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

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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. We show that the result is a substantial difference in Head Start funding and participation rates in those counties just above and below OEO's poverty-rate cutoff for technical assistance, differences that seem to have persisted through at least the 1970's. This discontinuity in Head Start funding and participation are associated with discontinuities in educational attainment.

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 childhood education programs is motivated in part by the fact that achievement gaps between rich and poor children, and between minorities and whites, are observed even before children start school, and by the possibility that human capital investments may be especially productive when made early in the life cycle.[1] Head Start is also of interest to economists because the program may reduce the external costs associated with a variety of anti-social behaviors that may have their antecedents in childhood poverty and early educational failure (Currie, 2001).

Controversy about whether Head Start produces lasting benefits for participants has surrounded the program since its inception. The first claim about "fade out" of Head Start's benefits was made in 1966 (Wolff and Stein, 1966, cited by Zigler and Meunchow, 1992). The general principle that early childhood intervention can produce lasting benefits is suggested by a number of randomized experimental 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 model 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]

To date the best available research on the longer-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). These sibling differences suggest that the program may have long-term effects on educational outcomes and achievement scores for whites, while reducing criminal involvement among African-Americans.[3] These papers substantially improve upon previous non-experimental evaluations by controlling for unmeasured family fixed-effects that may be associated with Head Start 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

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1 For evidence on early gaps in test scores see for example Phillips, Crouse and Ralph (1998), Phillips et al. (1998), and Fryer and Levitt (2004). For a discussion of how the rate of learning may change over the life course see Entwisle, Alexander and Olsen (1997) and Shonkoff and Phillips (2000), and more generally on the malleability of cognitive and non-cognitive skills at different ages see Carniero and Heckman (2003).

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-experimental regression that conditions on only a fairly basic set of socio-demographic characteristics, and so may confound the CPC program's impact with unobserved individual attributes associated with program participation.

3 On the other hand, using cross-section data from the National Longitudinal Survey of Youth 1997, Aughinbaugh (2002) does not find much 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.

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). A more pessimistic assessment of Head Start's long-term effects has contributed to the Bush Administration's push for, among other things, a shift in the program's focus from "comprehensive services to intellectual development" (Haskins, 2004).

The present paper provides new evidence on the long-term effects of the Head Start program as it was implemented in its original form – with a focus on comprehensive service delivery to poor children. Our paper complements those by CT and GTC by identifying Head Start's impacts using a very different 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 many 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). We demonstrate that the result is substantially higher Head Start funding in "treatment" counties with 1960 poverty rates that place them among the 300 poorest in the country – what we will call the "OEO cutoff" – compared to "control" counties with poverty rates just below this cutoff.

We show that the discontinuity in county-level Head Start funding around the OEO cutoff is mirrored by discontinuous changes in educational attainment in the 1990 Census, but not for labor market outcomes such as poverty or unemployment rates. Identification of the Head Start effect here comes in part from the assumption that potential outcomes vary smoothly around the OEO cutoff. This assumption seems plausible in our application given that 1960 county poverty rates were determined well before the War on Poverty was launched by President Lyndon Johnson, and so could not have been manipulated in response to possible funding advantages. Moreover as we demonstrate below, it appears that the arbitrary cutoff that OEO used for Head Start grant-writing assistance was not used to allocate funds for other federal social programs. 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).

Arguably the main challenge to these findings is from the possibility of selective migration: While the 1990 Census provides information on where people were living as teens or adults in 1990, the Census does not report on the counties in which people were living when they were of Head Start age or even their county of birth. County-level measures show some evidence for modest differences in population mobility around the OEO cutoff. One way to determine whether selective migration drives our results is to disaggregate the 1990 Census data by age, since in 1990 only some birth cohorts will have been young enough to be exposed to Head Start. We show that all of the estimated effect on educational attainment is concentrated among those birth cohorts that were young enough to have been "treated" by Head Start, either directly as program participants or indirectly as siblings or parents of participants. We find no evidence of schooling effects for cohorts too old to have been even indirectly exposed to Head Start. We also find no discontinuity in the population age distribution, suggesting that any selective migration that did occur does not appear to differ across age groups. (The next version of this paper will examine direct measures of population mobility by age within counties).

As an alternative way to address the problem of selective migration we replicate our results using data from the National Education Longitudinal Study of 1988 (NELS), which provides data through age 25 on a nationally representative sample of children who were in 8th grade in 1988. The strength of the NELS over the 1990 Census is that we can identify the county of residence for NELS respondents in 8th grade, around 8 to 10 years after these children would have been of Head Start age, which would seem to leave less room for selective migration than with the Census data where we only identify county of residence during adulthood.

The findings from the NELS are remarkably similar to those from the 1990 Census. We show that there is a discontinuity in Head Start participation rates for NELS respondents around the OEO cutoff; since NELS respondents would have been of Head Start age during the late 1970s, one implication is that the discontinuity in Head Start funding created by the program's launch in 1965 appears to have persisted for some time. We also show that there is a discontinuity in educational attainment around the OEO cutoff for NELS respondents, but not for labor market outcomes or achievement test scores. Moreover, we do not find any evidence for a 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.

Finally, as an additional specification check 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 the research design. Of course in the absence of random assignment there will inevitably be questions about any non-experimental study. While the federal government has in fact sponsored a recent randomized evaluation of Early Head Start, which extends Head Start services to pregnant women and children under age 3, this study will only provide information on short-term outcomes and so cannot answer questions about the program's long-term impacts (see Krueger, 2003, p. 29, and Haskins, 2004). In the meantime non-experimental findings such as those presented here may provide useful guidance to policymakers in this area.

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.

III. 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.[4] 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.

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 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.[5] In Section V we demonstrate that the decision to target grant-writing assistance to the poorest 300 counties produces a discontinuity in county-level Head Start program funding and participation rates.

How might Head Start affect long-term outcomes? A large literature documents differences among children in cognitive skills across race and social-class lines even before children start school (for example Phillips et al., 1998, Fryer and Levitt, 2004). Because learning

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4 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).

5 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).

is a cumulative process, these initial achievement differences 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.

From the beginning, Head Start administrators argued that the non-academic gains from the program may be at least as important as any changes in educational outcomes. Particularly relevant from a societal perspective may be the program's impacts on criminal behavior, given the enormous costs of crime to society – estimated to be on the order of $1 trillion per year (Anderson, 1999).[6] Head Start's effects on delinquency and crime may be non-trivial, given research from criminology that around 6 percent of each birth cohort – presumably drawn disproportionately from the set of low-income families eligible for Head Start – commits 50 to 60% of the total criminal activity of that cohort (Tracy, Wolfgang and Figlio, 1990). Given that aggressive children are more likely to become violent teens and adults (Reiss and Roth, 1993), Head Start could reduce long-term rates of criminal activity by improving socialization of young children. Of course any effect of Head Start on long-term educational outcomes may also help reduce anti-social behavior by increasing the opportunity costs of crime.

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 noted above, Head Start was originally part of OEO's Community Assistance Program, which was intended in part to provide jobs to poor families and involve them in the administration of local anti-poverty programs. As a result, from the first year of Head Start's existence many of the parents of children were employed in Head Start centers – up to 47,000 during the first year of the program (Zigler and Valentine, 1979, p. 69). In addition Head Start involved up to another 500,000 part-time volunteers to run the program, many of whom may have been parents of participants, and more generally explicitly included a focus on helping all Head Start parents and improving 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

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[6] Since aggressive children are more likely to become violent teens and adults (Reiss and Roth, 1993), any effects of Head Start on children's behavior may translate into long-term reductions in crime. Improvements in parenting practices that result from Head Start may also have some desirable effect on later criminal behavior (Loeber and Southamer-Loeber, 1986, Buka and Earls, 1993). Any improvements in long-term educational and economic outcomes may indirectly reduce anti-social behaviors by increasing the opportunity costs of crime.

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).[7] The alternative possibility is that OEO targeted grant-writing assistance using data on the proportion of families in the county 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.[8]

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.[9] The accuracy of these data are something less than perfect in part because these spending figures were reported to OEO by Executive Branch agencies through a special OEO request; the incentives for the agencies to do a careful budget accounting to assist

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

8 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.

9 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).

OEO are not clear. Moreover the electronic data on file with NARA have some other obvious errors (such as non-numeric characters included among some spending variables) and are poorly documented.[10] 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. A third reason that these data may have some measurement error at the county level is that in some cases federal spending by Executive Branch agencies 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. In any case, in addition to constructing a measure of Head Start spending we create variables for spending on other social programs.[11]

Our primary data source on long-term outcomes comes from county-level data from the 1990 Census STF4 files, which also report information for selected outcomes disaggregated for selected age groups. The outcomes that we examine below include educational attainment (high school completion or more; attendance of some college; college completion or more); unemployment; and poverty status. The next version of this paper will also examine criminal activity by using county-level data from the FBI's Uniform Crime Report (UCR) system, which compiles crimes reported by victims to police and then submitted to the FBI.[12]

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

10 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.

11 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 through the Department of Health, Education and Welfare, excluding those made through Head Start.

12 While analysis of these data is not included in the present draft, we are working to assemble UCR data for 1989-1991, including data on arrest rates by age to facilitate the same sort of difference-discontinuity design as implemented with the Census data. Of course the problems with the UCR are well known, and include differences in how crimes are defined and recorded across areas and over time, as well as variation in victim reporting of crimes to the police. Other problems come from incomplete reporting of local law enforcement agencies of crime in a given year, and the limitations of the imputation methods used by the FBI and other national agencies in adjusting for incomplete reporting. These imputation problems are particularly severe when using UCR data measured at the county level (Maltz, 1999, Maltz and Targonski, 2002).

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 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.[13] 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). Our descriptive and main findings below are all calculated using these sampling weights.

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.[14] 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

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[13] 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).

[14] 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.

the school that each respondent attended in 8th grade in 1988.[15]

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.[16] We also focus on self-reported arrests collected by self-administered pencil-and-paper questionnaires in the 1990 and 1992 interviews (U.S. Department of Education, 1994).[17] Students in school are asked about arrests during the past academic term, while dropouts are asked about the last academic term spent in school.[18] 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.[19]

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

15 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.

16 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).

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

18 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.

19 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.

with our NELS and county-level data.[20]

IV. Empirical Strategy

In Section V of the paper we show that the particular features of OEO's launch of Head 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, employment, earnings or criminal activity. 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 and UCR crime 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 and UCR data, (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

20 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.

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 of attending Head Start. One problem is that (as discussed below) across-county variation in funding may influence spending per participant as well as overall participation rates. 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).[21] 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.

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. We note that 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

21 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 of participating children learn about new parenting skills that they then share with others within their social networks.

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.

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 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).

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

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

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

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 Yc at P300 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:

(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 2. Given that we have less information near the OEO cutoff with the nationally-representative NELS sample, we use a larger bandwidth (equal to 6) 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) $Var[\alpha] = [\sigma^2(P300)\times cK\,] / [(Nh)\times f(P300)]$

where $\sigma^2(P300)$ = variance of υ_c at OEO cutoff
cK = complicated 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 funding and participation rates. We then show that this discontinuity is mirrored by 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 results from the NELS data are qualitatively similar, with discontinuities in educational attainment but not other outcomes such as employment or earnings, or achievement test scores.

A. Discontinuity in Head Start Funding and Participation Rates

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 NARA.[22] 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 349 "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 capita in 1968 was \$2.21 (in 1968 dollars). For the 228 "treatment" counties with 1960 poverty rates 10 percentage points above the OEO cutoff this figure is \$4.70 per capita, more than twice as much. In 1972 per capita Head Start spending is still about 60% higher in the counties with 1960 poverty rates 10 percentage points above the OEO cutoff compared to those with rates 10 points below the cutoff.

In Figure 1 we show that this difference in average per capita Head Start spending 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

22 Nationally, Head Start funding appears to be disproportionately concentrated among the most populated counties: While only 43% of all counties in the U.S. received some Head Start funding in 1968, fully 83% of the American population in 1968 was living in counties that received some Head Start funding.

points in time.[23]

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.

Table 2 and Figure 2 present the results of estimating the "first stage" effects of OEO's grant-writing assistance on Head Start funding and participation rates using the different estimation approaches discussed above in Section IV. The top panel of Table 2 shows that the point estimates for the discontinuity in Head Start spending per capita in 1968 or 1972 are about as large as in the raw data from Table 1 and Figure 1 – equal to 60-140% of the value of the control mean at the cutoff (that is, the left limit) – although the standard errors are somewhat large, presumably driven in part by the noise in these data. Table 2 also suggests that the estimated magnitude of this discontinuity is not overly sensitive to our choice of estimation approach, 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, controlling for other factors that vary with 1960 county poverty rates non-parametrically using the Porter partially-linear regression approach, and using different subsets of the county data around the OEO cutoff. The top panel of Figure 2 provides more intuition about these estimates by showing the Porter estimates calculated with a bandwidth of 2, as well as parametric estimates that use quadratics in 1960 county poverty rate calculated separately for counties on each side of the OEO cutoff.

The second panels of Table 2 and Figure 2 show that we estimate about a 10 percentage point difference in Head Start participation rates for NELS respondents at the OEO cutoff, which is equal to about two-thirds of the participation rate observed among NELS respondents in the control counties just to the left of the OEO cutoff. As with our results for Head Start spending, the estimated discontinuity in Head Start participation rates does not appear very sensitive to our particular choice of estimation approach although the standard errors for our point estimates are often somewhat large. Note that with a county poverty rate of about 60% at the OEO cutoff and a Head Start participation rate of around 25% (at the right-hand limit), the NELS data imply that somewhere around 40-50% of all poor children participate in Head Start in the treatment counties at this cutoff.

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23 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.

The bottom panel of Figure 2 shows that there is no detectable discontinuity in participation in other forms of preschool. Comparisons between panels B and C also suggest that our estimates below may be identifying the effects of increasing enrollments in Head Start compared to the counterfactual of time with parents or other forms of informal care, rather than center-based child care or other more formal preschool education programs.

B. Long-Term Outcomes from the 1990 Census

Table 3 and Figure 3 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 some older siblings as well as parents of Head Start participants. Head Start might produce positive spillover effects for siblings by improving parenting practices or household resources (Currie and Thomas, 1995), and has also been argued to have positive effects on parents as well, including the possibility of employment with the program. For both of these reasons we term people 35-54 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,[24] 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 3 summarizes our full set of results for alternative estimation approaches.

Table 3 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 4 percentage points across our different estimation approaches, equal to around 5 or 6 percent of the graduate rate for this cohort in control counties just to the left of the OEO cutoff. For college attendance the estimates are typically between 4 and 5 percentage points, about one-fifth of the left-hand (control) limit at the OEO cutoff. 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

24 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.

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, we never find statistically significant 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 3 and Figure 3 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.

A counter-explanation for our results is that the jump in educational attainment at the OEO cutoff reflects the influence of other factors, such as a discontinuity in selective migration out of these counties. Because we do not observe discontinuities in schooling for our untreated cohorts, this selective migration story would have to apply only to those people young enough to have been affected by Head Start but not for older people within these counties. Nevertheless those who are skeptical that Head Start could have effects on those not directly treated by Head Start (that is, our partially directly treated and indirectly treated groups) might conclude that the schooling discontinuities estimated for these age groups provide evidence for the influence of some other confounding factors.

If we assume that the discontinuity in schooling outcomes for the partially directly treated and indirectly treated groups is entirely due to unmeasured factors, and that the influence of these factors are the same on the directly treated group, then we can partial out the effect of such factors by focusing on the discontinuity in the difference in outcomes across age groups within counties across the OEO cutoff.[25] Table 4 summarizes the results of this "discontinuity in differences" estimation approach. For the contrasts between our directly treated group and the partially treated or indirectly treated groups, the point estimates are on average about half the size of those shown in Table 3. Given that the main effect for our oldest untreated group (55 plus) was around zero in all cases, contrasting the directly treated group to the untreated group yields estimates for the former that are of about the same magnitude as the main level effects in Table 3.

Table 5 shows that we do not observe any statistically significant discontinuities in the 1990 Census for the other outcomes that are reported separately for different age groups in the public-use STF4 Census files – living in poverty or below twice the poverty line, and unemployment rates. The poverty measure in particular may be problematic for our application

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25 Let Y_{ac} represent the average outcome of cohort (a) within county (c). Let $\Delta Y_c = Y_{ec} - Y_{nc}$ represent the difference in average outcomes between a cohort that was exposed to Head Start, that is, of Head Start age after the program was in operation, Y_{ec}, and the average outcome of a non-exposed cohort who were 3-5 years of age before Head Start began in 1965, Y_{nc}. In this case our regression discontinuity comes from choosing the value of β that minimizes the sum of squared deviations between our new dependent variable and our non-parametric estimate of $m(P_c)$, from the estimating equation: $(\Delta Y_c - G_c\beta) = m(P_c) + \eta_c$

since this reflects household rather than individual income, so for example differences across counties in individual earnings could be offset in household poverty figures by differences in household living arrangements.

Arguably the primary concern with the results from the 1990 Census data is the potential problem of selective migration. The Census data is vulnerable to this problem because we only observe where Census respondents were living as adults in 1990, not when they were of Head Start age as children.

Table 6 presents mixed evidence on the degree to which migration across counties may be a problem in practice with our 1990 Census estimates. The first row of Table 6 provides some evidence that the fraction of 1990 county residents who lived in the same county five years ago may be a bit lower in the treatment than control counties; while this result is somewhat sensitive to our choice of estimation approach, this finding provides at least some suggestive evidence that the treatment counties may be experiencing slightly more in-migration than are the control counties (qualitatively similar findings are suggested by the 1970 and 1980 census data). On the other hand the magnitude of this effect is quite small. Moreover this sort of migration difference does not appear to be different across age groups, as evidenced by the fact that we do not observe discontinuities in the share of each county's population that is in each of our age groups, and so might be taken care of with our discontinuity-in-differences estimates above. The next version of our paper will test this also by examining the fraction of each separate age group that was within the same county 5 years ago.

Another way to test for selective migration with the Census data is to examine whether we observe a discontinuity in basic county demographic characteristics at the cutoff. Table 7 shows that the percent black and percent urban remain balanced for treatment and control counties at the OEO cutoff from the 1950 through 1990 decennial censuses.

C. Results from the NELS

Another way to address concerns about selective migration is to replicate our estimates using data from the NELS, which allows us to identify county of residence for respondents in 8th grade, about 8 to 10 years after students would have been of Head Start age. While there is still the possibility of some selective migration in the NELS, the scope of this problem is presumably less severe than with the Census data. The fact that we observe a discontinuity in Head Start participation rates for NELS respondents on the basis of their 8th grade county of residence provides some support for this view.

Figure 4 shows that the results from the NELS are qualitatively similar to those from the 1990 Census presented above. The two panels of Figure 4 show that there are large discontinuities in educational attainment at the OEO cutoff in the 2000 wave of the NELS survey, when most respondents would have been around 25 years of age. The discontinuities in high school completion and college completion (in favor of the treatment counties) equal 15 and 13 percentage points, respectively, which are statistically significant at the 10 percent cutoff.

The NELS and Census data are also consistent with one another in revealing no statistically significant discontinuities in labor market outcomes, as seen in the two bottom panels of Figure 4. We return to the apparent discrepancy in findings for education versus labor market outcomes in the final section of the paper.

Consistent with previous findings that early childhood interventions may have more pronounced effects on behaviors than cognitive skills, the top two panels of Figure 5 shows that there are no statistically significant discontinuities at the OEO cutoff in 8th grade reading or math achievement scores in the NELS, and in fact the point estimates are of the opposite sign of what we would expect if Head Start increased test scores. In contrast the point estimates for self-reported arrests in the 1990 and 1992 NELS surveys are in the direction of a Head Start effect to reduce long-term criminal activity. Given that schooling and criminal activity are negatively related (Lochner and Moretti, 2004), we would expect Head Start to reduce arrests if the program also increases educational attainment. Interestingly the NELS point estimates for arrest probabilities are consistent with what we would expect given our findings for schooling in the NELS and Lochner and Moretti's (2004) estimates for the effects of schooling on crime, although our NELS arrest findings are not statistically significant.[26]

In principle these NELS results could still be driven by selective migration, even though we observe student's county of residence in 8th grade, "only" 8-10 years after they would have been of Head Start age. Yet in Table 8 we show that there are no statistically significant differences at the OEO cutoff with respect to the demographic characteristics of NELS respondents, either for the base year respondents or those who were surveyed during the 2000 wave of the NELS. Perhaps most importantly is our finding that there are no differences with respect to parent characteristics such as maternal education or log family income. It becomes more difficult to imagine a story in which the discontinuities in children's schooling outcomes is driven by selective migration, but these differences in youth outcomes are then not mirrored by differences in the characteristics of the parents of these children.

C. Specification and Sensitivity Checks

One natural concern is the possibility that the federal government may have disproportionately directed to the 300 poorest counties funding for a variety of other social programs as well beyond Head Start. In this case we will not be able to distinguish between the effects of Head Start funding and the effects of funding for other programs. Yet Figure 6 shows

26 Lochner and Moretti (2004, p. 175) find that each additional year of schooling reduces arrest rates by 11 to 16 percent (Table 10). In 1990 NELS respondents were about 15 years of age. In 1997 (the earliest year for which data were conveniently available) this age group had an arrest rate of about 0.1 per capita. If we interpret the top panel of Figure 5 as suggesting an increase in educational attainment of around four years at the OEO cutoff for about 15% of the sample, then the implied reduction in arrest probabilities of about 3 percentage points in the bottom left panel of Figure 6 would seem to be consistent with the Lochner and Moretti estimate, given that at least part of their estimate comes from reductions in the intensive rather than extensive margin of criminal offending and that Head Start is likely to enroll many of the 6% of each birth cohort that accounts for about 50% of each cohort's total criminal activity (Tracy, Wolfgang and Figlio, 1990).

that the discontinuity in federal funding for other social programs is extremely small both in relation to the estimate's standard error and as a proportion of the control county mean at the left limit for the OEO cutoff. Specifically, the discontinuity in other forms of federal social spending in 1972 is equal to $3.60 in 1972 dollars, less than 1% of the value of the left limit.

Another way to 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 9 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. We find that only 1 out of 21 point estimates is statistically significant at the 5% cutoff; 3 out of 21 are significant at the 10% cutoff. Moreover the pattern of these significant point estimates are quite different from those derived at the actual OEO cutoff. The only significant point estimate in the NELS is for high school completion and is equal to negative 8 percentage points ($p<.10$), the opposite of the positive schooling effect that we find at the actual OEO cutoff. In the Census data the few statistically significant differences that do arise are, unlike at the OEO cutoff, not concentrated among the directly treated group or among our high school completion or college attendance measures.

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 9. 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.

Finally, an alternative way to address the possibility of selective migration is to draw on the geo-coded version of the PSID. As discussed above, the PSID is not useful for estimating the effects of Head Start because the sample appears to include a "bad draw" with respect to Head Start enrollment rates in counties with 1960 poverty rates just above the OEO cutoff. However we can aggregate the PSID data up further and exploit the longitudinal address information to explore mobility patterns. Of those PSID respondents for whom we have address data at both age 3 and 18 between 1968 and 1992, 71% were in the same county at ages 3 and 13 (the first age at which we capture addresses in the NELS) and 66% were in the same county at 3 and 18. These figures are only slightly higher for those people living in the poorest counties. About 60 to 65% of people who were living in the poorest 600 counties at age 13 or 18 were living in the same counties when they were of Head Start age (3). More generally movers and stayers appear to have quite similar outcomes on average, at least in the national sample; larger differences are observed in the poorest counties, although the sample sizes here are quite small. Within our national PSID sample of 10 birth cohorts, age is not a significant predictor of mobility, suggesting that our discontinuity-in-differences estimator that focuses on within-county across-age group differences may take out most of any confounding effect from selective migration.

VI. Conclusions

In this paper we use data from the 1990 census and the NELS to examine the long term impact of the Head Start program. In particular, we exploit a discontinuity with regard to the 1960 poverty rate in the federal government's provision of Head Start grant writing assistance. We document that counties just above the threshold had larger levels of Head Start funding and head start enrolment rates. We also document that there is a corresponding discontinuity in some important long run outcomes, mainly educational attainment. However, the increased Head Start funding does not appear to lead to improved test scores or to improved employment outcomes.

Several considerations suggest that these results are reasonable. First, we find a qualitatively similar story emerging from two distinct data sets (NELS and the 1990 Census): an improvement in educational outcomes, but not for labor market outcomes. Second, we find stronger evidence for effects on behavioral outcomes such as educational persistence than for academic achievement test scores, which seems consistent with studies of other early childhood interventions (see Donohue and Siegelman, 1998, p. 21). Third, we find the strongest effects for the groups that should be most affected (younger adults in 1990), and no effects when we look for groups that should not be affected (the elderly in 1990). Fourth, there is little evidence of a discontinuity at the OEO cutoff in other federal spending, particularly for other social programs. Finally, we do not find a similar pattern of discontinuities in educational outcomes for “treated” cohorts at a pseudo-cutoff where there is no significant discontinuity in Head Start funding.

One apparent discrepancy between the results from the 1990 Census and the NELS is that we observe differences in educational attainment for “indirectly treated” cohorts in the Census, who are about the age to be the parents of children in Head Start, but do not see evidence for differences in parent schooling in the NELS. A candidate explanation is offered by the possibility that Head Start’s emphasis on parent involvement and services in the program declined somewhat over time as Head Start’s connections to the War on Poverty’s Community Action Programs dwindled.

Another puzzle is why the discontinuities in educational attainment at the OEO cutoff estimated in the Census and NELS data do not translate into improved labor market outcomes. One possibility may be that the cohorts directly treated by Head Start are still too early in their working careers for the increases in schooling to yield detectable differences in labor market outcomes.[27] An alternative possibility could be weak labor demand in these high-poverty counties combined with some friction that prevents people from seeking out better job opportunities elsewhere, as in the spatial mismatch hypothesis.[28]

Arguably the most important concern with our findings comes from the possibility of selective migration across counties between when people were of Head Start age and when we first identify their county of residence. This is particularly a concern with our 1990 Census results, where we first identify people's county of residence when they are adults. Perhaps our best evidence against the selective-migration story comes from the NELS data, which enables us to identify county of residence for people when they are still relatively young (in this case 8th grade, around age 13). The fact that we observe a discontinuity in educational attainment for NELS respondents similar to what is observed in the Census, but do not observe any discontinuity in parent characteristics appears to be at odds with what we would expect under a selective-migration counter-explanation for our findings. Presumably selective migration of families across county lines would show up as differences in parent as well as child attributes.

Are the magnitudes of the effects reasonable? We believe that the most meaningful measure of the "treatment dose" at the OEO discontinuity comes from Head Start spending per capita, rather than Head Start participation rates. The reason is that part of the difference in funding per capita across counties may go towards increased spending per program participant in addition to increasing enrollment rates, and so attributing the difference in outcomes at the OEO cutoff entirely to differences in participation rates may be misleading. Unfortunately our data are not very informative about how much of the spending differences are allocated towards increasing the intensive versus extensive margins of the Head Start program, given the large standard errors around our spending and participation estimates in Figure 2.

Taking the funding results from Figure 2 together with the schooling results from Figure 3 suggests that for the directly treated cohorts in the 1990 Census, the elasticity of high school completion to Head Start funding is equal to about +.08, while the elasticity for college attendance equals around +.3. Another way to think about this magnitude is to note that the top panel of Figure 2 suggests a difference in Head Start spending of about $2 per capita in 1968 / 1972 dollars. Assume that about 2% of the population is age-eligible for Head Start at any point in time, so that the $2 difference in per capita spending translates into a difference of about $100 in spending per age-eligible county resident. Our estimates suggest that this spending difference translates into an increase in high school completion and college attendance rates of about 4-5

27 For example, Card (1999, p. 1805) shows that log hourly wages for college graduates are much closer to those of high school graduates at age 25 than at later ages. Moreover some people in our directly treated Census group (18-24) or even the NELS sample may still be in school at the time we measure labor market outcomes.

28 The spatial mismatch hypothesis originates with Kain (1968), and suggests that minority workers in the inner-city have difficulty following job movement to the suburbs because of racial discrimination in the housing market. Limited access to informal social networks that can provide information about suburban jobs is also sometimes offered as a mechanism. A recent review suggests that most studies in this literature find evidence for spatial mismatch (Ihlanfdfeldt and Sjoquist, 1998), although the hypothesis has its skeptics (for example Ellwood, 1986 and Jencks and Mayer, 1990) and the most recent study of the Moving to Opportunity (MTO) residential mobility experiment finds little evidence for labor market effects (Kling *et al*., 2004).

percentage points, so that the (undiscounted) cost per additional high school graduate or college goer is on the order of about \$2,000, or about \$9,000 in current dollars. Ignoring for now the additional distortionary effects of raising tax revenue to fund Head Start, then the program would seem to pass a benefit-cost test if the benefits from a high school degree or extra year of college is more than \$9,000. If the Head Start program has a real effect on criminal activity then the program will almost surely pass a benefit-cost test, given the enormous costs of crime to society (Cohen et al., 2004). The arrest results in the NELS in this version of the paper are proportionately large but not quite statistically significant; in the next version of the paper we will include results on age-specific county-level arrest rates from the FBI's UCR data.

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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 capita 1968	2.21	(4.64)	4.7	(15.60)
Head Start Spending per capita 1972	3.11	(9.67)	4.92	(15.80)
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)

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

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

1990 Census

Variable	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	274	413	526	125	245	486	486
1968 Head Start Spending per capita	2.187	2.431	2.386	2.363	2.378	2.359	2.253
	(2.595)	(2.125)	(1.815)	(2.707)	(3.056)	(2.984)	(2.778)
1972 Head Start Spending, per capita	1.675	2.589	2.545	5.002	1.420	1.162	0.855
	(2.845)	(2.404)	(2.104)	(2.982)	(3.334)	(3.535)	(3.343)
1972 other social spending, per capita	14.252	5.238	13.010	25.447	17.217	12.945	-9.150
	(26.099)	(23.047)	(21.649)	(35.196)	(29.027)	(37.456)	(30.582)

NELS county averages

Variable	Porter			Parametric			
				Linear, same slope on both sides	Flexible Linear	Flexible quadratic	Flexible quadratic, covariates
Bandwidth	4	6	8				
Poverty Range				8	12	16	16
Number of observations with nonzero weight							
Head Start participation	0.110	0.102	0.100	0.133	0.104	0.146	0.089
	(0.081)	(0.064)	(0.052)	(0.105)	(0.085)	(0.102)	(0.121)
Other kindergardten participation							

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) and state dummies.

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

1990 Census

Variable	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	274	413	526	125	245	486	486
% High School Graduate, ages 18-24	0.038	0.025	0.010	0.062	0.042	0.054	0.042
	(0.017)	(0.014)	(0.013)	(0.023)	(0.019)	(0.021)	(0.019)
% High School Graduate, ages 25-34	0.015	0.011	0.004	0.045	0.009	0.023	0.016
	(0.014)	(0.011)	(0.010)	(0.021)	(0.015)	(0.016)	(0.015)
% High School Graduate, ages 35-54	0.010	0.009	0.002	0.015	0.005	0.020	0.018
	(0.015)	(0.013)	(0.011)	(0.023)	(0.017)	(0.019)	(0.016)
% High School Graduate, ages 55+	-0.009	-0.012	-0.012	-0.012	-0.010	-0.007	0.005
	(0.018)	(0.014)	(0.013)	(0.027)	(0.019)	(0.021)	(0.018)
% Some College, ages 18-24	0.048	0.036	0.020	0.039	0.053	0.060	0.054
	(0.022)	(0.019)	(0.016)	(0.030)	(0.026)	(0.027)	(0.025)
% Some College, ages 25-34	0.028	0.024	0.014	0.033	0.029	0.033	0.028
	(0.017)	(0.015)	(0.013)	(0.025)	(0.019)	(0.022)	(0.017)
% Some College, ages 35-54	0.031	0.022	0.011	0.030	0.034	0.042	0.036
	(0.016)	(0.014)	(0.012)	(0.023)	(0.018)	(0.020)	(0.016)
% Some College, ages 55+	0.002	-0.001	-0.001	0.009	0.002	0.000	0.003
	(0.012)	(0.010)	(0.008)	(0.017)	(0.013)	(0.014)	(0.012)
% College completion, ages 18-24	0.002	0.002	0.001	0.000	0.003	0.003	0.003
	(0.003)	(0.003)	(0.003)	(0.005)	(0.004)	(0.005)	(0.005)
% College completion, ages 25-34	0.015	0.011	0.006	0.021	0.016	0.019	0.017
	(0.008)	(0.007)	(0.006)	(0.012)	(0.010)	(0.011)	(0.010)
% College completion, ages 35-54	0.015	0.010	0.004	0.021	0.016	0.021	0.016
	(0.009)	(0.008)	(0.007)	(0.012)	(0.010)	(0.011)	(0.010)
% College completion, ages 55+	-0.001	0.000	0.000	0.001	0.000	-0.002	-0.001
	(0.006)	(0.005)	(0.005)	(0.009)	(0.007)	(0.007)	(0.007)

See note to Table 2

Table 4: Discontinuity in differences results

	1990 Census						
Variable		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	274	413	526	125	245	486	486
High School Graduation							
(18-24 minus 25-34)	0.023	0.014	0.006	0.017	0.033	0.031	0.026
	(0.015)	(0.012)	(0.011)	(0.021)	(0.016)	(0.018)	(0.017)
(18-24 minus 35-54)	0.028	0.016	0.008	0.047	0.037	0.034	0.025
	(0.018)	(0.015)	(0.014)	(0.027)	(0.021)	(0.023)	(0.019)
(18-24 minus 55 plus)	0.047	0.037	0.022	0.074	0.052	0.062	0.038
	(0.022)	(0.018)	(0.016)	(0.033)	(0.024)	(0.026)	(0.021)
Some college							
(18-24 minus 25-34)	0.020	0.013	0.006	0.006	0.025	0.027	0.026
	(0.016)	(0.014)	(0.012)	(0.022)	(0.019)	(0.021)	(0.020)
(18-24 minus 35-54)	0.017	0.014	0.009	0.009	0.020	0.018	0.018
	(0.018)	(0.015)	(0.013)	(0.026)	(0.021)	(0.022)	(0.020)
(18-24 minus 55 plus)	0.046	0.037	0.021	0.030	0.051	0.060	0.050
	(0.020)	(0.016)	(0.015)	(0.029)	(0.023)	(0.024)	(0.022)
College completion							
(18-24 minus 25-34)	-0.014	-0.010	-0.005	-0.021	-0.013	-0.016	-0.014
	(0.008)	(0.006)	(0.006)	(0.011)	(0.009)	(0.009)	(0.009)
(18-24 minus 35-54)	-0.014	-0.008	-0.003	-0.021	-0.014	-0.018	-0.012
	(0.008)	(0.007)	(0.006)	(0.012)	(0.009)	(0.010)	(0.009)
(18-24 minus 55 plus)	0.002	0.002	0.001	-0.001	0.003	0.005	0.005
	(0.006)	(0.005)	(0.005)	(0.009)	(0.007)	(0.008)	(0.007)

See note to Table 2

Table 5: Other census outcomes: employment and income

	1990 Census				
Variable	Porter		Parametric		
			Linear, same slope on both sides	Flexible quadratic	Flexible quadratic, covariates
Bandwidth	2	4			
Poverty Range			2	8	8
Number of observations with nonzero weight	274	526	125	486	486
pers18_24_inpoverty	0.015	0.033	-0.018	0.006	-0.003
	(0.020)	(0.016)	(0.022)	(0.026)	(0.021)
pers25_44_inpoverty	0.014	0.025	0.000	0.006	-0.004
	(0.014)	(0.011)	(0.015)	(0.019)	(0.014)
pers45_59_inpoverty	0.009	0.020	-0.011	0.002	-0.006
	(0.012)	(0.010)	(0.013)	(0.016)	(0.012)
pers60_64_inpoverty	0.001	0.014	-0.011	-0.002	-0.012
	(0.014)	(0.010)	(0.015)	(0.018)	(0.016)
pers65plus_inpoverty	0.002	0.011	-0.001	-0.003	-0.003
	(0.007)	(0.006)	(0.008)	(0.010)	(0.009)
pers18_24_incpovlt200perc	0.012	0.036	-0.010	-0.003	-0.017
	(0.025)	(0.018)	(0.027)	(0.031)	(0.025)
pers25_44_incpovlt200perc	0.004	0.022	-0.010	-0.006	-0.023
	(0.020)	(0.015)	(0.022)	(0.026)	(0.020)
pers45_59_incpovlt200perc	0.013	0.024	-0.001	0.003	-0.010
	(0.018)	(0.014)	(0.020)	(0.024)	(0.019)
pers60_64_incpovlt200perc	0.006	0.023	-0.024	-0.003	-0.016
	(0.020)	(0.015)	(0.021)	(0.024)	(0.022)
pers65plus_incpovlt200perc	0.023	0.026	0.012	0.020	0.016
	(0.010)	(0.008)	(0.011)	(0.013)	(0.012)
age16_24_unemployed	0.003	0.006	-0.008	0.002	-0.004
	(0.007)	(0.005)	(0.008)	(0.008)	(0.007)
age25_34_unemployed	0.004	0.007	-0.002	0.001	-0.004
	(0.007)	(0.005)	(0.007)	(0.008)	(0.006)
age35_54_unemployed	0.003	0.005	0.003	0.001	-0.003
	(0.004)	(0.003)	(0.004)	(0.005)	(0.004)
age55_64_unemployed	-0.002	-0.001	-0.004	-0.002	-0.004
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)

See note to Table 2

Table 6: Migration related outcomes

	1990 Census				
Variable	Porter		Parametric		
			Linear, same slope on both sides	Flexible quadratic	Flexible quadratic, covariates
Bandwidth	2	4			
Poverty Range			2	8	8
Number of observations with nonzero weight	274	526	125	486	486
% Same county as 5 year ago	-0.020	-0.006	-0.025	-0.027	-0.031
	(0.013)	(0.010)	(0.015)	(0.017)	(0.015)
Fraction ages 18-24	0.010	0.005	0.006	0.012	0.010
	(0.005)	(0.004)	(0.006)	(0.007)	(0.007)
Fraction ages 25-34	0.003	0.003	0.000	0.003	0.000
	(0.004)	(0.003)	(0.004)	(0.005)	(0.005)
Fraction ages 35-54	-0.003	-0.004	-0.001	-0.003	-0.003
	(0.005)	(0.004)	(0.005)	(0.006)	(0.005)
Fraction ages 55 plus	-0.021	-0.016	-0.003	-0.023	-0.016
	(0.011)	(0.008)	(0.012)	(0.013)	(0.011)

See note to Table 2.

Table 7: Test of Balance on Covariates

	alpha (t-statistic)
1990 Census	
Black	-.01 (0.13)
Urban	.02 (0.73)
1980 Census	
Black	.00 (0)
Urban	.00 (0.20)
1970 Census	
Black	.02 (0.49)
Urban	.04 (0.71)
1960 Census	
Black	.02 (0.48)
Urban	.04 (0.71)
1950 Census	
Non-white	.03 (0.71)
Urban	.01 (0.32)

Porter RD method, BW=2

Table 7: Test of Balance on Covariates

	alpha (t-statistic)
NELS Base Year	
Black	-.19 (1.59)
Hispanic	-.00 (0.04)
Log family inc 1987	.31 (0.69)
Mother's ed (years)	-.05 (0.05)
Urban	-.02 (0.29)
NELS 2000 survey	
Black	-.12 (1.03)
Hispanic	-.02 (0.17)
Log family inc 1987	-.05 (0.13)
Mother's ed (years)	.29 (0.22)

Porter RD method, BW=2

Table 9: Results for Pseudo-Cutoff (1960 Poverty Rate = 40%)

Dataset / variable	Alpha (t-statistic)
NELS	
Head Start participation	-.00 (0.13)
Reading scores, 1988	-0.83 (0.48)
Math scores, 1988	-0.76 (0.43)
Arrests, 1990	-.02 (0.56)
Arrests, 1992	-.02 (0.53)
High school 2000	-.08 (1.66)*
College 2000	.02 (0.38)
Work full time, 2000	-.03 (0.72)
Log earnings, 1999	.55 (1.50)
1990 Census	
High school 18-24	.018 (1.11)
High school 25-34	.026 (1.79)*
High school 35-54	.015 (1.07)
High school 55+	.027 (1.73)*
Some college, 18-24	-.002 (0.11)
Some college, 25-34	.028 (1.62)
Some college, 35-54	.010 (0.62)
Some college, 55+	.010 (0.91)
College 18-24	.010 (2.56)**
College 25-34	.012 (1.24)
College 35-54	.002 (0.20)
College 55+	-.001 (0.10)

Porter RD method, BW = 2

Figure 1A: Head Start Participation and 1968 Funding

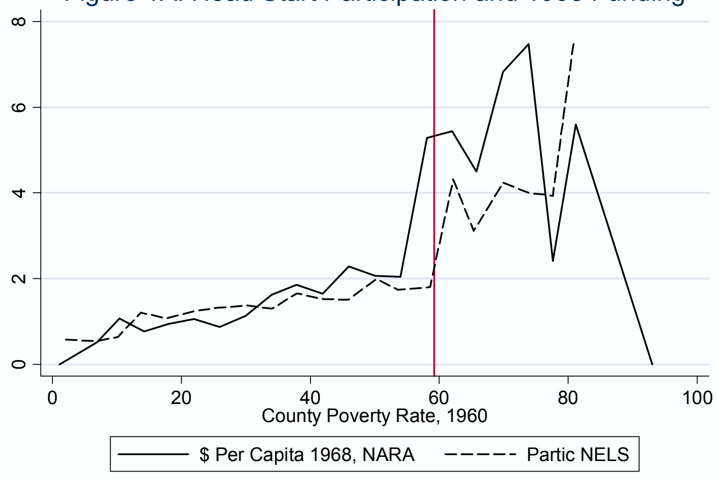
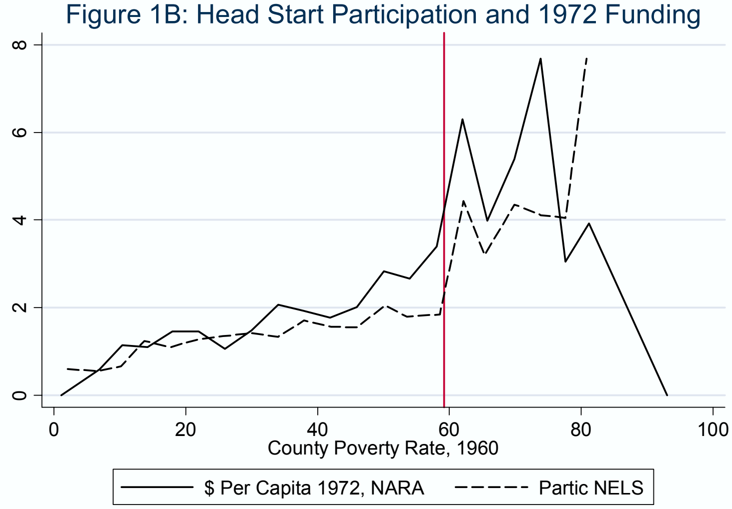


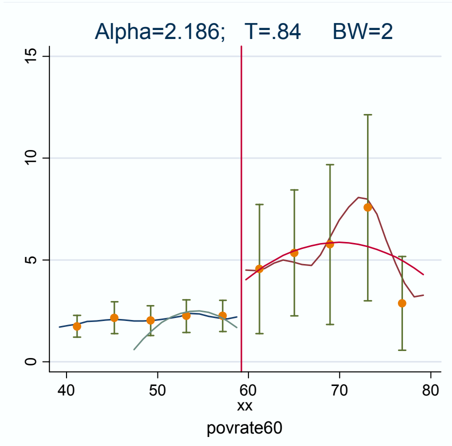
Figure 1B: Head Start Participation and 1972 Funding

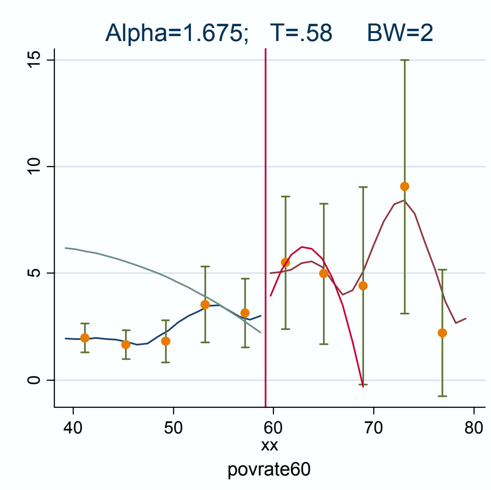


J:\julie2\u_graph_doug4.do & u_graph_doug68.do

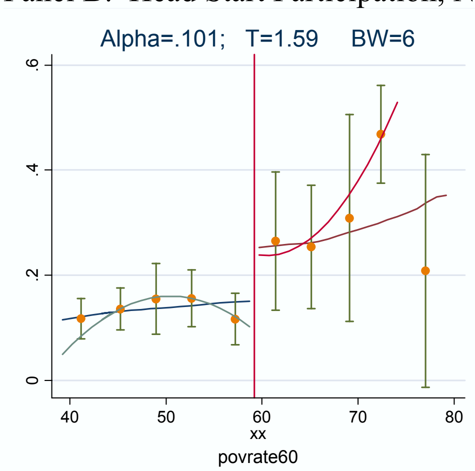
Figure 2: Estimated Discontinuity in Head Start Funding & Participation

Panel A: 1968 and 1972 spending per capita, National Archives data

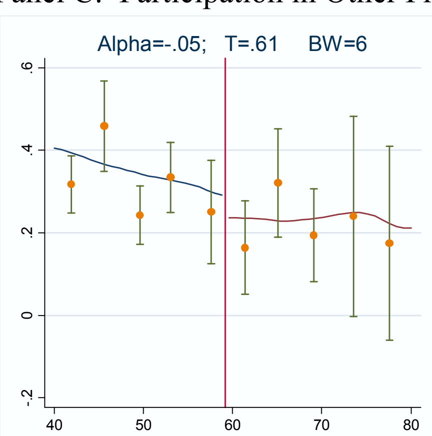




Panel B: Head Start Participation, NELS Base-Year Respondents



Panel C: Participation in Other Preschool Programs, NELS Base-Year Respondents



Top two panels from manyfirst.do (uses hsspend_per_cap72_c)
Bottom panel from [Jens] julie2\u_figure2_nels.do

Figure 3: Discontinuity in Educational Attainment by Age, 1990 Census
Panel A: High School or more

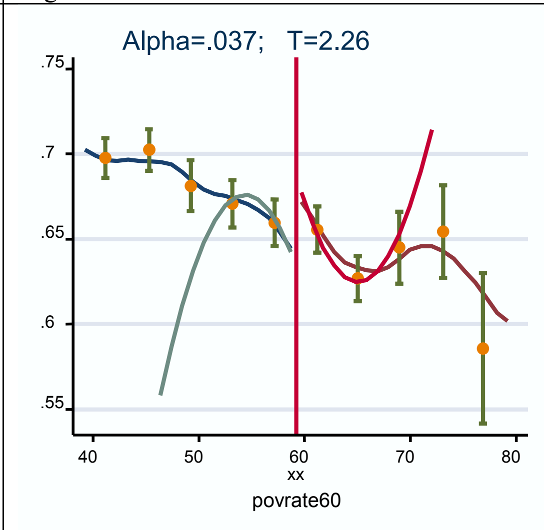
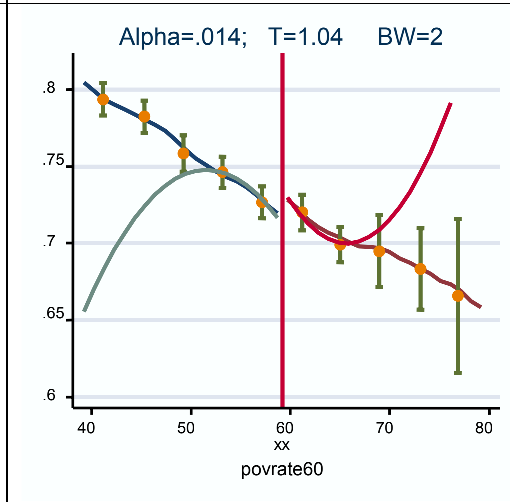
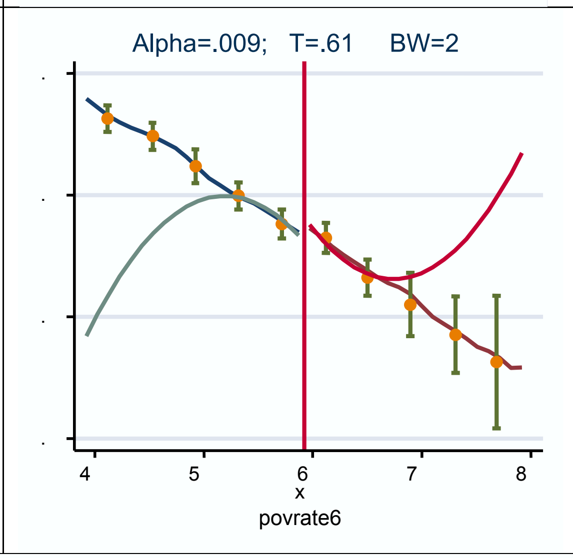
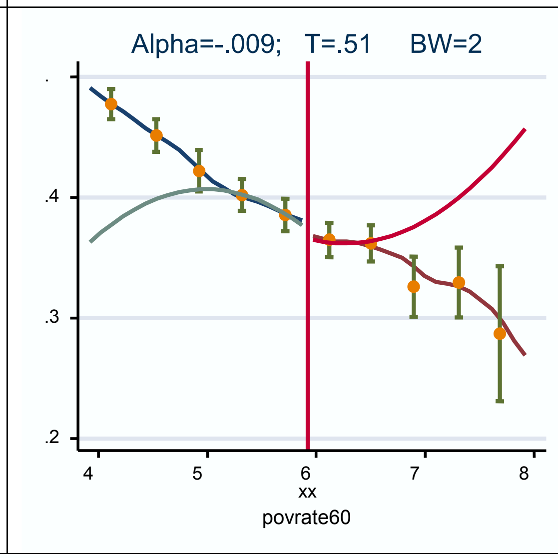
Age	High school or more	Age	
18-24		25-34	
35-54		55+	

Figure 3: Discontinuity in Educational Attainment by Age, 1990 Census
Panel B: Some college

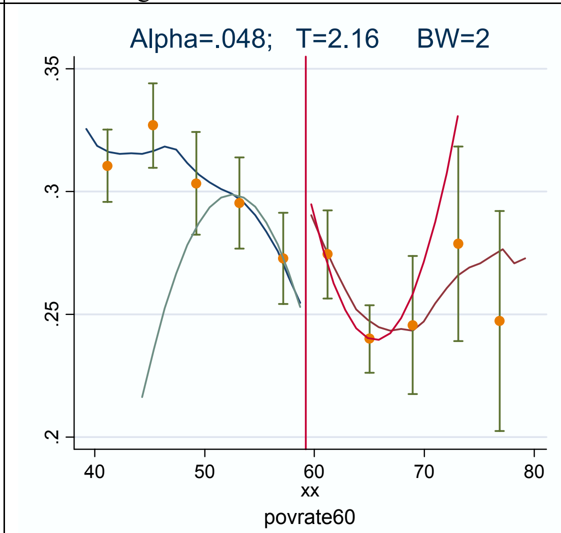
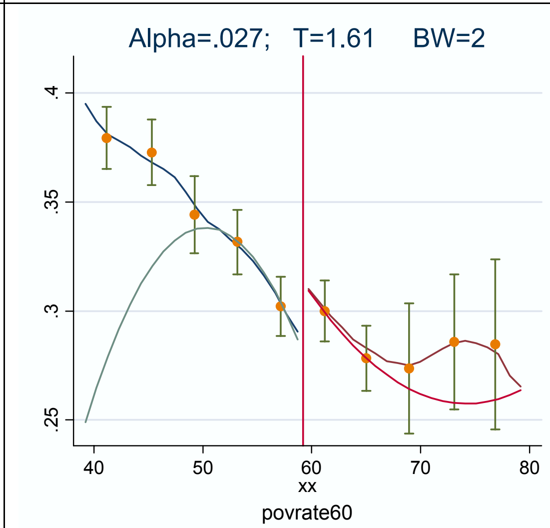
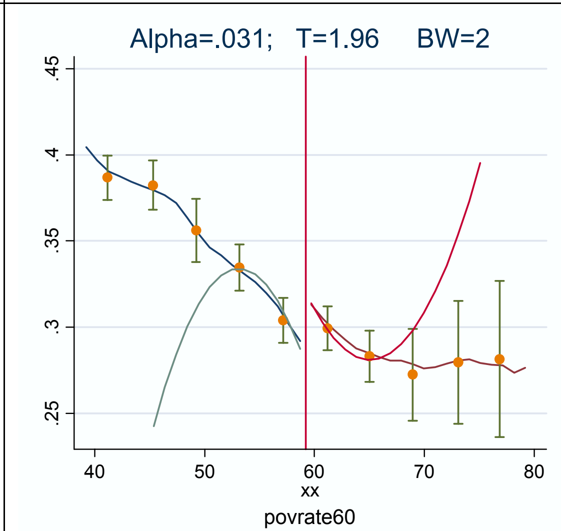
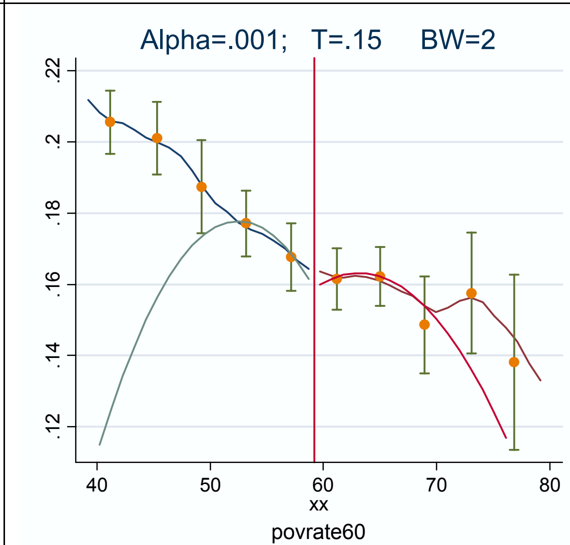
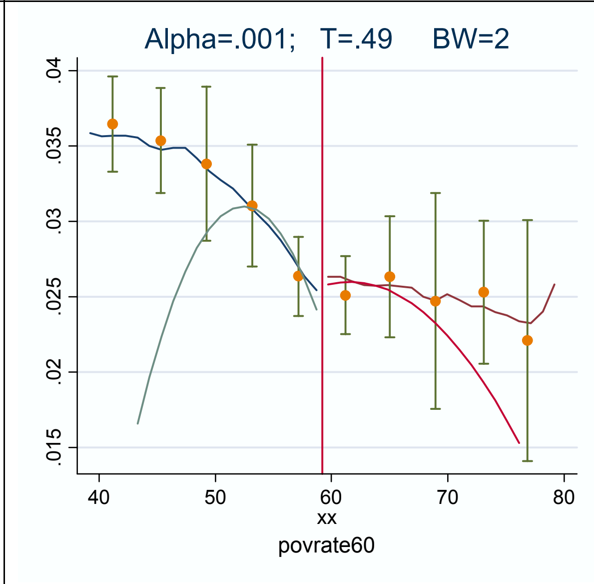
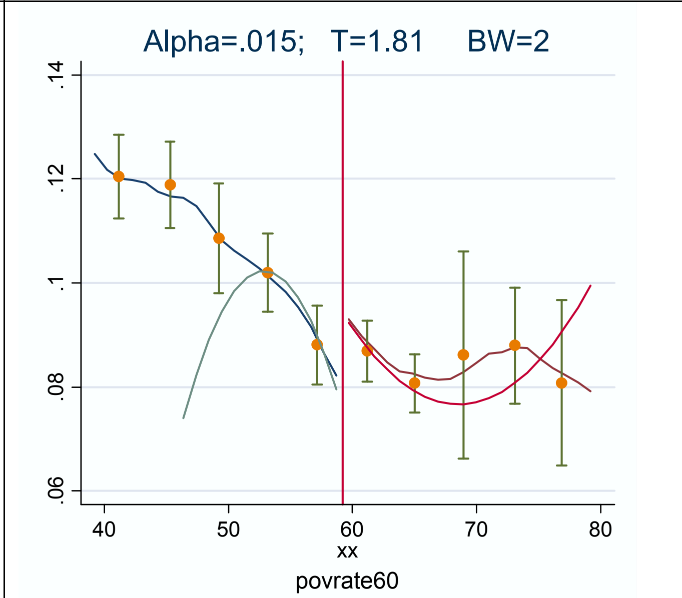
Age	Some college	Age	
18-24		25-34	
35-54		55+	

Figure 3: Discontinuity in Educational Attainment by Age, 1990 Census
Panel C: Completed college

<table>
<tr><td>Age</td><td>Completed college or more</td><td>Age</td><td></td></tr>
<tr><td>18-24</td><td>


</td><td>25-34</td><td>


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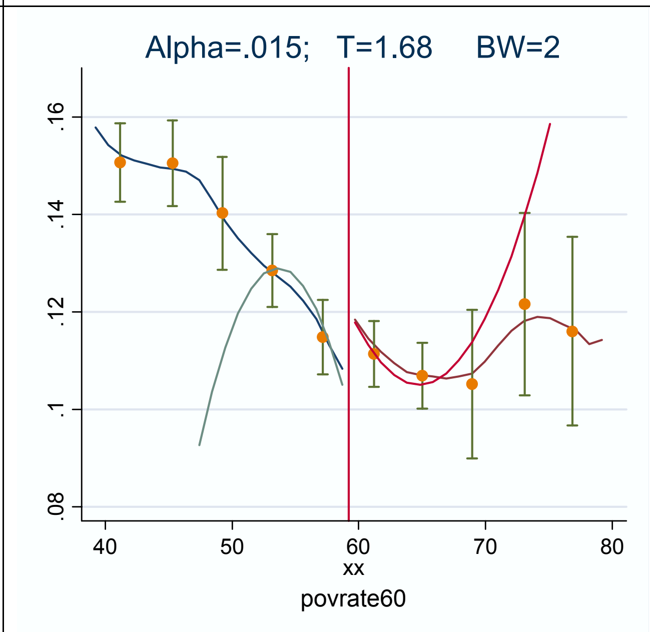
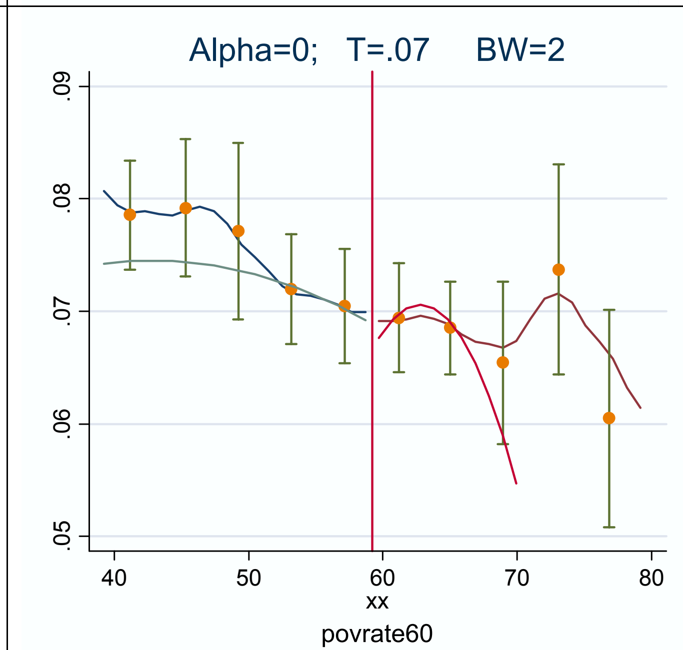


</td><td>55+</td><td>


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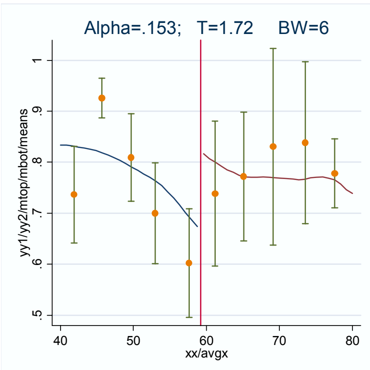
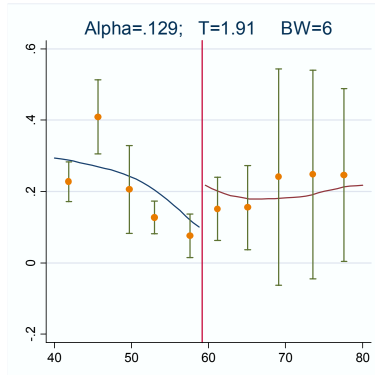
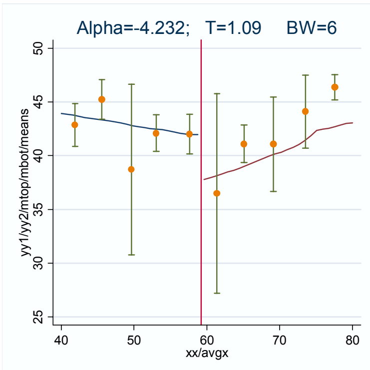
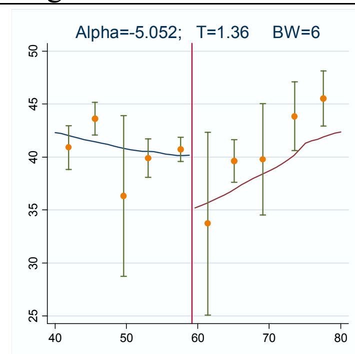
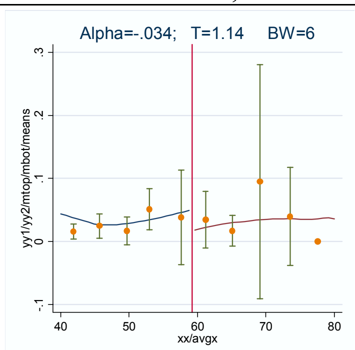
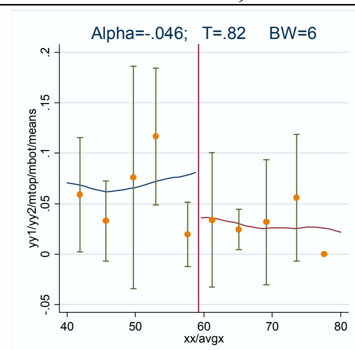
</table>

Figure 4
Discontinuity in Educational Attainment and Labor Market Outcomes,
NELS 2000 Follow-Up Survey

High school or more	College or more
	

Julie2\nels_porter2000.do. Above results calculated using f4pnlfl=1 / f4pnlwt sample and weight combination for 2000 survey; using the f4bypnwt=1 / f4bypnwt sample and weight produces stronger evidence here for effects on educational attainment, but yields more imbalance in background covariates, particularly mom's educational attainment and 1987 family income.

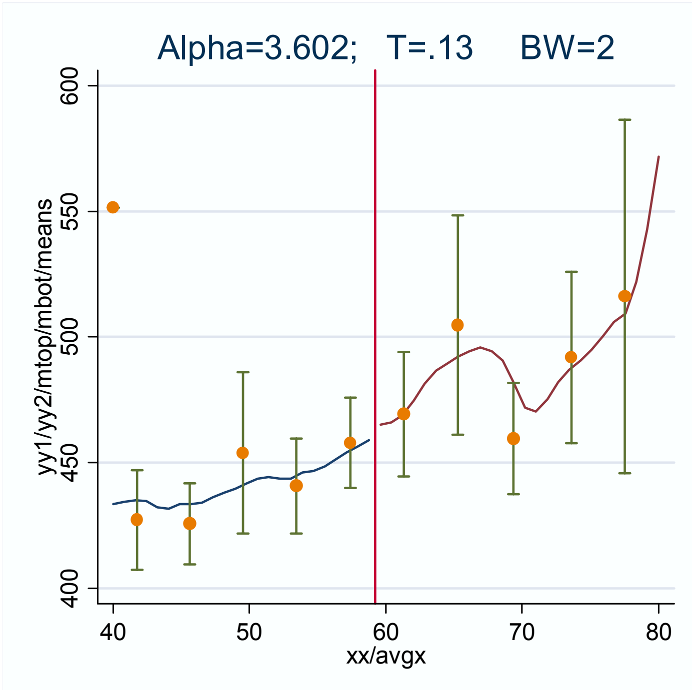
Figure 5
Discontinuity in Achievement Test Scores and Arrests,
NELS Base-Year and First Follow-Up Surveys

8th grade reading test scores	8th grade math test scores
	
Number of arrests, first follow-up survey	**Number of arrests, second follow-up survey**
	

Julie2\nels_porter2000.do

Figure 6
Discontinuity in Other Federal Spending, 1972 National Archives Data

Other Federal Social Spending



julie2\u_other_spending1.do, u_graph_otherfed.do, u_graph_otherfed2.do