DOES HEAD START IMPROVE CHILDREN'S LIFE CHANCES? EVIDENCE FROM A REGRESSION DISCONTINUITY DESIGN*

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This paper exploits a new source of variation in Head Start funding to identify the program's effects on health and schooling. In 1965 the Office of Economic Opportunity (OEO) provided technical assistance to the 300 poorest counties to develop Head Start proposals. The result was a large and lasting discontinuity in Head Start funding rates at the OEO cutoff for grant-writing assistance. We find evidence of a large drop at the OEO cutoff in mortality rates for children from causes that could be affected by Head Start, as well as suggestive evidence for a positive effect on educational attainment.

I. INTRODUCTION

Head Start was established in 1965 as part of the War on Poverty to provide preschool, health, and other social services to poor children age three to five and their families, and currently serves over 900,000 children each year at a cost of around $7 billion [HHS 2006]. This paper provides new evidence on the effects of the Head Start program on schooling and health by drawing on a new source of variation in program funding. Spe-

cifically, we exploit a discontinuity in program funding across counties that resulted from how the federal government's Office of Economic Opportunity (OEO) originally launched the program.

Interest in Head Start is motivated in part by large disparities in cognitive and noncognitive skills along race and class lines observed well before children start school [Tremblay et al. 2004; Brooks-Gunn and Markman 2005; Cunha et al. 2005], and by arguments that human capital interventions may be particularly promising for disadvantaged children during the early years of life [Heckman and Krueger 2003]. The potential value of intervening early is also suggested by evidence that neurodevelopmental plasticity may decline with age, together with the possibility of dynamic complementarities in learning [Shonkoff and Phillips 2000; Cunha et al. 2005; Knudsen et al. 2006]. "Two generation" programs like Head Start, which target parents as well as children, could affect parent outcomes and change the stream of investments they make in their children over the life course. And the nutrition and medical services provided by Head Start could improve child health, an important outcome in its own right that may also affect later economic outcomes [Case, Fertig, and Paxzon 2003; Almond 2005].

Whether Head Start yields lasting benefits in practice is an open question, the answer to which will necessarily rest on nonexperimental evidence for the foreseeable future. A recent federally sponsored randomized evaluation of Head Start finds impacts on test scores one year out of around 0.1 or 0.2 standard deviations compared with the mix of home- and center-based care received by controls [HHS 2005]. Whether these impacts will persist over time is unclear. Concern about "fade out" of Head Start impacts dates back to the 1960s [Vinovskis 2005], and more recently has led to proposals to change the program's focus from "comprehensive services to intellectual development" [Haskins 2004]. Even if the government's recent experiment included funding to track impacts through adulthood, evidence on long-term effects would not be available for decades. In the interim, policymakers might be tempted to draw inferences about Head Start's long-term effects from the encouraging results of model programs such as Perry Preschool and Abacedarian [Campbell et al. 2002; Schweinhart et al. 2005]. However, these programs are much smaller and more intensive (and expensive) compared with Head Start.

To date, the best available evidence for the lasting effects of

Head Start comes from within-family comparisons of siblings who have and have not participated in the program [Currie and Thomas 1995; Garces, Thomas, and Currie 2002, hereinafter CT and GTC]. CT find positive effects on test scores for children around age five. However, these gains fade out for Black children, perhaps due to lower quality schooling after Head Start [Currie and Thomas 2000]. CT is also one of the few studies to consider effects on health, and find Head Start improves immunization rates but has no detectable effects on height-for-age. GTC find long-term effects on high school completion for Whites (an increase of 22 percentage points), and reductions in criminal behavior among African-Americans (a decrease of 12 percentage points for being booked or charged).

While both CT and GTC substantially improve upon previous studies, as one recent review notes with respect to the program's long-term impacts: "The jury is still out on Head Start" [Currie 2001, p. 213]. As CT note, measurement error and spillover effects across siblings may bias their impact estimates toward zero. There also necessarily remains some uncertainty about what drives variation across siblings in Head Start participation. Furthermore, for policy purposes it is important to learn more about Head Start's long-term impacts on schooling, given the lack of evidence to date of lasting effects on Blacks, and on nonacademic outcomes like health, given debates about increasing the program's educational focus.

Our paper exploits a discontinuity in program funding across counties that resulted from OEO's implementation of the program. Specifically, during the spring of 1965 OEO provided technical assistance to the 300 poorest counties in the United States to develop Head Start funding proposals. We show that Head Start participation and funding rates appear to be 50–100 percent higher in counties with poverty rates just above OEO's cutoff (the "treatment" group) compared with those just below (controls). These findings come from (somewhat noisy) county-level federal spending data and from the National Education Longitudinal Study (NELS), which tracks a national sample of eighth graders in 1988. The funding difference at the heart of our design appears to have persisted through the late 1970s. The estimated discontinuity in other federal social spending is small and not significant.

We use this discontinuity in Head Start funding to identify impacts by comparing outcomes for people in treatment and con-

trol counties "near" the OEO cutoff. Identification comes from the assumption that potential outcomes are smooth around the cutoff. Because the cutoff was based on a predetermined variable, the usual concerns about strategic behavior with the regression discontinuity (RD) design do not seem to be an issue here [McCrary 2005].

Our main finding is that there appears to be a large drop in mortality rates of children five to nine years of age over the period 1973–1983 due to causes addressed as part of Head Start's health services. Our estimates imply that a 50–100 percent increase in Head Start funding reduces mortality rates from relevant causes by 33–50 percent of the control mean, enough to drive mortality rates from these causes in the treatment counties down to about the national average. There do not appear to be drops for other causes-of-death or cohorts that should not be affected by Head Start.

We also examine Head Start's effects on educational attainment by drawing on two separate data sources, neither of which is ideal but which taken together seem to provide at least suggestive evidence for a positive effect on schooling. The challenge is to measure adult schooling outcomes for those who were young children in counties "near" the OEO cutoff in the 1960s and 1970s. We show that there are signs of a positive discontinuity at the OEO cutoff in schooling in the decennial censuses from 1970 to 2000 among the cohorts born late enough to have enrolled in Head Start, and to some extent among birth cohorts who would have been parents or older siblings of participants, but not among "untreated" cohorts. The main drawback with these results is that we can only identify county of residence for people as adults.

To try to address the possibility of bias from selective migration, we show there is also suggestive evidence for a discontinuity at the OEO cutoff in schooling in the NELS, which improves upon the Census by recording each respondent's county of residence at age thirteen rather than adulthood, but at the cost of small samples local to the cutoff. Importantly, we do not find evidence of discontinuities in exogenous population attributes such as race, age, or urbanicity, which does not seem consistent with a story in which our results for children's outcomes are driven by selective migration of families across counties.

Our preferred estimates use a nonparametric approach to control for unobserved variables that vary with 1960 county poverty rates [Hahn, Todd, and Van der Klaauw 2001; Porter 2003].

We also show results for more parametric approaches, although on balance the nonparametric estimators seem to fit the data a bit better near the OEO cutoff. As with many RD studies statistical power is an issue, and in some cases estimates that are economically significant are not statistically significant. Nevertheless, the results point in the direction of sustained impacts of Head Start on health and schooling outcomes, including for Blacks as well as Whites and males as well as females.

The next two sections provide more details about our research design and Head Start. Our data and empirical strategy are discussed in Sections IV and V, while results for Head Start funding and participation rates are in Section VI, the main results for mortality and schooling are in Sections VII and VIII, and specification and robustness checks are summarized in Section IX. The final section discusses limitations and implications for public policy.

II. RESEARCH DESIGN

Our study provides new evidence on the long-term effects of Head Start by exploiting a natural experiment generated by how the program was launched by OEO. Head Start began as a summer program for children around ages three to five; by 1970 a majority of participants were year-round. Because Head Start involves direct federal funding of local service providers, the challenge for OEO in the spring of 1965 was to publicize Head Start, encourage local organizations such as schools and public health or welfare agencies to apply, review proposals, and fund enough local programs to launch Head Start in the summer of 1965 on the grand scale desired by President Lyndon B. Johnson—all within the span of a few months.

Despite OEO's efforts to publicize Head Start, officials were concerned that applications from poor counties would be underrepresented in a nationwide grant competition [Gillette 1996, p. 231]. In response to this concern, Head Start associate director Jule Sugarman initiated an effort to generate applications from the 300 poorest counties in the U. S. Volunteers from the federal Presidential Management Intern (PMI) program traveled to the selected counties for two to six weeks during the spring of 1965, located local actors who would be able to implement a Head Start program, work with them to develop proposals, and then flew the applications back to OEO

and defended them to reviewers. Importantly, this feature of Head Start's launch is widely documented in historical accounts, suggesting that the discontinuity in grant-writing assistance at the heart of our design is not the figment of a single historian's imagination.[1] Below, we show that the result is a discontinuity in county-level Head Start funding, which persists through the late 1970s.

We use the targeting of Head Start grant-writing assistance to just the 300 poorest counties to identify the effects of the program on people living in counties "near" the OEO cutoff using a standard RD design. Often with RD designs we are concerned about the possibility of strategic behavior that may push observational units above or below whatever cutoff is used for the assignment of treatment [McCrary 2005]. However, there does not seem to be much room for strategic behavior in our study given that Head Start grant-writing help was assigned on the basis of a predetermined variable (poverty rates five years earlier), and the problem facing OEO administrators was one of excess funding supply rather than demand, so there would be no incentive for "gaming" or favoritism by OEO. However, generalizability may be more of an issue with our estimates: the three hundredth poorest county in the United States at the time Head Start was launched had a 1960 poverty rate of around 59 percent, so counties near this cutoff are very poor counties indeed, and most are located in the South.[2] We return to this point in the conclusion.

III. Head Start Services

Widely perceived as a schooling program, early childhood education is only one of Head Start's six service components, accounting for around 40 percent of the program's budget [Rich-

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1. See, for example, Jones [1979, pp. 6–7], Gillette [1996, p. 222], President Johnson's speech on Head Start of May 18, 1965 [Zigler and Valentine 1979, pp. 69–70], GAO [1981, p. 17], and Vinovskis [2005, p. 88].
2. One-third of the 300 poorest counties in 1960 were in Mississippi, Kentucky, or Georgia. Almost all of the 300 poorest counties were in just ten states (the others are Alabama, Arkansas, Louisiana, North and South Carolina, Tennessee, and Texas). These ten states account for over two-thirds of the 300 "control" counties (1960 poverty rates that rank from three hundred first to six hundredth in the United States), with most of the rest in Florida, Oklahoma, Virginia, or West Virginia.

mond, Stipek, and Zigler 1979; Currie and Neidell 2006]. The other program elements include the following.

Parent involvement: The first year of the program 47,000 parents were employed in Head Start centers, and another 500,000 were part-time volunteers—about as many parents as children [Zigler and Valentine 1979, p. 69]. Parental involvement may affect earnings, parenting skills, and even parent schooling, given the focus during the early years on staff training and education.[3]

Nutrition: Head Start also provides children with nutritious meals and snacks. One early quasiexperimental study found that around 10 percent of children entering Head Start were receiving less than two-thirds of their daily caloric needs, and more than 40 percent received less than two-thirds of daily iron needs. Head Start was found to reduce the fraction with low caloric and low iron intake by around one-half and one-fifth, respectively [Fosburg et al. 1984, p. 175, 214].[4]

Social services: Poverty puts Head Start children at elevated risk for family problems.[5] As Lazear [1979, p. 289] notes, "The Head Start social worker serves a central role in strengthening family life—and family competence. She uses her casework skills in helping families cope with crises . . . and her skills in community organization in building and improving linkages between community professionals and agencies and Head Start."

Mental health services: Another goal of Head Start is to identify mental health problems and help access treatment. The prevalence of untreated problems among Head Start children may be quite high. Previous estimates suggest that nationwide 12–15 percent of all children suffer from one or more mental health disorders that are severe enough to require treatment, yet more than two-thirds of such children were not being treated [Keane et al. 1996, pp. 10–11].

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3. For example, an early OEO program announcement recommended one day per week release time for staff for either Head Start-specific training or adult education, such as high school equivalency or college [Trickett 1979, p. 320].

4. The study by Fosburg et al. [1984] was designed as a randomized experiment. However, of the 376 children who received pretests in the study fully 45 percent were lost to sample attrition by the time of the posttest [p. 370].

5. One study of Head Start children in Maine found that more than one in five were in foster homes or being followed by state agencies such as child protective services [Keith and Leeman 1993, cited in Zigler et al. 1994].

Health services: Around one-eighth of Head Start's budget goes to providing children with health services such as immunizations (polio, DTP, measles) and screening for conditions such as tuberculosis, nutritional deficiencies, anemias, and diabetes [North 1979; Keane et al. 1996; O'Brien, Connell, and Griffin 2004]. Additional health screening is intended to occur through classroom observation [North 1979; Keane et al. 1996, pp. 2–7]. Head Start also served as a broker between families and community health services, and in addition to medical referrals sometimes directly provided treatment or served as a last-dollar source of payment [North 1979].

This bundle of Head Start services might affect schooling outcomes through a variety of causal channels. In addition to the direct effects on schooling from early childhood education, nutrition, and health services, Head Start may indirectly affect children's schooling by influencing parents' schooling attainments or parenting practices.

Most important for health outcomes are likely to be Head Start's screening and treatment referrals, particularly given the limited medical services available otherwise to poor (particularly minority) children in the poorest parts of the South in the 1960s and 1970s.[6] A previous study of Head Start in Mississippi found the program increased the chances children saw a doctor for their health problems almost sixfold, and cut the fraction of children with health problems nearly in half (37 *vs.* 63 percent) [Fosburg et al. 1984, pp. 90–91]. To the extent to which some children would have received some of these health services later on anyway, our estimates capture the benefits of early detection and treatment of the chronic conditions targeted by Head Start. Whether children would have received screening and treatment otherwise is not clear, since the health services provided by schools and other institutions were quite different during our study period compared with today.[7] In any case Head Start's other features may also inter-

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6. For example, in 1962 the number of physicians per capita in the rural parts of the country's seventeen poorest states was about one-third the national average [Lesser and Hunt 1968, pp. 337–338, 351]. In 1965 fewer than half the children entering Mississippi's largest Head Start program had received *any* immunizations [Greenberg 1969, p. 183].

7. Through the 1970s fewer than half of states had school immunization requirements for measles [Hinman et al., 2004], and more generally health services offered by schools during this period seem to have been limited [Allensworth et al. 1997, pp. 41–42]. Health screening for children in the United States overall

act with or complement these health services as well. Parenting and social work services may reduce children's exposure to chronic stress, which may compromise immune system functioning [McEwen 1998; Lewis et al. 2000; Repetti et al. 2002], while malnutrition elevates the risk of complications from health problems such as measles. A more detailed discussion of the specific mortality causes that might be affected by Head Start is in Appendix I.

IV. Data

In what follows we provide a brief overview of the county- and individual-level data sources used in our analysis. Additional details on each data set are in Appendix I.

Perhaps the most important data question for our study is to understand how OEO's Head Start office identified the 300 poorest counties in the United States in spring 1965. The answer is not obvious because "poverty" had not yet been officially defined by the federal government at the time of the 1960 Census. OEO apparently relied on a special 1964 reanalysis of the 1960 Census data by the Census Bureau using the then-new federal poverty definition; we have obtained a copy of these data from the National Archives and Records Administration (NARA).[8]

Other county-level data for our study include OEO files from NARA for all federal expenditures by program for 1967 through 1980, although the accuracy of these data is less than perfect given poor documentation and some obvious errors. In the end, only data from 1968 and 1972 were usable, in the sense that the electronic data matched published Head Start and other federal spending figures at the national and state levels. Even here measurement error arises from complications such as providers that run Head Start programs in multiple counties but are listed as receiving federal funds only in the county with the organization's headquarters.

Data on child mortality come from the Vital Statistics, which include information on cause of death and the decedent's age. To

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has surely become more effective over time given that the number of physicians per capita increased by 80 percent from 1970 to 2000, while the number of registered nurses per capita increased by 120 percent [HHS, 2003 Table 101].

8. The alternative possibility is that OEO used the share of families with incomes below $3000, although our analysis reveals a larger discontinuity in funding at the three hundredth poorest county using the official poverty rate.

measure schooling outcomes, we use county-level data on schooling by age from the decennial censuses from 1960 through 2000. For 1990 we have also obtained a special tabulation of Census data from the Census Bureau that provides more detailed information on schooling attainment by age, race, and gender.

Our main source of individual-level data is a restricted-use geo-coded version of the NELS, which provides information for a nationally representative sample of eighth graders in 1988 with follow-up interviews through 2000. One key variable for our analysis is the NELS respondent's county of residence in the base year (1988) wave. A drawback with the NELS is the study is intended to provide a nationally representative sample, and so the base year sample includes only 649 respondents who lived in counties with 1960 poverty rates among the 300 poorest, and 674 who lived in one of the next 300 poorest counties. Other key variables include parent reports about whether children were in Head Start (which are on average consistent with aggregate enrollment figures), and the respondent's schooling as of the 2000 wave, when most respondents would have been around twenty-five years of age.

V. EMPIRICAL STRATEGY

The heart of our research design is to examine whether discontinuities in Head Start funding at the OEO cutoff are mirrored by discontinuities in other outcomes. Our analyses are conducted using county-level data, the level at which our Census and mortality data are reported. We aggregate the NELS to the county level as well, the simplest way to account within our nonparametric framework for the fact that students within counties are not independent units.[9]

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9. That is, for each county (c) and NELS respondent (i) we calculate the average outcome within the county as $Y_c = \Sigma_c(Y_{ic}w_{ic}) / \Sigma_c(w_{ic})$, where w_{ic} represents the sampling weight for the survey wave from which we draw the accompanying outcome measure Y_{ic}, and (c) indexes the county in which each NELS respondent lives in eighth grade. The estimates shown below do not weight by county population. When we recalculate all of our estimates with county population weights, which provides us with information about the effect on the average person rather than the average county, the results for the NELS Head Start participation, for the mortality results, and for the educational outcomes for the directly treated cohort in the Census are at least as strong as those shown below (in terms of the absolute magnitude of the point estimates and their size in relation to the standard errors). The results are weaker than those shown below for Head Start funding and NELS educational outcomes. However, the weighted estimates show somewhat more pronounced discontinuities for exogenous characteristics at the OEO cutoff, and show more pronounced discontinuities for

Let Y_c represent some average outcome for residents of county (c), such as child mortality rate, let P_c represent each county's poverty rate in 1960, and let the index (c) be defined over counties sorted in descending order by their 1960 poverty rate (so that $c = 1$ is the poorest county and the OEO cutoff occurs at $c = 300$). The provision of grant-writing assistance is a deterministic function of the county's 1960 poverty rate,

$$(1) \qquad G_c = 1 \ (P_c \geq P_{300}),$$

where $P_{300} = 59.1984$.

We can use the "sharp" RD implied by (2) to estimate discontinuities in outcomes at the OEO cutoff [Trochim 1984], which is in some sense like an "intent-to-treat" effect (ITT)—the effect of offering local providers help in securing Head Start funding. We focus on "reduced form" ITT-style estimates in part because the noise in our federal spending data limits our power to scale these estimates to recover the effects per dollar of funding.[10]

Our main estimating equation is given by

$$(2) \qquad Y_c = m(P_c) + G_c \alpha + v_c$$

where $m(P_c)$ is an unknown smooth function of 1960 poverty, and α is the impact of grant-writing assistance. The effect that we seek to identify is the one relevant for the poorest counties near the OEO cutoff.

Identification of the causal effects of Head Start grant-writing assistance—the ITT corresponding to a treatment of increased Head Start funding in the county—comes from assuming smoothness in potential outcomes near the OEO cutoff [Porter 2003]. This seems like a plausible assumption, since the cutoff was defined on the basis of a predetermined variable (poverty rates five years before Head Start's launch), and because (as we demonstrate below) this cutoff does not seem to have been used to distribute funding for other federal programs.

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schooling outcomes for the directly treated cohorts in the 1990 Census at a pseudo-cutoff at which there is no discontinuity in Head Start funding. To the extent to which this serves as a diagnostic test, this finding provides further empirical justification for preferring the unweighted estimates.

10. Another problem with estimating the effects per dollar of Head Start spending is that we have spending data across counties only for a few of the birth cohorts exposed to the Head Start funding discontinuity. The NELS captures discontinuities in Head Start participation but will miss across-county differences in funding that go toward increased spending per participant rather than expanded enrollment rates.

One practical question is how to model $m(P_c)$. We present "parametric" results that estimate (2) using different polynomial functions of P_c calculated using counties "near" the OEO cutoff. We obtain similar results when we control for observable county covariates from the 1960 Census such as total population, age or race distribution, or even state fixed effects [see Ludwig and Miller 2005]. This parametric approach assumes that we can adequately control for the other determinants of long-term outcomes that vary across counties using a sufficiently flexible polynomial function of P. We focus on linear and quadratic models, allowing the slope of these functions to vary on each side of the cutoff.

Our preferred estimates relax these functional form assumptions by using the nonparametric RD approach of Hahn, Todd, and Van der Klaauw [2001] and Porter [2003]. This method uses local linear regressions [Fan 1992] to estimate the left and right limits of the discontinuity, where the difference between the two is the estimated treatment impact. We estimate this in one step using kernel weights $w_c = K((P_c - P_{300})/h)$ for chosen bandwidth h.

$$(3) \qquad Y_c = b_0 + b_1(P_c - P_{300}) + \alpha G_c + b_2 G_c(P_c - P_{300}) + v_c.$$

We essentially estimate a kernel-weighted linear regression using data points to the left of the OEO cutoff, another kernel-weighted linear regression with data to the right of the OEO cutoff, and then calculate our estimate for the treatment impact (the parameter α) as the difference between the left and right limits of these regressions at the OEO cutoff. We use the triangle kernel $K(z) = (1 - |z|)I_{[0,1]}(|z|)$, which is boundary optimal [Cheng, Fan, and Marron 1997], although consistent with previous research, we find that our results are not very sensitive to choice of kernel [Fan and Gijbels 1996]. We present analytic standard errors below derived using the formula from Porter [2003]. We also show p-values from a paired-bootstrap percentile-T procedure with 2000 replications, which may offer more accurate asymptotic inference than the analytic standard errors [Cameron and Trivedi 2005].

The remaining estimation issue has to do with bandwidth selection. Because the RD design is identified only at the discontinuity, we try to balance the goals of staying as local to the OEO cutoff as possible while ensuring that we have enough data to yield informative estimates. Unfortunately there is currently no widely agreed-upon method for selection of optimal bandwidths

in the nonparametric RD context, and so our strategy is to present results for a broad range of candidate bandwidths. We use a bandwidth range from 9 to 36 for most of our data sets, with a focal preferred bandwidth of 18.[11] Our tables show how many counties receive nonzero weight for each bandwidth and data set. The decennial censuses provide more information near the cutoff so we use a range of 3.5–14, with a preferred bandwidth of 7.

VI. RESULTS FOR HEAD START FUNDING AND PARTICIPATION

Historical accounts suggest that OEO's Head Start grant-writing assistance substantially increased program funding in the 300 poorest counties—80 percent of these counties received Head Start funding [GAO 1981], compared with 43 percent of all counties nationwide. The findings from our own data analyses are qualitatively consistent with these historical accounts.

Table I shows that for the 228 "treatment" counties with 1960 poverty rates 10 percentage points above the OEO cutoff of 59.198, average Head Start spending per four-year-old in 1968 is about twice as high as in the 349 "control" counties with 1960 poverty rates within 10 points below the cutoff ($288 *vs* $134). In 1972 Head Start spending per four-year-old is still nearly 60 percent higher in the treatment than control counties.

In Table II, we demonstrate that the difference in Head Start funding around the OEO cutoff shown in Table I is driven in large part by a sharp drop-off in spending at the cutoff itself. The top panel presents results from estimating the "first-stage" effects of OEO's grant-writing assistance on Head Start participation rates in the NELS using the estimation approaches discussed previously. Since all of our tables are formatted similarly, we describe Table II in some detail. Each row presents results for a different dependent variable. Each column presents a different specification. The first three columns show results from nonparametric RD specifications, with varying bandwidths. For each bandwidth, we show the number of counties that receive nonzero weight in estimation. We also present the point estimate, analytic standard

11. For example, a bandwidth of 18 assigns positive weight to the 961 counties in the NARA spending data that have 1960 poverty rates within 18 percentage points of the (59.1984) cutoff. See Ludwig and Miller [2005] for a discussion of the limitations of using leave-one-out cross-validation for bandwidth selection in our application.

TABLE I
COUNTY CHARACTERISTICS

	Counties with 1960 poverty 49.198% to 59.198%		Counties with 1960 poverty 59.1984% to 69.1984%	
	Mean	Std. dev.	Mean	Std. dev.
Results from county-level spending and Census data				
No. of observations (counties)	349		228	
County-level federal spending				
Head Start spending per 4-year-old 1968	134	(277)	288	(915)
Head Start spending per 4-year-old 1972	183	(569)	289	(927)
Other social spending per capita 1972	446	(128)	483	(167)
1990 Census characteristics				
1990 County population	24,202	(24054)	21,371	(29799)
Fraction ages 18–24	0.0958	(0.03)	0.0954	(0.02)
Fraction ages 25–34	0.148	(0.02)	0.149	(0.02)
Fraction ages 35–54	0.243	(0.02)	0.238	(0.02)
Fraction ages 55 plus	0.243	(0.05)	0.232	(0.05)
1990 Percent urban	0.0254	(0.12)	0.0172	(0.10)
1990 Percent Black	0.163	(0.16)	0.266	(0.22)
1990 Per capita income	9520	(1537)	8488	(1434)
Results from the National Education Longitudinal Study				
No. of counties containing NELS respondents	28		17	
Head Start participation (base-year weights)	0.233	(0.16)	0.388	(0.24)
Head Start participation (first year follow-up weights)	0.244	(0.18)	0.423	(0.24)

All means are unweighted. Data are from OEO archival data on federal spending from NARA, 1990 census STF4 file, and from the NELS.

TABLE II

REGRESSION DISCONTINUITY ESTIMATES OF THE EFFECT OF HEAD START ASSISTANCE ON HEAD START SPENDING AND PARTICIPATION

Variable	Control mean	Nonparametric			Parametric	
					Flexible linear	Flexible quadratic
Results from the National Education Longitudinal Study						
Bandwidth or poverty range		9	18	36	8	16
Number of observations (counties) with nonzero weight		43	96	288	43	82
Head Start participation (base year sample)	0.259	0.168	0.135	0.126	0.168	0.217
		(0.142)	(0.094)	(0.056)	(0.148)	(0.142)
		[0.317]	[0.129]	[0.113]	[0.277]	[0.216]
Head Start participation (first year follow-up sample)	0.286	0.238	0.172*	0.148*	0.238	0.316*
		(0.150)	(0.100)	(0.061)	(0.156)	(0.151)
		[0.189]	[0.090]	[0.089]	[0.154]	[0.087]
Results from county-level federal spending data						
Bandwidth or poverty range		9	18	36	8	16
Number of observations (counties) with nonzero weight		527	961	2,177	484	863
1968 Head Start spending per child	137.505	137.251	114.711	134.491**	127.389	112.706
		(128.968)	(91.267)	(62.593)	(120.098)	(112.603)
		[0.157]	[0.138]	[0.045]	[0.225]	[0.297]
1972 Head Start spending per child	198.396	182.119*	88.959	130.153*	175.773	155.899
		(148.321)	(101.697)	(67.613)	(142.363)	(132.292)
		[0.085]	[0.352]	[0.090]	[0.109]	[0.152]
1972 other social spending, per capita	452.384	14.474	19.590	14.506	-2.361	5.659
		(28.356)	(19.612)	(14.929)	(25.624)	(27.471)
		[0.459]	[0.222]	[0.478]	[0.979]	[0.755]

Each cell presents a separate estimate of the discontinuity in the outcome measure listed in the left-hand column at OEO's threshold 1960 poverty level for providing counties with grant-writing assistance for Head Start funding. Within each cell the first number represents our point estimate of α from (3), with analytic standard errors in parentheses and percentile-T bootstrapped p-values in square brackets. Nonparametric estimates are based on the locally weighted kernel regression method discussed in Porter [2003], calculated using a triangle kernel. Parametric models give equal weight to observations within the range of the cutoff and model factors that vary with 1960 poverty rates using a linear or quadratic term in 1960 poverty with slopes allowed to differ on both sides of the OEO cutoff for Head Start grant-writing assistance.

*$p < .1$; **$p < .05$, using Percentile-T significance.

error, and for robustness the *p*-value from a bootstrap percentile-T procedure.

For example, in the first panel, second column we use a bandwidth of 9 to nonparametrically estimate the discontinuity in NELS Head Start participation using the base year (first row) and first follow-up weighted samples (second row). There are 43 counties that contain NELS respondents and receive nonzero weight with this bandwidth. The point estimate for the first follow up sample, which may be more relevant since our schooling outcomes come from later NELS follow-ups, implies an increase in probability of participation of .238.

Is this estimated impact in Head Start participation large? To help answer this we show in the first column the left-hand-side predictions of the nonparametric regression at the threshold using the midpoint bandwidth. This represents the no-program counterfactual against which the program impact can be compared. Henceforth, we refer to this as the "control mean." As the nonparametric model is reestimated using larger bandwidths, the point estimate goes from .24 to .15, the standard error gets much smaller as more data are incorporated, and as a result the bootstrapped percentile-T *p*-value declines from .19 to .09.

The final columns of Table II show the point estimates are somewhat larger with a more parametric approach, although visual inspection of our estimates (Figure I) suggests that the nonparametric estimator fits the data better than more parametric models near the OEO cutoff. The solid line shows the nonparametric estimates for $m(P)$ and α using a bandwidth of 18, the dashed lines show parametric estimates from the flexible quadratic model, and the triangles show raw cell means (and 95 percent confidence intervals) from grouping the data into five cells on each side of the cutoff for counties with 1960 poverty rates from 40 to 80 percent. The large gap in cell means just to the left and right of the OEO cutoff is consistent with the idea that our estimates are not driven by decisions about the functional form for $m(P)$. With a county poverty rate of about 60 percent at the cutoff and participation rates of around 45 percent at the right-hand limit, the data imply that around three-quarters of poor children participate in Head Start in the treatment counties.

The estimated discontinuities in Head Start funding per four-year-old are similarly large as a share of the control mean, as shown in the bottom panel of Table II and in Figure II. The point

Panel A: Discontinuity in Head Start participation, NELS base year sample
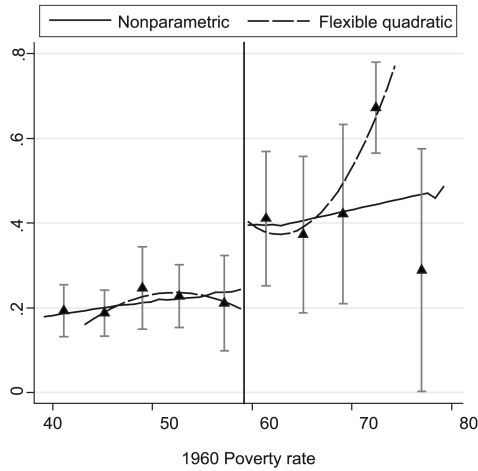
Panel B: Discontinuity in Head Start participation, NELS first follow up sample
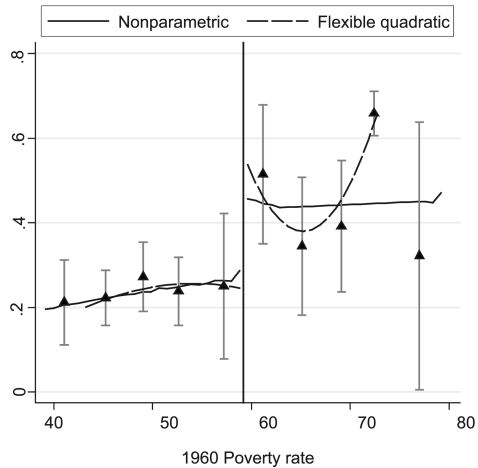
FIGURE I

Estimated Discontinuity in Head Start Participation in the NELS. (A) Discontinuity in Head Start participation, NELS base year sample and (B) discontinuity in Head Start participation, NELS first follow-up sample

Notes: Each panel shows the nonparametric estimate (solid line) for the function relating 1960 county poverty rate to the dependent variable [$m(P_c)$ from (3) in the text] as well as the implied discontinuity (α) using a bandwidth of 18, a parametric estimate (dashed line) that uses a quadratic to model $m(P_c)$, and raw cell means (triangles) and their 95 percent confidence intervals (bars) from grouping the data into five categories on each side of the cutoff for counties with 1960 poverty rates from 40 to 80 percent. Panel A Estimated nonparametric discontinuity = .135 T-stat = 1.36, bandwidth = 18. Panel B Estimated nonparametric discontinuity = .172 T-stat = 1.63, bandwidth = 18.

Panel A: 1968 Head Start funding per 4 year old
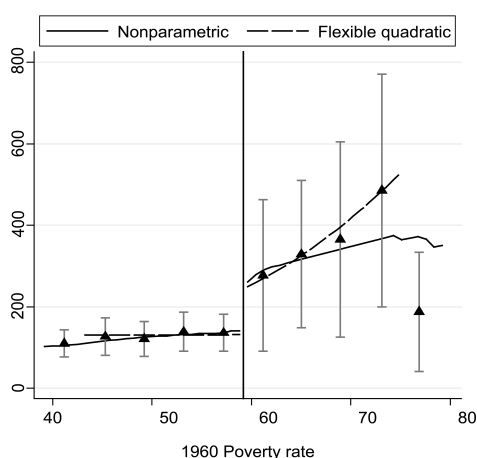
Panel B: 1972 Head Start funding per 4 year old
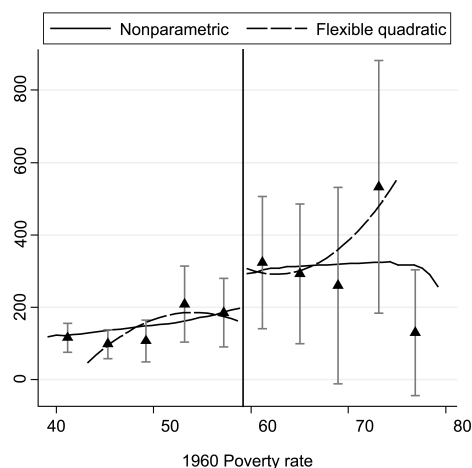
FIGURE II

Estimated Discontinuity in Head Start Funding per Four-Year-Old, National Archives. (A) 1968 Head Start funding per four-year-old and (B) 1972 Head Start funding per four-year-old

Notes: Each panel shows the nonparametric estimate (solid line) for the function relating 1960 county poverty rate to the dependent variable [$m(P_c)$ from (3) in the text] as well as the implied discontinuity (α) using a bandwidth of 18, a parametric estimate (dashed line) that uses a quadratic to model $m(P_c)$, and raw cell means (triangles) and their 95 percent confidence intervals (bars) from grouping the data into five categories on each side of the cutoff for counties with 1960 poverty rates from 40 to 80 percent. Panel A Estimated nonparametric discontinuity = 114.71 T-stat = 1.19, bandwidth = 18. Panel B Estimated nonparametric discontinuity = 89.96 T-stat = 0.83, bandwidth = 18.

estimates are generally not very sensitive to our choice of bandwidth.

How long did this discontinuity in Head Start funding at the OEO cutoff last? We do not have reliable county-level federal spending data that spans an extended time frame, and so must infer what we can from indirect sources. The fact that Head Start participation rates jump in the NELS at the cutoff indicate that the funding discontinuity across counties persisted at least through the late 1970s. By the late 1990s the Head Start funding disparity across counties at the OEO cutoff seems to have completely dissipated, as evidenced by the fact that we do not observe a discontinuity in Head Start participation rates among children in the Early Childhood Longitudinal Study, Kindergarten Class of 1998–1999 (ECLS-K).[12]

Finally, as noted above, a potential threat to identification with our research design comes from the possibility of discontinuities at the OEO cutoff in other forms of social spending. This seems unlikely since the decision to focus Head Start grant-writing assistance in the 300 poorest counties seems to have been made arbitrarily within the Head Start office rather than as some part of larger OEO-wide policy. And in fact Figure III and the last row of Table II show that the discontinuity in other forms of federal social spending at the OEO cutoff is never statistically significant and is very small expressed as a share of the control mean (ranging from -0.6 to $+4$ percent, vs. $+50$ to 100 percent for Head Start) or in absolute dollars ($-\$2$ to $+\$20$ per capita, vs. $\$100$ or more per four-year-old for Head Start).[13]

In other analyses (not shown) we also look specifically at federal spending on child and maternal health programs or on primary and secondary school programs, and find results that are statistically insignificant and modest (10–20 percent of the con-

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12. While the Community Partnership Act of 1974 required Head Start funding to become more equalized across areas (at least across states), limited growth in program funding during the 1970s [Haskins 2004] together with a "hold harmless" clause that prevented states from receiving funding below 1975 levels meant that funding equalization across areas began in earnest only in the late 1970s [Jones 1979; GAO 1981]. In addition, while funding contracts are fixed term, incumbents have traditionally been favored in the competition for funding.

13. This category includes all appropriations by OEO (other than Head Start), the Departments of Health, Education and Welfare, Housing and Urban Development, and Labor, and some selected programs run by the Department of Agriculture such as low-income housing programs and school lunches.
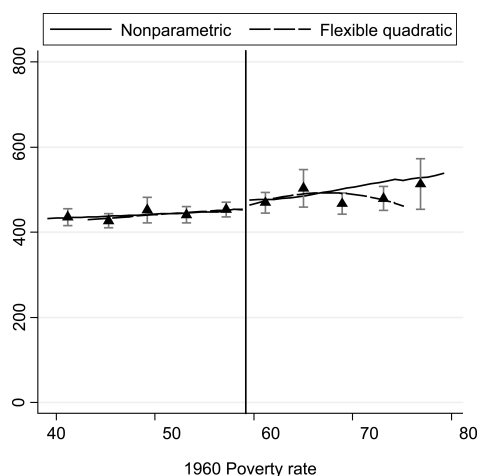
FIGURE III

Discontinuity in Other Federal Per-Capita Social Spending, 1972 National Archives Data

Note: Figure shows the nonparametric estimate (solid line) for the function relating 1960 county poverty rate to the dependent variable [$m(P_c)$ from (3) in the text] as well as the implied discontinuity (α) using a bandwidth of 18, a parametric estimate (dashed line) that uses a quadratic to model $m(P_c)$, and raw cell means (triangles) and their 95 percent confidence intervals (bars) from grouping the data into five categories on each side of the cutoff for counties with 1960 poverty rates from 40 to 80 percent. Estimated nonparametric discontinuity = 19.59 T-stat = 0.95, bandwidth = 18.

trol mean) in magnitude. Although we cannot definitively rule out the existence of any discontinuity in health or school spending, we can at least rule out discontinuities for these other programs of such a large scale as for Head Start funding. In the event that there was a discontinuity in other health or schooling programs that we cannot pick up with our data, then our main estimates would be capturing the effects of a package of War on Poverty interventions, of which Head Start was the dominant element, rather than a pure Head Start effect.

VII. HEAD START AND CHILD MORTALITY

To estimate the impact of Head Start on health, we draw on county-level data from the Vital Statistics Compressed Mortality Files (CMF). We find evidence for a negative discontinuity in causes of death targeted by Head Start's health services among

children, but not among other causes of death or cohorts that should not have been affected.

We term the causes of death that could be affected by the Head Start health services "Head Start susceptible causes" or "relevant causes" (see Section III and Appendix I for details). We focus on mortality of children in the age group five to nine, which should capture the ongoing health consequences of Head Start services provided to children ages three or four.[14] We focus on the 1973–1983 period because by 1973 all children five to nine would have been of Head Start age after the program was launched, while the NELS results above suggest that this age group would have been exposed to the discontinuity in funding through at least 1983. As seen by examining the control means shown in the first column of Table III, these Head Start susceptible causes account for just under 10 percent of all deaths to children five to nine during our study period in these poor counties. The most important causes of death *not* included in our Head Start-affected category are injuries (around 55 percent of deaths to children five to nine in 1973–1983) and neoplasms (around 15 percent of deaths).

Table III and Figure IV present evidence that the positive "jump" in Head Start at the OEO cutoff documented above appears to be mirrored by a jump down in 1973–1983 mortality rates for Head Start susceptible causes to children five to nine. The nonparametric estimates imply effects equal to one-third to one-half of the control mean with bootstrapped *p*-values from .03 to .08, large enough to eliminate the "excess risk" of death from these causes (defined as the difference in mortality rates from the national average). The parametric estimates are larger with bootstrapped *p*-values of .02, but as seen in Panel A of Figure IV, do not appear to fit the data as well as the nonparametric estimator near the OEO cutoff. Most of the averted deaths are distributed about equally across three specific causes of death, which together account for most of the deaths in our Head Start category and were much more common during our study period than

14. The alternative with the CMF would be to examine deaths to children ages one to four, most of whom would be too young to have enrolled in Head Start. Our focus on mortality of children five to nine also seems appropriate given that most of the causes of death susceptible to Head Start intervention are from chronic rather than acute conditions.

TABLE III

REGRESSION DISCONTINUITY ESTIMATES OF THE EFFECT OF HEAD START ASSISTANCE ON MORTALITY

Variable	Control mean	Nonparametric estimator			Parametric	
					Flexible linear	Flexible quadratic
Bandwidth or poverty range		9	18	36	8	16
Number of observations (counties) with nonzero weight		527	961	2,177	484	863
Main results						
Ages 5–9, Head Start-related causes, 1973–1983	3.238	−1.895**	−1.198*	−1.114**	−2.201**	−2.558**
		(0.980)	(0.796)	(0.544)	(1.004)	(1.261)
		[0.036]	[0.081]	[0.027]	[0.022]	[0.021]
Specification checks						
Ages 5–9, injuries, 1973–1983	22.303	0.195	2.426	0.679	−0.164	0.775
		(3.472)	(2.476)	(1.785)	(3.380)	(3.401)
		[0.924]	[0.345]	[0.755]	[0.998]	[0.835]
Ages 5–9, all causes, 1973–1983	40.232	−3.416	0.053	−1.537	−3.896	−2.927
		(4.311)	(3.098)	(2.253)	(4.268)	(4.295)
		[0.415]	[0.982]	[0.558]	[0.317]	[0.505]
Ages 25+, Head Start-related causes, 1973–1983	131.825	2.204	6.016	5.872	2.091	2.574
		(5.719)	(4.349)	(3.338)	(5.581)	(6.415)
		[0.700]	[0.147]	[0.114]	[0.749]	[0.689]

TABLE III
(CONTINUED)

Variable	Control mean	Nonparametric estimator			Parametric	
					Flexible linear	Flexible quadratic
Ages 25+, injuries, 1973–1983	121.191	5.697	7.276*	4.398	2.65	4.276
		(6.527)	(4.531)	(3.249)	(6.206)	(6.059)
		[0.256]	[0.060]	[0.261]	[0.596]	[0.426]
Ages 5–9, Head Start causes, 1959–1964	9.752	−3.327	−1.076	−0.066	−3.754*	−4.869**
		(5.066)	(3.341)	(2.075)	(5.136)	(5.016)
		[0.117]	[0.536]	[0.641]	[0.075]	[0.039]
Whites age 5–9, Head Start-related causes, 1973–1983	2.63	−1.105	−0.865	−0.749	−1.334	−1.746
		(1.056)	(0.862)	(0.618)	(1.061)	(1.332)
		[0.263]	[0.269]	[0.198]	[0.212]	[0.145]
Blacks age 5–9, Head Start-related causes, 1973–1983	4.688	−2.275	−2.719**	−1.589	−1.699	−1.93
		(3.758)	(2.163)	(1.706)	(4.094)	(3.718)
		[0.173]	[0.048]	[0.322]	[0.411]	[0.276]

Outcome of interest is one-year mortality rates per 100,000 Head Start-related causes include deaths due to tuberculosis, other infections, diabetes, nutritional causes, anemias, meningitis, and respiratory causes. Each cell presents a separate estimate of the discontinuity in the outcome measure listed in the left-hand column at OEO's threshold 1960 poverty level for providing counties with grant-writing assistance for Head Start funding. Within each cell the first number represents our point estimate of α from (3), with analytic standard errors in parentheses and percentile-*T* bootstrapped *p*-values in square brackets. Nonparametric estimates are based on the locally weighted kernel regression method discussed in Porter [2003], calculated using a triangle kernel. Parametric models give equal weight to observations within the range of the cutoff and model factors that vary with 1960 poverty rates using a linear or quadratic term in 1960 poverty with slopes allowed to differ on both sides of the OEO cutoff for Head Start grant-writing assistance.

*$p < .1$; **$p < .05$, using Percentile-*T* significance.

FIGURE IV

Estimated Discontinuities at OEO Cutoff in Mortality Rates per 100,000 for Children and Adults, from Causes Affected by Head Start and from Injuries

Note: Each panel shows the nonparametric estimate (solid line) for the function relating 1960 county poverty rate to the dependent variable [$m(P_c)$ from (3) in the text] as well as the implied discontinuity (α) using a bandwidth of 18, a parametric estimate (dashed line) that uses a quadratic to model $m(P_c)$, and raw cell means (triangles) and their 95 percent confidence intervals (bars) from grouping the data into five categories on each side of the cutoff for counties with 1960 poverty rates from 40 to 80 percent. Panel A, Estimated nonparametric discontinuity = −1.198 T-stat = 1.42, bandwidth = 18. Panel B, Estimated nonparametric discontinuity = 2.246 T-stat = 0.86, bandwidth = 18. Panel C, Estimated nonparametric discontinuity = 6.016 T-stat = 1.31, bandwidth = 18. Panel D, Estimated nonparametric discontinuity = −1.076 T-stat = 0.52, bandwidth = 18.

today—anemias, meningitis, and respiratory problems.[15] The estimate for mortality from *all* causes for children five to nine is usually negative and just slightly larger in absolute value than deaths from Head Start susceptible causes, but much less precisely estimated.

Is an effect of this magnitude plausible? One way to think about the answer is to note that the results shown in Table II imply a treatment–control difference in Head Start enrollment rates of around 12,000–30,000 per 100,000 four-year-olds. This enrollment difference, together with whatever discontinuity there is for spending per Head Start enrollee, leads to one or two fewer deaths per 100,000 four-year-olds. This effect strikes us as plausible given that a previous study of Head Start in Mississippi found that around two-thirds of comparison group children had some health problem (excluding self-limiting diseases like colds), a rate that appeared to be cut about in half by Head Start participation [Fosburg et al. 1984, pp. 90–91].

Some support for the idea that these mortality differences are due to Head Start rather than other factors comes from the fact that we do not observe a similar discontinuity in mortality rates for children five to nine from causes that should not be affected by Head Start—namely, injuries (second panel, Table III). Although we might expect Head Start to affect *contemporaneous* injury rates while children are in the program [Currie and Hotz 2004], we do not expect the program to affect accident rates for children ages five to nine.[16] The estimated discontinuities for injuries range from +8.8 percent to −8.7 percent of the control mean and are never close to statistically significant. Similarly rows 4 and 5 show that there is no discontinuity in mortality from either relevant causes or injuries for people ages 25 and older,

15. From 1968 to 1988 the death rate to children five to nine in the United States declined by 45 percent, including mortality rate reductions of 56 percent for meningitis, 61 percent for anemias, and 76 percent from respiratory problems. Health problems were especially prevalent in the 1960s and 1970s among minorities in poor, rural areas [Lesser and Hunt 1968]. The estimated impacts are negative and usually statistically significant for anemias and meningitis, but the standard errors for anemia get large at larger bandwidths. The estimated impacts for respiratory problems are also negative but are not very precisely estimated. Including all infectious diseases (not just those for which we can identify a clear mapping between Head Start services and mortality) yields results qualitatively similar to those shown in the table.

16. In principle, Head Start could have changed parent behaviors to affect injury rates. However, in practice, as North [1979, 1968, p. 245] notes, "Head Start's obvious potential for health education was, unfortunately, never attained." Further, health education for parents often emphasizes nutrition and hygiene [O'Brien, Connell, and Griffin 2004].

who would have been too old in 1973–1983 to have received health services from Head Start.

The sixth row of Table III shows that the estimated effect on Head Start causes for children age five to nine during the pre-Head Start years for which we could obtain data (1959–1964). In contrast to our primary (post-Head Start) findings, where the parametric and nonparametric estimates across bandwidth specifications are always large relative to the control mean and significant at least at the 10 percent level, for the pre-Head Start period there is no consistent pattern across specifications of a measured impact. The parametric models estimate relatively large and sometimes significant "impacts," but a visual inspection of Panel D in Figure IV indicates that this is a spurious finding; the right limit fitted by the quadratic polynomial is implausibly low while the nonparametric estimate seems to more accurately reflect the pattern in the data. The nonparametric estimate, in addition to being not significantly different from zero, is small relative to the control mean. Another way to see the program's impact is to note that before Head Start (1959–1964, in Panel D of Figure IV) the mortality rates in the full set of "treatment" counties with poverty rates above the OEO cutoff are almost always equal to or greater than the means for the "control" counties. But following Head Start (1973–1983, Panel A) the mortality rates in the set of treatment counties are typically below the control counties.

The final two rows of Table III show that the discontinuities in mortality for children five to nine from causes plausibly affected by Head Start are found among both Blacks and Whites. Because Blacks are proportionately overrepresented in Head Start, we may expect program impacts to be somewhat larger for this group. The magnitudes of the point estimates (as a share of the control mean) are somewhat larger for Blacks than Whites with the nonparametric estimates, although the reverse is true with our parametric results. In any case, strong conclusions about any race differences in Head Start's impact on child health are precluded by the fairly large standard errors for our estimates for Blacks.[17]

17. A final potential concern comes from the desegregation of hospitals in the South (particularly Mississippi) following the 1964 Civil Rights Act, which led to large declines in the mortality of Black infants [Almond, Chay, and Greenstone 2003]. However, hospital integration would only be a problem for our identification strategy if integration varied discontinuously at exactly the OEO cutoff used

VIII. HEAD START AND EDUCATIONAL ATTAINMENT

In this section we show that there is suggestive evidence for a discontinuity in educational attainment at the OEO cutoff in the Census among cohorts born late enough to have been affected by Head Start, but not among earlier cohorts. The main drawback with the Census is the possibility of selective migration as people move across counties from childhood to adulthood (when we observe their place of residence). We show the discontinuity in schooling for the Census is mirrored among NELS respondents, for whom we observe addresses at age thirteen.

VIII.A. Census Results

Estimates for educational attainment by cohort and decennial census are presented in Table IV. We expect schooling results to be most pronounced starting in 1990, when the cohorts who would have been able to enroll in Head Start first begin to reach adulthood, with more modest impacts on birth cohorts that would include parents and older siblings of enrollees. We expect no effects for any age group in the one "pretreatment" decennial census (1960) for which we have age-by-schooling data, and zero effects in the other censuses for cohorts too old to have been affected by Head Start. For the most part the results conform to this pattern.

The top panel of Table IV summarizes the results for the birth cohorts that reached Head Start age after the program was in existence.[18] The first row and Figure V show that among those eighteen to twenty-four years of age in 1990 (born 1966–1972), we see a positive discontinuity in completion of a high school or equivalent degree of 3–4 percentage points (around 5 percent of

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to allocate Head Start grant-writing assistance. Some evidence against this counterexplanation for our findings comes from the fact that the reduction in mortality among children five to nine is not limited to Blacks. Moreover, while Almond, Chay, and Greenstone. show large declines in deaths to infants or postneonates from hospital desegregation, we find that the discontinuities at the OEO cutoff in infant mortality from Head Start-affected causes are relatively small (5–20 percent of the control mean), *positive,* and usually not significant. See Ludwig and Miller [2005].

18. Given the large number of census years, birth cohorts, and schooling outcomes for which we show results in Table IV, for parsimony we present results for only two of our five main empirical specifications for each cohort-year-outcome. The other specifications generally yield similar results and are available upon request from the authors.

TABLE IV
ESTIMATES OF THE EFFECT OF HEAD START ASSISTANCE ON EDUCATIONAL OUTCOMES, CENSUS DATA

Variable	Fraction high school or more			Fraction some college or more		
	Control mean	Nonparametric	Parametric flexible linear	Control mean	Nonparametric	Parametric flexible linear
Bandwidth/poverty range		7	4		7	4
Number of observations (counties) with nonzero weight		429	244		429	244
Census year and age group						
Results for directly treated cohorts						
1990, Ages 18–24	0.647	0.030** (0.016) [0.032]	0.043** (0.019) [0.023]	0.228	0.037** (0.020) [0.031]	0.051** (0.024) [0.021]
2000, Ages 18–24	0.620	0 (0.016) [0.974]	0.024 (0.019) [0.191]	0.266	0.028** (0.019) [0.017]	0.042** (0.024) [0.022]
2000, Ages 25–34	0.773	0.006 (0.014) [0.666]	0.015 (0.016) [0.354]	0.362	0.04** (0.017) [0.009]	0.043** (0.021) [0.034]
Results for sibling and parent cohorts						
1970, Ages 18–24	0.472	0.044** (0.020) [0.003]	0.047** (0.023) [0.025]	0.022	0.002 (0.004) [0.577]	0 (0.004) [0.958]
1970, Ages 25+	0.285	0 (0.013) [0.990]	−0.007 (0.014) [0.582]	0.047	0 (0.004) [0.921]	0.002 (0.005) [0.648]

TABLE IV
(CONTINUED)

Variable	Fraction high school or more			Fraction some college or more		
	Control mean	Nonparametric	Parametric flexible linear	Control mean	Nonparametric	Parametric flexible linear
1980, Ages 18–24	0.620	0.017 (0.016) [0.231]	0.007 (0.020) [0.716]	0.149	0.03** (0.017) [0.037]	0.025 (0.021) [0.177]
1980, Ages 25+	0.440	0.008 (0.013) [0.449]	0.01 (0.014) [0.499]	0.159	0.009 (0.010) [0.312]	0.015 (0.011) [0.215]
1990, Ages 25–34	0.700	0.01 (0.013) [0.417]	0.005 (0.015) [0.775]	0.205	0.012 (0.012) [0.298]	0.012 (0.014) [0.381]
1990, Ages 35–54	0.621	0.009 (0.014) [0.454]	−0.004 (0.016) [0.818]	0.182	0.011 (0.010) [0.199]	0.017 (0.011) [0.126]
2000, Ages 35–44	0.756	0.008 (0.014) [0.468]	0.008 (0.014) [0.584]	0.343	0.026* (0.017) [0.087]	0.024 (0.019) [0.228]
2000, Ages 45–64	0.699	0.007 (0.015) [0.629]	0.006 (0.017) [0.758]	0.333	0.015** (0.017) [0.014]	0.026** (0.019) [0.019]

TABLE IV
(CONTINUED)

Variable	Fraction high school or more			Fraction some college or more		
	Control mean	Nonparametric	Parametric flexible linear	Control mean	Nonparametric	Parametric flexible linear
Results for untreated cohorts						
1960, Ages 25+	0.205	0.004 (0.013) [0.447]	0.007 (0.010) [0.606]		N/A	N/A
1990, Ages 55+	0.351	−0.014 (0.015) [0.336]	−0.011 (0.018) [0.575]	0.094	−0.003 (0.006) [0.637]	0.002 (0.007) [0.768]
2000, Ages 65+	0.463	−0.017 (0.019) [0.359]	−0.016 (0.023) [0.490]	0.210	−0.005 (0.013) [0.619]	−0.001 (0.016) [0.945]

Each cell presents a separate estimate of the discontinuity in the outcome measure listed in the column heading for the age group and census year listed in the left-hand column at OEO's threshold 1960 poverty level for providing counties with grant-writing assistance for Head Start funding. Within each cell the first number represents our point estimate of α from (3), with analytic standard errors in parentheses and percentile-T bootstrapped p-values in square brackets. Nonparametric estimates are based on the locally weighted kernel regression method discussed in Porter [2003], calculated using a triangle kernel. Parametric models give equal weight to observations within the range of the cutoff and model factors that vary with 1960 poverty rates using a linear or quadratic term in 1960 poverty with slopes allowed to differ on both sides of the OEO cutoff for Head Start grant-writing assistance.

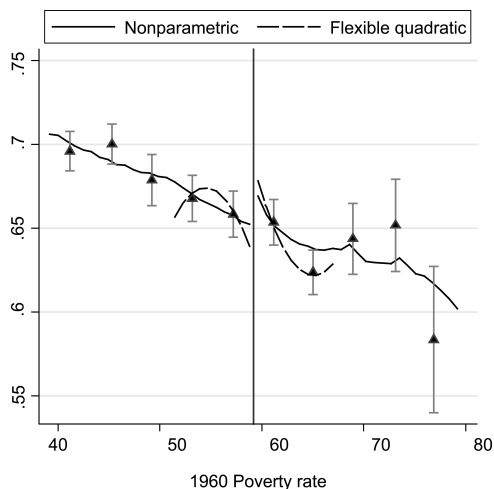*$p < .1$; **$p < .05$, using Percentile-T significance.

FIGURE V

Discontinuity in High School Completion, ages 18–24, 1990 Census

Note: Figure shows the nonparametric estimate (solid line) for the function relating 1960 county poverty rate to the dependent variable [$m(P_c)$ from (3) in the text] as well as the implied discontinuity (α) using a bandwidth of 18, a parametric estimate (dashed line) that uses a quadratic to model $m(P_c)$, and raw cell means (triangles) and their 95 percent confidence intervals (bars) from grouping the data into five categories on each side of the cutoff for counties with 1960 poverty rates from 40 to 80 percent. Estimated nonparametric discontinuity = .030 T-stat = 1.76, bandwidth =7.

the control mean), with bootstrapped p-values below .05.[19] We also observe statistically significant effects on fraction with some postsecondary school attendance of about the same magnitude. One explanation for the attenuated impact on high school completion by 2000 for this cohort is if Head Start affects the odds of receiving a regular diploma rather than a GED, as does Perry Preschool [Schweinhart et al. 2005], which would be important because GED-holders do not earn as much as graduates [Carniero and Heckman 2003]. While the Census does not distinguish between diplomas and GEDs, this explanation is consistent with the large increase in the control mean for these cohorts from 1990 to

19. While the smoothed trends and cell means in Figure V convey the visual impression that our discontinuity estimate is driven by the functional form of $m(P)$—specifically, the "flattening out" of the general downward trend in high school completion as 1960 county poverty increases—this impression is incorrect: in Ludwig and Miller [2005] we show that a less aggregated picture of the data (a scatter plot calculated with a bandwidth of 0.25) reveals a positive jump in high school completion rates at the OEO cutoff.

2000 (from 65 to 77 percent). For the cohorts who were eighteen to twenty-four in 2000 we observe little impact on high school or equivalent, but an effect on college attendance of about the same magnitude as for those who were eighteen to twenty-four in 1990.

Recall that these estimates reflect the reduced-form ITT-style effects of providing Head Start grant-writing assistance. Determining the "effects of treatment on the treated" (TOT) is complicated by the fact that our estimates for the discontinuity in Head Start funding are fairly imprecise. This means that we cannot determine whether there is a discontinuity in Head Start spending per participant above and beyond the discontinuity in participation rates that we find in the NELS, and as a result cannot recover a TOT impact by simply scaling the reduced-form ITT estimate by the estimated "first-stage" discontinuity in Head Start enrollment rates.

The second panel of Table IV summarizes the results across decennial censuses for those birth cohorts that could have included parents and older siblings of Head Start children. The first two rows show results for those who are ages eighteen to twenty-four and twenty-five or over during 1970, the first posttreatment census. Separate calculations from the 1970 census microdata suggest that around one-third of Southern poor people in each of these two age groups would have been living in households with children who reached Head Start age after the program began; the figure could be somewhat higher in the poorest counties. (Recall that there was about as many parents involved with Head Start as children in the early years). We find evidence for a positive discontinuity in high school completion for those eighteen to twenty-four in 1970 but not for those twenty-five and older. The available data make it difficult to follow these specific cohorts exactly across censuses. But most of the relevant cohorts are captured by the group of forty-five to sixty-four year olds in 2000, for whom we see that the discontinuity in high school completion has dissipated over time (as with the top panel of Table IV) but now we find a jump of around 2 or 3 percentage points in college attendance.

The other key age group in the second panel of Table IV consists of those eighteen to twenty-four in 1980, some of whom would have been older siblings of Head Start participants. For

this group we also see a positive discontinuity in college attendance equal to around 2 or 3 percentage points.[20]

The bottom panel of Table IV presents results for cohorts that should not have been affected by Head Start—those twenty-five or older in 1960, fifty-five and older in 1990, and sixty-five and older in 2000. We do not observe any statistically significant discontinuities in either of our schooling measures for any of these groups.

The pattern of estimated impacts across subgroups is also worth mentioning. We have obtained data disaggregated by race and gender as well as age for the 1990 census, from a special tabulation conducted for us by the Census Bureau. We find point estimates that are at least as large for Blacks as for the full sample, though the standard errors are large. Nevertheless, the results are intriguing because they differ from the sibling-difference estimates from CT and GTC, which show long-term schooling impacts from Head Start for Whites but not Blacks. Similarly, in a recent reanalysis of data from Perry Preschool and Abecedarian, Anderson [2005] finds schooling impacts concentrated among females. In our data we find impacts that are sometimes somewhat larger for females than for males, but typically large for males as well.

One concern with these results comes from the possibility of migration across counties between when people were of Head Start age (three or four) and when they are observed as adults in the decennial census. If exposure to Head Start made individuals more likely to move out of the state, then we might expect to find a discontinuity in the fraction of individuals born in the same state. The estimated discontinuities in the share of residents born in the state are usually not statistically significant, but the point estimates tend to be meaningfully large in absolute value for younger age groups, as well as for blacks (for Blacks ages eighteen to twenty-four, the point estimates are typically significant) [see Ludwig and Miller 2005].

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20. Variation across census years in the magnitude of the college attendance measure could in principle be due in part to changes in the way the Census asked about attendance of some college. In addition, "the question wording was changed in 2000 to more directly ask about completion of highest degree or level of school instead of including this in an instruction as was done in 1990" (www.census.gov/population/cen2000/90vs00.html, accessed 2/23/06).
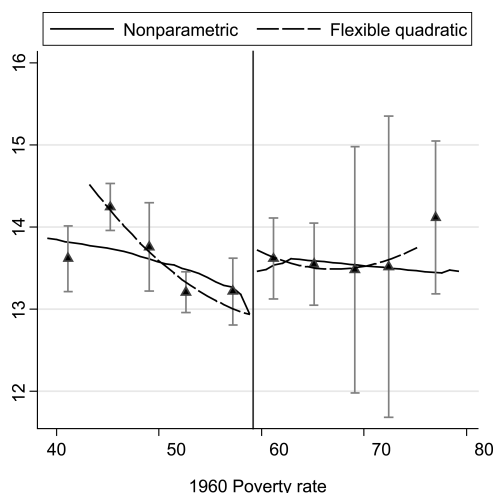
FIGURE VI

Estimated Discontinuity in Years of Schooling, NELS 2000 Survey

Note: Figure shows the nonparametric estimate (solid line) for the function relating 1960 county poverty rate to the dependent variable [$m(P_c)$ from (3) in the text] as well as the implied discontinuity (α) using a bandwidth of 18, a parametric estimate (dashed line) that uses a quadratic to model $m(P_c)$, and raw cell means (triangles) and their 95 percent confidence intervals (bars) from grouping the data into five categories on each side of the cutoff for counties with 1960 poverty rates from 40 to 80 percent. Estimated nonparametric discontinuity = .584 *T*-stat = 1.55, bandwidth = 18.

VIII.B. NELS Results

One way to address concerns about selective migration is to replicate our schooling estimates using the NELS, which allows us to identify county of residence for respondents in eighth grade when most students were around thirteen years old, about eight to ten years after Head Start age.

Figure VI and Table V show that the results from the NELS 2000 wave (when most respondents were around twenty-five years old) are qualitatively similar to those from the census data, although with less data than in the census our statistical power to detect impacts is more limited. The first row of Table V shows estimated discontinuities for educational attainment equal to around one-half year of schooling, compared with a control mean of 12.9 years of school completed. The next two rows provide suggestive evidence to support our conjecture from above —part of Head Start's effect on schooling may come in part from shifting participants from GEDs to actual high school diplomas, although

TABLE V
ESTIMATES OF THE EFFECT OF HEAD START ASSISTANCE ON EDUCATIONAL OUTCOMES, NELS OUTCOMES

Variable	Control mean	Nonparametric			Parametric	
					Flexible linear	Flexible quadratic
Bandwidth or poverty range		9	18	36	8	16
Number of observations (counties) with nonzero weight		43	96	288	43	82
Years of schooling, 2000	12.865	0.581	0.584*	0.201	0.581	0.821*
		(0.435)	(0.358)	(0.239)	(0.449)	(0.563)
		[0.206]	[0.095]	[0.457]	[0.180]	[0.082]
Completed high school, 2000	0.615	0.171	0.117	0.031	0.171	0.205
		(0.129)	(0.080)	(0.055)	(0.137)	(0.127)
		[0.260]	[0.208]	[0.644]	[0.223]	[0.213]
High school or equivalent, 2000	0.785	0.134	0.061	0.03	0.134	0.183
		(0.109)	(0.061)	(0.037)	(0.120)	(0.104)
		[0.295]	[0.386]	[0.546]	[0.264]	[0.146]
Some postsecondary school, 2000	0.531	0.084	0.151	0.092	0.084	0.165
		(0.135)	(0.097)	(0.064)	(0.138)	(0.151)
		[0.540]	[0.115]	[0.229]	[0.520]	[0.223]
Enrolled in school, 2000	0.164	0.104	0.066	0.105***	0.104	0.183**
		(0.095)	(0.076)	(0.045)	(0.100)	(0.131)
		[0.244]	[0.268]	[0.043]	[0.232]	[0.047]
Test score, reading, 8th grade	45.139	0.578	−0.272	−1.33	0.578	−0.372
		(2.119)	(1.771)	(1.101)	(2.305)	(3.052)
		[0.772]	[0.869]	[0.396]	[0.769]	[0.933]
Test score, math, 8th grade	44.397	−2.242	−1.54	−2.054	−2.242	−2.817
		(2.256)	(1.803)	(1.216)	(2.416)	(2.962)
		[0.346]	[0.327]	[0.240]	[0.307]	[0.266]

Each cell presents a separate estimate of the discontinuity in the outcome measure listed in the left column at OEO's threshold 1960 poverty level for providing counties with grant-writing assistance for Head Start funding. Within each cell the first number represents our point estimate of α from (3), with analytic standard errors in parentheses and percentile-T bootstrapped p-values in square brackets. Nonparametric estimates are based on the locally weighted kernel regression method discussed in Porter [2003], calculated using a triangle kernel. Parametric models give equal weight to observations within the range of the cutoff and model factors that vary with 1960 poverty rates using a linear or quadratic term in 1960 poverty with slopes allowed to differ on both sides of the OEO cutoff for Head Start grant-writing assistance.

* $p < .1$; ** $p < .05$, using Percentile-T significance.

our standard errors here are fairly large. Note that the NELS results are larger than those from census data, perhaps due to greater attenuation in the census data than in the NELS from migration across counties. It is also reassuring that the estimated discontinuities in schooling outcomes seem to be largest among those NELS respondents who are most likely to participate in the Head Start program.[21]

Rows 4 and 5 of Table V show point estimates for postsecondary school attendance and enrollment in school as of the 2000 NELS interview (around age twenty-five for most respondents) that are positive and large as a share of the control mean, though usually imprecisely estimated.[22] The fact that some NELS respondents are still enrolled in school as of the 2000 interview suggests that NELS is not a very good source of data for estimating Head Start impacts on adult labor market outcomes. Given that the NELS has small samples near the OEO cutoff, disaggregating further by race with NELS yields very noisy estimates.

Finally, our analysis yields no evidence of statistically significant discontinuities in eighth grade reading or math scores among NELS respondents (Table V), or in other noncognitive outcomes like absences, grades, or time spent on homework. In this sense, the overall pattern of estimates presented here are similar to previous studies of Head Start and Perry Preschool, which reveal some attenuation of test-score impacts after participation yet long-term effects on schooling attainment [see CT, GTC, and Schweinhart et al. 2005]. But recent research by Heckman, Stixrud, and Urzua [2006] emphasizes the point that even modest changes in cognitive or noncognitive skills could be enough to generate impacts on schooling persistence of the magnitudes that we estimate in the census or NELS. Because the

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21. For each NELS respondent we estimate the propensity to participate in Head Start as a function of family structure (father or other adult male in the home, and number of siblings), race/ethnicity, mother's and father's educational attainment, urbanicity, region, percent students in the respondent's eighth grade school who were minority, and percent free lunch. We then replicate our analyses separately for those with propensity score values above .15 (roughly the median score for NELS respondents in the counties with 1960 poverty rates of 40 percent or higher), and then again with those with propensity scores below .15. The results for our schooling outcomes are usually larger for those with high propensities to participate in Head Start, although the discontinuities tend to be very imprecisely estimated given that we are now working off of a subset of our already small NELS sample.

22. Among NELS respondents in the 600 poorest counties, about half of postsecondary school attendees do not complete a degree or certificate, while 15–20 percent earn a certificate or license, around 10 percent earn an associate's degree and only around 20 percent complete a bachelor's degree or more.

confidence intervals around our estimated discontinuities in NELS test scores and behavioral measures do not let us rule out modest effects, at the end of the day we cannot say much about the degree to which our estimated Head Start effects on educational attainment are driven by changes in cognitive versus noncognitive skills.

IX. EXTENTIONS AND SPECIFICATION CHECKS

In this section we address potential concerns with our findings from the possibility of selective migration across counties between early childhood and when we first observe people's county of residence, and whether our estimates are artifacts of functional form assumptions.

IX.A. Selective Migration

If migration were random (independent of Head Start participation or outcomes), then our estimates for program impacts would be attenuated, since treatment or control county residence is a noisy measure of actual exposure to Head Start funding intensity. More worrisome is the possibility of nonrandom migration, which is not a real concern with our mortality results (given that we measure outcomes just a few years after Head Start participation) but may be more of a problem with our schooling estimates.

However, there are a few reasons to believe that our schooling estimates are not simply artifacts of selective migration across counties. One data point against the selective-migration hypothesis comes from the qualitative similarity in results between the 1990 Census, which assigns people to treatment and control counties based on county of residence during adulthood, and the NELS, which assigns people to counties based on where they live at age thirteen. Migration across poor counties between ages three and thirteen appears to be modest, at least according to the Panel Study of Income Dynamics.[23]

23. The PSID provides very small and unrepresentative samples "close" to the OEO cutoff, as evidenced by the fact that we do not find a discontinuity in Head Start participation rates around the cutoff, but at least lets us compare people in all 300 poorest counties with the next 300 poorest counties (restricting our sample to those for whom we have addresses at both age three and thirteen and information on adult educational attainment). Of the respondents in the 300 poorest counties at age three, fully 86 percent were in one of these counties at age thirteen; of those in one of these counties at age thirteen, 64 percent were in one

IX.B. *Specification Tests*

Another way to test for selective migration is to test for balance at the discontinuity in exogenous characteristics that should not be affected by the intervention, since selective migration with respect to such characteristics would lead to imbalance. This also provides a more general test of our identifying assumptions about smoothness [McCrary and Royer 2003]. We have shown above that this is true with respect to causes of death that should not be affected by Head Start. This also seems to be true with respect to county-level sociodemographic characteristics such as race, urbanicity, or population age structure in the decennial censuses from 1950 to 1990 or the NELS, in the sense that there is no bandwidth that suggests imbalance across multiple exogenous characteristics.[24]

Yet another specification test comes from examining whether there are statistically significant discontinuities in outcomes that may be affected by Head Start at other "pseudo-cutoffs," which might occur if our estimates were artifacts of functional form assumptions. Yet we do not find similar discontinuities in outcomes to those shown above at pseudo-cutoffs where there are no discontinuities in Head Start funding [see Ludwig and Miller 2005].

IX.C. *Endogenous Break-Point Tests*

We have strong *a priori* reasons to believe that the discontinuity in Head Start assistance occurred at the three hundredth poorest county, although we are also interested in whether the data offer supportive evidence for identifying the location of the threshold. To the best of our knowledge, there is no literature on break-point tests for nonparametric RD models. As such, we performed a goodness-of-fit exercise that is in the same spirit of standard parametric tests [Piehl et al. 2003; Bai and Perron 1998]. As discussed in detail in Ludwig and Miller [2005], we find evidence that the discontinuities that fit our data best for the three first-stage variables (NELS Head Start participation, and

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at age three. The results are similar for the next 300 poorest counties, and if anything slightly more low-education people are leaving the "control" than "treatment" counties.

24. Another specification check is suggested by the fact that the across-county funding discontinuity at the OEO cutoff seems to have dissipated over time, and so we do not observe a discontinuity in Head Start participation among kindergartners in 1998 in the ECLS-K. Reassuringly, we also do not observe discontinuities in children's test scores.

Head Start funding in 1968 and 1972) are all very close to the OEO cutoff.

X. Discussion

One contribution of our paper is to highlight a new source of identifying information for the long-term effects of Head Start, generated by the discontinuity in program funding across counties by virtue of the grant-writing assistance given in 1965 to just the poorest 300 counties. We also demonstrate that the discontinuity in Head Start funding at the OEO cutoff is mirrored by a large discontinuity in child mortality rates for causes of death plausibly affected by Head Start. The impacts are proportionately large among both Blacks and Whites, although imprecisely estimated for Blacks. There are no statistically significant discontinuities in either other causes of death or age groups that should not be affected by Head Start.

Our evidence for positive Head Start impacts on educational attainment is more suggestive, and limited by the fact that neither of the data sources available to us is quite ideal. We show that there are signs of a positive discontinuity in high school completion and college attendance in the decennial censuses from 1970 to 2000 concentrated among cohorts that could have been affected by Head Start, although not among older cohorts or in the 1960 census that was taken before Head Start was launched. We find similar suggestive results for schooling outcomes in the NELS, which first identifies county of residence for people at age thirteen, and other data suggest that there is a great deal of overlap between those living in the poorest counties at age three and thirteen. The qualitative similarity in findings across data sources would seem to strengthen the findings from each, even though the NELS estimates typically suffer from large standard errors. Importantly, there is suggestive evidence for schooling impacts on Blacks as well as Whites.

A potential threat to our research design comes from the possibility of discontinuities at the OEO cutoff in other forms of federal social spending, although we do not detect evidence of this problem in our data. Additional evidence against the idea that our results for mortality and schooling are driven by discontinuities in other social spending comes from the fact that we generally do not observe impacts on birth cohorts that should not be affected by Head Start.

A separate concern is the possibility that the grant-writing assistance provided by OEO could have generated a difference in the Head Start "production function" across counties. While we cannot definitively rule this out, this seems unlikely in part because the PMIs sent out in Spring 1965 were not in the field long enough to do much training. Moreover, once Head Start grants were awarded, OEO conducted a massive national training effort that would presumably have helped standardize production technologies across areas. Consistent with this hypothesis, we find no lasting discontinuities at the OEO cutoff in the 1990s in allocation of program spending across service categories such as education or health, or specific program inputs such as teacher salaries or teacher–student ratios. We also find no significant increase (and if anything some evidence of a very slight decrease) in per-enrollee spending at the cutoff.[25]

In principle, the mechanism through which Head Start affects outcomes could come from community mobilization rather than the provision of preschool services. Yet, we find no discontinuities in voter registration rates at the OEO cutoff in any of the federal election years from 1968 to 1980 [Ludwig and Miller 2005].

Unfortunately, our data are not very informative about long-term Head Start impacts on other key outcomes such as work, earnings, or criminal activity. Many NELS respondents are still enrolled in school during the latest interview for which data are available, while estimates for impacts on self-reported arrests are noisy, perhaps in part because of underreporting (see, e.g., Kling, Ludwig, and Katz [2005]). Estimates for those labor market outcomes for which we can obtain county-by-age data from the Census are also quite noisy. Learning more about the long-term effects of Head Start on these outcome domains remains an important topic for future research.

The available data are also not very informative about the degree to which our estimates for Head Start's impact on educational attainment are driven by changes in cognitive versus noncognitive skills. The confidence intervals around our estimates for Head Start effects on achievement test scores or noncognitive outcomes like absences or homework time do not allow us to rule

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25. These results come from our analysis of data generously provided to us by Janet Currie and Matt Neidell from Program Information Reports for Head Start from 1988–2000 and from program budget data for 1990–2001. For additional details about these data sets, see Currie and Neidell [2006].

out modest effects, and even modest effects on either skill domain would be enough to generate impacts on schooling of the magnitudes that we estimate [Heckman, Stixrud, and Urzua 2006].

What do our findings imply for policy? Our findings seem to argue against the claim that Head Start has been a failure from the beginning. As one recent policy brief argues, "When the emotional appeals are cleared from the table, what is left is a costly but unsuccessful experiment. . . . Head Start is not working. Accept that, and let it go" [Olsen and Olsen 1999]. In contrast, our point estimates suggest that at least for children in the poorest counties of the South in the 1960s and 1970s, Head Start seems likely to have generated benefits in excess of costs.[26]

A more difficult question is whether Head Start as it operates today also generates net benefits to society. The alternative learning environments for children in our analytic sample were surely less developmentally enriching than for the average Head Start eligible child today: our sample comes from extremely disadvantaged family backgrounds, and in addition other high-quality kindergarten or pre-K options have expanded over time. On the other hand, K–12 school quality was surely lower for our analytic sample than for poor children today, which may contribute to relatively greater fade-out of program impacts among our sample. Our study in this sense suffers from the generic problem of trying to generalize estimates of long-term effects to current policies—long-term impacts can only be estimated for cohorts treated a long time ago.

Perhaps one lesson that can be drawn for current Head Start debates from our study together with other research in this area is that short- or even medium-term impacts on outcomes like test scores are not necessarily a reliable predictor of long-term impacts. We find no detectable Head Start impacts on achievement

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26. We assume that the "treatment" we study here is an extra $400 in spending on Head Start per four-year-old child in 2003 dollars (discontinuity in funding we estimate using 1972 data). On the benefit side our estimates suggest that mortality improvements yield benefits of around $120 per child. Our Vital Statistics estimates for children five to nine imply an effect on overall mortality rates of about 2 per 100,000, times a value per statistical life of $6 million, which is about the value used by the EPA and FDA [Sunstein 2004]. If our NELS estimate is correct that the funding discontinuity in Head Start increases schooling by about 0.5 years per child, if the return to an additional year of schooling is 5 to 10 percent higher earnings [Card 1999], then these earnings gains alone will easily exceed the costs of the treatment. See, for example, Krueger [2003].

test scores or noncognitive outcomes in middle school; yet, we find at least suggestive evidence for long-term effects on years of schooling completed. Recent concerns expressed by Besharov [2005] and others about the value of Head Start based on "disappointingly small" short-term impacts on cognitive or noncognitive outcomes may be misplaced, or at least premature.

Appendix I: Data Sources

A. County-Level Data

We have obtained from the National Archives and Records Administration (NARA) a copy of the reanalysis of the 1960 Census conducted by the Census Bureau for OEO that we believe OEO used to identify the 300 poorest counties in the United States to receive Head Start grant-writing assistance in 1965.[27] The alternative possibility is that OEO may have used information on the share of families per county with incomes below $3000, available with standard 1960 Census data, although our analysis reveals that a larger discontinuity in funding at the three hundredth poorest county using the official poverty rate suggesting OEO used that measure instead.

In order to document the discontinuity in Head Start funding around the OEO cutoff, and to examine whether there is a similar discontinuity in other forms of federal spending, we have also obtained from NARA a series of OEO data files on federal expenditures per county for the years 1967 through 1980.[28] The accuracy of these data is less than perfect, since the files are poorly documented and have some obvious errors. In the end only spending data from 1968 and 1972 were usable, in the sense that the electronic data matched published figures for total federal spending and Head Start spending at the national level, and data matched for Head Start at the state level as well. Another problem is that some federal spending is passed through state governments. In these cases OEO pro-

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27. NARA, Records of the Community Services Administration, Record Group 381: Putnam Print File, 1960.
28. Federal Outlays, County and State File [Machine-readable data file], 1967–1980 / conducted by the Office of Economic Opportunity for the Executive Office of the President. Washington: OEO [producer], 1968: Washington: National Archives and Records Service [distributor]. Record Group 381. File Number: 3-381-73-157(A).

rated state spending across counties, which might be reasonable on average but lead to error in measuring spending in the poorest areas. Finally some Head Start providers such as the Child Development Group of Mississippi operated Head Start programs in a number of counties [Greenberg 1969], but appear to be listed for funding only in the county in which the group's headquarters are located.

Our primary data source on long-term schooling outcomes comes from the censuses from 1960 through 2000. We rely on county-level Census data on schooling by age because public-use Census microdata do not identify county of residence for those living in the low-population counties near the OEO cutoff. For 1990 we obtained a special tabulation from the Census Bureau that provides more detailed schooling data by age, race and gender.

We also draw on county-level data from the Vital Statistics, which provides information from a census of all death certificates in the United States. For our primary observation period (1973–1983) we use data from the Compressed Mortality Files (CMF) that include detailed cause-of-death codes recorded using the International Classification of Diseases, 8th or 9th edition (ICD-8 and ICD-9). For our "pre-Head Start" specification check we analyze data from the annual mortality detail records for the period 1959–1964, generously provided to us by Doug Almond.

We identify the causes-of-death that may plausibly be affected by Head Start by consulting descriptions of the health services offered by the program. The detailed list of ICD-9 codes and cause-of-death descriptions that we count as plausibly affected by Head Start is given in Appendix II. Specifically, we include all of the ICD-9 codes for those health conditions against which children were immunized in Head Start, including polio, smallpox, diphtheria, tetanus, pertussis (whooping cough), and measles. The likely impact from these vaccinations may be modest, with the possible exception of measles: studies of Head Start children in the 1960s found that up to three-quarters had not been vaccinated against this disease (for example, Stone and Kudla [1967]), and even in 1976 only 55 percent of non-White children one to four years of age had been vaccinated [Jaynes and Williams 1989, p. 408]. As Jaynes and Williams [1989, pp. 408–409] note, "Measles is considered the most threatening of the preventable childhood contagious dis-

eases. Its frequent complications include pneumonia, ear in-
fections, and deafness. Brain inflammation (encephalitis) oc-
curs in about 1 of every 10,000 cases." The risk of severe
respiratory problems from measles is particularly severe
among malnourished children. Our list of Head Start-affected
ICD-9 codes as a result includes both measles and these po-
tential sequelae of the disease.

We include ICD-9 codes for diabetes and anemias because of
Head Start's screening for these problems. The possibility of
detection and treatment referral for sickle cell anemia also moti-
vates the inclusion of causes of death that involve infection, for
which sickle cell anemia puts children at elevated risk. Note that
the importance of Head Start's screening for anemias may be
more important during our study period (1960s and 1970s) than
today: while more than 40 states currently require that newborns
be screened for sickle cell anemia (a condition more common
among Blacks than Whites), most of these requirements were
enacted during the 1980s.[29] As discussed in the text, possible
effects of Head Start's nutrition, parenting, or social work com-
ponents on children's exposure to chronic stress may also affect
mortality from various infectious diseases given research about
effects of chronic stress on immune suppression.

Finally, infections that reach the central nervous system can
in some cases cause death from bacterial meningitis, so this
mortality code is motivated by the health services mentioned
above that may directly or indirectly affect infections among
Head Start children. In addition, as noted in the text, Head Start
over time focused classroom observation increasingly on the de-
tection of chronic health conditions such as severe otitis media
(ear infections) or asthma, which studies in the 1980s and earlier
find to be quite prevalent among Head Start children.

B. Individual-Level Data

Our main source of individual-level data is a restricted-use
geo-coded version of the NELS, sponsored by the U.S. Depart-
ment of Education to survey a nationally representative sample
of eighth graders in 1988 with follow-up interviews in 1990, 1992,

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29. Unpublished data from the National Newborn Screening and Genetics
Center, courtesy of Donna Williams.

1994, and 2000.[30] These individual-level data enable us to identify the long-term outcomes of Head Start participants directly, rather than compare countywide Head Start funding and average outcomes. These microdata also enable us to link the behavior of people as young adults to where they were living at around age thirteen, which is at least somewhat closer to when they would have been of Head Start age compared with when we first measure addresses for Census respondents.

The disadvantage of the NELS is that the study is intended to provide a nationally representative sample and so the number of respondents who live in counties with 1960 poverty rates "close" to the OEO cutoff is fairly limited. The original sample employed a two-stage sampling design, with 1052 schools selected in the first stage and 26 students per school selected in the second (excluded were students with mental handicaps, physical or emotional problems, and inadequate command of the English language). Base year participants were selected to participate in follow-up surveys in part on the basis of the number of other base-year NELS participants in the student's school at the time; dropouts were also retained in the sampling frame. The U.S. Department of Education [1994] provides weighting variables that account for the probability of participation in the base-year and follow-up surveys, as well as school administrator and student survey nonresponse.

Overall, the base-year sample includes students drawn from 568 different counties. The base-year sample includes 649 students who lived in counties with 1960 poverty rates among the 300 poorest, and 674 respondents who lived in one of the next 300 poorest counties with respect to 1960 poverty. The NELS base year sample sizes for the 100 counties with poverty rates just above the cutoff is $N = 183$; for 200 counties above, $N = 493$; for

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30. Note that there are a number of other micro-data sets that in principle could be used to study the effects of Head Start using our research design, but in practice wind up not being very helpful. The restricted-use version of the Panel Study of Income Dynamics includes geo-code information on respondents every year they are interviewed (including for some back to age three), but the relevant sample in high-poverty counties is small and does not seem to be representative, in the sense that we do not observe a discontinuity in Head Start participation rates. The High School and Beyond sophomores were born around 1964–1965 and so reached Head Start age when the program was transitioning from summer to year round, and in addition it is difficult to identify specific counties of residence for respondents in this data set, particularly in the high-poverty, low-population counties near the OEO cutoff.

100 counties with poverty rates just below the cutoff, $N = 185$; and for the 200 counties just below the cutoff, $N = 361$. Another potential concern with the NELS is that Head Start effects on grade retention could affect the age distribution across counties of who is enrolled in eighth grade in 1988, although, in practice, we do not find a discontinuity in age at the OEO cutoff.

The key explanatory variable of interest is whether the respondent has participated in Head Start, which is reported at baseline by the child's parent rather than taken from administrative records. The problem of recall errors with the NELS may be exacerbated by the fact that parents of eighth graders are asked to report on their child's involvement in Head Start or other preschool programs nearly ten years earlier (1977–1979). Nevertheless, the Head Start participation rate suggested by the NELS data (13 percent) is generally consistent with that implied by other national data sources (see CT and GTC). The other key explanatory variable for our analysis comes from the NELS respondent's county of residence, which we identify using information on the location of the school that each respondent attended in eighth grade in 1988.[31]

Our main measures of educational attainment and labor market outcomes come from responses to the 2000 follow-up survey, by which time respondents were around twenty years of age. Our measures of academic achievement come from standardized tests administered in 1988.[32] We also examine measure for noncognitive outcomes such as school absences, grades, and time spent on homework in the 1988 and 1990 NELS interviews, and arrests in the 1990 survey wave. Outcomes from the 1990 NELS interview include information from both students still enrolled in school and school dropouts.

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31. For students in public schools we identified counties by matching NELS school identifiers with information from the Common Core of Data, while for private-school students we identified the counties of their schools from the 1988 Private School Survey. Through this procedure we were able to identify the 1988 county of residence for 96 percent of base-year NELS respondents.

32. We only use achievement tests for the base year because test results are missing for an unusually large share of dropouts in later waves [U. S. Department of Education 1994; Grogger and Neal 2000].

APPENDIX II: CAUSES OF DEATH SUSCEPTIBLE TO HEAD START
INTERVENTIONS

ICD-9 codes	Cause of death
10–18	Tuberculosis
33	Whooping cough (pertussis)
34–35	Streptococcal sore throat, scarlatina, erysipelas
36	Meningococcal infection
38	Septicemia
45	Acute poliomyelitis
55	Measles
70	Viral hepatitis
250	Diabetes mellitus
260–269	Nutritional deficiencies
280–285	Anemias
320–322	Meningitis
466	Acute bronchitis, bronchiolitis
480–487	Pneumonia and influenza
490–491	Bronchitis, chronic and unspecified
493	Asthma
494–496	Other chronic obstructive pulmonary diseases andalliedconditions

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