta_3 \times (family\ size)_i \\ +\beta_4 \times (family\ size)_i^2 + \beta_5 \times (family\ \log income)_i \times (family\ size)_i + g(Z_i, X_i)] \\ -\Phi[\alpha + g(Z_i, X_i)] \end{array}\right\}.$$

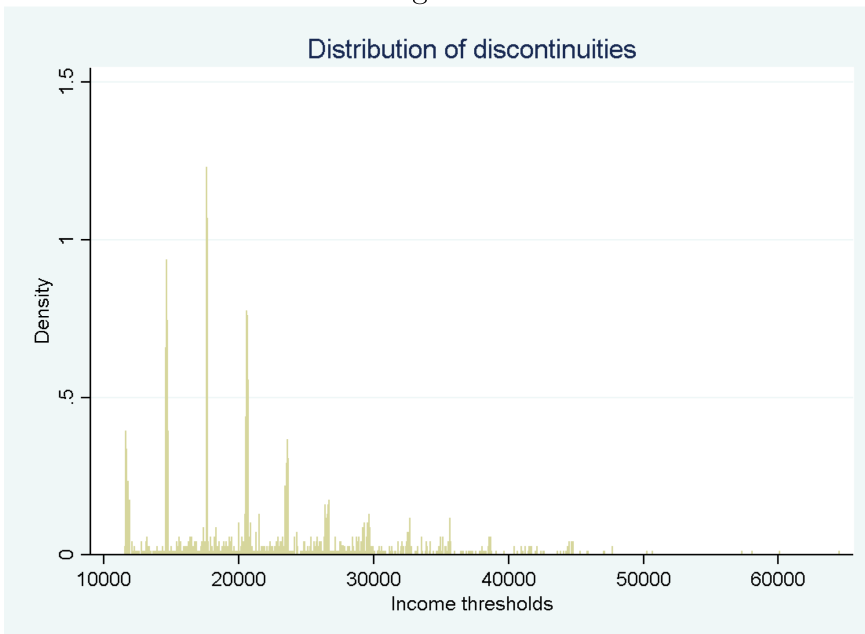
where N is the number of children in the sample, and Φ is the standard normal c.d.f., Y_i is Special Education Attendance for child i at ages 12 to 13, HS_i is a dummy variable indicating whether the child ever participated in Head Start, X_i is a vector of controls and Z_i is family income at age 4.

Two likelihood ratio tests are presented (and respective p-values). The first test tests the importance of heterogeneity in the effect of Head Start by testing the model with the complete set of interactions between the indicator of Head Start participation and the (log) income and family size at age 4 (the *unrestricted model*) against a restrict model where the effect of Head Start is constant for all income levels and family size. The second test tests the model with heterogenous effects against a restricted model without Head Start (testing the effect of Head Start).

The variance matrix of the Maximum Likelihood Estimator was obtained by the outer product of the gradient. * significant at 10%; ** significant at 5%; *** significant at 1%.

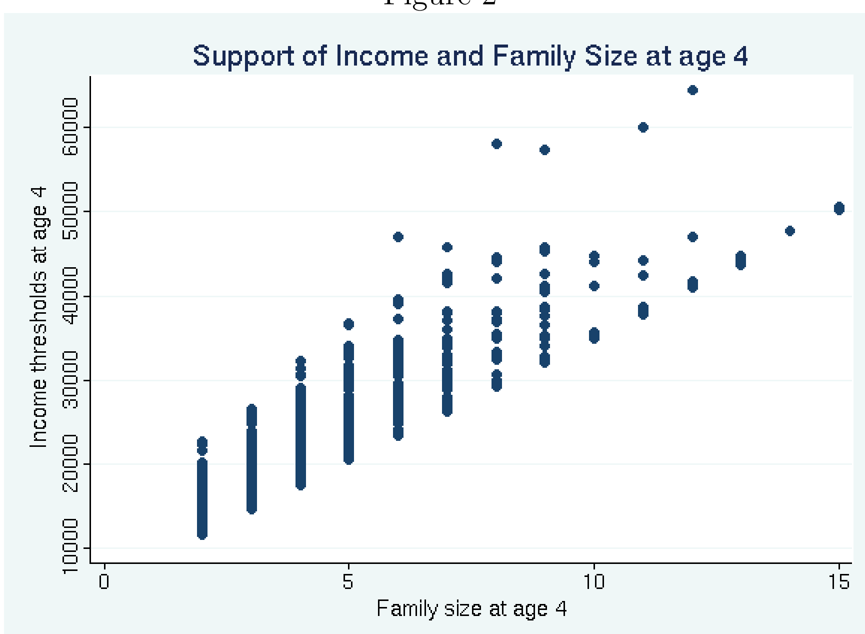
Figures

Figure 1



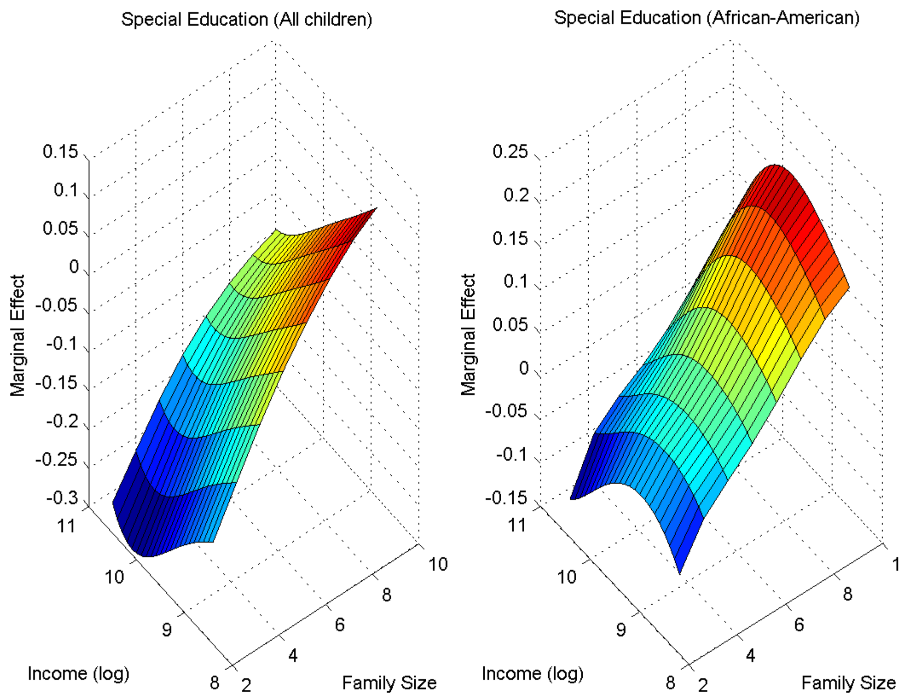
Note: This picture shows the support of income at age 4 over which we are able to perform estimation. The sample used contains all children (12-13 years old) and adolescents (16-17 years old) used in the regressions whose family income at age 4 is between 5 and 195 percent of the discontinuity level of income.

Figure 2



Note: This picture shows the joint support of income and family size at age 4 over which we are able to perform estimation. The sample used contains all children (12-13 years old) and adolescents (16-17 years old) used in the regressions whose family income at age 4 is between 5 and 195 percent of the discontinuity level of income.

Figure 3



Note: Estimation results are obtained fitting a bivariate probit model. The marginal effects represented are obtained allowing log of income and family size at age 4 to vary between the 5th and 95th percentiles of the distribution of each variable in the regressions samples and evaluating all the other controls at the mean. The model estimated is:

$$\begin{aligned} Y_i &= \alpha + h^*(Z_i, X_i) \times HS_i + g(Z_i, X_i) + \varepsilon_i && (9) \\ HS_i &= 1\left[\eta + \tau E_i + h(Z_i, X_i) + v_i > 0\right]. && (10) \end{aligned}$$

where Y_i is Attendance of Special Education at ages 12 or 13, HS_i is a dummy variable indicating whether the child ever participated in Head Start, X_i is the vector of controls, Z_i is family income at age 4, E_i is an indicator of income eligibility at age 4 and :

$$\begin{gathered} h^*(Z_i, X_i) = \beta_0 + \beta_1 \times (family\ (log)\ income\ at\ age\ 4)_i \\ +\beta_2 \times (family\ (log)\ income\ at\ age\ 4)_i^2 + \beta_3 \times (family\ size\ at\ age\ 4)_i \\ +\beta_4 \times (family\ size\ at\ age\ 4)_i^2 + \beta_5 \times (family\ (log)\ income\ at\ age\ 4)_i \times (family\ size\ at\ age\ 4)_i. \end{gathered}$$

The average marginal effects were computed according to the following expression:

$$\sum_{i=1}^{N} \{P[Y_i = 1|HS_i = 1, Z_i, X_i] - P[Y_i = 1|HS_i = 0, Z_i, X_i]\} =$$

$$\frac{1}{N}\sum_{i=1}^{N}\left\{\begin{array}{c} \Phi[\alpha + \beta_0 + \beta_1 \times (family\ \log income)_i + \beta_2 \times (family\ \log income)_i^2 \\ +\beta_3 \times (family\ size)_i + \beta_4 \times (family\ size)_i^2 + \beta_5 \times (family\ \log income)_i \times (family\ size)_i \\ +g(Z_i, X_i)] - \Phi[\alpha + g(Z_i, X_i)] \end{array}\right\}.$$

where N is the number of children in the sample, and Φ is the standard normal c.d.f.

Appendix A

Tables

Table A1: Description of Outcome Variables

Outcomes	
BPI	Behavior Problems Index. Standardized score with population mean 0 and standard deviation 1 (normed within gender).
Grade Repetition	Indicator for whether the child has ever repeated a grade up to a given age.
Special Education	Indicator for whether the child is attending classes for remedial work.
PIAT-Mathematics	Test that measures a child's attainment in mathematics as taught in mainstream education. Standardized score with population mean 0 and standard deviation 1 (normed within age).
PIAT-Reading Recognition	Test that measures word recognition and pronunciation ability. Standard score with population mean 0 and standard deviation 1 (normed within age).
PIAT-Reading Comprehension	Measures child's ability to derive meaning from sentences. Standard score with population mean 0 and standard deviation 1 (normed within age).
Peabody Picture Vocabulary Test	Measures an individual's receptive (hearing) vocabulary for Standard American English. Standard score with population mean 0 and standard deviation 1 (normed within age).
Overweight	Indicator that takes value 1 if the individual's Body Mass Index (BMI) is above the 95th percentile of the population for her/his age and gender.
Ever Smoke	Indicator for whether the child has ever tried a cigarrette up to a given age.
CESD	Center for Epidemiological Studies Depression Scale: percentile scale that measures symptoms of depression (higher scores are associated with more symptoms of depression).
Ever Sentenced	Indicator for whether the individual has ever been convicted of any charge other than minor traffic violations or sentenced to a corrections institution/jail/reform school.
Ever tried marijuana	Indicator that takes takes value 1 if the adolescent has ever used marijuana up to a given age.
Ever tried alcohol	Indicator that takes takes value 1 if the adolescent has ever tried any alcoholic drink up to a given age.
Enrolled in high school	Indicator that takes takes value 1 if the adolescent is enrolled in high school.
Outcomes measured before age 3	
Birth Weight	Child's birth weight (onces).
Breastfed	Indicator for whether the child was breastfed.
Mother's age at child birth	Mother's age at child's birth (in years).
Mother's AFQT	Mother's Armed Forces Qualification Test percentile score, measured in 1980 and revised in 1989.
Mother's Education	Mother's highest grade completed before child turned 4 years old.
Family Size (0-2)	Child's average family size before age 3.
Family (log) Income (0-2)	Child's average log family income before age 3 (in dollars of 2000).

Table A2.1: Sensitivity Analysis to different specifications - Outcomes at ages 12-13.

	Basic	No pre-HS age controls	Quadratic	Cubic	All	Welfare
	(1)	(2)	(3)	(4)	(5)	(6)
			BPI			
			Panel A: All children			
1 if Head Start eligible at age 4	-0.217	-0.185	-0.194	-0.202	-0.217	-0.295
	[0.117]*	[0.115]	[0.115]*	[0.114]*	[0.118]*	[0.118]**
Observations	1261	1261	1261	1261	1214	1259
			Panel B: African-American children			
1 if Head Start eligible at age 4	0.121	0.080	0.167	0.181	0.100	0.018
	[0.195]	[0.191]	[0.196]	[0.194]	[0.204]	[0.207]
Observations	537	537	537	537	518	535
			Grade Repetition			
			Panel C: All children			
1 if Head Start eligible at age 4	-0.364	-0.325	-0.260	-0.309	-0.339	-0.385
	[0.160]**	[0.159]**	[0.155]*	[0.158]*	[0.166]**	[0.162]**
Effect at Mean	-0.116	-0.106	-0.084	-0.099	-0.104	-0.122
Observations	1347	1347	1347	1347	1295	1344
			Panel D: African-American children			
1 if Head Start eligible at age 4	-0.265	-0.231	-0.245	-0.285	-0.338	-0.335
	[0.260]	[0.255]	[0.257]	[0.258]	[0.281]	[0.267]
Effect at Mean	-0.086	-0.077	-0.082	-0.093	-0.100	-0.108
Observations	572	572	572	572	553	570
			Overweight			
			Panel E: All children			
1 if Head Start eligible at age 4	-0.330	-0.303	-0.257	-0.264	-0.287	-0.340
	[0.176]*	[0.170]*	[0.171]	[0.172]	[0.185]	[0.179]*
Effect at Mean	-0.081	-0.076	-0.064	-0.065	-0.068	-0.084
Observations	1324	1324	1324	1324	1264	1322
			Panel F: African-American children			
1 if Head Start eligible at age 4	-0.690	-0.613	-0.631	-0.647	-0.676	-0.694
	[0.288]**	[0.276]**	[0.282]**	[0.284]**	[0.316]**	[0.296]**
Effect at Mean	-0.171	-0.156	-0.159	-0.160	-0.157	-0.172
Observations	553	553	553	553	525	551

Note: Tables A2.1 and A2.2 report reduced form regressions for selected outcomes using several specifications. Controls excluded from the table are as follows: "Basic" is specification used throughout the paper (fourth order polynomials in log family income and family size at age 4, an interaction between these two variables, a dummy for the presence of a father figure in the child's household at age 4, fourth order polynomials in average log family income and average family size between ages 0 and 2, an interaction between the two, and fourth order polynomials in birth weight, race and age dummies and dummies for year and state of residence at age 4); "No pre-Head Start age controls" includes controls for fourth order polynomials in log family income and family size at age 4, an interaction between these two variables, a dummy for the presence of a father figure in the child's household at age 4, race and age dummies, and dummies for year and state of residence at age 4; "Quadratic" is the same specification as "Basic" but with polynomials up to the second order on (log) income, family size and birth weight variables; "Cubic" is the same specification as "Basic" but with polynomials up to the third order on (log) income, family size and birth weight variables; "All" includes the same controls as "Basic" and polynomials up to the fourth order on mother's AFQT, on mother's age at child's birth (in years), on mother's highest grade completed when child was three years old and an indicator for whether the child was breastfed; "Welfare" includes the same controls as "Basic" and indicators of AFDC and SSI take-up at age 4. Robust standard errors are reported in brackets and are clustered at mother level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A2.2: Sensitivity Analysis to different specifications - Outcomes at ages 16-17.

	Basic	No pre-HS age controls	Quadratic	Cubic	All	Welfare
	(1)	(2)	(3)	(4)	(5)	(6)
			CESD			
			Panel G: All adolescents			
1 if Head Start eligible at age 4	-10.307	-9.832	-8.421	-8.623	-10.436	-11.620
	[4.004]**	[4.020]**	[3.910]**	[3.969]**	[4.082]**	[4.013]***
Observations	911	911	911	911	888	909
			Panel H: African-American adolescents			
1 if Head Start eligible at age 4	-4.22	-4.802	-5.81	-3.805	-4.193	-5.406
	[6.719]	[6.580]	[6.189]	[6.611]	[6.902]	[6.834]
Observations	402	402	402	402	391	401
			Ever Sentenced			
			Panel I: All adolescents			
1 if Head Start eligible at age 4	-0.146	-0.109	-0.005	-0.132	-0.043	-0.17
	[0.199]	[0.197]	[0.198]	[0.198]	[0.210]	[0.202]
Effect at Mean	-0.030	-0.023	-0.001	-0.028	-0.009	-0.035
Observations	1053	1053	1053	1053	1027	1051
			Panel J: African-American adolescents			
1 if Head Start eligible at age 4	-0.951	-0.701	-0.786	-0.910	-1.081	-0.890
	[0.402]**	[0.368]*	[0.389]**	[0.402]**	[0.466]**	[0.403]**
Effect at Mean	-0.182	-0.147	-0.153	-0.176	-0.195	-0.169
Observations	462	462	462	462	448	461
			Overweight			
			Panel K: All adolescents			
1 if Head Start eligible at age 4	-0.460	-0.433	-0.432	-0.439	-0.412	-0.526
	[0.210]**	[0.207]**	[0.202]**	[0.203]**	[0.215]*	[0.214]**
Effect at Mean	-0.113	-0.108	-0.106	-0.108	-0.098	-0.128
Observations	1034	1034	1034	1034	1001	1032
			Panel L: African-American adolescents			
1 if Head Start eligible at age 4	-0.136	-0.129	-0.275	-0.112	-0.119	-0.212
	[0.359]	[0.336]	[0.333]	[0.353]	[0.380]	[0.366]
Effect at Mean	-0.031	-0.030	-0.064	-0.025	-0.025	-0.048
Observations	446	446	446	446	425	445

Note: See note of table A2.1.

Table A3: Sensitivity to sample choice around distance to income cutoff.

Income Range	[75%-125%] (1)	[50%-150%] (2)	[25%-175%] (3)	[5%-195%] (4)	$\leq$ 300% (5)	Full sample (6)
			Outcomes at ages 12-13			
			BPI			
			Panel A: All children			
1 if Head Start eligible at age 4	0.229	-0.078	-0.203	-0.217	-0.153	-0.073
	[0.232]	[0.150]	[0.134]	[0.117]*	[0.098]	[0.083]
Observations	304	734	1067	1261	1721	2186
			Panel B: African-American children			
1 if Head Start eligible at age 4	0.047	0.211	0.213	0.121	0.188	0.1
	[0.444]	[0.270]	[0.232]	[0.195]	[0.176]	[0.162]
Observations	99	287	458	537	625	691
			Grade Repetition			
			Panel C: All children			
1 if Head Start eligible at age 4	-0.496	-0.414	-0.352	-0.364	-0.223	-0.137
	[0.353]	[0.209]**	[0.177]**	[0.160]**	[0.136]*	[0.119]
Effect at Mean	-0.128	-0.128	-0.113	-0.116	-0.068	-0.04
Observations	340	782	1144	1347	1824	2311
			Panel D: African-American			
1 if Head Start eligible at age 4	-6.077	-0.471	-0.277	-0.265	-0.128	-0.137
	[1.644]***	[0.383]	[0.311]	[0.260]	[0.226]	[0.227]
Effect at Mean	-0.225	-0.136	-0.089	-0.086	-0.041	-0.041
Observations	118	296	487	572	670	743
			Overweight			
			Panel E: All children			
1 if Head Start eligible at age 4	-0.710	-0.426	-0.288	-0.330	-0.086	-0.088
	[0.381]*	[0.221]*	[0.187]	[0.176]*	[0.148]	[0.130]
Effect at Mean	-0.155	-0.107	-0.073	-0.081	-0.022	-0.022
Observations	310	753	1094	1324	1796	2273
			Panel F: African-American			
1 if Head Start eligible at age 4	-30.018	-1.031	-0.714	-0.690	-0.356	-0.470
	[3.816]***	[0.412]**	[0.326]**	[0.288]**	[0.254]	[0.246]*
Effect at Mean	-0.383	-0.252	-0.178	-0.171	-0.088	-0.118
Observations	74	280	472	553	655	719
			Outcomes at ages 16-17			
			CESD			
			Panel G: All adolescents			
1 if Head Start eligible at age 4	-15.813	-7.209	-9.944	-10.307	-7.69	-5.508
	[8.033]*	[5.469]	[4.306]**	[4.004]**	[3.339]**	[2.885]*
Observations	213	516	774	911	1213	1503
			Panel H: African-American			
1 if Head Start eligible at age 4	-9.444	5.131	-1.358	-4.22	-6.999	-2.737
	[14.819]	[8.834]	[7.556]	[6.719]	[5.518]	[5.125]
Observations	73	200	341	402	471	514
			Ever Sentenced			
			Panel I: All adolescents			
1 if Head Start eligible at age 4	0.108	-0.282	-0.001	-0.146	0.214	0.207
	[0.459]	[0.285]	[0.222]	[0.199]	[0.182]	[0.155]
Effect at Mean	0.019	-0.057	0.000	-0.030	0.041	0.038
Observations	222	585	890	1053	1443	1792
			Panel J: African-American			
1 if Head Start eligible at age 4	-48.054	-3.468	-0.836	-0.951	-0.772	-0.718
	[0.000]	[1.113]***	[0.457]*	[0.402]**	[0.399]*	[0.390]*
Effect at Mean	-0.667	-0.367	-0.171	-0.182	-0.134	-0.116
Observations	30	219	376	462	534	583
			Overweight			
			Panel H: All adolescents			
1 if Head Start eligible at age 4	-0.833	-0.745	-0.405	-0.46	-0.296	-0.284
	[0.452]*	[0.296]**	[0.234]*	[0.210]**	[0.185]	[0.163]*
Effect at Mean	-0.165	-0.174	-0.097	-0.113	-0.068	-0.063
Observations	242	575	871	1034	1418	1787
			Panel I: African-American adolescents			
1 if Head Start eligible at age 4	70.803	-0.799	-0.399	-0.136	-0.282	-0.412
	[222.910]	[0.611]	[0.417]	[0.359]	[0.330]	[0.310]
Effect at Mean	0.236	-0.181	-0.091	-0.031	-0.065	-0.095
Observations	72	220	384	446	515	567

Note: This table reports reduced form estimates for different windows of data around the income cutoff at age 4. First row shows the bandwidth used. See table 5 for a description of controls excluded. Robust s.e. are reported in brackets and are clustered by mother. *,**,*** significant at 10%, 5% and 1%, respectively.

Table A4: Reduced Form Regressions: Alternative specification.

	Ages 12-13			Ages 16-17		
	BPI	Special Education	Grade Repetition	CESD	Overweight	Ever Sentenced
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: All					
Effect at Mean	-0.349	-0.040	-0.061	-10.488	-0.122	0.003
	[0.130]***	[0.046]	[0.048]	[4.753]**	[0.051]***	[0.034]
Observations	1261	1308	1347	911	1324	1053
	Panel B: African-American					
Effect at Mean	-0.007	0.113	-0.019	-5.756	-0.015	-0.056
	[0.200]	[0.076]	[0.066]	[8.568]	[0.094]	[0.088]
Observations	537	548	572	402	446	462

Note: This table presents the marginal effect of income eligibility at age from reduced form regressions for selected outcomes at ages 12-13 and 16-17 allowing for $f(Z_i, X_i)$ in equation (2) to be a different function in either side of the discontinuity. The model estimated is:

$$Y_i = \phi + \gamma E_i + g(Z_i) + 1[Income \leq Cutoff] \times h(Z_i) + f(X_i) + \varepsilon_i.$$

where Z_i is the log family income, g and h represent fourth order polynomial in family (log) income at age four and 1[.] is the indicator function. If $Income \leq Cutoff$: $E\left[Y_i|Z_i \leq \bar{Z}(X_i)\right] = \phi + \gamma + g(Z_i) + h(Z_i) + f(X_i)$. Let $\delta > 0$ and sufficiently close to zero. Then

$$\lim_{\delta \to 0^-} E\left[Y_i|Z_i \leq \bar{Z}(X_i)\right] = E\left[Y_i|Z_i = \bar{Z}(X_i) - \delta\right] = \phi + \gamma + g(\bar{Z}(X_i)) + h(\bar{Z}(X_i)) + f(X_i)$$

If $Income > Cutoff$: $E\left[Y_i|Z_i > \bar{Z}(X_i)\right] = \phi + g(Z_i) + f(X_i)$. Then

$$\lim_{\delta \to 0^+} E\left[Y_i|Z_i > \bar{Z}(X_i)\right] = E\left[Y_i|Z_i = \bar{Z}(X_i) + \delta\right] = \phi + g(\bar{Z}(X_i)) + f(X_i)$$

The partial effect of Head Start is estimated by: $E\left[Y_i|Z_i = \bar{Z}(X_i) - \delta\right] - E\left[Y_i|Z_i = \bar{Z}(X_i) + \delta\right] = \gamma + h(\bar{Z}(X_i))$. This model allows for heterogeneous effects of the program as the effect is a function of the discontinuity level of income. See table 5 for a description of controls included in X_i. Standard errors for the "Effect at Mean" are reported in brackets and were computed using 249 block bootstrap replications, where mothers are the unit that defines blocks. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A5: Reduced Form Regressions: Regressions of outcomes at ages 16-17 on Head Start income eligibility at age four.

	Ever tried alcohol		Ever tried marijuana		Enrolled in high school	
	No Controls	Controls	No Controls	Controls	No Controls	Controls
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: All			
1 if Head Start eligible at age 4	0.046	-0.126	0.165	0.057	-0.214	0.106
	[0.089]	[0.193]	[0.082]**	[0.173]	[0.126]*	[0.260]
Effect at Mean	0.014	-0.032	0.061	0.020	-0.033	0.014
Mean	0.774		0.358		0.911	
S.D.	0.418		0.480		0.284	
Observations	1085		1111		992	
			Panel B: African-American			
1 if Head Start eligible at age 4	0.104	-0.139	0.246	-0.112	-0.422	1.069
	[0.137]	[0.334]	[0.145]*	[0.327]	[0.266]	[0.865]
Effect at Mean	0.037	-0.038	0.085	-0.035	-0.070	0.154
Mean	0.699		0.324		0.885	
S.D.	0.459		0.468		0.320	
Observations	469		476		252	

Note: This table reports results of probit regressions of outcomes measured at ages 16-17 on Head Start income eligibility at age 4. A description of the outcomes is provided in table A1 in Appendix A. Mean and S.D. are the mean and standard deviation, respectively, of the outcome variable. Two columns are presented for each outcome: one regression without any controls ("No controls") and other with all controls ("Controls"). See table 5 for the description of controls excluded from the table and how "Effect at Mean" is computed. Robust standard errors are reported in brackets and are clustered at mother's level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A6: Probit regression of Head Start participation on Head Start income eligibility for non-Black children and adolescents.

Age groups	12-13		16-17	
	(1)	(2)	(3)	(4)
	No Controls	Controls	No Controls	Controls
1 if Head Start eligible at age 4	0.781	0.447	0.844	0.502
	[0.116]***	[0.253]*	[0.131]***	[0.274]*
Mean change in take-up probability	0.228	0.110	0.247	0.127
Observations	740	740	599	599

Note: This table reports results of probit regressions of Head Start participation on income eligibility. The mean change in marginal take-up probability is computed by the average marginal change in the probability of Head Start participation across individuals as the eligibility status changes and all other controls are kept constant. See table 3 for a description of controls excluded from the table. Robust standard errors are reported in brackets and are clustered at mother's level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A7: Reduced Form Regressions: Regressions of outcomes at ages 12-13 on Head Start income eligibility at age four for non-Black children.

	BPI		Overweight		Ever Smoke		Grade Repetition		Special Education	
	No Controls	Controls	No Controls	Controls	No Controls	Controls	No Controls	Controls	No Controls	Controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1 if Head Start eligible at age 4	0.038	-0.463	0.178	-0.252	0.216	-0.225	0.062	-0.397	0.130	-0.380
	[0.079]	[0.159]***	[0.110]	[0.233]	[0.095]**	[0.212]	[0.103]	[0.214]*	[0.105]	[0.230]*
Effect at Mean	0.038	-0.463	0.048	-0.059	0.083	-0.071	0.022	-0.116	0.042	-0.103
Mean	0.536		0.190		0.389		0.325		0.254	
S.D.	0.987		0.393		0.488		0.469		0.436	
Observations	678		737		759		751		735	

Table A8: Reduced Form Regressions: Regressions of outcomes at ages 16-17 on Head Start income eligibility at age four for non-Black adolescents.

	CESD		Ever Sentenced		Ever Smoke		Overweight	
	No Controls	Controls	No Controls	Controls	No Controls	Controls	No Controls	Controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1 if Head Start eligible at age 4	-0.619	-10.511	0.426	0.117	0.282	0.181	-0.018	-0.440
	[2.646]	[5.618]*	[0.128]***	[0.257]	[0.108]***	[0.237]	[0.132]	[0.300]
Effect at Mean	-0.619	-10.511	0.109	0.026	0.104	0.053	-0.005	-0.101
Mean	43.782		0.182		0.656		0.192	
S.D.	29.112		0.386		0.475		0.394	
Observations	476		527		622		543	

Note: These tables reports results of regressions of outcomes measured at ages 12-13 and 16-17 on Head Start income eligibility at age 4. A description of the outcomes is provided in table A1 in Appendix A. A probit model is estimated for discrete outcomes (Grade Repetition, Attending Special Education, Ever Smoke, Overweight, and Ever Sentenced). Mean and S.D. are the mean and standard deviation, respectively, of the outcome variable. Two columns are presented for each outcome: one regression without any controls ("No controls") and other with all controls ("Controls"). See tables (5) and (6) for a description of the controls excluded from the table and computation of "Effect at mean". Robust standard errors are reported in brackets and are clustered at mother's level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A9: Eligibility to Head Start at different ages (0 to 7) and Head Start participation between 3 and 5 years old.

Age of eligibility = X	0	1	2	3	4	5	6	7
				Ages 12-13				
				Panel A1: All				
1 if Head Start eligible at age X	-0.052	0.184	0.064	0.053	0.602	0.275	0.497	0.438
	[0.204]	[0.197]	[0.188]	[0.178]	[0.177]***	[0.183]	[0.177]***	[0.199]**
Mean change in marginal take-up probability	-0.016	0.056	0.020	0.016	0.174	0.083	0.153	0.136
Observations	862	1012	1083	1099	1349	1029	1012	922
				Panel A2: African-American				
1 if Head Start eligible at age X	-0.387	0.055	0.019	-0.095	0.890	0.520	0.163	0.550
	[0.326]	[0.363]	[0.325]	[0.314]	[0.298]***	[0.309]*	[0.289]	[0.308]*
Mean change in marginal take-up probability	-0.118	0.016	0.006	-0.028	0.256	0.156	0.048	0.168
Observations	407	464	508	500	581	469	461	441
				Ages 16-17				
				Panel B1: All				
1 if Head Start eligible at age X	0.01	0.246	0.103	0.156	0.634	0.329	0.654	0.677
	[0.230]	[0.214]	[0.208]	[0.192]	[0.202]***	[0.195]*	[0.194]***	[0.227]***
Mean change in marginal take-up probability	0.003	0.075	0.033	0.046	0.184	0.099	0.202	0.204
Observations	679	812	878	937	1104	937	900	816
				Panel B2: African-American				
1 if Head Start eligible at age X	-0.252	0.039	0.307	-0.074	0.850	0.369	0.622	0.900
	[0.380]	[0.397]	[0.367]	[0.336]	[0.367]**	[0.333]	[0.327]*	[0.345]***
Mean change in marginal take-up probability	-0.075	0.011	0.092	-0.021	0.242	0.111	0.183	0.259
Observations	317	375	418	433	476	428	406	390

Note: This table reports probit estimations for the probability of Head Start take-up between ages 3 and 5, when children may participate in the program, and eligibility is measured at different ages between 0 and 7 years old (age "X" in each column). Each column represents a regression measuring eligibility and controlling for its determinants at a given age. Controls excluded from the table are fourth order polynomials in log family income and family size at age "X", an interaction between these two variables, a dummy for the presence of a father figure in the child's household at age "X", fourth order polynomials in average log family income and average family size between ages 0 and 2, an interaction between the two, and fourth order polynomials in birth weight, race and age dummies and dummies for year and state of residence at age "X". Mean change in marginal take-up probability is given by the average marginal change in the probability of Head Start participation across individuals as the eligibility status changes and all other controls are kept constant. Robust standard errors are reported in brackets and are clustered at mother level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A10: Reduced Form Regressions: Regressions of outcomes at ages 12-13 on Head Start income eligibility at age four.

	PIAT RC		PIAT R		PIAT M		PPVT	
	No Controls	Controls	No Controls	Controls	No Controls	Controls	No Controls	Controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: All children				
1 if Head Start eligible at age 4	-0.438	-0.157	-0.498	-0.289	-0.434	-0.048	-0.705	0.020
	[0.055]***	[0.109]	[0.066]***	[0.133]**	[0.053]***	[0.102]	[0.118]***	[0.239]
Mean	-0.433		-0.174		-0.204		-0.878	
S.D.	0.933		1.119		0.918		1.225	
Observations	1223		1238		1242		428	
				Panel B: African-American children				
1 if Head Start eligible at age 4	-0.279	-0.179	-0.431	-0.416	-0.294	-0.046	-0.435	-0.118
	[0.081]***	[0.168]	[0.104]***	[0.204]**	[0.083]***	[0.170]	[0.211]**	[0.428]
Mean	-0.669		-0.42		-0.436		-1.236	
S.D.	0.868		1.079		0.865		1.179	
Observations	538		543		545		206	

Note: This table reports estimates of reduced form regression of several test scores on Head Start eligibility at age 4. A description of the outcomes is provided in table A1 in Appendix A. Controls excluded from the table are described in table 5. Robust standard errors are reported in brackets and are clustered at mother's level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A11: Heterogeneity in the effects of Head Start.

	Ages 12-13				Ages 16-17	
	Eversmoke	Overweight	Grade Repetition	Special Education	CESD	Ever Sentenced
	(1)	(2)	(3)	(4)	(1)	(2)
			Panel A: All			
Head Start	-6.436	-11.977	1.907	0.000	2,069.91	-1.506
	[11.897]	[13.025]	[9.510]	[11.863]	[884.744]**	[11.993]
HSXln(Income at age 4)	1.617	2.658	-0.343	0.045	-464.636	-6.29e-13
	[2.686]	[2.973]	[2.142]	[2.635]	[199.323]**	[2.708]
HSXln(Income at age 4)2	-0.108	-0.164	0.002	-0.022	25.905	-1.45e-11
	[0.155]	[0.174]	[0.123]	[0.150]	[11.208]**	[0.155]
HSXFamily Size at age 4	-0.475	-0.379	-0.521	0.219	35.415	2.51e-12
	[0.773]	[1.127]	[0.632]	[0.872]	[22.220]	[0.816]
HSX(Family Size at age 4)2	-0.009	-0.039	0.008	-0.015	-0.018	3.04e-11
	[0.019]	[0.034]	[0.015]	[0.021]	[0.369]	[0.019]
HSXln(Income at age 4)X(Family Size at age 4)	0.064	0.079	0.05	0.011	-3.873	1.47e-11
	[0.084]	[0.128]	[0.070]	[0.090]	[2.579]	[0.087]
Head Start Partial Effects	-0.148	-0.178	-0.340	-0.129	-0.774	-0.312
Likelihood ratio (Wald) test	2.178	4.166	4.780	11.908	1.390	0.000
P-Value	0.824	0.526	0.443	0.036	0.225	1.000
Observations	1360	1324	1347	1308	911	1053
			Panel B: African-American			
Head Start	-47.384	-12.737	-37.189	-21.6	-3,622.05	-1.581
	[30.917]	[39.643]	[27.952]	[26.469]	[16,546.683]	[58.478]
HSXln(Income at age 4)	10.392	3.365	7.81	4.766	789.04	5.39e-10
	[6.652]	[8.321]	[6.031]	[5.674]	[3,646.152]	[12.080]
HSXln(Income at age 4)2	-0.577	-0.225	-0.397	-0.268	-42.862	4.48e-09
	[0.364]	[0.452]	[0.332]	[0.310]	[200.752]	[0.627]
HSXFamily Size at age 4	-0.569	-2.193	0.522	0.005	-30.513	4.64e-10
	[1.255]	[2.734]	[1.232]	[1.373]	[181.905]	[2.148]
HSX(Family Size at age 4)2	-0.022	-0.009	0.024	-0.004	0.099	3.81e-09
	[0.032]	[0.079]	[0.034]	[0.034]	[1.435]	[0.041]
HSXln(Income at age 4)X(Family Size at age 4)	0.102	0.238	-0.08	0.021	3.356	4.01e-09
	[0.135]	[0.326]	[0.141]	[0.138]	[21.961]	[0.219]
Head Start Partial Effects	0.109	-0.203	0.164	0.068	-1.944	-0.307
Likelihood ratio (Wald) test	9.028	3.216	6.920	6.100	0.280	0.004
P-Value	0.108	0.667	0.227	0.297	0.892	1.000
Observations	577	553	572	548	402	462

Note: This table reports effects of Head Start participation when the effects vary across income levels and family size. Controls excluded from the table are described in table (5). For continuous outcomes, the partial effect of HS_i is

$$\{E[Y_i|HS_i = 1, Z_i, X_i]\widehat{\ -\ }E[Y_i|HS_i = 0, Z_i, X_i]\} =$$

$$\frac{1}{N}\sum_{i=1}^{N}\left\{\begin{array}{c} \beta_0 + \beta_1 \times (family\ \log income)_i + \beta_2 \times (family\ \log income)_i^2 + \\ \beta_3 \times (family\ size)_i + \beta_4 \times (family\ size)_i^2 + \beta_5 \times (family\ \log income)_i \times (family\ size)_i \end{array}\right\},$$

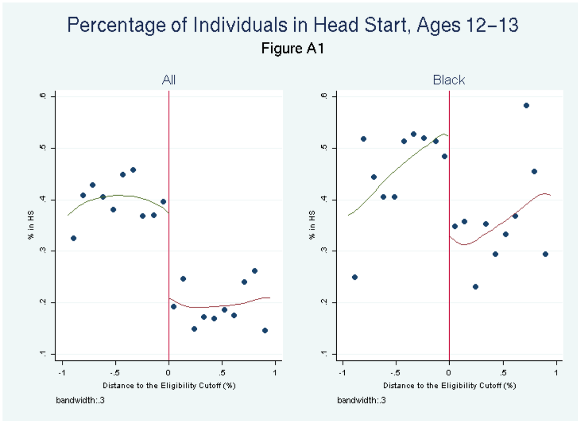
whereas for discrete outcomes is computed by

$$\sum_{i=1}^{N}\{P[Y_i = 1|HS_i = 1, Z_i, X_i] - P[Y_i = 1|HS_i = 0, Z_i, X_i]\} =$$

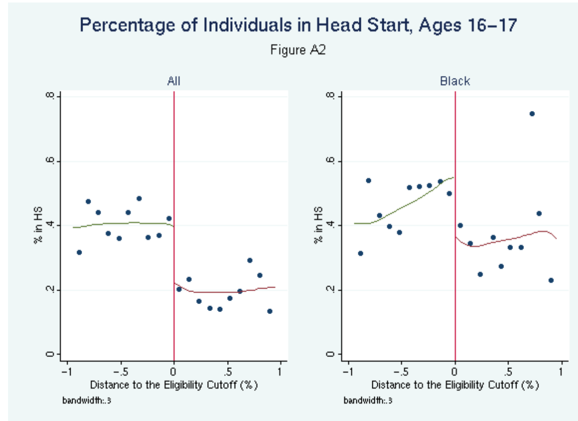
$$\frac{1}{N}\sum_{i=1}^{N}\left\{\begin{array}{c} \Phi[\alpha + \beta_0 + \beta_1 \times (family\ \log income)_i + \beta_2 \times (family\ \log income)_i^2 + \beta_3 \times (family\ size)_i \\ +\beta_4 \times (family\ size)_i^2 + \beta_5 \times (family\ \log income)_i \times (family\ size)_i + g(Z_i, X_i)] - \Phi[\alpha + g(Z_i, X_i)] \end{array}\right\}.$$

where N is the number of children in the sample, and Φ is the standard normal c.d.f. For discrete outcomes the Likelihood Ratio Test tests the model estimated in this table against a model with homogenous effects (whose estimates are presented in tables (8) and (9)); for continuous outcomes (CESD) we present the Wald test for the joint significance of $(\beta_1, \beta_2, \beta_3, \beta_4, \beta_5)$. The $p - Value$ is the p-value for the Likelihood Ratio (or Wald) Test. For discrete outcomes the variance matrix of the Maximum Likelihood Estimator was obtained by the outer product of the gradient and robust standard errors are presented for CESD. * significant at 10%; ** significant at 5%; *** significant at 1%.

Figures

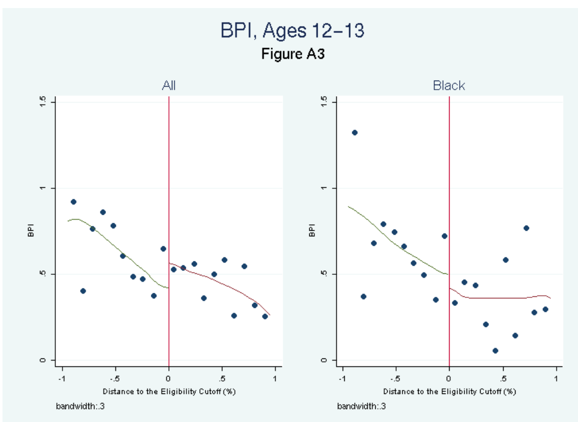


Percentage of Individuals in Head Start, Ages 12–13
Figure A1

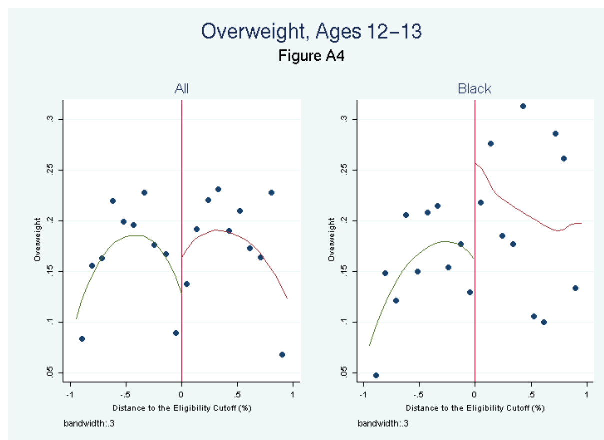


Percentage of Individuals in Head Start, Ages 16–17
Figure A2

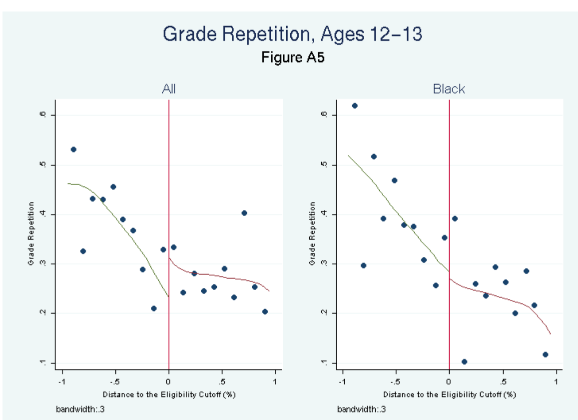
Note: The continuous lines in Figures A1 and A2 present local linear regression estimates of Head Start participation on percentage distance to cutoff; regressions were run separately on both sides of the cutoff and the bandwidth was set to 0.3. Circles in figures represent mean Head Start participation by cell within intervals of 9.5% of percentage distance to cutoff.



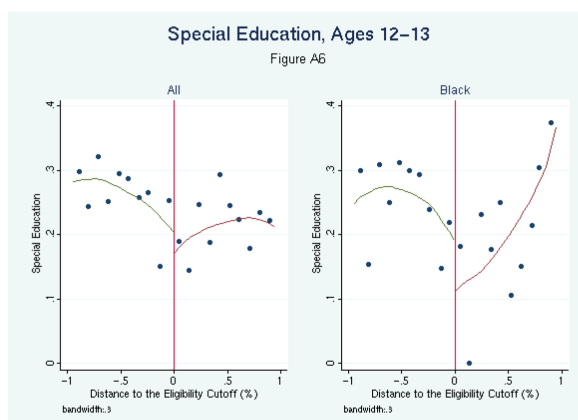
BPI, Ages 12–13
Figure A3



Overweight, Ages 12–13
Figure A4

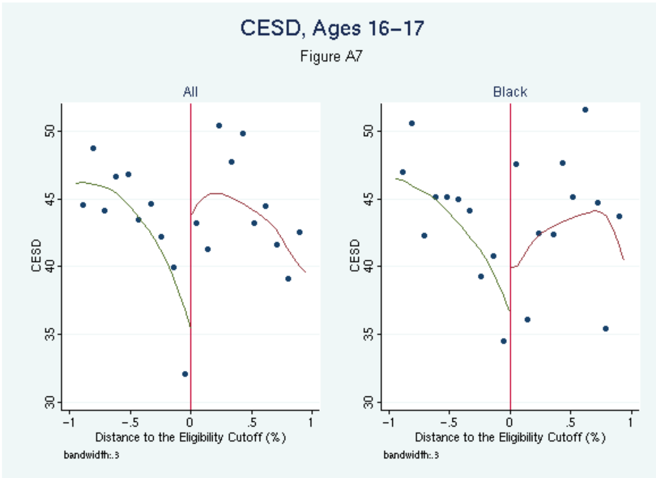


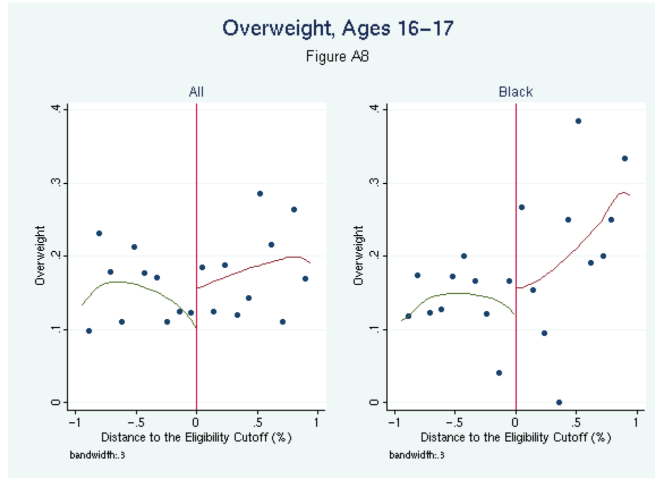
Grade Repetition, Ages 12–13
Figure A5

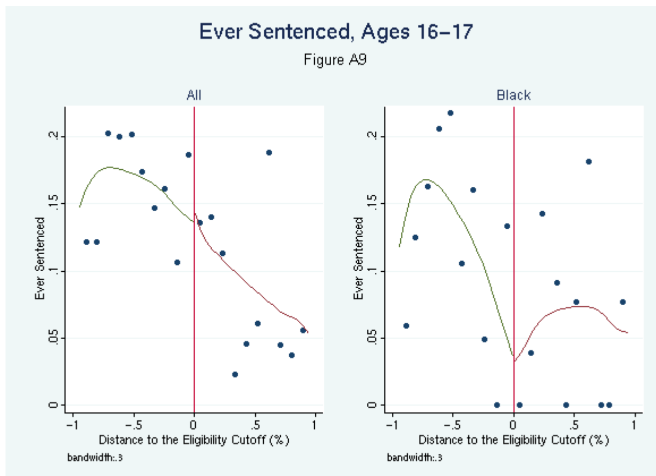


Special Education, Ages 12–13
Figure A6

Note: The continuous lines in Figures A3 to A6 present local linear regression estimates of BPI, Overweight, Grade Repetition and Special Education indicators, respectively, on percentage distance to cutoff; regressions were run separately on both sides of the cutoff and the bandwidth was set to 0.3. Circles in figures represent the mean outcome by cell within intervals of 9.5% of percentage distance to cutoff.







Note: The continuous lines in Figure A7, A8 and A9 present local linear regression estimates of CESD, Overweight and Ever Sentenced indicators, respectively, on percentage distance to cutoff; regressions were run separately on both sides of the cutoff and the bandwidth was set to 0.3. Circles in figures represent the mean outcome by cell within intervals of 9.5% of percentage distance to cutoff.

Appendix B

Eligibility to Head Start

According to the Head Start Act, Sec. 645(a)(2)(A) "children from low-income families shall be eligible for participation in programs assisted under this subchapter (*Head Start*) if their families' incomes are below the poverty line, or if their families are eligible or, in the absence of child care, would potentially be eligible for public assistance"[27]. Alternatively, grantees may enroll up to 10% of children from "over-income" families, as allowed by 45 CFR Part 1305 and, if applicable, by Section 645(a)(2) and (d) of the Head Start Act (*the latter refers to Head Start centers that operate in an Indian tribe*)[28]. See table A for a summary of Head Start's legislation since the program was launched in 1965. The eligibility criteria have been unchanged throughout the period of analysis.[29]

[27] See Title VI, Subtitle A, Chapter 8, Subchapter B, of the Omnibus Budget Reconciliation Act of 1981, Public Law 97-35 (42 USC 9840) and its amends (http://www.law.cornell.edu/uscode/html/uscode42/usc_sec_42_00009840—-000-.html).

[28] Indian Tribes meeting the conditions specified in 45 CFR 1305.4(b)(3) are excepted from this limitation (see 45 CRF Part 1305 - source 57 FR 46725, Oct. 9, 1992, as amended at 61 FR 57226, Nov. 5, 1996).

[29] See www.eric.ed.gov and Zigler and Valentine, 1979.

A low-income family is a family whose income before taxes is below the poverty line or a family that is receiving public assistance, even if the family's income exceeds the poverty line. The U.S. Department of Health and Human Services considers public assistance as AFDC/TANF and SSI (See 45 CFR Part 1305.2). In section 3 we explain why we did not impute SSI eligibility.

The Department of Health and Human Services considers that the income period of time to be considered for eligibility is the 12 months immediately preceding the month in which application or reapplication for enrollment of a child in a Head Start program is made, or the calendar year immediately preceding the calendar year in which the application or reapplication is made, whichever more accurately reflects the family's current needs. We use income from last calendar year because it is the income measure available in NLSY79. As of our knowledge D.H.H.S. does not issue any specific definition of "family unity" and therefore we use NLSY79's definition.

To check the veracity of declared income, centers are required to verify the following proofs: Individual Income Tax Form 1040, W-2 forms, pay stubs, pay envelopes, written statements from employers, or documentation showing current status as recipients of public assistance, and should keep a signed statement by an employee identifying which of these documents was examined and stating that the child is eligible to participate in the program. Some centers do not keep an accurate register. [30]

Given that there are two routes of eligibility to Head Start for each child we perform two separate comparisons:

1. Impute child's poverty status: the child is in a poor family if the annual family gross income available in NLSY79 is below or equal to the Federal Poverty Guideline for each year of data available.

2. Impute child's family AFDC/TANF eligibility. See "AFDC Eligibility Requirements" below for a detailed description.

Finally, the child is eligible to participate in Head Start if she is in a poor family or if she is in an AFDC/TANF eligible family.

In order to restrict our analysis to a comparable group of individuals we restrict our sample to children whose family income at age four was between 5 percent and 195 percent of the maximum level of income that would enable them to be Head Start eligible. To obtain this level of income we perform several comparisons:

1. If the child's family does not verify the categorical requirements to be entitled to AFDC/TANF, the maximum gross income that would have allowed Head Start eligibility is the Federal Poverty Guideline.

2. If the child's family is categorically eligible to AFDC/TANF, several scenarios may emerge[31]:

   (a) if the family is not receiving income from AFDC, or if this information is missing, then two income tests must be verified in order to become AFDC income eligible:

      i. if the *gross income test* is not valid, the maximum level of income that would allow Head Start eligibility is $MAX(m \times Need\ Standard, Federal\ Poverty\ Guideline)$ where m is 1.5 for the years of 1982, 1983 and 1984, and 1.85 from then onwards. We use the Need Standard in the state of residence and year at age four and the Federal Poverty Guideline of the year in which the child turned four years old.

      ii. if *the gross income test* is verified, then the relevant cutoff point will be given by $MAX(MIN(m \times NS, NS + Deductions), Federal\ Poverty\ Guideline)$ where NS is the Need Standard in the state of residence at age four.

[30] See http://eclkc.ohs.acf.hhs.gov/hslc for the Head Start Program Definition of income and Federal Poverty Guidelines.

[31] See "AFDC Eligibility Requirements" for detailed description of the gross and countable income tests.

(b) since 1982, if the family is currently receiving income from AFDC/TANF only *the gross income test* is performed and the maximum level of income above which the family no longer is income eligible is given by $MAX(m \times Need\ Standard, Federal\ Poverty\ Guideline)$

(c) the gross income test had not been implemented as of 1979, 1980 and 1981, and the cutoff is given by $MAX(NS + Deductions, Federal\ Poverty\ Guideline)$

We then define a variable we call *"percentage distance to cutoff"* which results from the percentage difference between the family income and the threshold income level that results from the previous set of comparisons, and use it to restrict our sample to the set of individuals located near their relevant discontinuity cutoffs.

AFDC Eligibility Requirements [32]

Eligibility for AFDC requires that household contains at least one child less than eighteen years old, and has sufficiently low income and assets levels. AFDC-UP (Unemployed Parent) requires that children in two-parents families may be eligible if they satisfy the work history requirement and work less than 100 hours per month while on welfare. The Family Support Act of 1988 mandates that states set up AFDC-UP programs, but it allows states to limit benefits to six months per year.

There are two income tests that an applicant family must pass in order to become AFDC income eligible (U.S. Congress, 1994):

- *the gross income test*: a gross income limit for AFDC eligibility of 150 percent of the state's standard was imposed by The Omnibus Reconciliation Act (OBRA) of 1981, and raised to 185 percent by The Deficit Reduction Act of 1984;

- *the countable income test*: it requires that family income after some disregards must be less than the state's need standard. The countable income is the gross income subtracted of work related expenses, child care expenses, child support disregards up to a maximum.[33]

Eligibility is re-assessed annually, and for those who are already recipients of AFDC/TANF only the first income test is required. To impute the threshold for AFDC/TANF income eligibility for each child we merge the need standard, child support disregards, child care expenses and work related expenses information with the child-level data from the CNLSY79 by state of residence and family size for each year.

Federal AFDC law requires that all income received by an AFDC recipient or applicant must be counted against the AFDC grant except that income explicitly excluded by definition or deduction. The disregards can be computed as follows. Prior to 1981 there was no allowance for work related expenses and child care expenses were capped at 160 dollars per month. The OBRA of 1981 continued to cap the deduction for child care costs at 160 dollars per month and set the work incentive disregard for work expenses at 75 dollars per month. These allowances were increased by the Family Support Act of 1988 that raised work expenses disregards to 90 dollars per month and the child care expenses to 200 dollars per each child under two years old and 175 dollars for month per each child two years or older. This was effective from October 1, 1989, but as our income values are annual we used it from 1990 onwards. In 1996, work related expenses were subsequently raised to 100 dollars, 200 dollars in 1997 and 250 dollars per month since 1998. Between 1997 and 1999, child care expenses were set at 200 dollars per each child either she was under or older than two years old. Additionally, the Deficit Reduction Act of 1984 established a monthly disregard of 50 dollars of child support received by family, that is valid from 1985 (inclusive) onwards. As the last age in which we

[32] See Hoynes, 1996, for a description of AFDC eligibility rules.
[33] Details on all state-specific values can be found in the Welfare Rules Database of the Urban Institute.

impute program eligibility is 7 years old (see tables 7 and A9) and the youngest child in our sample was born in 1992, 1999 in the last year in which eligibility status should be imputed.[34]

Since NLSY79 does not contain systematic collection of child care and work related expenses we assume that families fully deduct the full disregard of child care expenses for all children under 6 years old and no disregard for older children (as is imposed by AFDC requirements), and deduct the full amount of work related expenses if the mother or her spouse is working.

Need standard, work related expenses, child care expenses and child support disregards are defined in monthly levels but were converted into annual values to be comparable with the annual gross income measure available in the NLSY79.

Treatment of Earned Income Tax Credit (EITC) has changed over time. Prior to 1981, EITC was counted only when received, however the OBRA of 1981 requires to assume that working AFDC recipients received a monthly EITC if they appeared eligible for it and regardless of when or if the credit was actually available. The 1984 legislation returned to prior law policy with respect to the EITC: it was to be counted only when actually received. The Family Support Act of 1988 required to disregard the EITC in determining eligibility for and benefits under the AFDC program. As EITC information in NLSY79 started to be recovered with the 2000 wave we ignore the EITC in our analysis.[35]

States are also required by Federal law to disregard certain earned income when determining the amount of benefits to which a recipient family is entitled, which we did not take into account as we only impute income eligibility to the program.

Our treatment of the data regarding stepparent's or mother's partner income was as follows. The OBRA of 1981 required that a portion of the stepparent income to count as part of the income, however, as NLSY79 total income does not include mother's partner income, we do not include it in the definition of income, but as long as child's mother is married, her husband's income is included in the definition of family income (regardless of whether she is married or not with the child's natural father). Also, if mother is cohabiting, her partner will not be included in the family size variable.

When determining AFDC/TANF eligibility we took into account the program categorical requirements with respect to the family structure. Eligibility under the traditional AFDC program requires that a child resides in a female-headed household, which we considered as a family where a father-figure is missing. However, children in two-parents households may still be eligible under the AFDC-Unemployed Parent program in those states in which the program is available[36]. Eligibility for AFDC-UP is limited to those families in which the principal wage earner is unemployed but has a history of work. We consider that the principal wage earner has a "history of work" if the father was employed for less than forty weeks in the previous calendar year (as Currie and Gruber, 1996).[37] We do not perform the assets test required by AFDC, as information on assets is only available after 1985 in NLSY79.

[34]NLSY79 and CNLSY79 surveys were not conducted in 1995, 1997 and 1999, and income, family size, child's mother cohabiting status and state of residence were not imputed for these years.

[35]Given the extensive set of robustness checks performed, we are convinced that our results are not sensitive to this.

[36]In 1988, the Family Support Act required all States, effective October 1, 1990, to provide AFDC-UP (except American Samoa, Puerto Rico, Guam, and the Virgin Islands until funding ceilings for AFDC benefits in these areas are removed). The two-parent program reverted to optional status for all States after September 30, 1998.

[37]Since 1971, Federal regulations have specified that an AFDC parent must work fewer than 100 hours in a month to be classified as unemployed, unless hours are of a temporary nature for intermittent work and the individual met the 100-hour rule in the two preceding months and is expected to meet it the following month. Attachment to the labor force is one condition of eligibility for AFDC-UP. See U.S. Congress, 1994, for the specific requirements.

Table A: Summary of Legislation Related to Head Start

Date	Law Number	Title	Description
1964	88-452	Economic Opportunity Act	Anti-poverty bill to "strengthen, supplement, and coordinate efforts in furtherance" of a policy of the U.S. "to eliminate the paradox of poverty in the midst of plenty". HS was not mention in the original act, but it was considered part of the Community Action Program.
1966	P.L. 89-794	Economic Opportunity Act Amendments of 1966	A section was added to Title II making HS a part of the Economic Opportunity Act.
1967	P.L. 90-222	Economic Opportunity Act Amendments of 1967	"Follow Through" was added in Title II, to continue services for HS children when they enter kindergarten and elementary school. This program was administered by the Office of Education.
1969	P.L. 91-177	Economic Opportunity Act Amendment of 1969	A provision was added allowing children from families above the poverty level to receive Head Start services for a fee.
1972	P.L. 92-424	Economic Opportunity Act Amendment of 1972	A fee schedule for non-poor participants in Head Start was required; fees were prohibited for families below the poverty line. Added a requirement that at least 10 percent of HS's enrollment include children with disabilities.
1973	93-202	Postponement of a Head Start Fee Schedule	Prior approval by Congress was required before any Head Start fee schedule could be established.
1974	P.L. 93-644	Head Start, Economic Opportunity, and Community Partnership Act of 1974	Reauthorized HS through the fiscal year of 1978. HS funds should be allocated to states proportionately based upon each state's relative number of children living in families with income below the poverty line and the relative number of public assistance recipients in each state.
1978	P.L. 95-568	Economic Opportunity Act Amendment of 1978	Reauthorized Head Start for three more years. Minor changes to the law.
1981	P.L. 97-35 (42 USC 9831 et. Seq.)	Economic Opportunity Act Amendment of 1981	The HS Act was attached to the OBRA of 1981 and its goals are to "promote school readiness by enhancing the social and cognitive development of low-income children."
1984	P.L. 98-558	Human Services Reauthorization Act of 1984	HS Reauthorization for 2 years. The Indian and Migrant branches of HS became separate regions; prohibited changes in methods for determining eligibility for low income if they would reduced participation of persons in the program. HS may provide services to children age 3 to the age of compulsory school attendance.
1989	P.L. 101-120	Head Start Supplemental Authorization Act of 1989	Reauthorized Head Start for FY of 1990.
1990	P.L. 101-597	National Health Service Corps Revitalization Amendments of 1990	Minor amend to Head Start Act.
1990	P.L. 101-501	HS Reauthorization Act of 1990.	Reinforced importance of parental involvement, improved information on HS programs.
1992	P.L. 102-763	Head Start Improvement Act	Facilities purchase; Extended waivers for non-federal regulations; Establishment of transportation regulations; Health services to younger siblings; Protection of the quality set-aside; Literacy and child development training to parents; Elimination of priority status to a grantee once funded.
1994	P.L. 103-218	Head Start Act Amendments of 1994	Reauthorized HS for the years of 1995 through 1998. Required the development of quality standards (including revising the Program Performance Standards), the development of performance measures, and improved monitoring of local programs. It authorized family-centered programs for infants and toddlers. It established new standards for classroom teachers and family service workers.
1998	P.L. 105-285	Coats Human Services Reauthorization Act of 1998	Reauthorized Head Start for 5 years.
2007	110-134	Improving Head Start for School Readiness Act of 2007	Allows grantees to serve additional children from families with income up to 130% of poverty to be served; formula allocation remains at 100% of poverty; expansion of both HS and Early HS programs with additional funds going to states serving fewer than 60 percent of eligible children; establishes standards for the curriculum of teachers.

The regulations relevant to Head Start 45 CFR, Parts 1301 to 1311. Additional Program Instructions and Information Memorandums can be found at the Early Childhood Learning and Knowledge Center web site: http://eclkc.ohs.acf.hhs.gov/hslc.

Appendix C: Calculation of adjusted p-values

This appendix describes our algorithm for calculating familywise adjusted p-values. It is based on Algorithms 4.1 and 4.2 of Romano and Wolf, 2005.

Let T be the sample size and $s = \{1, ..., S\}$ the number of hypothesis to test. Consider an individual test statistic $z_{T,s} = \hat{\beta_{T,s}} / \hat{\sigma_{T,s}}$, where $\hat{\beta_{T,s}}$ is the estimated coefficient on the Head Start eligibility indicator and $\hat{\sigma_{T,s}}$ is the estimated standard deviation of $\hat{\beta_{T,s}}$. Let X_T^* denote a data matrix generated by bootstrap, and $\hat{\beta^*_{T,s}}$ is the estimated coefficient on Head Start eligibility obtained using X_T^* and $\hat{\sigma^*_{T,s}}$ is the respective estimated standard deviation, such that $z^*_{T,s} = \hat{\beta^*_{T,s}} / \hat{\sigma^*_{T,s}}$. Let $\hat{d}_j$ be a data-dependent critical value obtained as in Algorithms 4.1 and 4.2 of Romano and Wolf, 2005.

We start by re-labelling the hypotheses in ascending order of their p-value. In the first step, we reject the null hypothesis that $\hat{\beta_{T,s}} = 0$ if $|z_{T,s}| > \hat{d}_1$, for all $s = \{1, ..., S\}$. If none of the null hypothesis is rejected, the process stops. If at least one is rejected, we remove it from the data and treat those left as the original data. We construct a new critical value with remaining data, which we denote by $\hat{d}_2$. Again, we reject the null hypothesis of $\hat{\beta_{T,s}} = 0$ if $|z_{T,s}| > \hat{d}_2$, for all $s = \{1, ..., S\}$ (excluding those hypothesis rejected in the first step). The process stops when no hypothesis is rejected.