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Does Head Start Improve Long-Term Outcomes? Evidence from a Regression Discontinuity Design

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Does Head Start Improve Long-Term Outcomes?
Evidence from a Regression Discontinuity Design

Abstract

This paper exploits a new source of variation in Head Start funding to identify the program's long-term effects. In 1965 the Office of Economic Opportunity (OEO) provided technical assistance to the 300 poorest counties in the U.S. to develop Head Start funding proposals, but did not provide similar assistance to other counties. As we demonstrate, the result is Head Start participation and funding rates are nearly twice as high in counties with poverty rates just above versus just below OEO's cutoff for grant-writing assistance. We do not find evidence for similar discontinuities in other forms of federal social spending. This discontinuity in Head Start funding is associated with a discontinuity in mortality rates for children ages 5-9 from health problems addressed by the program, but not for other causes-of-death that should not be affected by Head Start (such as injuries) or for age groups that should not be affected (adults). We also find a discontinuity in educational attainment in the 1990 Census, which is concentrated among those age groups young enough to have been exposed to Head Start, and a similar discontinuity in schooling among sample members of the National Education Longitudinal Study of Youth 1988, who would have been of Head Start age during the late 1970s.

I. Introduction

Head Start was established in 1964 as part of the War on Poverty to provide educational, health and other services to poor children, and currently serves more than 800,000 children each year at a total annual cost of $6.7 billion (Haskins, 2004). Interest in compensatory early education programs is motivated in part by evidence for differences in achievement test scores between rich and poor or minority and white children even before they start school (Phillips et al., 1998, Fryer and Levitt, 2004). The possibility that neurodevelopmental plasticity may decline with age and of dynamic complementarities in learning suggest that human capital investments may be especially productive when made early in the life cycle (Shonkoff and Phillips, 2000, Carniero and Heckman, 2003, Bruer, 2004, Lombroso and Pruett, 2004). Head Start is also of interest to economists because the program may reduce external costs from a variety of anti-social behaviors that may have antecedents in early childhood (Currie, 2001).

Whether Head Start yields lasting benefits to program participants in practice is an open question, the answer to which will rest on non-experimental evidence for the foreseeable future. The federal government's recent randomized evaluation of Head Start's short-term impacts finds beneficial treatment-control differences 1 year after program participation with respect to letter-word identification (around .2 standard deviations), pre-writing and vocabulary scores (~.1sd), and the frequency with which parents read to children (~.15sd), with no detectable impacts on oral comprehension or early math skills (HHS, 2005).[1] With a per-pupil expenditure level in 2002 of around $7,200 (Haskins, 2004), the magnitudes of both the costs and test score gains of Head Start are about the same as those associated with reducing class sizes in early grades from 22 to 15 in the Tennessee STAR experiment (Krueger, 1999). If these achievement gains from Head Start were to persist over time, the program would appear to pass a benefit-cost test just from the effect of higher test scores on earnings alone (Krueger, 2003). However controversy about whether Head Start benefits "fade out" date back to the program's beginning (see for example Wolff and Stein, 1966), and has led to recent proposals for changing the program's focus from "comprehensive services to intellectual development" (Haskins, 2004).

Even if the government's new randomized evaluation of Head Start included funding to study long-term impacts (which unfortunately it does not), experimental evidence on how the program influences outcomes during adolescence or adulthood would not be available for 15 or 20 years. In the interim policy makers might be tempted to draw inferences about Head Start's long-term effects from the encouraging experimental evaluations of other model early childhood programs such as Perry Preschool and Abacedarian (Barnett, 1992, 1995; Donohue and Siegelman, 1998; Karoly *et al*., 1998; Campbell *et al*., 2002; Currie, 2001). However these programs are much smaller and more intensive than Head Start, and as a result may not be very informative about what we might expect from the less-costly, larger-scale Head Start program.[2]

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1 Effects on problem behaviors, access to health services, overall health outcomes and parent discipline strategies (spanking) were observed for 3 year olds but not 4 year olds (HHS, 2005).

2 One exception is the Chicago Child-Parent Center (CPC) Program, which, like Head Start, is a federally funded large-scale public program. Reynolds et al. (2001) suggest that CPC participants are more likely to complete high school and less likely to be arrested compared to non-participants. However these conclusions stem from a non-

To date the best available evidence for the long-term 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, hereafter CT and GTC). The sibling differences suggest that the program may have long-term effects on educational outcomes and achievement test scores for whites, while reducing criminal involvement among African-Americans.[3] These studies substantially improve upon previous non-experimental evaluations of Head Start by controlling for unmeasured family fixed-effects that may be associated with program participation. Yet there necessarily remains some uncertainty about what drives variation across siblings in Head Start participation. Of particular concern is the possibility that participation is related to unmeasured child or time-varying family characteristics that also affect children's outcomes. As one recent review notes, with respect to long-term impacts: "The jury is still out on Head Start" (Currie, 2001, p. 213).

The present paper provides new evidence on the long-term effects of the Head Start program by drawing on a new (actually an old) source of variation in program participation. Specifically, we exploit a discontinuity in program funding across counties that resulted from how the Office of Economic Opportunity (OEO) launched the program during the spring of 1965. Unlike other federal social programs, Head Start provides funding directly to local service providers. Out of concern that the most disadvantaged communities would be unable to develop proposals for Head Start funding, OEO sent staff to the 300 poorest counties in the country to identify potential service providers and help them develop proposals (Jones, 1979, pp. 6-7). As we demonstrate below, the result is Head Start participation and funding rates that are 50% to 100% higher in counties with poverty rates just above OEO's cutoff (the "treatment" group) compared to those with poverty rates just below the cutoff (controls). These findings come from the National Education Longitudinal Study, which provides information about a nationally representative sample of 1988 eighth graders, as well as from federal expenditure data from the National Archives, although the latter are noisy and so yield somewhat imprecise estimates. These funding differences appear to have persisted through at least the late 1970s. We use this discontinuity in funding across counties to identify Head Start impacts under the assumption that potential outcomes are smooth around the cutoff. Because the cutoff was based on a predetermined variable (poverty rates measured 5 years earlier), strategic behavior by county actors does not seem like a concern.

Our data suggest that the discontinuity in Head Start funding across counties is mirrored by discontinuities in children's health outcomes (mortality) and educational attainment. Our main challenge is to assemble data capable of linking the discontinuity in Head Start funding across counties generated by the way the program was launched 40 years ago to the long-term outcomes of children exposed to Head Start in those counties. Our paper in this sense operates at the intersection of econometric program evaluation and cliometrics, and as is often the case with

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experimental regression that conditions on only a fairly basic set of socio-demographic characteristics.

[3] Using cross-section data from the NLSY97, Aughinbaugh (2002) does not find evidence for long-term impacts of Head Start participation. Currie and Thomas (2000) find little long-term effect on Head Start in the NELS data for blacks, although this may be contingent on the quality of the public schools that children go on to attend.

the latter, many of the data sources available to us are less than ideal along some dimensions.[4] In what follows we present a mosaic of evidence derived from multiple different data sources that generally seem to provide consistent findings.

Using data from the Vital Statistics census of death certificates for the period 1973-83, we show that there is a large discontinuity in mortality rates to children 5 to 9 years of age due to causes-of-death that were directly or indirectly screened for and addressed as part of Head Start's health services, including asthma, anemias, infections, meningitis, and diabetes. This mortality effect is quite large, equal to 50-90% of the left limit at the cutoff (the "control mean"), which is enough to drive mortality rates from these causes in the treatment counties down to about the national average. To rule out other explanations for this finding we demonstrate that there is no discontinuity for other causes-of-death that should be affected, nor for mortality rates among adults whose health outcomes should not have been affected by Head Start. While these mortality rates are measured during childhood they nevertheless represent an important long-term outcome in the sense that survival to adulthood mediates all other long-term measures.

We also demonstrate that there is a discontinuity in educational attainment at the OEO cutoff using data from the 1990 Census. This discontinuity in schooling outcomes is limited to those age groups that were young enough to have been exposed to Head Start. The virtue of the Census data is that we have large samples of people living in counties near the cutoff, although the disadvantage is that we only have information on county of residence when respondents are adults in 1990 rather than when they were of Head Start age. To address the possibility of bias from selective migration we show that there is also evidence for a discontinuity in educational attainment in the NELS, which improves upon the Census by recording each respondent's county of residence at age 13 rather than adulthood. We also do not find any discontinuity in key parent characteristics that are strongly predictive of children's outcomes, such as family income or maternal schooling. This last finding does not seem consistent with a story in which our findings for students in the NELS are driven by selective migration of families across county lines.

Do these estimates identify the effects of Head Start? One challenge for identification comes from the possibility that other forms of government spending might also vary discontinuously around the cutoff. However we show that the estimated discontinuity around the OEO cutoff in other forms of federal social spending is very small and statistically insignificant.

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4 For example, Goldin (1998) examines state-level secondary school graduation rates from 1910 to 1960, where the starting date for the data series is constrained by the fact that few schools reported information to the Bureau of Education before that time; in addition, the data for the period 1920-38 include two series, one collected from schools and the other from states, that show some unexplained disagreements (see Goldin, 1994). Gentzkow, Glaeser and Goldin (2004) examine political behavior by newspapers using a dataset on papers for the largest cities from 1870 to 1910, although in principle one might also wish to study papers in smaller cities over this period as well. Goldin and Katz (2000) generate estimates for the returns to schooling before 1940 by drawing on a state census from 1915 available for Iowa (but not other states). And Collins and Margo (2004) examine the effects of the race riots of the 1960s on urban housing markets using historical data for different dimensions of riot severity that do not include direct measures of economic damage. The results of each of these studies are in our view interesting and informative despite these data limitations.

Another challenge to identification comes from the possibility that Head Start, like other OEO activities, may have helped mobilize the community, which in turn could have affected political behavior and changes in local or state spending. Yet we show that there is no evidence of a discontinuity in voter registration rates across counties in any year from 1968 through 1980.

A final potential concern comes from the fact that our estimates – like those from every regression discontinuity design – rely on functional form assumptions for identification. Our findings do not appear sensitive to decisions about the functional form used to control for other determinants of long term outcomes across counties, including flexible polynomials in 1960 county poverty rates and even non-parametric functions of 1960 poverty using the partially linear regression approach of Porter (2003). As another way to demonstrate that our results are not an artifact of our various estimation procedures, we look for discontinuities in outcomes at other cutoffs where there are no discontinuities in Head Start funding. We find no discontinuities in outcomes at our pseudo-cutoff, which enhances our confidence in our research design.

The remainder of the paper is organized as follows. The next section provides more background on Head Start, with a particular focus on how Head Start might affect long-term outcomes and the features of the program's rollout that generates the natural experiment underlying our research design. The third and fourth sections discuss our data and empirical strategy, the fifth section presents our results and the final section discusses limitations with our estimates and implications for public policy.

II. Background on Head Start

Planning for Project Head Start began in the fall of 1964 as part of OEO's Community Action Program (CAP). Head Start began as a summer program, although by 1970 a majority of program participants were year-round (nine months) and now the program is almost entirely year-round for the 800,000 children served each year.[5] While Head Start is widely perceived to be an educational intervention, the program as originally conceived and implemented was more than that: Head Start is (or at least was) also a health program, a nutritional program, a social services program, a parenting program, and even a jobs program.

Unlike most other federally-funded social programs, Head Start involves direct federal funding of local service providers, who can be either local government agencies or non-profit organizations. The challenge for OEO administrators in the spring of 1965 was to publicize Head Start, encourage local organizations to submit proposals, review the proposals and fund enough local programs to launch Head Start in the summer of 1965 on the grand scale desired by President Johnson – all within the span of several months.

5 The number of total participants (summer # in parentheses) by year (Jones, 1979): 1965 – 561,000 (561,000); 1966 – 733,000 (573,000); 1967 – 681,000 (466,000); 1968 – 693,825 (476,825); 1969 – 635,121 (421,665); 1970 – 434,880 (195,328); 1971 – 419,971 (123,485); 1972 – 379,000 (86,400); 1978 – 389,500 (26,000).

Despite OEO's efforts to publicize the new Head Start program among local school principals, welfare administrators and public health officials, federal officials were concerned that in a nationwide grant competition many poor counties would be unable to develop acceptable proposals. Julius Richmond, national director of Project Head Start in 1965, noted that OEO administrations were "making a very determined effort to get the communities with greatest need in" (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 were provided with funding to travel to the selected counties for two to six weeks during the spring of 1965, locate local actors who would be able to implement a Head Start program, work with them to develop a suitable proposal, fly the completed application back to Washington and then defend the proposal to OEO reviewers. Importantly, this particular feature of Head Start's launch is widely documented in accounts of the program's history, suggesting that the discontinuity in grant-writing assistance that is at the heart of our research design is not the figment of a single historian's imagination.[6] Below we demonstrate that the result is a discontinuity in county-level Head Start program funding and participation rates.

How might Head Start affect long-term outcomes? Because learning is a cumulative process, early achievement deficits may affect what children learn during their K-12 careers. As Carniero and Heckman note, "Learning begets learning; skills (both cognitive and non-cognitive) acquired early on facilitate later learning" (2003, p. 90). In this view any Head Start impacts on short-term academic achievement could translate into long-term schooling gains through dynamic complementarities in educational outcomes.

There may also be dynamic complementarities between child health and other forms of human capital investment. In addition to serving meals, Head Start involved an immunization regime more rigorous than even modern requirements for participating in kindergarten in most states, including vaccinations for DTP, polio, smallpox, and measles (North, 1979, O'Brien, Connell, and Griffin, 2004). Head Start also provided health screenings that are more comprehensive than those usually given to children of this age, including blood, urinalysis and other tests to check for tuberculin, anemias (such as sickle cell), diabetes, and hearing and vision problems (North, 1979). The daily observation of children by Head Start teachers could in principle also help detect and lead to treatment for problems such as asthma, bronchitis or pneumonia in cases where parents might not realize either the severity of the problem or how to access treatment.[7] Previous research has found that Head Start may increase immunization rates by 10 to 20 percentage points (Fosberg, 1984 cited in O'Brien, Connell and Griffin, 2004, Currie and Thomas, 1995), although Head Start may have even more pronounced effects on the health services to very low-income (especially minority) children in the poorest parts of the South in the 1960s and 1970s.

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6 See for example Jones (1979), pp. 6-7, and Gillette's (1996, p. 222) interview with Jule Sugerman of OEO. The White House's notes to accompany President Johnson's speech on Head Start of May 18, 1965 makes explicit reference to the number of the nation's 300 poorest counties that received Head Start funding (Zigler and Valentine, 1979, pp. 69-70; see also GAO, 1981, p. 17).

7 Thanks to Romi Webster for detailed discussions about the possible health effects of Head Start.

A separate pathway through which Head Start could affect children's long-term outcomes is by changing household environments, and thus the long-term stream of parental investments in children. As part of OEO's Community Assistance Program, Head Start was intended in part to provide jobs to poor families and involve them in the administration of local programs. As a result up to 47,000 parents were employed in Head Start centers the first year of the program (Zigler and Valentine, 1979, p. 69). Head Start also involved up to another 500,000 parents and other part-time volunteers to help run the program, and more generally tried to help all Head Start parents and improve their parenting skills (GAO, 1981, p. 13). Head Start could have improved employment rates or disposable income of many poor families by providing subsidized child care, and the social services offered as part of the program may have reduced stress among some parents. These aspects of Head Start imply that parents and even siblings of program participants may be "treated" by the intervention as well.

Despite the numerous plausible mechanisms through which Head Start could produce lasting benefits, previous studies often find evidence of "fade out" in program gains, particularly for African-American children (Currie and Thomas, 1995). However most of the evidence on the effects of Head Start comes from non-experimental comparisons of program participants with non-participants. These comparisons are of course susceptible to bias from unmeasured variables associated with both program participation and children's outcomes (see Currie, 2001). Even the best studies that rely on within-family across-sibling comparisons may be susceptible to such bias to some degree. In sum, there are theoretical reasons to suspect that Head Start could produce lasting effects on the outcomes of low-income children, but a somewhat limited body of empirical research that raises questions about whether Head Start realizes this goal in practice.

III. Data

In what follows we discuss the various county-level data sources that we use to estimate the long-term impacts of Head Start, and then discuss our student-level data.

A. County-Level Data

How did OEO identify the 300 poorest counties in the U.S. in 1960 to which the agency should provide Head Start grant-writing assistance? The answer is not immediately obvious because the official federal poverty rate was only invented in 1964, and so the 1960 Census does not include a measure of persons living in poverty. OEO apparently identified the poorest counties using a special 1964 re-analysis of the 1960 Census conducted by the Census Bureau for OEO using the then-newly-defined federal poverty rate, a copy of which we have obtained from the National Archives and Records Administration (NARA).[8] The alternative possibility is that OEO targeted grant-writing assistance using data on the proportion of families in the county

[8] NARA, Records of the Community Services Administration, Record Group 381: Putnam Print File, 1960.

with incomes below $3,000, although our analysis reveals that a larger discontinuity in funding at the 300th poorest county using the official poverty rate, suggesting OEO used that measure instead.[9]

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.[10] The accuracy of these data is less than perfect. The electronic data files have some obvious errors and are poorly documented.[11] In the end only spending data from 1968 and 1972 were usable, in the sense that the data from the electronic files matched published figures for total federal spending and Head Start spending at the national level, and the 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-rated state spending across counties, which might be reasonable on average but lead to error in measuring spending in the poorest areas.[12] In any case, in addition to constructing a measure of Head Start spending we create variables for spending on other social programs.[13]

Our primary data source on long-term outcomes comes from a special tabulation

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9 Since the official poverty threshold for a family of four in 1960 dollars is $3,002 (Citro and Michael, 1995, p. 35), it is not surprising that the percent of a county's families with incomes less than $3,000 is highly correlated with the official poverty rate for 1960 (+.95). The correlation between the two measures is not perfect because the income level used to define whether a family is in poverty varies with the family's composition, and the two measures of disadvantage produce slightly different rankings of which counties were the "poorest" in 1960.

10 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).

11 The records for 1968 and 1972 still contain a number of glitches such as alphabetic characters or brackets in the last columns of the program expenditure and beneficiary files, which we infer should be zeros based on comparisons with published expenditure and beneficiary data.

12 A related challenge specific to the 1968 spending data is that the Child Development Group of Mississippi (CDGM) operated Head Start programs in a number of counties in Mississippi, yet CDGM's total funding is concentrated in the Mississippi county (Clay Co.) that housed CDGM headquarters. We use information from the appendix map in Greenberg (year) to identify other Mississippi counties in which CDGM operated and interpolate spending across these counties, assuming that CDGM allocated their Head Start spending across counties in proportion to county population. Results setting CDGM spending to missing are qualitatively similar. The CDGM problem is not an issue for the 1972 data because the agency lost OEO funding in the late 1960s.

13 For 1968 the data on program beneficiaries for Head Start matches up with published figures for the U.S. as a whole, although the beneficiary variable seems to be largely missing for most other federal programs. Interpretation of Head Start beneficiary data is complicated in 1968 because the program at that time was mostly summer-only, although some areas had shifted towards a year-round program. For 1972 even the beneficiary variable for Head Start records are generally missing. For 1972 we define total Head Start expenditures as federal spending dedicated to three OEO programs with activity codes listed Head Start ($328.0 million), OEO's Follow Thru program ($25.3 million), and OEO Community Services spending devoted to early childhood education ($11.7 million). The sum of these three programs is approximately equal to published figures for total Head Start spending for this year (see notes 8/18/03). Spending on other social programs is defined as expenditures by HEW, OEO (excluding those made through Head Start), the Department of Labor, HUD, and selected programs from the Department of Agriculture.

conducted for us by the U.S. Census Bureau for data from the 1990 Census. These data include information on educational attainment, employment, earnings, and residence 5 years ago disaggregated by age group, gender, and race. We use data from the 1990 rather than 2000 Census because the funding disparity across counties seems to have smoothed out over time, which means that the discontinuity in funding will be less pronounced for young people in 2000 compared to 1990. At the same time the additional 10 years worth of mobility for older cohorts from 1990 to 2000 will further attenuate our estimates for these age groups.

We also draw on county-level data from the Vital Statistics, which provides information drawn from a census of all death certificates in the U.S., including detailed cause-of-death codes recorded during our observation period (1973-83) using the ICD-8 or -9 systems. Using data from the Compressed Mortality Files we focus on mortality to children in the age group 5-9, which should capture the health effects of Head Start services to children ages 3-4.

Finally, we draw on county-level voter registration records collected from state officials by the Inter-University Consortium on Political and Social Research (ICPSR Study 9405) for the period 1968-88. We focus on the ratio of registered voters to population for the years 1968 through 1980 to capture possible effects of Head Start or related OEO activities on voter registration rates. Note that voter data are not available for all states although this becomes less of a problem over time,[14] and voter registration totals may be subject to some upward bias from voters who have left the county or state but are not immediately removed from the voting rolls.

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. Department of Education to survey a nationally representative sample of 8th graders in 1988 with follow-up interviews in 1990, 1992, 1994 and 2000. These individual-level data enable us to identify the long-term outcomes of Head Start participants directly, rather than compare county-wide Head Start funding and average outcomes. These micro-data also enable us to link the behavior of people as young adults to where they were living at around age 13, which is at least somewhat closer to when they would have been of Head Start age compared to when we first measure addresses for Census respondents. The disadvantage is that the NELS 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 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. Overall the base year sample includes students

14 The ICPSR dataset is missing voter information for Alabama for 1968-72, Iowa for 1968-74, Kansas for 1968-70, Minnesota for 1968-72, Texas for 1968-74, and Virginia for 1968-72. Note that by 1976 our dataset will include complete information for all of the Southern states that account for most of the 600 with the poorest 1960 county poverty rates, as mentioned above. The number of counties for which we have valid voter registration data in our sample increases over time as follows: 1968, N=1,991; 1970, N=1,994; 1972, N=2,111; 1974, N=2,135; 1976, N=2,732; 1978, N=2,879; and 1980, N=2,845.

drawn from 568 different counties.[15] 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 8th grade in 1988, although in practice this does not seem to be a concern since 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 10 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 data.[16] 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 8th grade in 1988.[17]

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 25 years of age. Our measures of academic achievement come from standardized tests administered in 1988.[18] We also focus on self-reported arrests collected by self-administered pencil-and-paper

15 The original sample employed a two-stage sampling design, with 1,052 schools selected in the first stage and 26 students per school selected in the second. Excluded from the NELS sample in 1988 were students with mental handicaps, physical or emotional problems, and inadequate command of the English language. In most cases, 24 of the 26 students per school included in NELS were randomly sampled, while the other two students were selected from among the Hispanic and Asian Islander students (U.S. Department of Education, 1994). 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 (U.S. Department of Education, 1994). The Department of Education 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 non-response (U.S. Department of Education, 1994).

16 This figure is similar to that reported by parents in the 1979 National Longitudinal Survey of Youth Child-Mother file (NLSCM), in which 14 percent of white and 32 percent of African-American children participated in Head Start (Currie and Thomas, 1995), and to figures reported in the PSID suggesting participation rates of between 10 and 12 percent for children born in the 1970's (Garces, Thomas and Currie, 2000). Head Start participation in the NELS is also consistent with the figures implied by administrative data collected by the Federal government: If we assume each Head Start participate is in the program for only one year, then around 12 percent of children four years old in 1978 were enrolled in Head Start. In 1978, the year in which the average NELS child would have been four years of age, a total of 337,531 children participated in Head Start (GAO, 1981). Since each cohort under the age of 5 in 1978 averaged around 3 million children (U.S. Census Bureau, 1979), the ratio of program participants to children age four was on the order of 0.11. Put differently, if children were only allowed to participate in Head Start at age four, the available administrative data would suggest that 11 percent of the cohort of children enrolled in eighth grade in 1988 (the NELS cohort) participated in Head Start.

17 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% of base-year NELS respondents.

18 We only use achievement tests for the base year because follow-up achievement test results are missing for an unusually large share of dropouts in later waves (U.S. Department of Education, 1994, Grogger and Neal, 2000).

questionnaires in the 1990 and 1992 interviews (U.S. Department of Education, 1994).[19] Students in school are asked about arrests during the past academic term, while dropouts are asked about the last academic term spent in school.[20] This raises the possibility that students and dropouts may be reporting on arrests at a different point in calendar time, which is of some concern given that crime rates were changing quite dramatically during the 1990's (Levitt, 2004). This is likely to be more of a problem with the 1992 than the 1990 NELS survey.[21]

In principle an alternative micro-data source for our project would be the Panel Study of Income Dynamics (PSID), which in 1995 asked all respondents ages 18 to 30 about their participation in Head Start and other preschool programs and serves as the data source for GTC. One advantage of the PSID relative to the NELS is the ability to identify where respondents live when they are actually of Head Start age rather than at some later point in time. In practice the PSID, which like the NELS is intended to be representative at the national but not the state (much less county) level, appears to provide an unrepresentative draw of people in the treatment counties just above the OEO cutoff. The result is that among PSID sample members who answered the 1995 Head Start question, we do not see the discontinuity in Head Start participation that we observe in the NELS and the county-level federal spending data. For this reason we do not use the PSID to directly estimate the effects of Head Start on outcomes using the OEO discontinuity. However we do exploit the availability of geo-coded data for the larger PSID sample in all poor counties to explore the problem of selective across-county migration with our NELS and county-level data.[22]

IV. Empirical Strategy

In Section V of the paper we show that the particular features of OEO's launch of Head

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19 Self-administered questionnaires seem to yield somewhat lower estimates for the prevalence of sensitive behaviors than computer-assisted methods (Turner *et al.*, 1998).

20 The arrest rates reported by NELS teens are quite similar to those implied by national arrest data. For example, in the first NELS follow-up in 1990 (when most students were 15 or 16), 6 percent of male students had been arrested during the previous term. By comparison, data from the Federal Bureau of Investigation's Uniform Crime Report system suggest that 10 percent of teens age 15 and 12 percent of teens age 16 were arrested during 1990 (FBI, 1991). Since the NELS question covers half a school year, and a fair proportion of juvenile criminal activity may occur over the summer, the NELS results seem reasonable.

21 This is for two reasons. First, the fraction of NELS respondents who have dropped out is much lower in 1990 than 1992. Second, those who have dropped out are likely to have dropped out more recently prior to the interview for the 1990 than the 1992 waves, suggesting that in the 1990 interview a larger fraction of dropouts will be reporting on the same calendar period as are enrolled students.

22 An alternative explanation for the difference between the PSID and NELS in documenting a Head Start discontinuity at the OEO cutoff is that the Head Start variable with the former may suffer from relatively greater measurement error. The reason is that while the NELS asks parents of potential Head Start participants to report on program involvement 10 years after their children would have been age-eligible to participate, the PSID asks people to self-report on whether they were in Head Start from 15 to 25 years after they would have been of Head Start age. We believe that sampling variability rather than measurement error is more likely to explain the PSID pattern of Head Start participation around the OEO cutoff because we do not see any difference in outcomes for PSID respondents at the cutoff.

Start in 1965 generated a discontinuity in Head Start program funding and participation rates across counties. The heart of our research design is to then examine whether we also observe discontinuities around the OEO cutoff in long-term outcomes such as schooling or earnings. Like all regression discontinuity estimates, identification rests crucially on issues related to functional form – other determinants of long-term outcomes are assumed to vary smoothly around the OEO cutoff, while only the intensity of Head Start treatment changes discontinuously at this point. In what follows we first discuss our approach for estimating discontinuities in long-term outcomes around the OEO cutoff controlling for flexible polynomial terms in each county's 1960 poverty rate. To determine whether our results are sensitive to functional form assumptions about how we control for other factors that vary with 1960 county poverty, we also estimate the discontinuity in long-term outcomes using the partially linear regression approach of Porter (2003), which non-parametrically controls for determinants of outcomes related to 1960 county poverty rates.

A. Regression Discontinuity Design

Our analyses are conducted using county-level data, which is the geographic unit for which our Census data are reported. We aggregate the NELS data up to the county level as well using the sampling weights, the simplest way within the partially linear regression framework discussed below to account for the non-independence of NELS observations living within the same county. That is, for each county (c) and NELS respondent (i) we calculate the average outcome within the county as $Y_c= \sum_c(Y_{ic}w_{ic}) / \sum_c(w_{ic})$, 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 8th grade (about 10 years after they would have been of Head Start age). In the Census, (c) indexes county of residence 1990 when we observe adult long-term outcomes. Below we return to the problem of unmeasured county of residence at Head Start age (3-5) for Census and NELS respondents.

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 for Head Start grant-writing assistance occurs at c=300). Each county's 1960 poverty rate is a function of a set of "fundamental" factors p_c as well as a random component ε_c, as in equation (1). The provision of grant-writing assistance is a deterministic function of the county's 1960 poverty rate, as in equation (2), where P_{300}=59.1984.

(1) $P_c = p_c + \varepsilon_c$

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

We can use the "sharp" regression discontinuity 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 service providers assistance in securing Head Start funding. If the offer of grant-writing assistance has no effect beyond increasing the amount of funding, then we can calculate the effect on long-term outcomes per dollar of additional Head

Start funding by dividing the ITT effect for some educational or other outcome by the ITT effect on Head Start funding.

Less clear is whether we can estimate the effects from an extra dollar of Head Start spending or actual program participation. While the NELS provides information about enrollment rates, variation in funding across counties could affect spending per participant as well as participation probabilities. At the same time our estimates for Head Start county-level spending shown below are imprecise. In short we can generate fairly precise "ITT" estimates on long-term outcomes but have less power to reliably scale these estimates to recover the effects per dollar of Head Start spending. For these reasons, we focus our analysis primarily on the "reduced form" ITT-style estimates for the overall discontinuity in outcomes at the OEO cutoff.[23]

Given that the unit of treatment in this setup is the county, our empirical analysis focuses on estimating the effect for the average county rather than the average child. In the next section we show that the results estimated for the average child (that is, weighting by county population) are qualitatively similar to our preferred (un-weighted) estimates. Our main estimating equation is given by (3), where Y_c is the outcome for county c, $m(P_c)$ is an unknown smooth function of 1960 poverty levels, and α is the impact of grant writing assistance. The effect that we seek to identify is the one relevant for the poorest counties with 1960 poverty rates near the OEO cutoff.

(3) $$Y_c = m(P_c) + G_c\alpha + \upsilon_c$$

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). It strikes us as plausible that in the absence of OEO's grant-writing assistance to the poorest 300 counties, outcomes would vary smoothly around the cutoff, particularly because this cutoff does not seem to have been used to distribute funding for other federal programs. Below we present empirical evidence on funding patterns for other federal programs that is consistent with this conclusion.[24]

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23 Estimating the effects of Head Start enrollment itself also requires the assumption that program effects on participants and non-participants alike are not related to the county's overall Head Start funding or participation rates. This "stable unit treatment value assumption" (SUTVA) may be violated if social interactions among children or parents affect children's long-term outcomes, in which case Head Start's impacts may be amplified by "social multipliers" (Glaeser, Sacerdote and Scheinkman, 2003). For example, Head Start funding might affect the probability that a given classroom contains a "rotten apple" that disrupts everyone's learning (Lazear, 2001), or that parents learn about new parenting skills that they then share with others within their social networks.

24 An alternative way to think about identification in this RD model comes from Lee (2003). If the probability density of ε_c (the stochastic component of each county's 1960 poverty rate) is continuous at the OEO cutoff, then the allocation of technical grant-writing assistance for Head Start, G, can in the limit be thought of as randomized in the neighborhood of P_{300}. This assumption strikes us as plausible given that each county's 1960 poverty rate was determined before the War on Poverty was launched, and OEO's decision to target grant-writing assistance on the poorest 300 counties seems to have been an unannounced, *ad hoc* decision made in the rush to launch a nationwide Head Start program within the span of a few months. There would appear to be little room for strategic behavior on the part of local officials, and little incentive for strategic behavior on the part of OEO officials (given that the

B. Estimation Issues

Our "parametric" estimates come from estimating (3) using different functions of P_c calculated using counties "near" the OEO cutoff, as in (4). This setup assumes that OEO grant-writing assistance (and the resulting change in Head Start program funding) produces a constant shift in outcomes. Alternatively we can also estimate a model as in equation (5) that allows Head Start funding to change the slope of the relationship between Y and P as well. We can also refine each of these estimators by further controlling for observable county covariates from the 1960 Census, such as total population and age or race distribution.

(4) $\quad Y_c = \alpha_1 G_c + \Sigma_k \beta_{1k} (P_c)^k + \upsilon_{1c}$

(5) $\quad Y_c = \alpha_2 G_c + \Sigma_k \beta_{2k} (P_c)^k + \Sigma_k \delta_{2k} G_c (P_c)^k + \upsilon_{2c}$

[Note: the remainder of this subsection needs to be revised to reflect new, "partially linear" method described in Porter (2003). This method involves a locally linear regression, using Kernel weights based on proximity to the threshold, and estimating the LHS and RHS means at the discontinuity independently from one another. That is, the LHS mean is based only on data from the LHS, and similarly for the RHS mean. We compute inference using the formulas in Porter (2003), which we have verified for ourselves as providing accurate inference via monte carlo analysis.]

Both (4) and (5) assume 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. As a check on whether our estimates are sensitive to functional form assumptions about m(.) we also calculate our estimates using the partially linear regression approach of Porter (2003). The approach is "partially linear" in the sense that we use a kernel estimator to non-parametrically model $m(P_c)$, which gives more weight to data points that are closer to the OEO cutoff and allows us to control for the variety of factors that vary across counties and affect long-term outcomes without having to impose strong functional-form assumptions. Following Porter we re-write equation (3) as follows:

(6) $\quad (Y_c - G_c\alpha) = m(P_c) + \upsilon_c$

Our estimate for α comes from finding the value that minimizes the average squared deviation between the new dependent variable in equation (6) and the nonparametric estimate of $m(P_c)$. That is, our estimate for the change in the conditional expectation of Y_c at P_{300} comes from choosing the value of α that minimizes the following value, where the summations for c, j and k are all from 1 to N:

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concern in Spring 1965 was one of excess supply of federal funding rather than excess demand). So long as the mapping between P and Y is also smooth in the neighborhood of the OEO cutoff then potential outcomes will be independent of G given P, the necessary condition for identification (Hahn, Todd and van der Klaauw, 2001).

(7) $\min \Sigma_c [\ Y_c - G_c\alpha - \Sigma_j\ w_j^c(Y_j - G_j\alpha)]^2$

where $w_j^c = K_h(P_c - P_j) / \Sigma_k\ K_h(P_c - P_k)$

To estimate (7) we use the Epanechnikov kernel, $K(z) = (.75)(1-.2z^2)/\sqrt{5}$ for $|z| < /\sqrt{5}$. For the estimates below that use county-level Census data we use a bandwidth of 3. Given that we have less information near the OEO cutoff with the nationally-representative NELS sample, we use a larger bandwidth (equal to 8) with those data. We chose these bandwidths in part by examining whether they produced balance on our background covariates, before we looked at results for our outcome measures of interest. We explore the sensitivity of our estimates to the choice of bandwidth in the next section. In the next version of our paper we will also present the results from cross-validation tests for optimal bandwidth.

We present standard errors below that come from analytically estimating the variance of our parameter estimate using the formula as in equation (8), from Porter (2000). While the analytic formula for the variance has changed slightly from Porter (2000) to Porter (2003), Monte Carlo simulations suggest that the 2000 formula seems to work well (in the sense that we appropriately reject a true null hypothesis only 5% of the time in simulated data) and, perhaps more importantly, bootstrapped standard errors are quite similar to those shown below.

(8) $\text{Var}[\alpha] = [\sigma^2(P300)\times cK\ ] / [(Nh)\times f(P300)]$

where $\sigma^2(P300)$ = variance of υ_c at OEO cutoff
cK = constant function based on shape of kernel
$f(P300)$ = density of P at cutoff

V. Findings

We begin this section by using the regression discontinuity method discussed above to estimate the discontinuity in Head Start participation and funding rates using data from the NELS and National Archives discussed above. The NELS estimates suggest that Head Start participation rates are 50-100% higher in the treatment than control counties, and are precisely enough estimated to be statistically significant at the conventional cutoff. The National Archives data shows a similarly large discontinuity in Head Start spending at the cutoff, although these spending data are noisy as discussed above and so this is estimate is somewhat imprecise.

We then present our results for mortality rates for children 5-9 years old over the period 1973-83. These mortality data reveal a discontinuity in mortality rates for children for causes of death that should be affected by Head Start, but we do not observe discontinuities in causes-of-death or age groups that should not be affected by the program. These mortality results reinforce the idea that OEO's grant-writing assistance led to a pronounced discontinuity in program funding, and are of course also of considerable interest in their own right as well.

The third sub-section presents our estimates for educational attainment using data from the 1990 Census and the NELS. We present evidence for discontinuities around the OEO cutoff in educational attainment among adults in the 1990 Census, although this is not true for household poverty or unemployment rates. Moreover the discontinuity in schooling is concentrated among those cohorts young enough in 1990 to have been directly or indirectly "treated" by Head Start. The NELS data also show a discontinuity in schooling outcomes.

The section closes by demonstrating that there are no discontinuities in other forms of spending or in one measure of community political mobilization (voting registration rates). We also show that our method does not find evidence for discontinuities in schooling outcomes at a "pseudo-cutoff" where there is no discontinuity in Head Start funding.

A. Head Start Participation Rates and Funding

Historical accounts note that as a result of OEO's grant-writing assistance to the 300 poorest counties in the U.S., Head Start providers in 240 of these counties (80%) received funding (GAO, 1981). By comparison 43% of all counties nationwide had some Head Start funding according to the 1968 expenditure data from the National Archives. To provide some sense of the geography of our "treatment" and "control" groups, 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 (Alabama, Arkansas, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, and Texas). These ten states also account for more than two-thirds of the 300 "control" counties (with 1960 poverty rates that rank from 301st to 600th in the U.S.), with most of the rest located in Florida, Oklahoma, Virginia or West Virginia. Put differently, most of the variation that we use to identify the effects of Head Start comes from differences in Head Start funding across very poor counties within the South.

Table 1 provides some initial empirical evidence for a discontinuity in Head Start funding across counties that is consistent with historical accounts of the program's launch. For the 347 "control" counties with 1960 poverty rates that are within 10 percentage points of the OEO cutoff from below (49.198 to 59.198), average Head Start spending per 4 year old in 1968 was equal to $134 (in 1968 dollars). For the 228 "treatment" counties with 1960 poverty rates 10 percentage points above the OEO cutoff this figure is $288 per 4 year old, more than twice as much. In 1972 per capita Head Start spending is still nearly 60% higher in the treatment than control counties.

In Figure 1 we show that this difference in Head Start funding around the OEO cutoff is driven in large part by a sharp drop-off in spending at the cutoff itself. The solid line in the top panel presents a histogram of county-level Head Start spending per capita in 1968, calculated using a bin width of 4; the bottom panel shows Head Start spending in 1972.

Figure 1 also demonstrates that the discontinuity in Head Start spending in 1968 and 1972 is mirrored by a discontinuity in Head Start participation rates among respondents to the NELS data. In both panels of Figure 1, the dashed line shows Head Start participation rates in the NELS, calculated using the same bin width as with the spending data and re-scaled to fit on the same axis. In fact the overall similarity in patterns across all counties across the three different data sources is striking, particularly given that all of these data sources are noisy (as noted above, the NARA spending data are error-ridden, and the NELS sampling scheme is intended to be representative at the national not state or county levels) and all capture different points in time.[25]

The fact that we observe a discontinuity in Head Start participation rates for NELS respondents, who would have been of Head Start age during the late 1970s, speaks to the stability of the cross-sectional differences in county Head Start resources, particularly at the OEO cutoff, that we use to identify the program's impact. The persistence of the discontinuity in county-level program funding presumably results from the flat trajectory of Head Start appropriations over time after the first few years of the program's launch (Haskins, 2004) together with a "hold harmless" rule that prevented states from experiencing declines in Head Start funding levels from one year to the next (Jones, 1979). The persistence of the discontinuity in Head Start funding means that many cohorts of disadvantaged children were exposed to the "natural experiment" that we use to evaluate the program's effects.

The top panel of Table 2 presents results from estimating the "first stage" effects of OEO's grant-writing assistance on Head Start participation rates in the NELS using the different estimation approaches discussed in the preceding section. We present these results both for the sample of NELS respondents who participated in the base year questionnaire ("by") and, perhaps more relevant for estimated outcomes shown below from follow-up waves, the slightly smaller sample of respondents who participated in the first follow up survey ("f1").

The point estimates for the discontinuity in Head Start participation rates in the NELS from the semi-parametric estimates range from 50% to more than 100% of the left limit at the cutoff (that is, the control mean). While the pattern and magnitudes of the estimates and standard errors are generally similar for the base year and follow up samples the point estimates tend to be slightly larger for the latter group, with t-statistics that range from 1.8 to 2.0 for the semi-parametric estimates. The final four columns of Table 2 suggest that the estimated magnitude of this discontinuity is not overly sensitive to using different parametric approaches instead, such as controlling for different polynomials in 1960 county poverty rates, allowing these functions to have different slopes on both sides of the OEO cutoff, and using different subsets of the county data around the OEO cutoff; these different approaches yield point estimates qualitatively similar to the Porter estimates and have t-statistics that range from 1.5 to 2.1. Figure 2 provides more intuition about these estimates by showing the semi-parametric estimates, as well as parametric estimates that use quadratics in 1960 county poverty rate calculated separately for counties on each side of the OEO cutoff.

Note that with a county poverty rate of about 60% at the OEO cutoff and a Head Start

25 Of the 1,346 counties that received any Head Start funding in 1968, 72% received Head Start funding in 1972. Of the 1,084 counties that received Head Start funding in 1972, 90% had received Head Start funding in 1968. The set of counties funded in 1968 and 1972 may not overlap perfectly because of noise in the NARA data on federal spending (noted above), termination of OEO funding for some of the original Head Start programs, and the addition of new Head Start programs in some areas.

participation rate of around 40-50% at the right-hand limit, the NELS data imply that around two-thirds or more of all poor children participate in Head Start in the treatment counties at the OEO cutoff.

The bottom panel of Table 2 and Figure 3 shows that the estimated discontinuities in Head Start spending per 4-year-old are similarly large, although the noise in these expenditure data (described above) leads these estimates to be somewhat imprecise. The combination of the numerous historical accounts of OEO's launch of the Head Start program, the statistically significant effects on Head Start participation estimated from the NELS, the similarly large discontinuities in Head Start funding estimated from the National Archives data, and the discontinuity in child mortality rates in the 1970s from causes targeted by Head Start (presented next) taken together seem to provide a reasonably strong case that the "first stage" discontinuity in program funding at the hear to four research design is real and substantial in magnitude.

B. Child Mortality

Table 3 presents evidence that the discontinuity in Head Start participation and funding described above is mirrored by a discontinuity in child mortality rates over the period 1973-83 for causes-of-death addressed by the program. (Graphs for the semi-parametric and quadratic polynomial estimates are presented in Figure 4). The first row shows the control mean and estimated discontinuities in mortality rates per 100,000 children ages 5 to 9 from tuberculosis, whooping cough, infections, polio, measles, diabetes, malnutrition, meningitis, anemias, and respiratory problems such as asthma, bronchitis or pneumonia. By 1973 all children in the 5-9 age range would have been of Head Start age after the program was in existence. We also know that most 5-9 year olds through 1983 would have been exposed to the Head Start funding discontinuity by virtue of the NELS results presented in the previous section.

Note that while most of these causes-of-death are rare today, they were much more common during our observation period. For example from 1960 to 2000 the all-age mortality rate in the U.S. declined by 80% for tuberculosis and 55% for deaths from influenza and pneumonia.[26] We assume these problems were more severe in poor parts of the South during the 1960s and 1970s since mortality rates are still higher in such areas today. For example Mississippi has an overall mortality rate that is 17% above the national average and an infant mortality rate that has hovered around 33% to 50% above the national average (US Statistical Abstracts, 1998, pp. 96, 98).

Table 3 shows that there is a proportionately very large discontinuity in mortality from the causes of death affected by Head Start for children ages 5-9 over the 1973-83 time period. The semi-parametric estimates imply discontinuities equal to 50% to 80% of the left limit (control mean) of 3.5 per 100,000, with t-statistics ranging from 1.6 to 1.9. The various parametric estimates fall in the same range. Are estimates of this magnitude plausible? To help

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26 See edcp.org/tb/pdf/tb_Incidence_Rates_USMD_60-00.pdf and www.bcbs.com/mcrg/mcrg_stats.pdf, accessed on May 6, 2005.

answer this question it is useful to note that nationwide over this time period the mean and median mortality rate across counties for these causes-of-death to children age 5-9 equaled around 2.0 and 1.8 per 100,000, respectively. Thus our estimates suggest that for children in treatment counties, Head Start essentially eliminates the "excess risk" of death from these causes (defined as the difference in mortality rates from the national average).

Some support for the idea that these mortality differences are due to Head Start comes from the fact that we do not observe a similar discontinuity in mortality rates for children 5-9 from causes that should not be affected by Head Start – namely, injuries.[27] While our estimates for causes-of-death that could have been affected by Head Start are always negative, proportionately very large and usually statistically significant or close to significant, the estimated discontinuities for injury deaths are never close to statistically significant and range in magnitude from +8.8% to -8.7% of the control mean. Similarly we do not observe a discontinuity in mortality rates for either of these cause-of-death categories for people who would have been too old to have been affected by Head Start's health services, people ages 25 and older.

C. Long-Term Outcomes from the 1990 Census

Table 4 and Figure 5 present our estimates from the 1990 Census for discontinuities in educational attainment (completion of high school or more, attendance of at least some college, or completion of college or more) for four different age groups. The cohort of primary interest consists of people ages 18-24 in 1990, who were born 1966-1972 and so all came of Head Start age (typically between 3 and 5 years old) while the program was in existence. We call this the "directly treated" group. Also of some interest are people 25-34 in 1990, born 1956-1965. Because about one-third to one-half of this cohort might have been of Head Start age after the program was in operation, we call this the "partially directly treated" group. The set of people ages 35-54 in 1990 might include parents of Head Start participants, who as noted above were also involved in the program in a variety of ways by design, and older siblings who might have benefited from spillover effects. We call this group the "potentially indirectly treated" group. People ages 55 and older in 1990 are unlikely to have been parents (much less older siblings) of Head Start participants,[28] and so we label this the "untreated" group. Figure 3 shows results from the Porter partially linear regression approach as well as a parametric model that controls for a flexible quadratic in county poverty, while Table 4 summarizes our full set of results for alternative estimation approaches.

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27 In principle the health education component of Head Start could have changed parent behavior in ways that affected injury rates. However in practice, as North (1979, p. 245) notes, "Head Start's obvious potential for health education was, unfortunately, never attained." One recent review of current health education for parents emphasizes information about nutrition and hygiene (O'Brien, Connell, and Griffin, 2004, p. 173).

28 In 1990 the oldest person who could have participated in Head Start would have been about 30 years of age (born 1960, and so five years old when Head Start went into operation in 1965). Data from the Vital Statistics for 1960 suggests that about one-half of all births that year were to parents 25 or older (55 or older in 1990), and only about one-quarter were to parents 30 or older (HEW, 1960). This means that most of the parents of Head Start participants born in 1960 or later were younger than 25 in 1960 and so of course younger than 55 in 1990.

Table 4 shows that there are relatively large discontinuities in educational attainment for the directly treated group (18-24 years old in 1990). For high school graduation these estimated discontinuities are typically between 3 and 6 percentage points across our different estimation approaches. If Head Start spending is 50% to 100% higher in the treatment than control counties our findings would imply an elasticity of high school graduation to Head Start spending of around +.05 to +.20. For college attendance the estimates are typically between 4 and 6 percentage points, about one-fifth of the control mean. We do not find a discontinuity in college completion rates for this group, which makes sense given that most people in this group will be too young to have graduated from college. In the final section of the paper we say more about the magnitude of these estimated impacts and their implications for the costs and benefits of the Head Start program.

We also find some evidence of statistically significant effects on high school completion and college attendance for the partially directly treated group (25-34) and the potentially indirectly treated group (35-54), which are typically no more than about half the magnitude of the point estimates for our directly treated group. For the partially and indirectly treated groups we also see signs of some effect on college completion. In contrast, there are no discontinuities in educational attainment for any of our schooling outcomes or estimation approaches for the age group that is too old to have been directly or indirectly "treated" by Head Start (those 55 and older in 1990). This pattern of estimates across age groups in Table 4 and Figure 5 is consistent with what we would predict if Head Start has a positive effect on educational attainment for participants and also produced positive effects for siblings and parents.

Table 5 shows that we do not find statistically significant effects on labor market outcomes. The findings for schooling in Table 4 and labor market outcomes in Table 5 may be reconciled by evidence from the NELS (shown below) demonstrating that there is a discontinuity in the probability that people are still enrolled in some form of schooling through their mid-20s.

One concern with these results comes from the possibility of migration across counties between when people were of Head Start age (3-4) and when they are observed as adults in the 1990 Census. Table 4 shows that while the discontinuities in the fraction of county residents who were born in the same state are usually not statistically significant, the point estimates tend to be somewhat larger (in absolute value) for the younger than older age groups. For example for our 18-24 year old group the discontinuity is equal to around 3-5 percentage points (not significant), compared to basically no discontinuity in this measure for our untreated group of people 55 and older. Unfortunately the Census data do not capture county of birth, nor are they directly informative about flows of migration. In any case these findings suggest that the treatment counties might be experiencing slightly more in-migration or out-migration than the control counties, especially for the younger age groups for which we observe the largest discontinuity in educational attainment.

Can across-county population mobility explain away these Census findings? If migration were random (that is, unrelated to either Head Start participation or educational attainment) the effect would be to simply attenuate our estimates, since residence in a treatment versus control county as an adult is an error-prone measure of treatment-county residence during early childhood. Selective migration could in principle produce other types of bias. Using data from the PSID on respondents whose addresses at both age 3 and 18 are observed during the period 1968-1992, 84% of those who were in one of the poorest 600 counties at both age 3 and 18 graduated from high school, compared to 64% for those who were in such a county at age 3 but not 18 (out-movers) and 88% of those who were in such a county at age 18 but not 3 (in-movers). While these PSID samples are small, they suggest that the discontinuity in population mobility across counties would need to be much larger than what is reflected by our proxy (percent born in state) in order for selective migration to explain away our findings for high school graduation. Some additional evidence against a selective migration counter-explanation for our findings comes from Table 6, which shows that there is no significant discontinuity in total county population (in thousands), percent black, or percent urban in any of the Censuses from 1950 to 1990. We return to this issue in the next section.

D. Extensions

The main findings presented above raise three primary concerns: the possibility of selective migration of people across counties with the 1990 Census data; whether there are discontinuities in other forms of social spending across the OEO cutoff; and the possibility that our results are simply an artifact of functional form assumptions underlying our different estimation approaches. We address each of these concerns in turn.

Another way to address concerns about selective migration is to replicate our estimates for long-term outcomes using the NELS, which allows us to identify county of residence for respondents in 8th grade when most students were around 13 years old, about 8-10 years after Head Start age. Using data from the geo-coded PSID for respondents whose addresses we observe at both ages 3 and 13, we find that 75% of those who were in the treatment counties "near" the OEO cutoff at age 13 (with 1960 poverty rates that ranked them 150 to 300) were living in the same set of counties at age 3. The proportion is similar for those living in control counties at age 13. While there is still in principle the possibility of some selective migration in the NELS, the scope of this problem is less severe than with the Census data.

The first few rows of Table 7 show that the results from the NELS are qualitatively similar to those from the 1990 Census presented above, using the schooling variables measured in the 2000 wave of the NELS when respondents were around 25. Using a bandwidth of 6 our semi-parametric estimator reveals a discontinuity in high school graduation rates of around 19 percentage points (nearly a third of the control mean), with a t-statistic of 2.0. Using larger bandwidths reduces the size of the estimated discontinuity (columns 2 and 3), while parametric approaches yield point estimates of about the same magnitude although are generally less precisely estimated. The patterns for college attendance (second row) are similar, which together imply a discontinuity in educational attainment of between .5 and 1.0 years of school (third row). The fact that NELS respondents in the treatment counties may also be more likely than those in the controls to still be enrolled in school (fourth row) may help explain why we do not observe positive effects on labor market outcomes such as employment or annual earnings (not shown).

Table 7 also shows that there are proportionately large discontinuities in the likelihood that the respondent has been arrested during the previous semester in the 1990 and 1992 surveys. While these findings are imprecisely estimated they at least point in the direction we would expect based on the schooling results described above. We do not find any evidence for a discontinuity in standardized achievement test scores in reading or math in 8th or 10th grade.29

In principle these NELS results could still be driven by selective migration even though we observe student's county of residence in 8th grade, much closer to when they were of Head Start age than the adult county residence measures available with the Census. Yet Table 7 shows that there are no statistically significant differences with respect to the demographic characteristics of NELS respondents, including family socio-economic status (maternal education and family income), urbanicity, or Hispanic background. The one exception may be for our measure of student race (black), where the point estimates are sometimes large in relation to the control mean. While the Census data in Table 6 shows there is no systematic difference in county percent black at the OEO cutoff there could be some difference for the NELS sample. In any case the lack of a discontinuity in parent outcomes (schooling and income) in the NELS would seem to argue against a counter-explanation for our results that rests on systematic differences over time across counties in county development efforts or the like.

This last point raises the more general concern of possible discontinuities in other forms of social spending at the OEO cutoff. In theory this seems unlikely because the decision to provide grant-writing assistance to just the 300 poorest counties seems to have been arbitrarily selected by the Head Start office within OEO rather than established as part of some considered OEO-wide policy. Empirical evidence seems to confirm this hypothesis: Figure 7 shows that the discontinuity in other forms of social spending[30] at the OEO cutoff is very small as a proportion of the control mean (less than 2%), compared to a 50% to 100% discontinuity in Head Start funding. The dollar value of the discontinuity per eligible county resident is equal to $10 or less, compared to a difference of $100 or more per 4 year old for the Head Start program.

A related concern is the possibility that because Head Start was part of OEO's Community Action Program, differences across counties in Head Start funding could generate differences in political mobilization that could in turn affect local and state spending. Community mobilization could also directly affect children's schooling if "social capital" matters for long-term outcomes. To address this concern we analyze data on the fraction of

29 One concern is that the NELS excludes children from testing if they are in special education or other reasons. This proportion is about twice as high in the baseline survey for NELS respondents in the 300 treatment counties compared to the 300 control counties (4.5% versus 2.2%).

30 This spending category includes all appropriations by HEW, HUD, the US Department of Labor, and OEO, plus some selected programs run by the Department of Agriculture such as low-income housing programs and school lunches.

county residents who are registered voters biannually from 1968 to 1980, and do not find any statistically significant discontinuities in this measure (results available upon request).

Finally, we check to see whether our results are spurious or instead may reflect the effects of Head Start is to examine whether we see discontinuities in long-term outcomes at other "pseudo-cutoffs." We must be careful in conducting this sort of specification check because as shown in Figure 1, there are significant differences in Head Start funding and participation rates across counties even away from the OEO cutoff. If we arbitrarily choose a cutoff where there is an actual program funding difference we might detect in part the effects of Head Start funding.

Table 8 shows the results of generating new estimates at a pseudo-cutoff equal to 1960 county poverty rate of 40%. This cutoff was chosen because we do not see evidence of a discontinuity in 1972 Head Start spending per capita here. We should note that we chose this cutoff before looking at our county-level Census outcome data or any of our NELS data. With our Census data only 5 out of 85 point estimates are statistically significant at the 5% level at our pseudo-cutoff; moreover the pattern of the few significant coefficients are quite different from those derived at the actual OEO cutoff in the sense that the effects are not concentrated among the directly treated group for high school completion or college attendance. The only significant point estimate in the NELS is for high school completion and is of the opposite sign to what we find at the actual OEO cutoff.

When we re-calculate all of our estimates weighting by county population, which provides us with information about the effect on the average person rather than the average county, the results for the directly treated cohort in the Census and in the NELS data are at least as strong as those shown above (in terms of the absolute magnitude of the point estimates and their size in relation to the standard errors). However the weighted estimates show somewhat more pronounced discontinuities in educational outcomes for the directly treated group at the pseudo-cutoff used in Table 8. To the extent to which this serves as a diagnostic test on our model specification, this finding provides further empirical justification for preferring the un-weighted to the weighted estimates.

VI. Conclusions

One objective of our empirical analysis is to document that the discontinuity in OEO grant-writing assistance reported in the Head Start histories translates into a non-trivial discontinuity in Head Start program funding. Our empirical evidence on this point rests on three findings. First, we demonstrate that among respondents to the NELS there is a very large and statistically significant discontinuity in Head Start participation rates, on the order of around 50-100% of the control group mean. Second, we show that there are similarly large differences in Head Start spending per 4 year old across counties, although these are imprecisely estimated. Third, there are discontinuities in children's health outcomes (mortality rates) from causes targeted by Head Start but not for other causes of death or unaffected age groups. These mortality effects are very large, equal to 50%-80% of the control mean, serving to reduce mortality rates in these counties to around the national average, and are consistent with the other results suggesting a large discontinuity in program funding. Of course these mortality results are important in their own right as well.

We then demonstrate that these discontinuities in Head Start participation rates, funding, and child mortality rates are mirrored by discontinuities in educational attainment in both the 1990 Census and the NELS. The effects in the Census are limited to those age groups that would have been exposed to Head Start. While the possibility of selective migration is the primary concern with the Census results, the NELS results are qualitatively similar and would seem to rule out selective migration as a counter-explanation for the findings: We identify county of residence for NELS respondents 8-10 years after children were of Head Start age, rather than in adulthood as in the Census, and more importantly we do not see any discontinuities in parent outcomes. Moreover the Vital Statistics data we analyze show discontinuities in mortality rates for children 5-9 years of age, just a few years after they were age-eligible for Head Start.

Two additional concerns come from the possibility of a discontinuity in other types of spending across county lines or in community political mobilization, or from concerns about functional form assumptions with the regression discontinuity research design. We show that there is no discontinuity in other forms of social spending or in voter registration rates across counties. We also show that for each of our results the findings are (usually) qualitatively similar across different estimation approaches, including a flexible semi-parametric estimator from Porter (2003), nor do we find analogous discontinuities in child mortality rates or schooling outcomes at a pseudo-cutoff where there is no discontinuity in Head Start spending.

A final potential concern with our estimates is the possibility that the grant-writing assistance provided by OEO could have produced a difference in the Head Start "production function" across counties. While we cannot definitively rule out this possibility, we find this counter-explanation unlikely in part because the PMIs sent out in the spring of 1965 were not in the field long enough to do much training. A second reason is that after Head Start grants were awarded in Spring 1965, a massive nationwide training effort was launched for all grantees that would have helped standardize the production technology across local programs.

Our findings nonetheless have three limitations for public policy purposes. The first limitation is related to the generic observation that any non-experimental findings will be subject to more uncertainty than those derived from the idealized randomized experiment. For example the fact that there may be some difference in the racial composition of the NELS sample around the OEO cutoff, even though race is balanced in county-level data in the 1950 through 1990 Censuses, highlights the challenges of both non-experimental evaluation in general and linking data on long-term outcomes to children living near our cutoff in particular. However it is important to note that the federal government has only very recently launched a randomized study of Head Start. Even if the government appropriated resources for a long-term study, which to date has not been done, evidence on long-term effects would not be available for 15 or 20 years. Non-experimental evidence on the long-term impact of Head Start is likely to be an important guide for policy for the foreseeable future. We believe that the results here are an important complement to the sibling-difference estimates presented by CT and GTC, and in fact are quite consistent with theirs along a number of dimensions. A strength of our study is that we have a clear source of identifying variation in Head Start funding.

A second limitation has to do with generalizability. Our estimates are identified off of a discontinuity in Head Start funding among the poorest counties in the South around 30 or 40 years ago. Over time schooling and health opportunities have improved, especially in the South, and so we might expect the findings presented here to provide an upper bound for the long-term effects of Head Start on a current nationally representative population of children.

Third, we will have some difficulty providing a sharp answer to the benefits per extra dollar of Head Start spending given that our estimate for spending per se is imprecise. The more precisely measured NELS estimate for Head Start participation rates are not useful for this exercise because additional funding across counties could in principle be used to increase spending per participant rather than overall participation rates.

With this final caveat in mind, we can at least provide a rough sense for how the benefits implied by our estimates might compare to the costs. For this exercise we focus on the benefits from increased earnings and reductions in and criminal offending from our estimated Head Start impact on high school graduation rates, together with the estimated effect of Head Start on child mortality rates. Our Vital Statistics estimates for children 5-9 imply an effect on overall mortality rates of about 3 per 100,000. Using a value per statistical life of $6 million, which is about the value used by the EPA and FDA (Sunstein, 2004), these mortality results would imply a modest benefit of around $180 per 4-year-old in a county. For crime, Lochner and Moretti (2004) demonstrate that each additional year of schooling reduces arrest rates by 11-16 percent (Table 10). While our NELS estimates for arrests are not statistically significant, they are large as a proportion of the control mean and the 95% confidence intervals include the sort of impact we would expect from Lochner and Moretti's results if NELS also increases average educational attainment by something around 1 year. If the social costs averted per decline in youth arrest is around $120,000 (Lochner and Moretti, 2004, Table 13), using Lochner and Moretti's estimates together with the control group's mean arrest rate for NELS respondents in 1992 imply a social savings on the order of $1,400 to $2,300 per 4-year-old. Similarly the estimates presented here do not recover an effect of Head Start on earnings but that is probably due to the fact that those groups in the Census and NELS affected by Head Start are still in their mid-20s and enrolled in school. If we use a conservative estimate for the returns to an additional year of schooling of 5 to 10% (Card, 2000), and if Head Start does in fact increase educational attainment by around .5 years, then the benefits from increased earnings (even assuming a relatively high discount rate of 6% and no annual productivity growth over time) would be on the order of nearly $9,000 per 4-year-old (results from Krueger, 2003, Table 5, multiplied by around 3). By comparison the 1972 spending difference presented above (in current dollars) is on the order of $700 per 4-year-old.

This rough benefit-cost calculation ignores both some benefits (the short-term value to mothers from subsidized child care) and costs (distortions from raising tax revenues), and more generally assumes that the estimated Head Start effects on educational attainment translate into earnings and criminal offending gains even though our data have weak power in detecting effects on these outcomes directly. In any case the findings taken together are consistent with the idea that Head Start seems more likely to pass a benefit-cost test than not, at least for the very disadvantaged populations exposed to Head Start in the 1960s and 1970s that are studied here.

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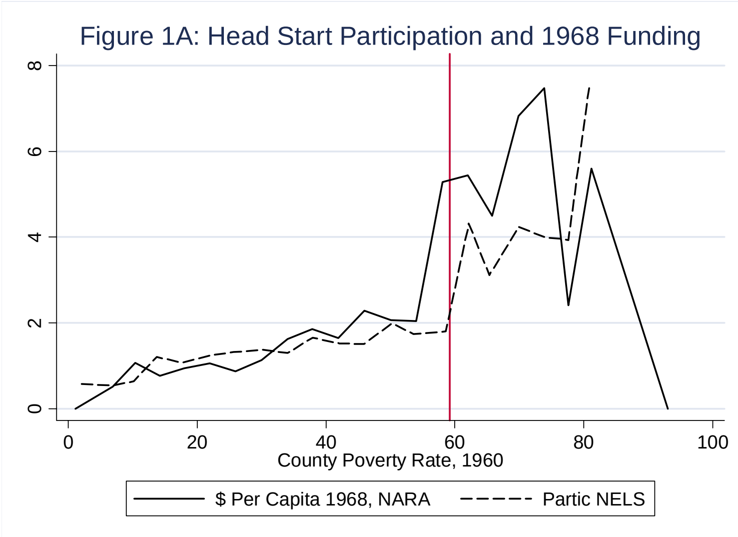

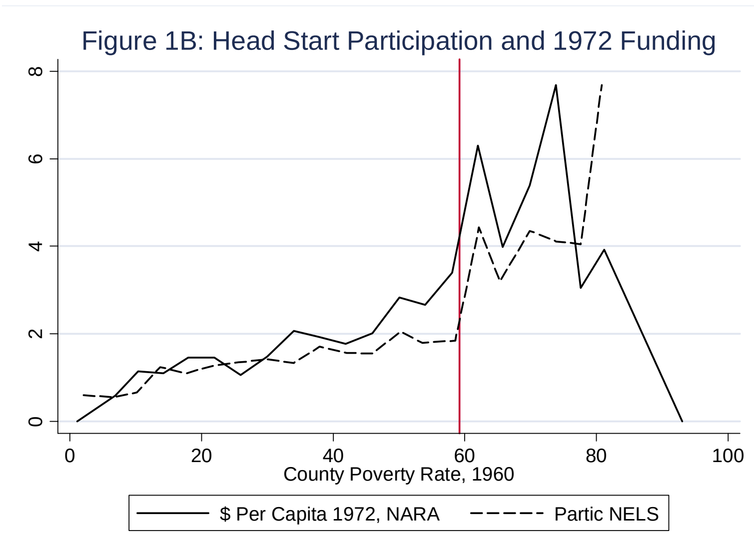

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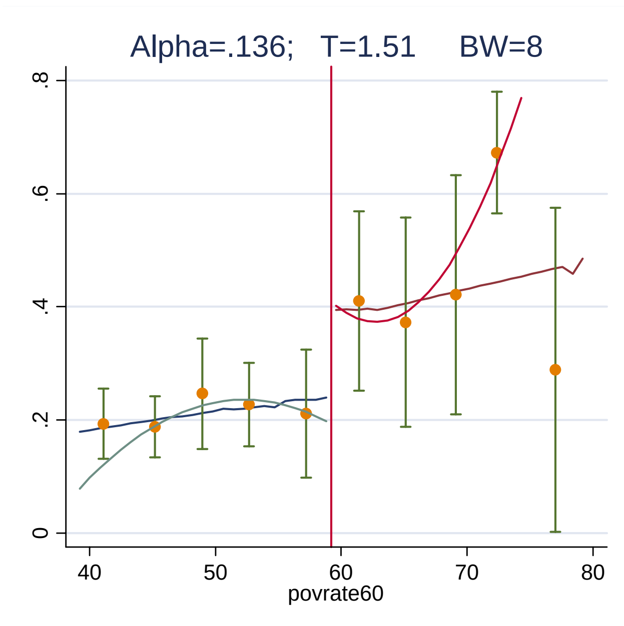
Figure 2: Estimated Discontinuity in Head Start Participation in the NELS

Panel A: Discontinuity in Head Start participation, NELS base year sample ("by")



Panel B: Discontinuity in Head Start participation, NELS first follow up sample ("f1")

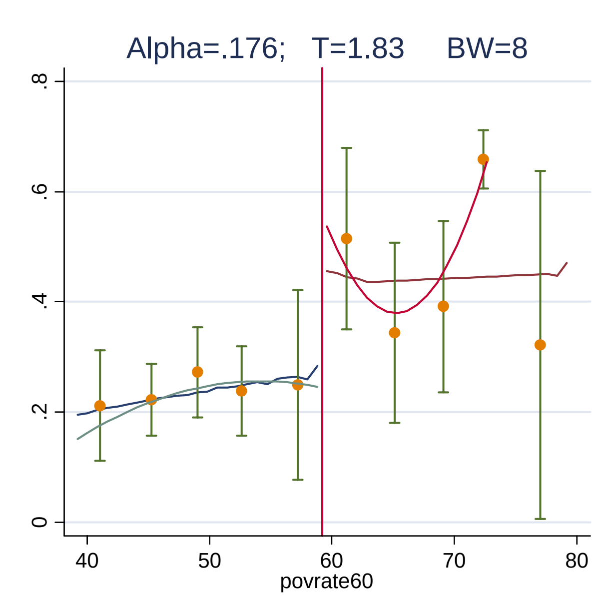
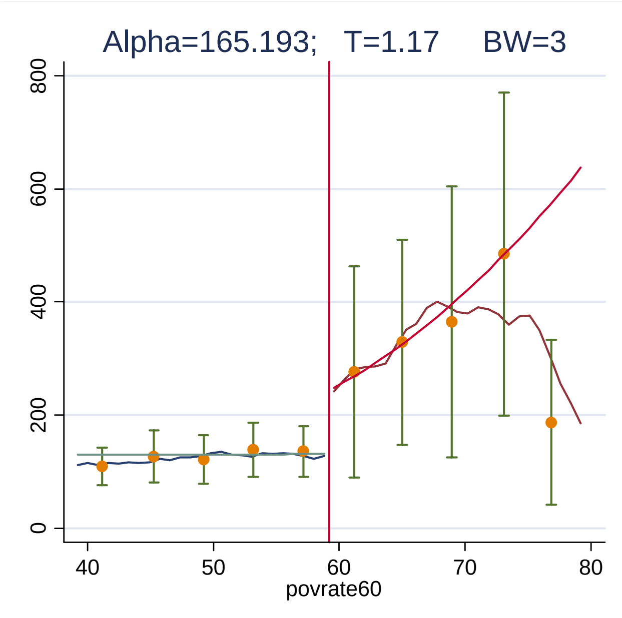


Figure 3: Estimated Discontinuity in Head Start Funding per 4 year old, National Archives

Panel A: 1968 Head Start funding per 4 year old



Panel B: 1972 Head Start funding per 4 year old

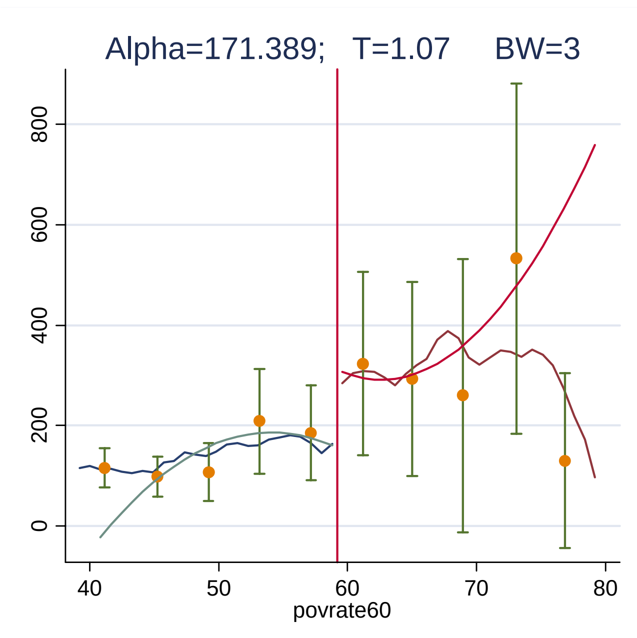


Figure 4: Mortality Rates per 100,000 from 1973-83, for Children and Adults, from Causes Affected by Head Start and from Injuries

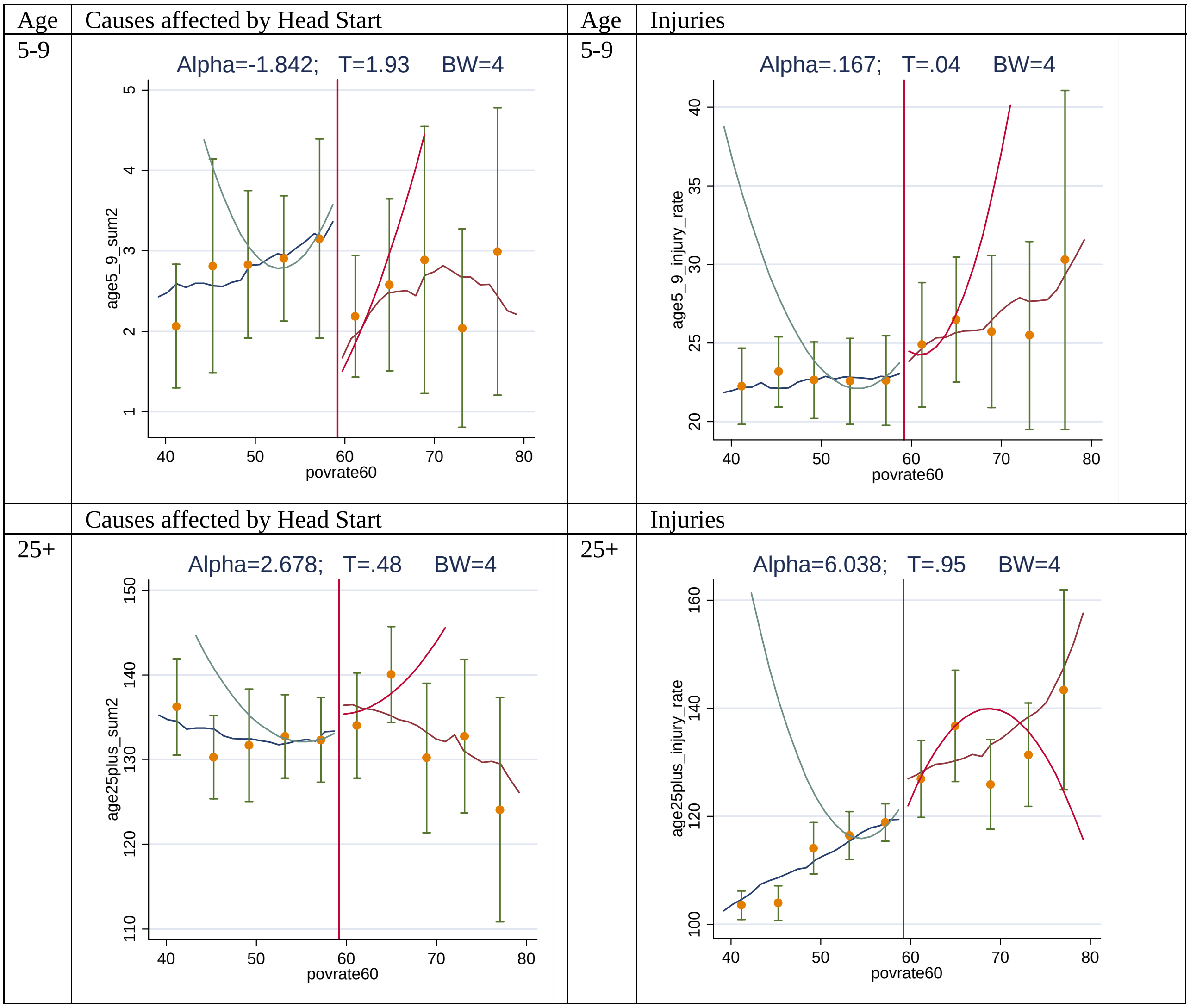


Figure 5: Discontinuity in High School Completion by Age, 1990 Census

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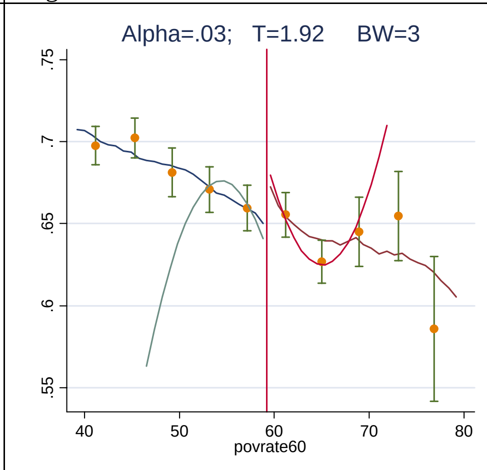
24</td><td>
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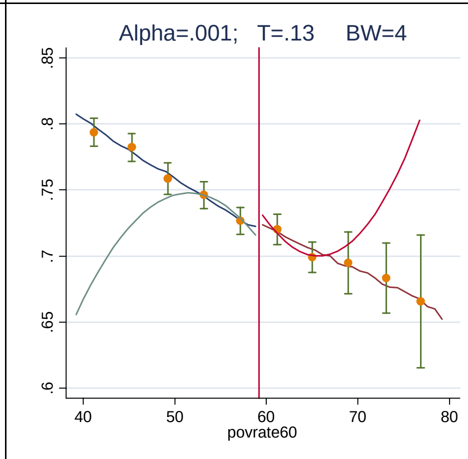
34</td><td>
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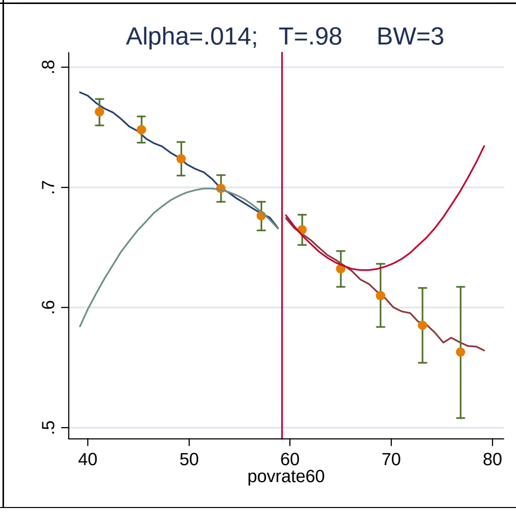
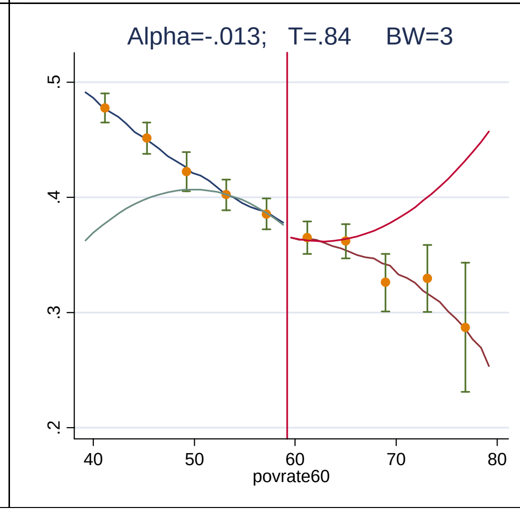
54</td><td>
</td><td>55+</td><td>
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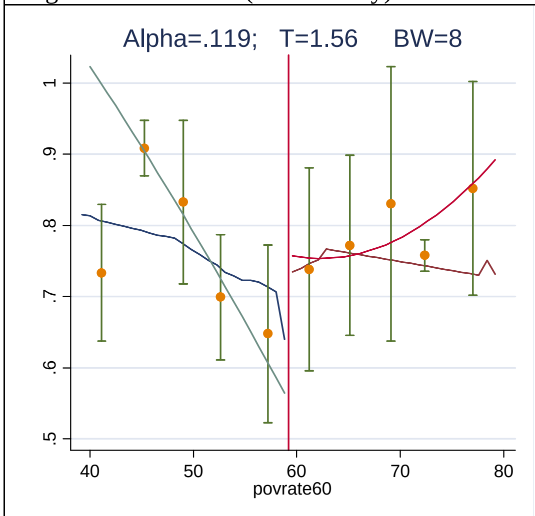
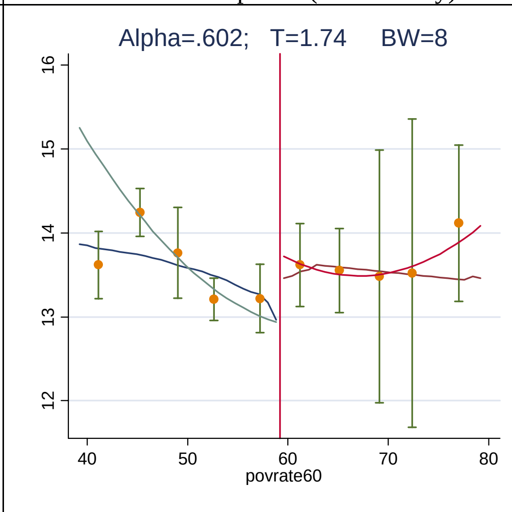
Figure 6

Discontinuity in Schooling and Arrest Outcomes, NELS

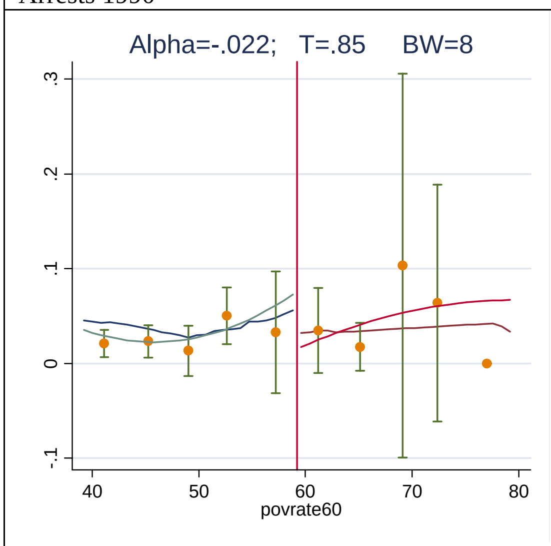
High school or more (2000 survey)



Years of school completed (2000 survey)



Arrests 1990



Arrests 1992

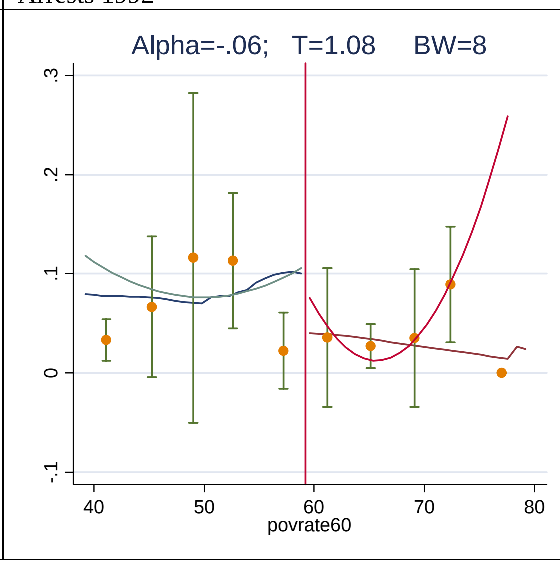


Figure 7
Discontinuity in Other Federal Social Spending, 1972 National Archives Data

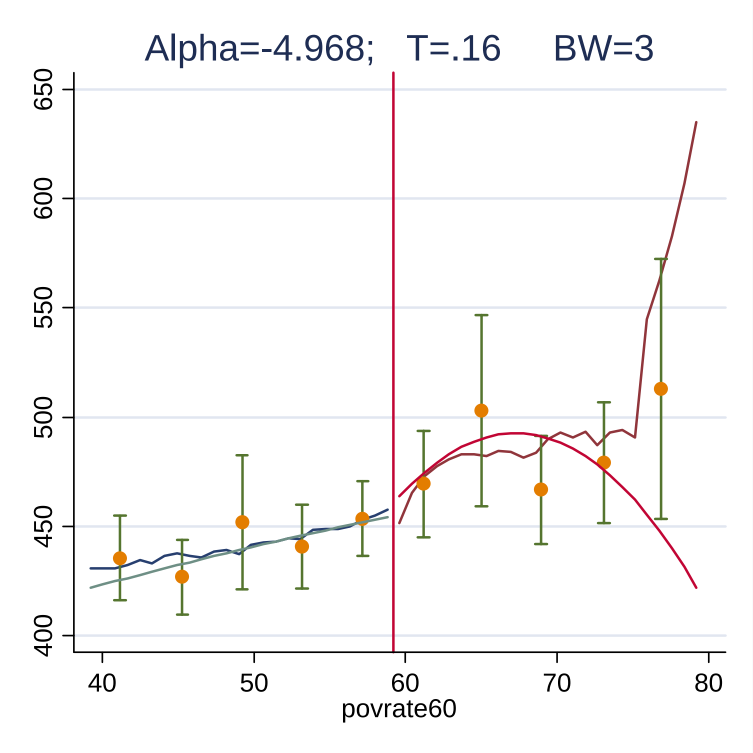


Table 1:
County Characteristics

Variable	Counties with 1960 Poverty 49.198% to 59.198% Mean	Std. Dev.	Counties with 1960 Poverty 59.1984% to 69.1984% Mean	Std. Dev.
# observations	349		228	
Head Start Spending per 4-year-old 1968	134	(277.00)	288	(915.00)
Head Start Spending per 4-year-old 1972	183	(569.00)	289	(927.00)
Other Social Spending per capita 1972	446	(128.00)	483	(167.00)
age18_24_high school or more	0.67	(0.09)	0.644	(0.07)
age25_34_hsormore	0.74	(0.06)	0.709	(0.06)
age35_54_hsormore	0.693	(0.07)	0.647	(0.07)
age55plus_hsormore	0.396	(0.09)	0.36	(0.08)
age18_24_college	0.288	(0.12)	0.258	(0.09)
age25_34_college	0.321	(0.09)	0.289	(0.08)
age35_54_college	0.324	(0.08)	0.291	(0.07)
age55plus_college	0.174	(0.06)	0.16	(0.04)
age18_24_collegecomplete	0.0295	(0.02)	0.0257	(0.02)
age25_34_collegecomplete	0.0968	(0.05)	0.0847	(0.04)
age35_54_collegecomplete	0.124	(0.05)	0.109	(0.04)
age55plus_collegecomplete	0.0714	(0.03)	0.0685	(0.02)
1990 County population	24202	(24054.00)	21371	(29799.00)
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	(1537.00)	8488	(1434.00)
NELS results				
# observations	28		17	
Head start participation ("by" weights)	0.233	(0.16)	0.388	(0.24)
Head start participation ("f1" weights)	0.244	(0.18)	0.423	(0.24)

Standard deviations in parentheses. All means are unweighted. Data from the 1990 census STF4 file, and from the NELS

Table 2: Regression Discontinuity Estimates of the Effect of Head Start assistance on Head Start Spending and participation

NELS county averages

Variable	LHS Mean	Porter			Parametric			
					Linear, same slope on both sides	Flexible Linear	Flexible quadratic	Flexible quadratic, covariates
Bandwidth		6	8	10				
Poverty Range					8	12	16	16
Number of observations with nonzero weight		64	95	125	43	53	82	82
Head Start participation ("by")	0.258	0.120	0.137	0.144*	0.175	0.165	0.217	0.178
		(0.105)	(0.090)	(0.077)	(0.146)	(0.119)	(0.142)	(0.148)
Head Start participation ("f1")	0.281	0.219**	0.176*	0.169**	0.225	0.241*	0.316**	0.292*
		(0.109)	(0.096)	(0.085)	(0.154)	(0.125)	(0.151)	(0.160)

National Archives Federal spending data

Variable	LHS Mean	Porter			Parametric			
					Linear, same slope on both sides	Flexible Linear	Flexible quadratic	Flexible quadratic, covariates
Bandwidth		2	3	4				
Poverty Range					2	4	8	8
Number of observations with nonzero weight		273	411	524	125	244	484	484
1968 Head Start Spending per capita	122.351	109.419	165.194	139.041	136.545	148.113	145.514	145.240
		(196.721)	(140.484)	(125.468)	(159.170)	(179.677)	(175.592)	(164.102)
1972 Head Start Spending, per capita	150.415	36.631	171.390	184.876	294.232*	83.543	68.374	61.495
		(219.430)	(159.446)	(144.428)	(175.390)	(196.134)	(207.948)	(198.495)
1972 other social spending, per capita	458.031	9.842	-4.969	5.826	25.447	17.217	12.945	21.654
		(31.756)	(30.146)	(26.145)	(35.196)	(29.027)	(37.456)	(27.030)

Note: Standard errors in parentheses. RD estimates of treatment effects estimate the jump in educational outcomes associated with receiving 1965 PMI assistance. RD methodology based on Porter (2000), locally weighted kernel regression, as discussed in text, with an Epanechnikov kernel. Parametric models give equal weight to observations within the range of the cutoff. The model the last column includes controls for log(1960 population), 1960 %urban and 1960 %black. For the census data, the last column also includes state fixed effects. * = p<.1; ** = p<.05

Table 3: Regression Discontinuity Estimates of the Effect of Head Start assistance on Mortality

Compressed Mortality files, 1973-1983

Variable	LHS Mean	Porter			Parametric			
					Linear, same slope on both sides	Flexible Linear	Flexible quadratic	Flexible quadratic, covariates
Bandwidth		2	3	4				
Poverty Range					2	4	8	8
Number of observations with nonzero weight		272	409	521	124	243	482	482
Ages 5-9, Head start related causes	3.523	-2.771*	-1.762	-1.842*	-2.823	-3.173**	-2.331	-1.909
		(1.482)	(1.104)	(0.954)	(2.349)	(1.494)	(1.473)	(1.505)
Ages 5-9, Injuries	24.525	2.164	-0.259	0.168	-2.145	0.025	0.607	0.291
		(4.983)	(3.962)	(3.373)	(5.722)	(4.788)	(4.960)	(4.871)
Ages 25+, Head start related causes	132.814	-0.856	2.279	2.678	10.977	3.672	1.905	1.445
		(8.152)	(6.387)	(5.560)	(10.983)	(7.862)	(8.194)	(7.203)
Ages 25+, injuries	121.753	-2.551	1.098	6.038	-4.638	0.027	-2.706	-3.689
		(8.815)	(7.274)	(6.348)	(7.969)	(7.559)	(9.106)	(6.577)

See note to Table 2 for methodology. Outcome of interest is 1-year mortality rates per 100,000 Head start related causes include deaths due to: tuberculosis, other infections, diabetes, nutritional causes, anemias, meningitits, and repriratory causes.

Table 4: Regression Discontinuity Estimates of the Effect of Head Start assistance on Educational outcomes

1990 Census

Variable		Porter			Parametric			
	LHS Mean				Linear, same slope on both sides	Flexible Linear	Flexible quadratic	Flexible quadratic, covariates
Bandwidth		2	3	4				
Poverty Range					2	4	8	8
Number of observations with nonzero weight		274	413	526	125	245	486	486
% High School Graduate, ages 18-24	0.647	0.039*	0.031*	0.008	0.062**	0.042**	0.054**	0.041**
		(0.020)	(0.016)	(0.014)	(0.023)	(0.019)	(0.021)	(0.019)
% High School Graduate, ages 25-34	0.715	0.011	0.016	0.001	0.045**	0.009	0.023	0.013
		(0.016)	(0.013)	(0.011)	(0.021)	(0.015)	(0.016)	(0.015)
% High School Graduate, ages 35-54	0.663	0.007	0.015	0.003	0.015	0.005	0.02	0.021
		(0.017)	(0.015)	(0.013)	(0.023)	(0.017)	(0.019)	(0.016)
% High School Graduate, ages 55+	0.375	-0.006	-0.014	-0.017	-0.012	-0.01	-0.007	0.01
		(0.020)	(0.016)	(0.014)	(0.027)	(0.019)	(0.021)	(0.017)
% Some College, ages 18-24	0.249	0.057**	0.042**	0.023	0.039	0.053**	0.06**	0.061**
		(0.026)	(0.021)	(0.018)	(0.030)	(0.026)	(0.027)	(0.025)
% Some College, ages 25-34	0.28	0.032	0.031*	0.015	0.033	0.029	0.033	0.031*
		(0.020)	(0.017)	(0.015)	(0.025)	(0.019)	(0.022)	(0.017)
% Some College, ages 35-54	0.282	0.035*	0.028*	0.01	0.03	0.034*	0.042**	0.039**
		(0.019)	(0.016)	(0.014)	(0.023)	(0.018)	(0.020)	(0.016)
% Some College, ages 55+	0.161	0.005	-0.002	-0.004	0.009	0.002	0.000	0.006
		(0.013)	(0.011)	(0.009)	(0.017)	(0.013)	(0.014)	(0.012)
% College completion, ages 18-24	0.022	0.003	0.004	0.000	0.000	0.003	0.003	0.004
		(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	(0.005)
% College completion, ages 25-34	0.078	0.016	0.014*	0.005	0.021*	0.016	0.019*	0.020**
		(0.010)	(0.008)	(0.008)	(0.012)	(0.010)	(0.011)	(0.010)
% College completion, ages 35-54	0.103	0.017*	0.013	0.003	0.021*	0.016	0.021*	0.017
		(0.010)	(0.009)	(0.008)	(0.012)	(0.010)	(0.011)	(0.010)
% College completion, ages 55+	0.069	0.001	0.000	-0.001	0.001	0.000	-0.002	-0.001
		(0.007)	(0.006)	(0.005)	(0.009)	(0.007)	(0.007)	(0.007)
% in same state of birth, ages 18-24	0.788	-0.045	-0.039	-0.029	-0.046	-0.043	-0.042	-0.058**
		(0.030)	(0.024)	(0.023)	(0.036)	(0.028)	(0.032)	(0.029)
% in same state of birth, ages 25-34	0.745	-0.026	-0.009	0.002	-0.044	-0.020	-0.025	-0.050*
		(0.030)	(0.024)	(0.023)	(0.035)	(0.028)	(0.032)	(0.027)
% in same state of birth, ages 35-54	0.78	-0.019	-0.005	0.003	-0.004	-0.014	-0.016	-0.039
		(0.027)	(0.022)	(0.020)	(0.035)	(0.026)	(0.029)	(0.024)
% in same state of birth, ages 55+	0.789	0.012	0.012	0.000	0.019	0.009	0.027	-0.001
		(0.033)	(0.026)	(0.023)	(0.045)	(0.032)	(0.034)	(0.027)

See note to Table 2

Table 5: Other census outcomes: employment and income

	1990 Census					
Variable			Porter			Param
	LHS Mean				Linear, same slope on both sides	Flexible Linear
Bandwidth		2	3	4		
Poverty Range					2	4
Number of observations with nonzero weight		273	411	524	125	244
% people 18-24 employed	0.545	-0.013	-0.015	-0.029	0.014	-0.013
		(0.027)	(0.022)	(0.019)	(0.033)	(0.025)
% people 25-44 employed	0.694	0.004	0.001	-0.012	0.009	0.003
		(0.022)	(0.018)	(0.016)	(0.027)	(0.021)
% people 45-64 employed	0.706	-0.007	-0.006	-0.016	0.002	-0.014
		(0.018)	(0.015)	(0.013)	(0.024)	(0.017)
% people 65+ employed	0.222	-0.011	-0.007	-0.008	-0.012	-0.012
		(0.011)	(0.010)	(0.008)	(0.015)	(0.011)
Avg income people 18-24	5895.217	-544.31	-535.065*	-571.956**	-179.166	-511.872
		(352.808)	(300.539)	(258.497)	(478.000)	(332.393)
Avg income people 25-44	12000.000	22.113	-114.06	-288.017	-39.628	-16.087
		(466.670)	(429.094)	(375.460)	(546.260)	(432.841)
Avg income people 45-64	16000.000	651.162	333.555	-35.384	1416.191**	512.878
		(581.274)	(510.340)	(447.537)	(670.430)	(542.026)
Avg income people 65+	11000.000	-144.346	-356.511	-361.836	-106.196	-425.904
		(429.382)	(385.940)	(335.083)	(550.889)	(393.486)

See note to Table 2

Table 6: Balance of covariates

1990 Census

Variable	LHS Mean	Porter			Parametric: Linear, same slope on both sides	Parametric: Flexible Linear	Parametric: Flexible quadratic	Parametric: Flexible quadratic, covariates
Bandwidth		2	3	4				
Poverty Range					2	4	8	8
Number of observations with nonzero weight		274	413	526	125	245	486	486
pop_50	20.946	3.073	1.183	1.471	-3.405	2.507	2.218	-0.603
		3.032	2.437	2.123	4.042	3.038	3.094	1.912
pop_60	20.128	1.006	0.425	0.792	-5.994	0.884	-0.069	-3.819
		3.706	2.885	2.625	5.858	3.89	4.146	2.573
pop_70	18.003	1.677	0.178	-0.092	-2.201	1.869	0.908	0.634
		3.421	2.738	2.361	3.99	3.52	3.556	2.059
pop_80	21.536	4.086	2.077	2.584	2.246	4.483	2.946	0.15
		4.093	3.242	2.897	4.839	4.158	4.199	2.731
pop_90	22.682	3.462	1.788	2.998	3.03	4.206	1.808	-0.853
		4.996	3.935	3.597	5.647	5.067	5.27	3.933
pctnonwhite_50	0.248	0.019	0.036	0.049	0.001	0.011	0.019	0.023
		0.04	0.034	0.03	0.052	0.04	0.042	0.032
pctblk_60	0.247	0.001	0.014	0.035	-0.015	-0.005	-0.004	0.015
		0.038	0.033	0.029	0.051	0.038	0.04	0.03
pctblk_70	0.204	-0.024	-0.008	0.003	-0.036	-0.03	-0.014	0.018
		0.036	0.031	0.027	0.049	0.036	0.037	0.027
pctblk_80	0.209	0.004	0.011	0.025	-0.032	-0.004	0	0.014
		0.034	0.029	0.026	0.045	0.034	0.035	0.026
pctblk_90	0.201	0.007	0.013	0.026	-0.04	-0.002	0.006	0.015
		0.034	0.029	0.026	0.045	0.034	0.035	0.026
pcturb_50	0.128	0.025	0.003	0.005	0	0.028	0.013	0.003
		0.029	0.025	0.023	0.04	0.03	0.035	0.029
pcturb_60	0.163	0.024	-0.004	-0.003	0.011	0.032	0.01	-0.004
		0.034	0.03	0.026	0.044	0.034	0.04	0.03
pcturb_70	0.19	0.003	-0.015	-0.014	0.024	0.009	0.004	0.013
		0.038	0.033	0.029	0.049	0.038	0.043	0.032

Notes: See Table 2. Population figures in thousands.

Table 7: NELS outcomes

Variable	LHS Mean	Porter (NELS)			Parametric (NELS)			
					Linear, same slope on both sides	Flexible Linear	Flexible quadratic	Flexible quadratic, covariates
Bandwidth		6	8	10				
Poverty Range					8	12	16	16
Number of observations with nonzero weight		64	95	125	43	53	82	82
At least high school grad, 2000	0.613	0.189**	0.12	0.09	0.186	0.171	0.205	0.216
		(0.093)	(0.077)	(0.076)	(0.136)	(0.114)	(0.127)	(0.134)
At least some college, 2000	0.526	0.257**	0.157*	0.159*	0.082	0.162	0.165	0.2
		(0.106)	(0.094)	(0.086)	(0.136)	(0.118)	(0.151)	(0.155)
Years of schooling, 2000	12.847	1.082**	0.602*	0.462	0.597	0.783*	0.821	1.023*
		(0.372)	(0.345)	(0.324)	(0.441)	(0.406)	(0.563)	(0.573)
Enrolled in school, 2000	0.161	0.148*	0.068	0.072	0.106	0.104	0.183	0.191
		(0.087)	(0.073)	(0.061)	(0.098)	(0.083)	(0.131)	(0.138)
read88	41.417	0.699	-0.209	-0.781	0.521	0.166	-0.106	0.437
		(1.465)	(1.459)	(1.307)	(1.972)	(1.809)	(2.595)	(2.697)
math88	39.170	-0.697	-1.282	-1.680	-1.906	-1.841	-2.388	-1.746
		(1.704)	(1.549)	(1.403)	(2.212)	(2.059)	(2.625)	(2.746)
arrests90	0.055	-0.066**	-0.023	-0.022	-0.033	-0.056	-0.061	-0.059
		(0.033)	(0.027)	(0.023)	(0.046)	(0.041)	(0.048)	(0.050)
arrests92	0.101	-0.086	-0.061	-0.061	-0.013	-0.026	-0.024	-0.051
		(0.061)	(0.056)	(0.043)	(0.086)	(0.086)	(0.125)	(0.132)
momed_years	12.981	0.266	0.267	0.113	-0.037	0.128	-0.138	0.249
		(0.289)	(0.274)	(0.231)	(0.309)	(0.270)	(0.553)	(0.535)
income87	21000.000	1702.594	-1100	-3200	593.384	-1500	-3600	3212.463
		(5787.727)	(5233.072)	(4307.888)	(8482.904)	(7107.203)	(10000.000)	(10000.000)
black	0.421	-0.214*	-0.191*	-0.117	-0.107	-0.168	-0.146	0.069
		(0.127)	(0.102)	(0.088)	(0.180)	(0.159)	(0.186)	(0.157)
hisp	0.074	0.030	0.044	0.029	-0.102	-0.017	-0.054	-0.132
		(0.105)	(0.088)	(0.079)	(0.152)	(0.118)	(0.130)	(0.124)
urban	0.019	-0.024	-0.004	0.034	-0.106**	0.004	-0.115	-0.108
		(0.065)	(0.069)	(0.073)	(0.048)	(0.096)	(0.159)	(0.147)

Table 8: Regression Discontinuity Estimates, False cutoff (at 40% poverty rate)

1990 Census

Variable	LHS Mean	Porter			Parametric			
					Linear, same slope on both sides	Flexible Linear	Flexible quadratic	Flexible quadratic, covariates
Bandwidth		2	3	4				
Poverty Range					2	4	8	8
Number of observations with nonzero weight		462	699	925	193	403	828	828
% High School Graduate, ages 18-24	0.688	0.014	0.008	0.009	0.031	0.026	0.016	0.014
		(0.018)	(0.015)	(0.013)	(0.027)	(0.018)	(0.019)	(0.017)
% High School Graduate, ages 25-34	0.79	0.023	0.009	0.012	0.038	0.035	0.023	0.019
		(0.016)	(0.013)	(0.011)	(0.023)	(0.016)	(0.016)	(0.013)
% High School Graduate, ages 35-54	0.767	0.015	-0.001	0.001	0.022	0.023	0.013	0.015
		(0.016)	(0.013)	(0.011)	(0.023)	(0.016)	(0.017)	(0.013)
% High School Graduate, ages 55+	0.475	0.031	0.017	0.014	0.020	0.031	0.027	0.022
		(0.018)	(0.015)	(0.013)	(0.024)	(0.018)	(0.019)	(0.016)
% Some College, ages 18-24	0.318	-0.003	-0.012	-0.005	0.012	-0.001	-0.008	-0.002
		(0.022)	(0.019)	(0.017)	(0.031)	(0.021)	(0.025)	(0.024)
% Some College, ages 25-34	0.379	0.029	0.007	0.012	0.038	0.040	0.024	0.024
		(0.020)	(0.017)	(0.014)	(0.028)	(0.019)	(0.021)	(0.018)
% Some College, ages 35-54	0.393	0.012	-0.005	0.001	0.005	0.018	0.005	0.013
		(0.018)	(0.016)	(0.014)	(0.026)	(0.018)	(0.020)	(0.017)
% Some College, ages 55+	0.204	0.013	0.004	0.006	-0.006	0.015	0.006	0.010
		(0.013)	(0.011)	(0.010)	(0.017)	(0.013)	(0.014)	(0.012)
% College completion, ages 18-24	0.03	0.011	0.009	0.009	0.013	0.012	0.009	0.008
		(0.005)	(0.004)	(0.004)	(0.006)	(0.004)	(0.005)	(0.005)
% College completion, ages 25-34	0.115	0.013	0.008	0.005	0.010	0.016	0.011	0.007
		(0.010)	(0.010)	(0.009)	(0.015)	(0.010)	(0.013)	(0.012)
% College completion, ages 35-54	0.15	0.004	0.000	-0.001	-0.009	0.006	0.002	0.003
		(0.011)	(0.010)	(0.009)	(0.016)	(0.011)	(0.013)	(0.012)
% College completion, ages 55+	0.078	0.001	0.001	-0.001	-0.012	0.002	-0.001	0.000
		(0.007)	(0.006)	(0.006)	(0.010)	(0.007)	(0.009)	(0.007)
			NELS					
Bandwidth		6	8	10				
Poverty Range					8	12	16	16
Number of observations with nonzero weight		201	277	349	111	181	243	243
At least high school, 2000	0.841	-0.073	-0.017	-0.016	-0.199**	-0.115**	-0.272**	-0.271**
		(0.058)	(0.051)	(0.045)	(0.070)	(0.058)	(0.076)	(0.077)
At least some college, 2000	0.742	-0.047	-0.003	-0.042	-0.045	-0.042	-0.082	-0.094
		(0.059)	(0.050)	(0.044)	(0.073)	(0.061)	(0.079)	(0.080)
Years of schooling, 2000	13.906	-0.010	0.174	0.007	-0.132	-0.031	-0.392	-0.473
		(0.248)	(0.208)	(0.178)	(0.326)	(0.255)	(0.333)	(0.336)
Still in school, 2000	0.129	0.05	0.037	0.02	0.035	0.044	0.061	0.059
		(0.046)	(0.039)	(0.034)	(0.058)	(0.046)	(0.060)	(0.061)
read88	45.305	-0.242	0.005	0.023	-1.311	-0.387	-2.262	-2.257
		(1.081)	(0.929)	(0.789)	(1.492)	(1.124)	(1.433)	(1.453)
math88	43.671	-0.334	-0.361	-0.301	-0.649	-0.499	-1.30	-1.218
		(1.197)	(1.008)	(0.862)	(1.582)	(1.245)	(1.595)	(1.598)

See note to Table 2