THE EFFECT OF MEDICAID ON EDUCATIONAL ATTAINMENT:
EVIDENCE FROM CHICAGO

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ABSTRACT

Medicaid is a public health insurance program designed to shield poor children from the economic insecurity of sickness and improve their health. Recent evidence suggests that it may also have a lasting impact on non-health outcomes. I study a federal law that expanded Medicaid eligibility discontinuously for low-income children born after September 30, 1983. Using administrative data on students in Chicago Public Schools, I demonstrate that Medicaid enrollment increased significantly for those children likeliest to be affected by the expansion. I also offer suggestive evidence that these children were more likely to graduate high school, and that this effect is particularly strong for males. These findings suggest potentially large, long-term benefits to non-health outcomes from expanding children's access to health insurance.

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I. INTRODUCTION

Medicaid is the largest health insurance provider in the US, covering more than 1 in 3 children (Kaiser Family Foundation 2013). Between 1987 and 1996, the fraction of children enrolled in Medicaid nearly doubled, from 12 to 21 percent, and over 9 million children became eligible for the program (Weigers et al. 1998). Only recently have researchers been able to estimate the long-term impact of this major expansion to children's public health insurance. These early findings suggest that affected children live longer, are in better health, and earn more in adulthood (Wherry and Meyer 2015; Brown, Kowalski, and Lurie 2015).

I contribute to this literature by studying the effects of Medicaid eligibility on the educational attainment of low-income students in Chicago Public Schools. Using a federal law that expanded eligibility discontinuously for poor children born after September 30, 1983, I find that Medicaid enrollment increased by 2.5 to 4 months for those likeliest to gain eligibility. In addition to improving their insurance coverage, this eligibility expansion may have also raised children's high school graduation rates. My estimates suggest that affected male children were 3.5 percentage points (9 percent) more likely to complete high school, while estimates for female children are insufficiently precise to be informative.

These findings add to our knowledge about the far-reaching effects of public insurance coverage. The benefits of such coverage for children's contemporaneous health are well documented (Levy and Meltzer 2008). Further, children born into poor health have greater difficulty developing their human capital (e.g., Almond 2006; Oreopoulos et al. 2008; Royer 2009; Figlio et al. 2014), suggesting that health casts a long shadow over educational attainment. Yet very few studies directly examine the relationship between this major health intervention (public insurance) and this important outcome (schooling).

One recent study that does this is by Cohodes, Grossman, Kleiner, and Lovenheim (2015). The authors find that increased Medicaid eligibility in childhood reduces the likelihood of high school dropout. I focus on the same important question, but arrive at its answer differently. First, instead of using eligibility variation resulting from changes to state Medicaid and welfare policies, I rely on differences in children's eligibility due to their birthdate relative to the September 30, 1983 cutoff. This allows me to estimate the effects of Medicaid eligibility without assuming the exogeneity of state policies or common trends across states in factors affecting different birth cohorts. Second, while my findings are most relevant for low-income students in Chicago, this loss in generalizability is arguably offset by gains in data quality. Using administrative records on children's Medicaid enrollment and schooling, I directly measure the effects of eligibility on insurance coverage, graduation, absences, and grade repetition.

The gender asymmetry in my results is surprising in light of evidence from multiple areas of the human capital literature that suggest females benefit more from childhood interventions than males. For example, expansions of nutritional assistance, disease eradication efforts, and intensive schooling at young ages have all been shown to improve human capital outcomes for females more than for males (Hoynes, Schanzenbach, and Almond 2014; Bleakley 2007; Anderson 2008). One possible explanation for this result is that males are likelier to exhibit symptoms of behavioral disorders, such as attention deficit hyperactivity disorder (ADHD), that inhibit their ability to learn in school. If Medicaid improves access to the care necessary for managing these conditions, then it may explain why males are more responsive to expanded eligibility than females.

These findings imply that the benefits of children's public health insurance extend beyond health and childhood. They also imply that older children—whose responsiveness to health interventions is understudied relative to pregnant women and infants, and who experienced the largest growth in Medicaid coverage during the 1990s (Currie, Decker, and Lin 2008)—may benefit substantially from improved access to care. It is also unclear *a priori* how responsive the health of older children is to insurance coverage; it may, for example, be less malleable then the health of younger children, as recent evidence from the child development literature suggests is true of skill formation (Phillips and Shonkoff 2000). On the other hand, if the relationship between health and income in adulthood originates in the ability to manage chronic conditions in childhood (Case, Lubotsky, and Paxson 2002), then the benefits of expanding Medicaid to older children in poverty may be large.

II. RESEARCH DESIGN

The research design in this study relies on a federal law that expanded Medicaid eligibility for older children born after September 30, 1983. The law took effect when children were almost 8 years old and the discontinuity it created lasted until just after they turned 14, providing affected children with up to 6.5 years of additional eligibility. The context necessary for understanding this law, and why it is well suited to estimating the effects of Medicaid eligibility, are discussed in this section.

II.A. Broadening of Medicaid eligibility

Medicaid evolved from a program targeting children poor enough to qualify for cash welfare assistance[1] to one that serves the broader low-income population. One of the federal laws that shaped this transformation forms the basis of this study's research design. I briefly review the relevant legislative history below.

[1] Cash assistance and welfare are used interchangeably throughout this paper.

Medicaid was established in 1965 with the goal of reducing income-based inequality in health and access to care. Operated as a federal-state insurance program, eligible individuals are entitled to receive a variety of medical services—inpatient and outpatient hospital, physician, nursing home, laboratory and x-ray—with no cost sharing (Goodman-Bacon 2015).

At the program's inception, states were the gatekeepers of Medicaid eligibility. Low-income children automatically qualified for Medicaid if they were eligible to receive cash assistance through the Aid to Families with Dependent Children (AFDC) program,[2] and states had considerable leeway in determining who was eligible for AFDC. For example, in January 1990, the income threshold for AFDC averaged 47 percent of the federal poverty level, ranging from 13.4 percent in Alabama to 78.9 percent in California.[3] This effectively limited Medicaid access to children in dire poverty with a single parent or guardian.

Under pressure to address children's limited and geographically inequitable access to Medicaid, Congress passed a series of laws beginning in the mid-1980s that weakened its link to AFDC. These laws followed a common pattern: they first encouraged, and later required, states to base children's Medicaid eligibility on a family's income as a fraction of the poverty level, rather than on eligibility for AFDC (Appendix Table 1). While children receiving AFDC remained eligible for Medicaid, states began an aggressive push in the mid-1990s to reduce the number of families receiving welfare, culminating in the enactment of a federal "welfare reform" law that replaced the AFDC program altogether. As a result, the number of children qualifying for Medicaid due to welfare receipt fell, while the number qualifying due to the expansions grew.

A feature of one of these expansions, the Omnibus Budget Reconciliation Act of 1990 (OBRA90), makes it possible to credibly estimate the effects of Medicaid eligibility. Effective

[2] A family must meet both income and composition criteria to qualify for AFDC. See Moffitt (2003) and Currie and Gruber (1996a) for details.
[3] National Governors Association (1990)

July 1991, OBRA90 extended eligibility to all children in poverty born after September 30, 1983.[4] Since very low-income children often qualified for Medicaid on the basis of cash assistance receipt—and did so regardless of birthdate—those most directly affected by OBRA90 were children in families between the AFDC income threshold and the poverty level. As a result, the probability of being eligible changed sharply for children in this income segment born near the cutoff, making it well suited for study using a regression discontinuity (RD) research design (Lee and Lemieux 2010). The discontinuity created by OBRA90 remained in place until states adopted the Children's Health Insurance Program (CHIP) in 1998.[5] A child in poverty born in October 1983 was almost 8 years old when OBRA90 took effect and just over 14 when CHIP was adopted, and therefore gained 6.5 years of Medicaid eligibility relative to a child born a month earlier (Figure 1).

II.B. Approaches to studying the effects of Medicaid eligibility

Comparing the educational outcomes of eligible and ineligible children to recover the effects of Medicaid eligibility is susceptible to two sources of bias: selection and simultaneity.[6] Selection (omitted variable) bias arises from the fact that eligible children differ from ineligible children in ways that, apart from their eligibility status, are correlated with outcomes, and these differences are difficult or impossible to control for entirely. Simultaneity (reverse causality) bias

[4] Two other eligibility expansions enacted prior to OBRA90—the Deficit Reduction Act of 1984 (DEFRA84) and the Omnibus Budget Reconciliation Act of 1987 (OBRA87)—also feature the September 30, 1983 discontinuity (see Appendix Table 1). These expansions targeted children under ages 5 and 7, respectively, and in families meeting AFDC's income but not its composition criteria. Volatility in poor families' incomes makes it likely that some children gaining eligibility under OBRA90 were affected at earlier ages by DEFRA84 or OBRA87. In this paper, I refer only to the OBRA90 expansion as it is the largest of the three, but DEFRA84 and OBRA87 may also contribute to the results.

[5] Introduced in the Balanced Budget Act of 1997, CHIP provides states with matching funds to expand health insurance to children in higher income families regardless of birthdate. Within two years, all 50 states and the District of Columbia had developed and submitted plans to the Health Care Financing Administration for implementing CHIP.

[6] I leave aside discussion of a third source of bias, measurement error, to focus on the conceptual problems arising from estimating the effects of Medicaid eligibility.

occurs when the outcome of interest can affect eligibility, the opposite of the causal relationship of interest. For example, family income may be reduced, and the likelihood of Medicaid eligibility increased, if a parent must stay home to care for a sick child.

Circumventing these problems requires identifying a source of variation in Medicaid eligibility that is uncorrelated with individuals' characteristics (to address selection) and not a function of their outcomes (to address simultaneity). The eligibility discontinuity created by OBRA90 convincingly deals with both selection and simultaneity bias: children's characteristics vary smoothly across the cutoff, while birthdate is immutable.[7] The variation in childhood Medicaid eligibility between individuals born on either side of the cutoff is exogenous, and the law passed after it could affect any fertility decisions.

A handful of researchers have used the variation generated by OBRA90 to study the effects of expanding Medicaid eligibility. Card and Shore-Sheppard (2004) use this approach to estimate Medicaid take-up by comparing the enrollment rates of children born before and after the cutoff, in families above and below the poverty level. Their difference-in-differences estimates imply that the OBRA90 expansion caused Medicaid eligibility and enrollment to rise by 91.8 and 6.9 percentage points, respectively. Wherry and Meyer (2015) estimate the expansion's effect on mortality using vital statistics data. They find that black children born after the cutoff experienced a 19 percent decrease in internal mortality between the ages of 15-18, with no similar effect for external mortality or among white children.[8] Wherry et al. (2015) use the same approach to study effects on hospitalizations and emergency department visits using state-level data covering between 20 and 34 percent of the US population. They estimate an 8 to

[7] In other contexts, we might worry about efforts by parents to misrepresent a child's birthdate in order to gain eligibility. While that may happen in response to a Medicaid expansion, the documentation required to obtain Medicaid coverage (e.g., birth certificates) likely deters this behavior.
[8] Based on the authors' simulations, black children were more likely to gain eligibility under the expansion than white children.

13 percent decline in hospitalizations for blacks in 2009, with larger estimates for hospitalizations involving chronic illness (13 to 17 percent), individuals in low-income ZIP codes (15 to 21 percent), and the combination of the two (22 to 29 percent).

III. MECHANISMS AND PRIOR EVIDENCE

Medicaid can improve children's educational outcomes through two mechanisms: health and family income. This section reviews evidence on how Medicaid affects these two channels and how they, in turn, may improve school attainment. I conclude with a discussion of a study by Cohodes, Grossman, Kleiner, and Lovenheim (2015), the paper most similar to this one in the literature.

III.A. Mechanisms for Medicaid to improve schooling outcomes

By making care more affordable and accessible, Medicaid can improve children's health and reduce family poverty, factors that are both linked to school performance. For example, children suffering from chronic conditions like asthma and ADHD are more likely to miss school, repeat grades, or drop out altogether (Fowler et al. 1992; Diette et al. 2000; Barkley 2002), while children in poverty perform worse on a range of measures, including educational attainment (Brooks-Gunn and Duncan 1997; Mayer 1997; Case, Lubotsky, and Paxson 2002; Case, Fertig, and Paxson 2005). While these studies make a compelling case for the association between education and children's physical, mental, and financial well being, they do not provide causal evidence of one affecting the other.

Many studies, however, do provide causal evidence of Medicaid's effects on health and income. Medicaid expansions have been shown to improve contemporaneous health outcomes, such as infant and child mortality (Currie and Gruber 1996a, 1996b), and long-run health outcomes, such as hospitalizations and adult mortality (Wherry et al. 2015; Wherry and Meyer 2015). Gross and Notowidigdo (2011) find that Medicaid expansions resulted in fewer personal bankruptcies, over a quarter of which, by the authors' estimates, are attributable to a lack of health insurance. Adults given the chance to apply for Medicaid experience a 25 percent reduction in the probability of having an unpaid medical bill and a 35 percent reduction in having any out-of-pocket medical expenses (Finkelstein et al. 2012). Evidence on whether Medicaid affects household income by influencing labor supply decisions is mixed, with recent studies failing to uncover any effect.[9]

Whether improved health affects educational outcomes remains an active area of research.[10] Most work in this area focuses on the effects of prenatal care or low birth weight on later outcomes.[11] Other researchers suggest that improving physical and mental health, even among older children, can lead to fewer absences and a greater ability to focus while in class (Grossman and Kaestner 1997; US DHHS 2000; Currie and Stabile 2006). A particularly stark example of this is the eradication of hookworm in the American South during the early 20th century, which significantly increased school enrollment, attendance, and literacy, as well as income in adulthood (Bleakley 2007).

Recent studies provide mixed evidence on whether the positive relationship between family income and children's outcomes is causal. Dahl and Lochner (2012), Duncan Morris, and Rodrigues (2011), Milligan and Stabile (2011), and Akee et al. (2010) provide experimental or quasi-experimental evidence that raising household incomes improves children's academic

[9] Yelowitz (1995) finds that Medicaid expansions increased parents' labor force participation, while Meyer and Rosenbaum (2001) and Ham and Shore-Sheppard (2001) fail to uncover any sizable effect.
[10] Currie (2009) provides an excellent review of this literature.
[11] See, for example, Almond (2006); Black, Devereux, and Salvanes (2007); Oreopoulos et al. (2008); Almond, Edlund, and Palme (2009); Royer (2009); Almond and Mazumder (2011); and Figlio et al. (2014).

achievement. In contrast, Jacob, Kapustin, and Ludwig (2015) find little, if any, impact on children in families that won a lottery for housing vouchers, which generate large income effects.

An important point to note when considering Medicaid's income effects is that they will accrue disproportionately to families with the largest medical expenses to offset, which are likely to be disadvantaged relative to families with fewer medical needs. Akee et al. (2010) note that the effects of transfer payments from casino profits on children's graduation rates differ by a family's baseline poverty status, with children in poorer households benefiting more. This suggests that the potential income effects from a Medicaid eligibility expansion may be large, as adverse selection will cause the most disadvantaged households to enroll. While the low-income children in Chicago studied by Jacob, Kapustin, and Ludwig (2015) bear more than a superficial resemblance to the sample studied here, families electing to participate in a housing voucher lottery may be positively selected and therefore less likely to benefit from additional income than families enrolling in Medicaid.

III.B. Evidence on the effects of health insurance on education

Very few studies examine the effects of public health expansions on non-health outcomes, including education. Levine and Schanzenbach (2009) test the effects of eligibility at different points throughout children's lives on their fourth and eighth grade test scores. They estimate that increasing eligibility at birth by 50 percentage points improves reading scores by 0.09 standard deviations. Effects on math scores, and of eligibility at older ages, are statistically insignificant. Brown, Kowalski, and Lurie (2015) use tax return data from the IRS to estimate the effects of childhood Medicaid eligibility on college attendance and a range of labor market outcomes in adulthood. They find that children whose eligibility increased were more likely to attend college, paid more in taxes, and earned higher wages.

The paper related most closely to this one is a study of how health insurance expansions affect educational attainment by Cohodes, Grossman, Kleiner, and Lovenheim (2015) (henceforth CGKL). The authors find that increasing Medicaid eligibility by 10 percentage points reduces the rate of high school drop out by 0.39 percentage points (4.1 percent). Additional analyses suggest that this effect is driven by eligibility expansions during childhood, rather than at birth, with eligibility at ages 4 through 8 yielding the most significant impact on high school completion.

This paper differs from the CGKL study in three respects: identification, sample, and measures. First, the variation I use to identify the effects of Medicaid eligibility is generated by a single expansion, OBRA90, which increased eligibility dramatically and discontinuously for children born just after September 30, 1983. In contrast, CGKL rely on spatial and time variation in childhood eligibility resulting from state and federal Medicaid and welfare policies.[12] Introduced by Currie and Gruber (1996a, 1996b) and widely adopted since then, this approach uses a simulated instrument to isolate eligibility variation based on federal and state policies while removing variation based on individual characteristics.

One major concern with this approach is legislative simultaneity (Gruber 2003). States may change their Medicaid or other anti-poverty policies in response to economic conditions, complicating efforts to estimate the effects of those policies on outcomes of interest (Besley and

[12] This variation is the result of two types of discretionary state policies: adoption of optional federal Medicaid expansions, and AFDC eligibility rules. Children's eligibility within a state changed as a result of these policies, and also as a result of mandatory federal expansions, such as OBRA90, that differed in their impact based on the state's prior policies. For example, children in states adopting earlier optional expansions were less affected by later mandatory expansions because they were more likely to already be eligible. Likewise, low-income children in states with high AFDC income thresholds were likelier to have coverage than similar children in states with low AFDC income thresholds, and were thus also less affected by federal policy.

Case 2000).[13] For example, in addition to adopting optional Medicaid expansions, many states received federal waivers to drastically change their AFDC programs during the mid-1990s (Moffitt 2003). CGKL present a version of their results using time variation only from changes to federal laws and spatial variation from states' pre-existing welfare policies. Although qualitatively similar, these results are often substantially larger, suggesting that state policies have a meaningful impact on the size of their estimates.

The other major differences between this work and that of CGKL concern samples and measures. CGKL rely on two national datasets to conduct their analysis: the Current Population Survey (CPS) is used to estimate first stage effects of simulated eligibility on actual eligibility, and the American Community Survey (ACS) provides measures of educational attainment. In contrast, this study focuses exclusively on low-income students in Chicago. The trade-off with using a less generalizable sample is access to higher quality administrative data on Medicaid enrollment, school attendance, and graduation.

Medicaid enrollment is an outcome usually unavailable to researchers, or obtained from survey data where it is substantially underreported (Lewis, Ellwood, and Czajka 1998). For example, the 1992 panel of the Survey of Income and Program Participation, used by Card and Shore-Sheppard (2004) to estimate Medicaid take-up, underreports enrollees by approximately 15 percent compared to administrative data from the Health Care Financing Administration. Parents often do not know whether their children are enrolled in Medicaid, which goes by different names in different states, and have difficulty accurately remembering their enrollment status over the recall period.

[13] An example from a different context involves the estimated effects of compulsory schooling laws. These laws also vary across and within states, and estimates of their effects change dramatically when the common trends assumption is relaxed (Stephens and Yang 2014).

Most researchers in this area, including CGKL, report effects on outcomes per increase in children's eligibility, rather than enrollment.[14] However, since Medicaid eligibility is unobserved in the CPS, researchers impute it using families' reported income and composition over the previous calendar year. This results in three problems, as Yazici and Kaestner (2000) note. First, because the incomes and composition of poor families are more volatile than those of non-poor families, the eligibility of low-income children is likely to be imputed with significant error. Second, while actual Medicaid eligibility is often re-assessed on a monthly basis, imputed eligibility is based on data from the prior year and fails to capture these fluctuations. Finally, because the simulated eligibility instrument is also measured with error, the resulting estimates may be biased if the instrument and individual eligibility errors are correlated. Administrative enrollment data are not subject to imputation issues, are reported at high frequency, and, although not directly comparable to eligibility, more closely reflect use of Medicaid benefits.

IV. DATA AND SAMPLE

The sample under study comprises low-income school children in Chicago. The degree to which these children's Medicaid eligibility was affected by OBRA90 differs based on their cash assistance receipt from July 1991 through December 1997, the period during which the discontinuity existed in Illinois. This section describes the data sources from which the sample and outcomes are drawn, how the sample is defined, and addresses internal and external validity concerns.

[14] As Currie and Gruber (1996a) note, most studies estimate the effects of Medicaid eligibility, rather than enrollment, "since this is the margin that is directly affected by Medicaid eligibility policy." However, some researchers have studied policies that target the enrollment margin directly without affecting eligibility (Aizer 2003, 2007).

IV.A. Data sources

The sample and outcomes used in this analysis are drawn from two sources. First, the Illinois Department of Human Services (IDHS) maintains monthly enrollment information for recipients of three major public assistance programs: cash welfare, Food Stamps, and Medicaid.[15] These data are limited to individuals enrolled in these services, rather than those eligible, and encompass the entire period during which the OBRA90 discontinuity existed.

The second source of data contains the enrollment and graduation status of students in Chicago Public Schools (CPS). These data are available from the 1994-95 academic year onward. In this analysis, I work with student data that have been averaged at the level of birth (month) cohort, race, sex, and a measure of welfare receipt during the OBRA90 discontinuity period.[16]

IV.B. Sample definition

The analysis sample is derived from a larger sample of individuals living in Cook County, Illinois, which includes the city of Chicago, who ever enrolled in AFDC, Food Stamps, or Medicaid between July 1994 and July 1997. From this sample, I retain children born within five years of September 30, 1983 enrolled in a CPS school at any point from the 1994-95 academic year onward. I exclude any children who meet the inclusion criteria only through enrollment in Medicaid, as they may compromise the internal validity of the study, as discussed in further detail below. This definition yields 89,453 children, most of whom are black or Hispanic and live in a female-headed household (Table 1).

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[15] IDHS enrollment data include the period prior to July 1997, when its predecessor agency, the Illinois Department of Public Aid (IDPA), administered the state's AFDC, Food Stamps, and Medicaid programs.

[16] Any cell containing fewer than 10 sample members (less than 1 percent of the sample) is dropped.

Because cash assistance receipt automatically entitles a child to Medicaid eligibility regardless of birthdate, not all children in the sample are equally likely to be affected by OBRA90. Almost 20 percent of children received cash assistance continuously from July 1991 through December 1997, the period during which the OBRA90 discontinuity existed. The remaining 80 percent, however, went without welfare for some or all of this period, either due to ineligibility, administrative error, or residency outside the state; many, though not all, gained eligibility as the result of OBRA90.

To facilitate the analysis, the sample is divided into quartiles based on the number of months a child went *without* welfare during the OBRA90 period (Figure 2). Children in the first quartile, who received welfare continuously or missed at most a month of coverage, were virtually unaffected by the law. Children in the fourth quartile went without cash assistance for at least 60 percent, and on average 86 percent, of the period, and therefore received the largest potential "dose" of treatment. Most of the analysis will focus on children in this last quartile.

IV.C. Sample limitations

Defining the sample using public assistance receipt raises concerns about internal and external validity. The most pressing concern is that the treatment under study—an expansion of Medicaid eligibility—can bias my estimates by affecting the sample's composition. For this reason, children only enrolled in Medicaid and no other form of public assistance between July 1994 and July 1997 are excluded from the analysis. Still, enrollment in AFDC or Food Stamps may be affected by the OBRA90 expansion if, for example, a family exits welfare or begins receiving Food Stamps upon learning that a child is newly eligible for Medicaid.[17] If this

[17] The income threshold to receive Food Stamps is 130 percent of the poverty level. All children eligible for Medicaid under OBRA90 were eligible for Food Stamps as well.

happens, then sample members born after the cutoff may have higher family incomes or be in worse health than those born before the cutoff.[18] This would violate the central principle of RD designs: that treatment status (e.g., Medicaid eligibility) be "as good as" randomly assigned for individuals near the cutoff.

I test for non-random selection into the sample using two methods. First, I look for discontinuities in baseline characteristics around the cutoff. Visual inspection (Appendix Figure 1) suggests, and an omnibus test confirms, that I cannot reject the null of differences in several baseline characteristics being jointly zero among children born within six months of the cutoff.[19] Second, as suggested by McCrary (2008), I look for a discontinuity in the density of the birth month distribution at the cutoff. Using a bin size of 1 and an automatic bandwidth selection procedure, the test suggests a small, negative discontinuity: children born after September 1983 are slightly underrepresented in the sample (Appendix Figure 2, Panel A). However, as McCrary (2008) notes, the best choice of bandwidth may be based on subjective inspection of the distribution itself, which shows a number of "dips" apart from the one near the cutoff. Using a slightly larger bandwidth to account for this potential under-smoothing, the discontinuity is no longer significant (Appendix Figure 2, Panel B).[20] Although these tests cannot definitively rule out the existence of systematic, unobserved differences among individuals around the cutoff, they provide some assurance that this is unlikely to be the case.

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[18] Unlike children eligible for Medicaid as the result of cash assistance receipt who are enrolled automatically by caseworkers, those made eligible by OBRA90 must voluntarily enroll, and some may do so only upon requiring medical care.

[19] This test is conducted by combining the results of separate regressions of an indicator for being born after the cutoff against the baseline characteristics in Table 1. I adjust for non-independence of these characteristics within birth cohort by clustering standard errors at that level. See Table 1 for details.

[20] As a robustness check, performing this test using placebo birth month cutoffs around the true cutoff suggests that both positive and negative discontinuities are occasionally detected, and the default bandwidth results in over-rejection of the null that no discontinuity exists (Appendix Figure 2, Panels C and D).

Another concern deals with external validity: do the analysis results generalize to the full population of children affected by OBRA90? Based on the sample's definition, which excludes children not receiving public assistance, the answer must be no. Because individuals who receive public assistance are typically more disadvantaged and in worse health than those who do not (Currie 2009), the results obtained here likely overstate the effects of expanding Medicaid eligibility. As Blank and Ruggles (1996) note, individuals eligible for public aid broadly fall into two groups: those who enroll immediately because they are disadvantaged and expect to remain that way, and those who do not enroll because they expect (correctly) their eligibility to be brief. This sample includes only the former.

V. EMPIRICAL STRATEGY

I use a discontinuity in birth cohort affecting the probability of being eligible for Medicaid to estimate its effects on educational attainment. Consider child *i* born in cohort c_i, where $c_i = 0$ represents October 1983. Due to the OBRA90 expansion, the probability that child i is eligible for Medicaid (D_i) increases discontinuously as c_i crosses zero:

$$\lim_{c_i \downarrow 0} P[D_i = 1|c_i] - \lim_{c_i \uparrow 0} P[D_i = 1|c_i] > 0$$

Because a child's birth cohort is not the sole determinant of her eligibility—other factors, such as income, also play a role—the above difference does not equal one. The discontinuity at $c_i = 0$ is therefore "fuzzy" rather than "sharp."

Because I do not observe eligibility, I instead estimate the reduced form effect of being born after the cutoff on an outcome of interest, Y_i:

$$\tau = \lim_{c_i \downarrow 0} \mathbb{E}[Y_i|c_i] - \lim_{c_i \uparrow 0} \mathbb{E}[Y_i|c_i]$$

For this estimate to have a causal interpretation, additional assumptions are required (Imbens and Angrist 1994). Specifically, if c_i crossing zero only increases the probability of a child becoming Medicaid-eligible (monotonicity), and if it cannot affect the outcome except through its effect on eligibility (exclusion), then τ represents an estimate of the "intent to treat" effect: the causal effect of increasing the probability that a child is Medicaid-eligible as the result of being born just after the cutoff.

I estimate this effect using three methods—two parametric and one non-parametric—that differ in the stringency of their assumptions and the precision of their estimates. The first parametric method takes advantage of the fact that most children in the sample—those in the first, second, and third quartiles—experience little to no treatment because they receive welfare during most or all of the time the discontinuity exists. Though no discontinuity in Medicaid enrollment or any educational outcome is expected for these children, they nevertheless provide additional statistical power for estimating shared terms with children in the fourth quartile, who are most exposed to treatment. For example, consider estimating the following equation using least squares:

$$Y_i = \alpha + X_i\beta + \sum_{j=1}^{2}\left(\kappa_j {c_i}^j + \kappa_j^* T_i {c_i}^j\right) + \pi Q_i^{(4)} + \tau_1 T_i + \tau_2 Q_i^{(4)} T_i + \varepsilon_{it} \qquad (1)$$

where $T_i = 1[c_i \geq 0]$ is an indicator for child *i* being born after the cutoff, and $Q_i^{(4)} = 1\left[w_i \geq w^{(75)}\right]$ is an indicator for child *i* being in the fourth quartile of months without welfare during the discontinuity period.[21] All observations contribute to the estimation of β, the

[21] If w_i represents the number of months a child is without welfare from July 1991 through December 1997, and $w^{(75)}$ is the 75th percentile of that distribution for all children in the sample, then $Q_i^{(4)}$ represents a child in the fourth quartile of the observed distribution of w.

coefficient on a vector of demographic characteristics,[22] and $\{\kappa_j, \kappa_j^*\}$, which fit a quadratic in birth cohort separately on either side of the October 1983 cutoff. Only observations from children in the fourth quartile contribute to the estimation of π and τ_2, which reflect differences in the intercept for children born before ($T_i = 0$) and after ($T_i = 1$) the cutoff, relative to children in the lower quartiles.

The coefficient of interest in equation (1) is the linear combination $\tau_1 + \tau_2$. The first term, τ_1, measures the discontinuity between children in the lower quartiles born before and after the cutoff, which in most cases is approximately zero due to the small dose of treatment these children receive. The second term, τ_2, captures any additional discontinuity experienced by children in the fourth quartile. The combination of these two terms, therefore, captures the total treatment effect of having a higher probability of being eligible for Medicaid.

Pooling observations for all children, regardless of their exposure to the treatment, improves precision by estimating common terms on the full dataset. However, the implicit assumption is that these coefficients—β, κ_j, κ_j^*—do not vary substantially between children in the upper and lower quartiles. In particular, if the cohort trends of children most exposed to treatment differ from those of children least exposed, then the estimate of τ_2 could exhibit substantial bias.[23]

An alternative parametric estimation technique that trades off greater variance for a reduction in bias is to estimate equation (1) using only children in the upper quartile:

$$Y_i = \alpha + X_i\beta + \sum_{j=1}^{2}\left(\kappa_j {c_i}^j + \kappa_j^* T_i {c_i}^j\right) + \tau T_i + \varepsilon_{it} \tag{2}$$

[22] Characteristics include race, sex, birth calendar month, and the sex and birth year of the household head.
[23] This is analogous to the common trend assumption in a difference-in-differences estimation strategy.

This is a straightforward parametric RD estimation, as used by Wherry and Meyer (2015) and others. The coefficient of interest, τ, measures the discontinuity for children born before and after the cutoff in the fourth quartile only.

The third and most taxing estimation technique involves the use of non-parametric methods, which have been shown to address many of the shortcomings of parametric techniques (see, e.g., Gelman and Imbens 2014).[24] The most common non-parametric approach, local linear regression, involves estimating a kernel-weighted linear regression on observations within a fixed bandwidth. A number of data-driven approaches for choosing an asymptotically mean squared error optimal bandwidth have been developed in recent years; I utilize the one proposed by Calonico, Cattaneo, and Titiunik (2014).[25] The authors' main contribution, however, is the development of a novel variance estimator that yields bias-corrected confidence intervals for the local linear regression RD estimator. Failing to account for this bias results in confidence intervals with lower than expected empirical coverage and that lead to over-rejection of the null hypothesis.

VI. RESULTS

The analysis proceeds in three steps. First, I offer evidence that Medicaid enrollment follows the expected pattern: children in the fourth quartile born after September 30, 1983 are more likely to be enrolled in Medicaid during the months in which the discontinuity existed. Second, I estimate the cumulative increase in Medicaid enrollment these children experience

[24] These include the introduction of bias from using observations far from the cutoff and sensitivity to the degree of polynomial used.

[25] As a robustness check, estimates using alternative bandwidth selection methods, such as those proposed by Imbens and Kalyanaraman (2012) and Ludwig and Miller (2007), are provided in the Appendix.

over the duration of that period. Finally, I present results that suggest these children, and particularly males, are more likely to graduate high school.

VI.A. Are children born after the cutoff more likely to be enrolled in Medicaid?

From July 1991 through December 1997, the period when the OBRA90 discontinuity existed, children in the fourth quartile born after the cutoff are more likely to be enrolled in Medicaid than those born earlier, relative to children in the first quartile. This can be demonstrated using a difference-in-differences estimation, calculated using least squares separately for each month during this period:

$$Medicaid_i = \alpha + \pi T_i + \sum_{j=2}^{4} \left(\beta_j Q_i^{(j)} + \delta_j Q_i^{(j)} T_i \right) + \varepsilon_i \tag{3}$$

The coefficients δ_j, plotted in Figure 3, capture the change in Medicaid enrollment between children born before and after the cutoff in quartile j (first difference), relative to children in the first quartile (second difference).

The top panel of Figure 3 presents results for children in the second quartile, who receive welfare approximately 94 percent of the time between July 1991 and December 1997. Relative to children in the first quartile, those born after the cutoff are no more likely to be enrolled in Medicaid than those born before throughout most of this period. Beginning in the middle of 1997 and coinciding with the enactment of federal welfare reform, a gap emerges as families exit welfare and children born after September 30, 1983 enroll with greater frequency than those born before. A similar pattern holds for children in the third quartile (middle panel of Figure 3).[26]

[26] The upward trend for children in the third quartile begins in early 1996, shortly after Illinois implemented reforms to its AFDC program under a federal waiver that imposed additional requirements on recipients (see Appendix A).

Children born after the cutoff in the fourth quartile are, on average, 5.6 percentage points more likely to be enrolled in Medicaid during each month of the discontinuity period than those born before the cutoff, relative to children in the first quartile. The expansion of Medicaid eligibility in Illinois that occurred alongside the implementation of CHIP in January 1998 marks the point at which the enrollment gap between children born before and after the cutoff begins to dissipate.

To summarize, OBRA90 increased the likelihood that a child born after September 30, 1983 was enrolled in Medicaid between July 1991 and December 1997. With the exception of the months surrounding the enactment of welfare reform, this effect is concentrated almost entirely among children who received cash assistance least often during that period. I now turn to estimating the cumulative impact that OBRA90 had on these children's Medicaid enrollment.

VI.B. How much additional Medicaid coverage did affected children gain?

Table 2 presents regression estimates of the expansion's effect on cumulative enrollment over the discontinuity period. Column 1 reports estimates from the pooled quadratic specification (equation 1) estimated on the full sample, while columns 2 and 3 report estimates from quadratic and local linear regressions estimated only on children in the fourth quartile. Combining males and females, children in the fourth quartile born after the cutoff were enrolled in Medicaid for an additional 2.5 to 4 months between July 1991 and December 1997, an increase over the baseline mean of between 7 and 11 percent. Visual evidence of this discontinuity is provided in Figure 4.

When considering males and females separately, a pattern emerges: males appear to be likelier to take up Medicaid than females.[27] Point estimates from the quadratic and local linear

[27] Although eligibility is unobserved, neither the wording of the OBRA90 law nor the observable characteristics of the sample suggest that males are more likely to gain eligibility than females, or vice versa.

regressions are larger and, in the case of the quadratic, more precisely estimated for males than for females.[28] Males born after September 30, 1983 are enrolled in Medicaid for, on average, 3 to 4 additional months, while females gain approximately 1 month.[29] Several factors may explain this divergence. Male children are more likely to obtain care from an emergency department,[30] thereby increasing their odds of being enrolled in Medicaid by a hospital. Further, male children are more likely to be identified by a parent or teacher as having a learning disability, both for cultural reasons and due to differences in how these disorders present by gender (Boyle et al. 2011). If differential demand for medical care by gender is driving this phenomenon, then males are more likely than females to enroll and therefore benefit from the additional eligibility that OBRA90 provides.

VI.C. Does additional Medicaid eligibility affect educational outcomes?

Table 3 presents regression estimates of the expansion's effect on high school graduation rates. Across genders, the pooled quadratic specification (column 1) implies that 2.5 to 4 months of additional Medicaid coverage, on average, increased graduation rates by 2.3 percentage points (5 percent), and rules out an increase smaller than 0.4 percentage points (0.9 percent). Estimates from the quadratic and local linear specifications (columns 2 and 3) are considerably smaller and less precise.

As with Medicaid take-up, males appear more responsive to the treatment than females. Across the three specifications, estimates of the improvement in their graduation rates range

[28] The imprecise point estimate for the enrollment effect on males obtained using a local linear regression is sensitive to the choice of bandwidth. Estimates obtained using bandwidths chosen with the procedures proposed by Imbens and Kalyaranaman (2012) and Ludwig and Miller (2007) are all significant and of similar magnitude (Appendix Table 2).

[29] Estimates from the pooled quadratic specification (column 1) appear uniformly larger than those from the quadratic or local linear regression estimated only on children in the fourth quartile, possibly suggesting that the bias introduced by including children less affected by the treatment is driving this disparity.

[30] http://www.cdc.gov/nchs/data/hus/hus14.pdf#079

from 3.5 to over 6 percentage points, or 9 to almost 18 percent. Only the quadratic estimate (column 2) fails to reject a null effect. The analogous estimates for females are generally small and statistically insignificant. The local linear regression estimate of the effect on females (column 3), which implies that eligibility *reduces* graduation rates, should be taken with a grain of salt: it is sensitive to bandwidth choice (Appendix Table 2), and likelier to be the result of under-smoothing when yielding a precise estimate where parametric estimation methods do not.

The CPS data allow for a limited exploration of the potential mechanisms that may be driving these results. One possibility is that by improving children's health, Medicaid coverage reduces their absences from school. Another is that children are better able to focus in class, improving their academic performance and minimizing disruptive behavior. I measure the first possibility directly, albeit using only data on high school absences,[31] and the second using data on grade repetition throughout a student's time in CPS.

Tables 4 and 5 present regression estimates of the expansion's effect on absences and grade repetition, respectively. Neither the pooled nor quadratic estimates on absences are statistically distinguishable from zero, for each gender considered separately or together. A similar caution about under-smoothing applies to the local linear regression estimates on absences, which are statistically significant while those from parametric estimation methods are not. Each panel of the grade repetition results includes estimates of varying sign, most of which are statistically insignificant.

Overall, the results suggest that Medicaid eligibility improves graduation rates for males. This finding is robust to the use of several estimation methods, though a quadratic estimated only using those children most likely to gain eligibility fails to rule out a null effect. Evidence on the mechanisms through which such an improvement in graduation rates takes place is murkier.

[31] Absence data from earlier grades are unavailable.

There is no discernable impact of eligibility on grade repetition or high school absences. However, it is worth noting that the eligibility discontinuity ceases to exist by the time children near the cutoff enter high school. Therefore, it may be that absences in elementary and middle school—periods contemporaneous with the OBRA90 discontinuity—were reduced, and the resulting human capital gains persisted into high school.

VII. DISCUSSION

The results of this analysis suggest that expanding Medicaid eligibility may raise high school graduation rates significantly for males. This section provides answers to two follow-up questions: why are effects larger for males than females, and are estimates of this magnitude plausible?

VII.A. Why might males respond more positively to Medicaid eligibility than females?

High rates of Medicaid enrollment among male children suggest they are more likely to receive care when made eligible than female children. The likelihood of this care improving their educational outcomes, however, depends on the conditions it is meant to address. One hypothesis that may explain the gender divergence in educational attainment results is that males are more likely to exhibit behavioral disorders that inhibit their school performance, and these disorders may be addressed via better access to healthcare. For example, males are nearly three times as likely to exhibit clinically significant symptoms of ADHD, the most common chronic mental health condition among children in the US (Cuffe et al. 2005). Children diagnosed with ADHD are more likely to drop out of school, be expelled, or repeat a grade (Barkley 1998; Weiss and Hechtman 1993). Currie and Stabile (2006) show that children with ADHD perform worse on a range of schooling outcomes, and the effects are large relative to those of chronic physical health problems like asthma.

Medicaid eligibility could significantly improve the educational outcomes of children with ADHD by making the care necessary to manage their symptoms more accessible. For several decades, the standard of care for children with ADHD has centered on a combination of counseling and medication using stimulants, the latter of which generates a positive response in over 70 percent of children on the first trial (Cantwell 1996). Between half and three quarters of children diagnosed with ADHD are prescribed some type of stimulant, usually methylphenidate, sold under the brand name Ritalin. In 1995, 1.5 million children aged 5 through 18, or almost 3 percent of this age group, were prescribed this medication (Robison et al. 1999). If access to counseling or medication to manage ADHD symptoms is improved when children become eligible for Medicaid, then this may account for the positive response of male children to the OBRA90 expansion.

VII.B. Are estimates of this magnitude plausible?

The smallest of the three reported point estimates for the graduation rate effect on male children—a 3.5 percentage point increase, relative to a mean of 38.3 percent—is substantial. Although the 95 percent confidence intervals admit considerably smaller effects, it is important to place this estimate in perspective. Evidence of childhood interventions producing large improvements in graduation rates is not unheard of. For example, the Perry Preschool Program increased the likelihood that a female participant graduated high school or obtained a GED at age 27 by 49.4 percentage points (Anderson 2008). However, Perry and similar interventions operate through different channels, on different samples, and at a much different scale than the Medicaid expansion studied here. A program whose participants closely resemble those in this study is the Chicago Child-Parent Center (CPC), which provides education, family, and health services to disadvantaged children from ages 3 to 9. A non-randomized evaluation of the CPC program found that, relative to children in non-participating schools, those who took part for 1 to 2 years were 11.2 percentage points more likely to complete high school, and this effect was even larger for male children (13.6 percentage points) (Reynolds et al. 2001).

The closest estimate in the literature on the effect of a Medicaid expansion on children's school completion is from the study by CGKL. Drawing a direct comparison with their estimate—that increasing Medicaid eligibility throughout childhood by 10 percentage points reduces the drop out rate by 4.1 percent—is difficult without observing children's eligibility. One way to facilitate this comparison is to use an estimate of the Medicaid take-up rate from the OBRA90 expansion, which range from 7.7 percent (Card and Shore-Sheppard 2004) to 34 percent (Wherry et al. 2015). However, applying these estimates naively to this sample is problematic given children's high baseline rate of Medicaid enrollment and, therefore, eligibility. For example, male children in the fourth quartile born before September 30, 1983 are enrolled in Medicaid during the discontinuity period for, on average, 37 out of 78 months, implying a minimum take-up rate of 48 percent, already in excess of the 34 percent estimated by Wherry et al. (2015). This is not surprising when considering how the sample was constructed: children receiving public assistance are, by definition, taking up benefits for which they are eligible.

To do a back-of-the-envelope comparison with the CGKL results, assume that children in this sample made eligible for Medicaid enroll 75 percent of the time. This implies that male children in the fourth quartile born before the cutoff were eligible for 49.3 of the 78 months the discontinuity existed.[32] If male children born after the cutoff enrolled in Medicaid for 4 additional months during this period (Table 1), then a 75 percent take-up rate suggests they gained 5.3 months of eligibility, an increase of 10.8 percentage points. This is roughly comparable to the magnitude of the eligibility increase reported by CGKL. If I reframe my estimates as an effect on high school non-completion, a 3.5 percentage point decline relative to a mean of 61.7 percent is a reduction of almost 6 percent, close to the magnitude CGKL report (4.1 percent).

As other studies of children affected by the OBRA90 expansion note, these effects are not experienced uniformly across all children born after the cutoff. For example, Wherry and Meyer (2015) and Wherry et al. (2015) find that OBRA90 significantly reduced mortality and hospitalization in adulthood among black children born after the cutoff. In simulation exercises, the authors determine that the average black child born in October 1983 gained 0.82 years of Medicaid eligibility during her childhood, relative to one born a month earlier. However, this increase is distributed unevenly: among those gaining any eligibility, the average increase was 4.8 years. If the children experiencing these large eligibility gains are also in worse health, then the estimated effects are not implausible. In a similar vein, the effects on Medicaid take-up and high school graduation estimated in this paper are probably not uniform: the sickest children were the likeliest to enroll in Medicaid and also to benefit from insurance coverage.

VIII. CONCLUSION

This paper studies the effects of Medicaid eligibility on children's educational attainment. Using a discontinuity in federal policy that expanded eligibility among poor children born after

[32] If average Medicaid enrollment for this group was 37 months, then a 75 percent take-up rate implies that eligibility averaged 37/.75 = 49.3 months.

September 30, 1983, I demonstrate that low-income students in Chicago Public Schools were likelier to be enrolled in Medicaid throughout the period during which the discontinuity existed. This additional insurance coverage may have also increased their high school graduation rates, and did so more for males than females.

These findings are consistent with a recent literature documenting sizable long-term effects of Medicaid eligibility on health (Wherry and Meyer 2015; Wherry et al. 2015) and human capital (Cohodes, Grossman, Kleiner, and Lovenheim 2015; Brown, Kowalski, and Lurie 2015). Taken together, these results imply that expanding children's access to health insurance can produce large and durable improvements in even their non-health outcomes.

Appendix A: Changes to AFDC in Illinois[33]

The Aid to Families with Dependent Children (AFDC) program in Illinois underwent several changes in the 1990s prior to being phased out and replaced by the Temporary Assistance to Needy Families (TANF) program. Section 1115 of the Social Security Act authorizes the Secretary of Health and Human Services (HHS) to waive requirements in order to allow states to carry out pilot or demonstration projects. Between January 1993 and August 1996, HHS approved waivers in 43 states, including Illinois. Many of the policies in these waivers were later incorporated into the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996 that replaced AFDC with TANF.[34]

The waivers granted to Illinois changed several aspects of the state's AFDC program: sanctions, time limits, family caps, income disregards, and child support enforcement.[35] Some of these changes, such as the increased generosity of income disregards, made the program more attractive. Others introduced new conditions, such as work requirements, that increased the likelihood of recipients exiting the program. With the exception of changes to provisions concerning income disregards and child support enforcement, most elements of the waiver took effect October 1995.

Sanctions

Previously, AFDC recipients were required to participate in work-related activities as part of the Job Opportunities and Basic Skills Training (JOBS) program, or otherwise face sanctions for non-compliance.[36] Under the waiver, these sanctions were expanded to include loss of benefits for the entire family for up to six months. Turning down a job offer would also result in the loss of a family's AFDC benefits for three months or until the recipient is employed.

Time limits

Previously, families could receive AFDC benefits for as long as they remained eligible. Under the waiver, families with children aged 13 or older were subject to a time limit of 24 months without earned income. Recipients who failed to find employment within 12 months were required to accept a subsidized work assignment of up to 60 hours per month. Once time limits were imposed, the birth of an additional child did not exempt an individual from complying with them. Families reaching the time limit became ineligible to receive assistance for two years. Extensions were provided to families complying with requirements and making a good faith effort to secure employment who were nevertheless unable to find, or maintain, work that paid at

[33] This section summarizes information helpfully collected by the Office of Human Services Policy, Office of the Assistant Secretary for Planning and Evaluation, U.S. Department of Health & Human Services. <http://aspe.hhs.gov/hsp/isp/waiver2/waivers.htm>

[34] Section 1931 of the Social Security Act mandated that individuals in families that met AFDC income requirements in their state as of July 16, 1996 remain eligible for Medicaid once AFDC ceased to exist. Although federal law did not mandate Medicaid coverage for TANF enrollees, Illinois continued to provide coverage for this group.

[35] A waiver for a pilot program focused on homeless families additionally relaxed asset limits and provided transitional Medicaid coverage for those leaving AFDC.

[36] Recipients exempt from participation in JOBS include those who are ill or incapacitated, underage or enrolled in school, employed, pregnant, caring for an ill or incapacitated family member, or providing care to a young child.

least the maximum AFDC benefit. Families with children under the age of 13, or recipients who were incapacitated or needed to care for someone who is incapacitated, were exempt.

Family caps
Previously, a family's AFDC benefit amount, an increasing function of family size, would rise when a child was born. Under the waiver, families were denied any increase in benefits for children conceived after the family applied for, or was notified of the provision while recipients of, AFDC. To compensate for the reduction in benefits per person, families were allowed to keep more of their earnings while enrolled. The family cap also applied to children conceived while the family was off AFDC for less than three months.

Income disregards
Previously, employed AFDC recipients were entitled to certain disregards when calculating eligibility and benefit levels. Specifically, each recipient received a $90 disregard for work expenses, in addition to the first $30 of earned income and one-third of the remainder for the first four months of AFDC receipt (the "thirty-and-one-third" rule). After four months and up to one year, only $30 of earned income could be disregarded. After one year, the disregard went to zero and the entirety of a recipient's earned income reduced, dollar for dollar, their benefit amount. Under the waiver, effective November 1993, two-thirds of earned income could be disregarded with no time limit, significantly decreasing the effective tax rate on earnings received by AFDC recipients.[37]

Child support enforcement
Previously, AFDC recipients were required to assist in the enforcement of child support orders, under threat of sanction for the custodial parent if they failed to cooperate. Under the waiver, effective June 1996, this sanction was extended to include the AFDC benefits of children if cooperation from the custodial parent was not obtained within six months.

[37] This program was titled Work Pays (Lewis, George, and Puntenney 1999).

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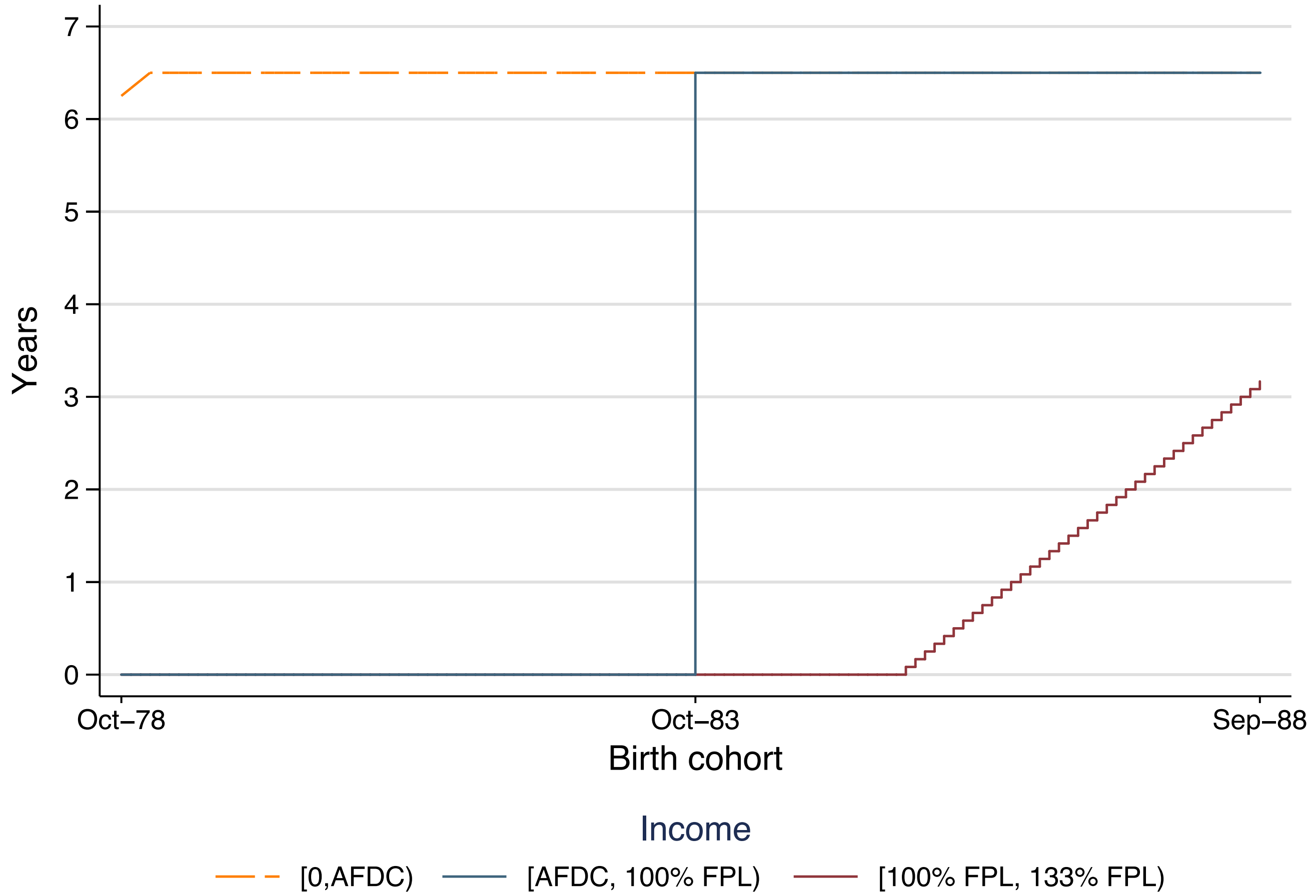
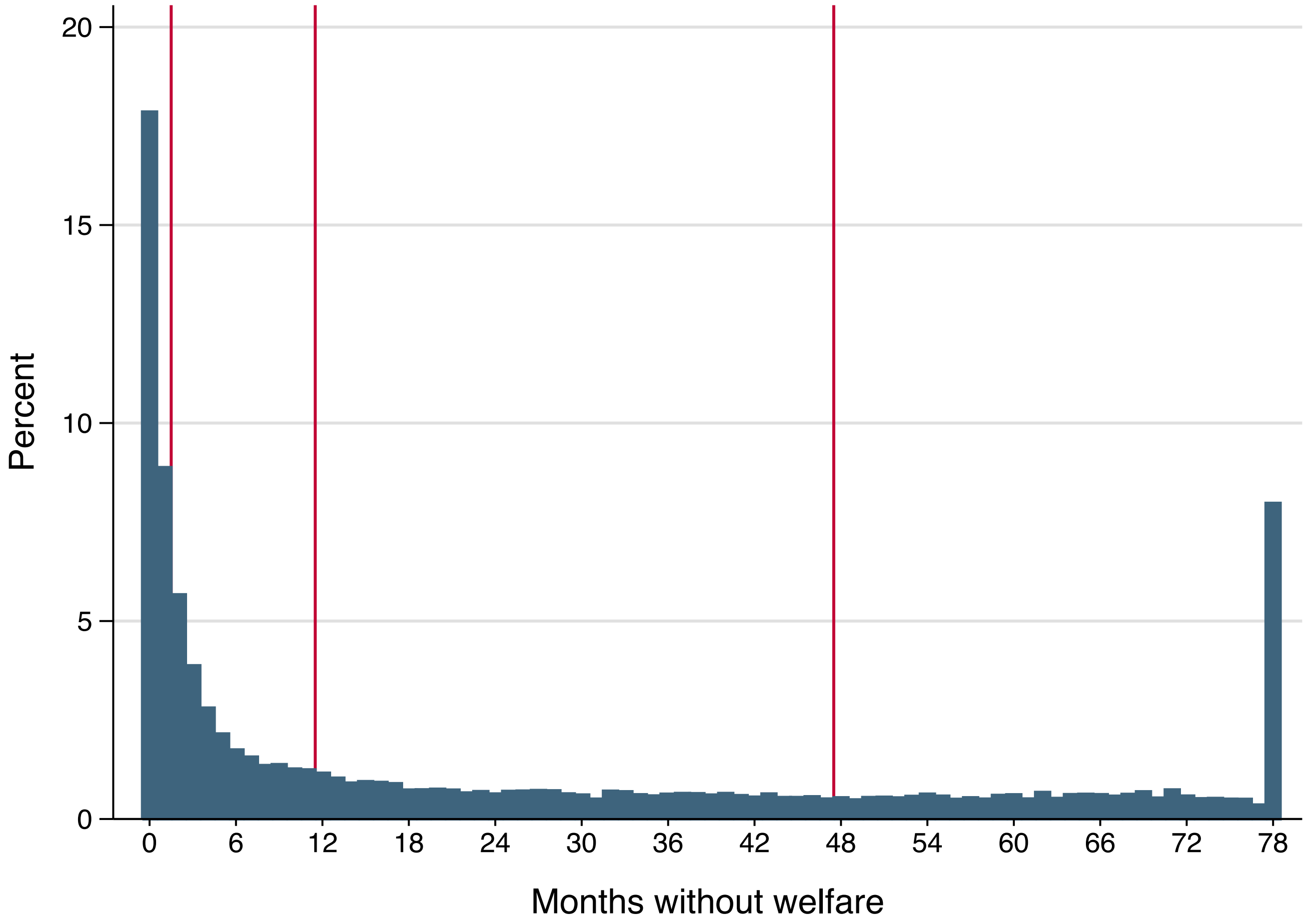
Figure 1: Years of Medicaid eligibility during OBRA90 discontinuity period (Jul. 91 – Dec. 97)



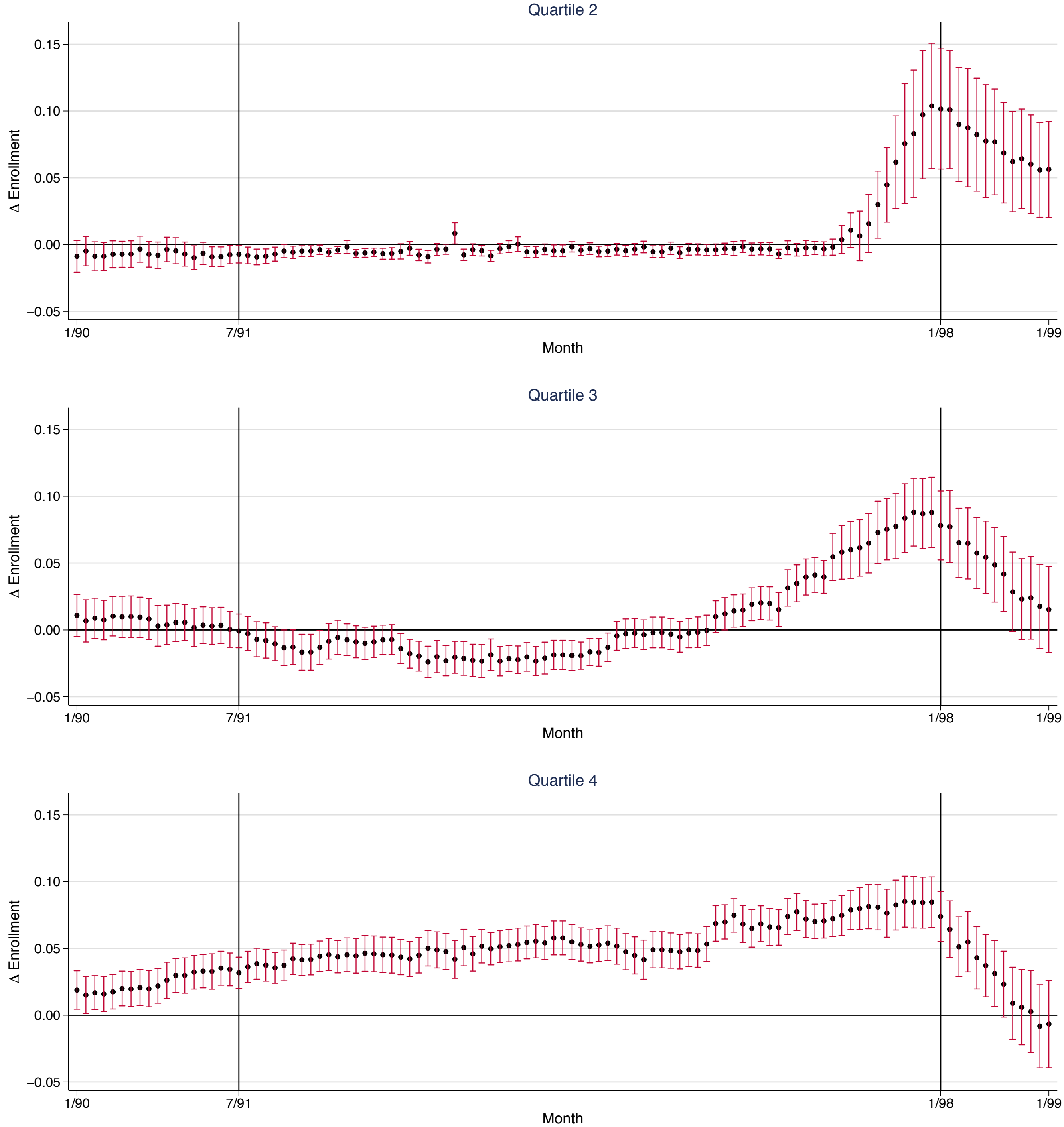
Note: Figure presents the total number of years (up to 6.5) that a child could be eligible for Medicaid between July 1991 and December 1997, the period during which the OBRA90 discontinuity existed in Illinois, by her birth cohort and family income. (This assumes that family income remains fixed during this period.) The AFDC income threshold in Illinois ranged from 41.7 percent of the federal poverty level (FPL) in 1990 to 34.9 percent in 1996 (National Governors Association). The birth cohort-eligibility gradient for children in the highest income category is the result of later cohorts being eligible, up to age 6, under the OBRA89 expansion. See Appendix Table 1 for details. The large discontinuity at October 1983 for children between the AFDC threshold and the poverty level is due to OBRA90.

Figure 2: Months without welfare during OBRA90 discontinuity period (Jul. 91 – Dec. 97)



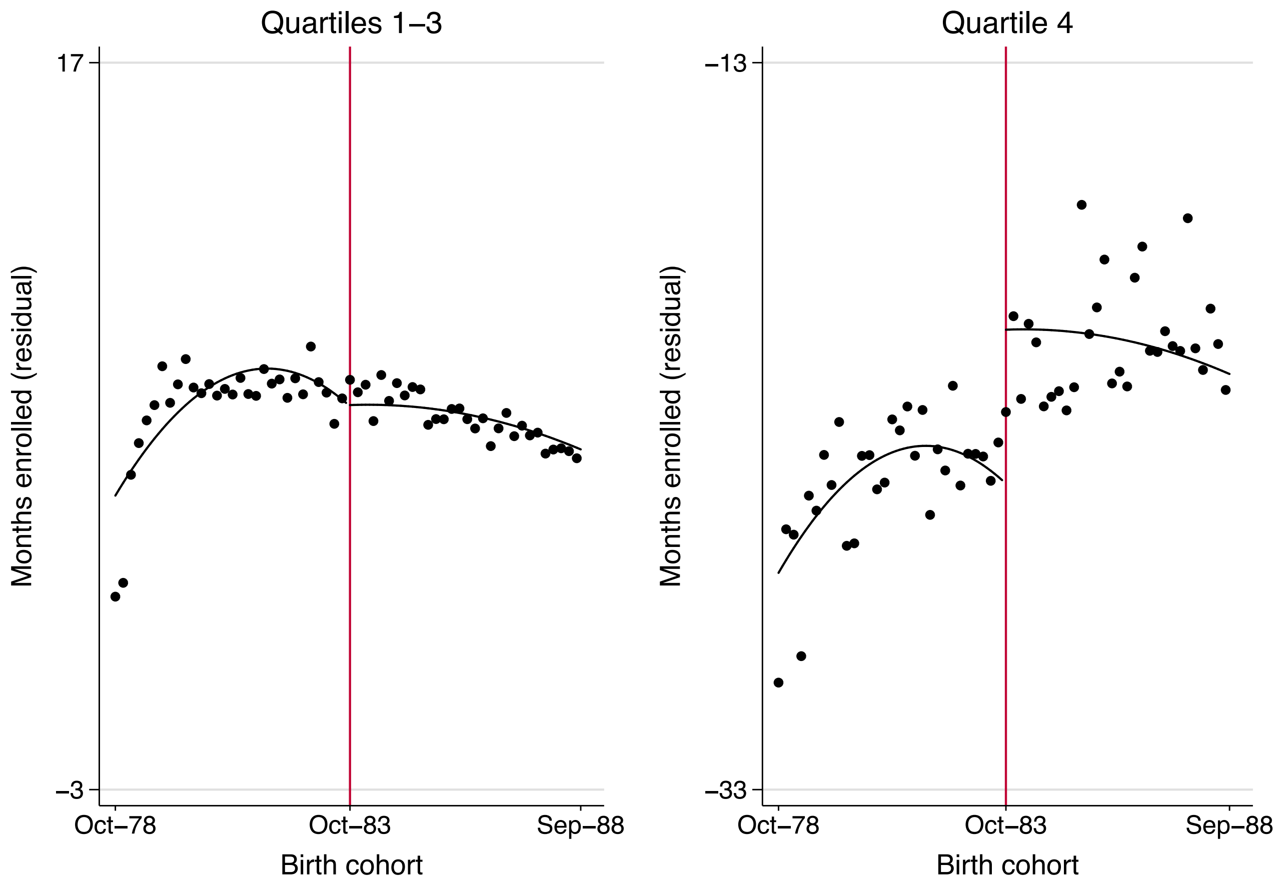
Note: Figure presents the distribution of sample children by months without welfare receipt during the OBRA90 discontinuity period. Red lines indicate quartiles of the sample: 0-1 month (quartile 1), 2-11 months (quartile 2), 12-47 months (quartile 3), and 48-78 months (quartile 4).

Figure 3: Difference-in-differences estimates of monthly Medicaid enrollment



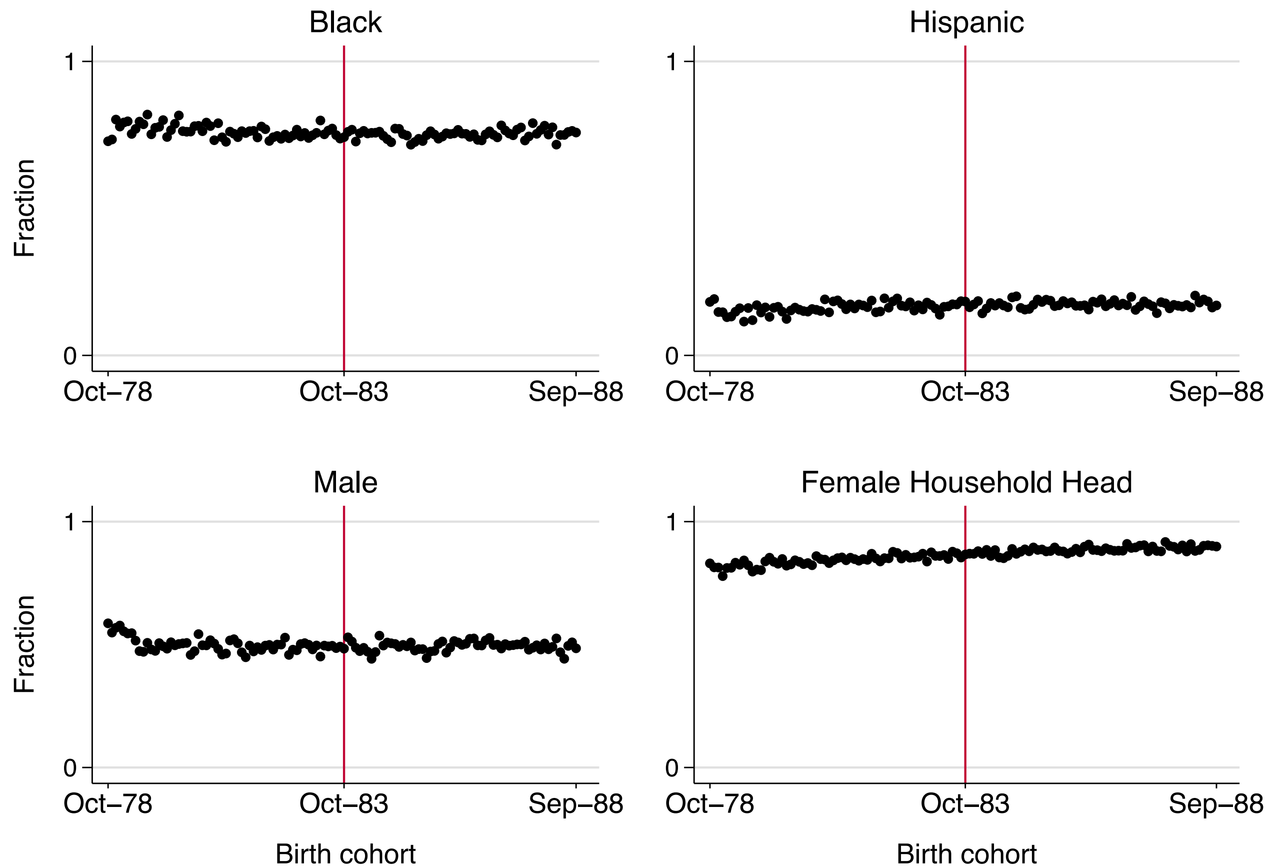
Note: Each panel presents estimates of and 95 percent confidence intervals around δ_j, the difference-in-differences estimator of Medicaid enrollment from equation (3). This parameter represents the change in Medicaid enrollment between children born before and after the cutoff in quartile j (first difference), relative to children in the first quartile (second difference). Black lines indicate the period when the OBRA90 discontinuity was in effect. Standard errors are clustered by birth cohort.

Figure 4: Months enrolled in Medicaid (Jul. 91 – Dec. 97)



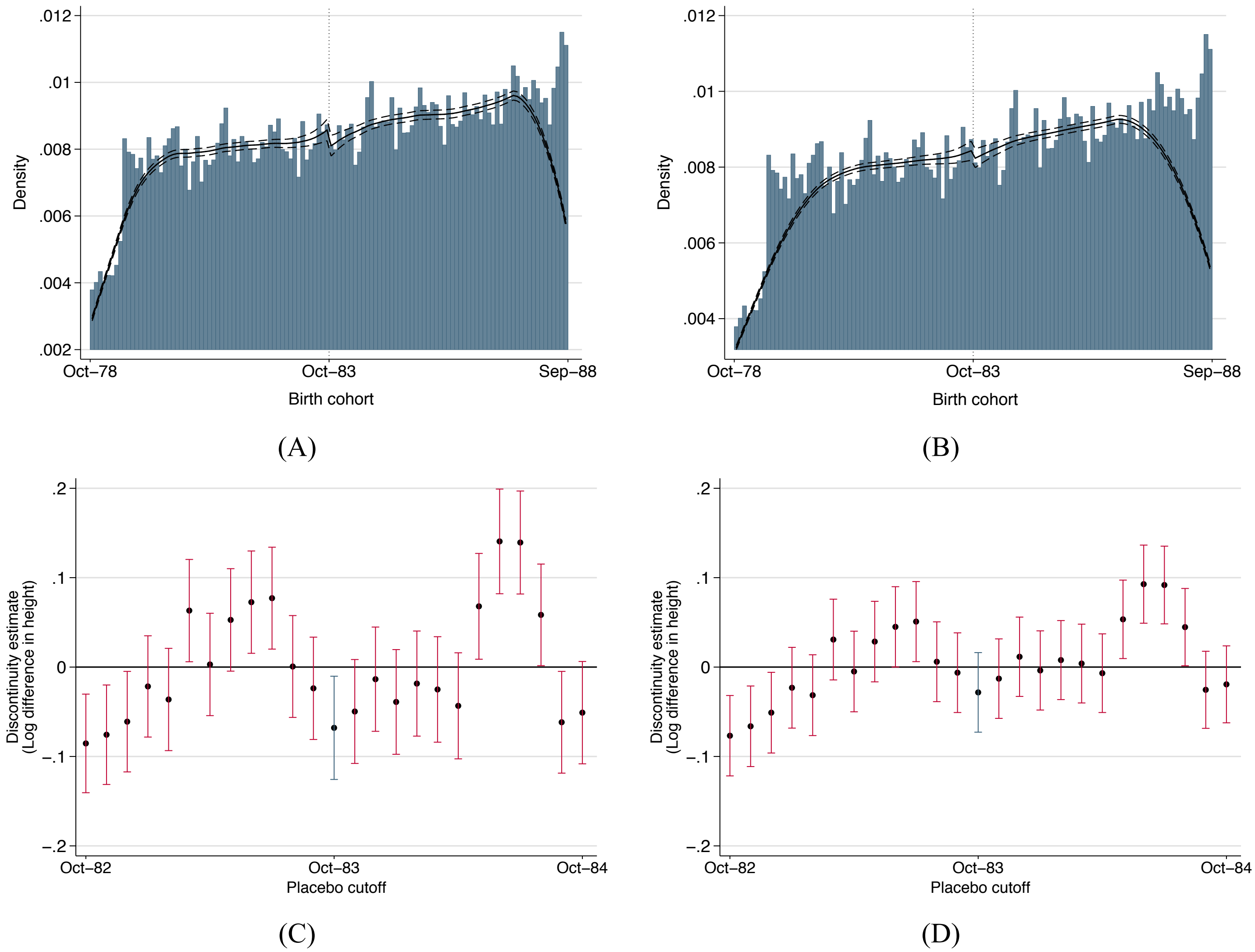
Note: Figure presents residuals from a regression of months a child is enrolled during the OBRA90 discontinuity period on race, sex, birth calendar month, and the sex and birth year of the child's household head. Residuals are averaged and displayed in bins of two birth cohort months. A quadratic in birth cohort is fitted separately on either side of the cutoff, as detailed in equation (1). The left panel includes children receiving welfare relatively more often during the discontinuity period, while the right panel includes children receiving welfare relatively less often during this time.

Appendix Figure 1: Sample members' baseline characteristics by birth cohort



Note: Figure presents averages of four baseline characteristics by birth cohort for sample members.

Appendix Figure 2: Density of the birth month distribution



Notes: Panels (A) and (B) present densities of the sample birth month distribution, overlaid with a local linear smoother and 95 percent confidence intervals. Both linear smoothers are generated with a bin size of 1 month. In panel (A), the bandwidth (14.8) is chosen using the automatic selection procedure outlined in McCrary (2008). In panel (B), a slightly larger bandwidth (25) is used. The corresponding estimates of the discontinuity at October 1983 (log difference in height) are -0.068 (0.029) in panel (A) and -0.028 (0.023) in panel (B). Panels (C) and (D) present discontinuity estimates using the automatically selected and fixed bandwidths, respectively, for several placebo cutoffs. Estimates for the actual cutoff are shaded blue.

Table 1: Summary statistics

	Mean
Male	0.495
Black	0.757
Hispanic	0.169
Female head of household	0.867
Joint test of balanced observable characteristics across the cutoff[1]	
Chi-squared statistic	9.2
p-value	0.100
N (Individuals)	89,453

Notes: Analysis sample includes students in Chicago Public Schools born within five years of September 30, 1983 and enrolled in AFDC or Food Stamps at least once between July 1994 and July 1997. See text for details.

[1] Test of the null hypothesis that differences in the observable characteristics (e.g., race, sex, household head sex and birth year) of individuals born six months on either side of September 30, 1983 are jointly zero.

Table 2: Effect of Medicaid eligibility on Medicaid enrollment

	Months Enrolled: July 1991 - December 1997		
	Pooled	**Quadratic**	**Local Linear Reg.**
	(1)	**(2)**	**(3)**
		Males and Females	
Mean	35.7	35.7	36.2
Estimate	3.9	2.4	2.5
	[2.9, 4.8]	[0.5, 4.2]	[-0.3, 5.3]
N (Indiv.)	89,453	22,327	9,366
		Females only	
Mean	34.4	34.4	35.3
Estimate	3.7	1.3	1.0
	[2.4, 5.0]	[-1.5, 4.1]	[-3.5, 5.2]
N (Indiv.)	45,211	10,720	3,528
		Males only	
Mean	36.9	36.9	37.2
Estimate	4.0	3.2	4.2
	[2.7, 5.4]	[0.6, 5.8]	[-0.4, 8.7]
N (Indiv.)	44,242	11,607	3,708

Notes: Table displays regression discontinuity estimates of the effect of Medicaid eligibility on Medicaid enrollment. Column 1 is estimated using equation (1) on the full sample of children. Column 2 is estimated using equation (2) on children in the fourth quartile only. Column 3 is estimated using the `rdrobust` Stata package with a triangular kernel and bandwidth selection procedure proposed by Calonico, Cattaneo, and Titiunik (2014) on children in the fourth quartile only. Estimates in columns 1 and 2 include controls for a child's race, sex, birth calendar month, and the sex and birth year of the household head, and cluster standard errors by birth month. 95 percent confidence intervals shown in brackets. Means reported for columns 1 and 2 are simple averages for children in the fourth quartile born before October 1983; for column 3, they are kernel-weighted averages for children in the fourth quartile born within the chosen bandwidth before October 1983.

Table 3: Effect of Medicaid eligibility on high school graduation

	Graduated High School		
	Pooled	Quadratic	Local Linear Reg.
	(1)	(2)	(3)
	Males and Females		
Mean	0.459	0.459	0.457
Estimate	0.023	0.015	0.007
	[0.004, 0.043]	[-0.021, 0.051]	[-0.007, 0.025]
N (Indiv.)	89,453	22,327	4,298
	Females only		
Mean	0.551	0.551	0.541
Estimate	0.009	-0.016	-0.030
	[-0.019, 0.037]	[-0.069, 0.036]	[-0.048, -0.017]
N (Indiv.)	45,211	10,720	1,325
	Males only		
Mean	0.383	0.383	0.348
Estimate	0.035	0.040	0.062
	[0.005, 0.066]	[-0.006, 0.087]	[0.035, 0.086]
N (Indiv.)	44,242	11,607	1,261

Notes: Table displays regression discontinuity estimates of the effect of Medicaid eligibility on graduation from public high school in Chicago. Column 1 is estimated using equation (1) on the full sample of children. Column 2 is estimated using equation (2) on children in the fourth quartile only. Column 3 is estimated using the `rdrobust` Stata package with a triangular kernel and bandwidth selection procedure proposed by Calonico, Cattaneo, and Titiunik (2014) on children in the fourth quartile only. Estimates in columns 1 and 2 include controls for a child's race, sex, birth calendar month, and the sex and birth year of the household head, and cluster standard errors by birth month. 95 percent confidence intervals shown in brackets. Means reported for columns 1 and 2 are simple averages for children in the fourth quartile born before October 1983; for column 3, they are kernel-weighted averages for children in the fourth quartile born within the chosen bandwidth before October 1983.

Table 4: Effect of Medicaid eligibility on school attendance

	Average Absences per Year (High School)		
	Pooled	Quadratic	Local Linear Reg.
	(1)	(2)	(3)
		Males and Females	
Mean	24.2	24.2	20.2
Estimate	0.8	0.0	-1.0
	[-0.3, 1.9]	[-1.4, 1.4]	[-1.5, -0.7]
N (Indiv.)	89,453	22,327	3,943
		Females only	
Mean	24.2	24.2	19.1
Estimate	0.6	0.7	0.9
	[-0.7, 2.0]	[-1.6, 3.1]	[0.3, 2.1]
N (Indiv.)	45,211	10,720	976
		Males only	
Mean	24.3	24.3	19.5
Estimate	0.8	-0.5	-0.7
	[-0.7, 2.4]	[-2.9, 1.9]	[-1.6, -0.2]
N (Indiv.)	44,242	11,607	1,438

Notes: Table displays regression discontinuity estimates of the effect of Medicaid eligibility on average absences per year in high school. Column 1 is estimated using equation (1) on the full sample of children. Column 2 is estimated using equation (2) on children in the fourth quartile only. Column 3 is estimated using the `rdrobust` Stata package with a triangular kernel and bandwidth selection procedure proposed by Calonico, Cattaneo, and Titiunik (2014) on children in the fourth quartile only. Estimates in columns 1 and 2 include controls for a child's race, sex, birth calendar month, and the sex and birth year of the household head, and cluster standard errors by birth month. 95 percent confidence intervals shown in brackets. Means reported for columns 1 and 2 are simple averages for children in the fourth quartile born before October 1983; for column 3, they are kernel-weighted averages for children in the fourth quartile born within the chosen bandwidth before October 1983.

Table 5: Effect of Medicaid eligibility on grade repetition

	Repeated Grade		
	Pooled	Quadratic	Local Linear Reg.
	(1)	(2)	(3)
	Males and Females		
Mean	0.449	0.449	0.426
Estimate	0.008	0.038	-0.011
	[-0.016, 0.032]	[0.005, 0.072]	[-0.034, 0.005]
N (Indiv.)	89,453	22,327	2,404
	Females only		
Mean	0.375	0.375	0.346
Estimate	-0.002	0.038	-0.011
	[-0.033, 0.030]	[-0.005, 0.080]	[-0.030, 0.005]
N (Indiv.)	45,211	10,720	1,677
	Males only		
Mean	0.511	0.511	0.498
Estimate	0.014	0.039	-0.026
	[-0.023, 0.050]	[-0.009, 0.087]	[-0.052, -0.011]
N (Indiv.)	44,242	11,607	1,076

Notes: Table displays regression discontinuity estimates of the effect of Medicaid eligibility on the likelihood of repeating a grade. Column 1 is estimated using equation (1) on the full sample of children. Column 2 is estimated using equation (2) on children in the fourth quartile only. Column 3 is estimated using the `rdrobust` Stata package with a triangular kernel and bandwidth selection procedure proposed by Calonico, Cattaneo, and Titiunik (2014) on children in the fourth quartile only. Estimates in columns 1 and 2 include controls for a child's race, sex, birth calendar month, and the sex and birth year of the household head, and cluster standard errors by birth month. 95 percent confidence intervals shown in brackets. Means reported for columns 1 and 2 are simple averages for children in the fourth quartile born before October 1983; for column 3, they are kernel-weighted averages for children in the fourth quartile born within the chosen bandwidth before October 1983.

Appendix Table 1: Federal & state laws expanding Medicaid eligibility for children in Illinois

Legislation	Effective	Children Covered
Deficit Reduction Act, 1984 (DEFRA84)	October 1984	Under age 5 Born after Sept. 30, 1983 Family income-eligible for AFDC
Omnibus Budget Reconciliation Act, 1987[1] (OBRA87)	July 1988	Under age 1 (infants) Family income below 100% FPL
	October 1988	Under age 7 Born after Sept. 30, 1983 Family income-eligible for AFDC
Family Support Act, 1988[2] (FSA88)	April 1990	All ages 1 year of coverage if leaving welfare Family income below 185% FPL
Omnibus Budget Reconciliation Act, 1989 (OBRA89)	April 1990	Under age 6 Family income below 133% FPL
Omnibus Budget Reconciliation Act, 1990 (OBRA90)	July 1991	Under age 19 Born after Sept. 30, 1983 Family income below 100% FPL
Children's Health Insurance Program Act (KidCare)	January 1998	Under age 1 (infants) Family income below 200% FPL Under age 19 Family income below 133% FPL

Notes: For more detailed legislative history, see Currie and Gruber (1996a), Shore-Sheppard (2008), and Wermuth (1998). FPL = federal poverty level.

[1] OBRA87 provided states the option (not exercised by Illinois) of covering infants up to 185% FPL.

[2] FSA88 expanded the Transitional Medicaid Assistance (TMA) program to provide up to a year of Medicaid coverage for families leaving AFDC/TANF due to increased earnings. Qualifying families must receive cash assistance in at least three of the preceding six months. States may optionally charge a premium for the second six months of assistance.

Appendix Table 2: Alternative bandwidths, Medicaid enrollment

	Months Enrolled: July 1991 - December 1997		
	CCT	**IK**	**CV**
	(1)	**(2)**	**(3)**
		Males and Females	
Mean	36.2	36.2	36.2
Estimate	2.5	2.3	2.3
	[-0.3, 5.3]	[0.4, 4.7]	[0.0, 5.2]
N (Indiv.)	9,366	15,927	15,927
		Females only	
Mean	35.3	35.4	35.1
Estimate	1.0	0.7	1.6
	[-3.5, 5.2]	[-3.3, 4.1]	[-1.9, 4.2]
N (Indiv.)	3,528	4,901	10,539
		Males only	
Mean	37.2	36.8	37.6
Estimate	4.2	3.9	2.8
	[-0.4, 8.7]	[0.9, 7.5]	[0.8, 7.4]
N (Indiv.)	3,708	5,666	10,077

Notes: Table displays regression discontinuity estimates of the effect of Medicaid eligibility on Medicaid enrollment from local linear regressions estimated with alternative bandwidth selection procedures on children in the fourth quartile only. Column 1 uses the procedure proposed by Calonico, Cattaneo, and Titiunik (2014). Column 2 uses the procedure proposed by Imbens and Kalyaranaman (2012). Column 3 uses the procedure proposed by Ludwig and Miller (2007). Reported means are kernel-weighted averages for children in the fourth quartile born within the chosen bandwidth before October 1983.

Appendix Table 3: Alternative bandwidths, high school graduation

	Graduated High School		
	CCT	IK	CV
	(1)	(2)	(3)
	Males and Females		
Mean	0.457	0.458	0.365
Estimate	0.007	0.001	0.085
	[-0.007, 0.025]	[-0.011, 0.035]	[0.017, 0.070]
N (Indiv.)	4,298	9,366	916
	Females only		
Mean	0.541	0.553	0.511
Estimate	-0.030	-0.034	-0.009
	[-0.048, -0.017]	[-0.057, -0.027]	[-0.029, 0.025]
N (Indiv.)	1,325	3,004	976
	Males only		
Mean	0.348	0.355	0.206
Estimate	0.062	0.057	0.205
	[0.035, 0.086]	[0.044, 0.086]	[0.105, 0.186]
N (Indiv.)	1,261	3,133	469

Notes: Table displays regression discontinuity estimates of the effect of Medicaid eligibility on graduation from public high school in Chicago from local linear regressions estimated with alternative bandwidth selection procedures on children in the fourth quartile only. Column 1 uses the procedure proposed by Calonico, Cattaneo, and Titiunik (2014). Column 2 uses the procedure proposed by Imbens and Kalyaranaman (2012). Column 3 uses the procedure proposed by Ludwig and Miller (2007). Reported means are kernel-weighted averages for children in the fourth quartile born within the chosen bandwidth before October 1983.

Appendix Table 4: Alternative bandwidths, school attendance

	Average Absences per Year (High School)		
	CCT	IK	CV
	(1)	(2)	(3)
	Males and Females		
Mean	20.2	20.7	18.8
Estimate	-1.0	-0.8	1.1
	[-1.5, -0.7]	[-1.6, -0.8]	[0.6, 1.6]
N (Indiv.)	3,943	7,822	916
	Females only		
Mean	19.1	20.2	17.2
Estimate	0.9	-1.0	3.9
	[0.3, 2.1]	[-1.2, 0.0]	[1.8, 4.1]
N (Indiv.)	976	3,004	447
	Males only		
Mean	19.5	21.3	20.5
Estimate	-0.7	-0.8	-1.7
	[-1.6, -0.2]	[-3.0, -1.7]	[-1.2, -0.2]
N (Indiv.)	1,438	3,517	469

Notes: Table displays regression discontinuity estimates of the effect of Medicaid eligibility on average absences per year in high school from local linear regressions estimated with alternative bandwidth selection procedures on children in the fourth quartile only. Column 1 uses the procedure proposed by Calonico, Cattaneo, and Titiunik (2014). Column 2 uses the procedure proposed by Imbens and Kalyaranaman (2012). Column 3 uses the procedure proposed by Ludwig and Miller (2007). Reported means are kernel-weighted averages for children in the fourth quartile born within the chosen bandwidth before October 1983.

Appendix Table 5: Alternative bandwidths, grade repetition

	Repeated Grade		
	CCT	**IK**	**CV**
	(1)	**(2)**	**(3)**
		Males and Females	
Mean	0.426	0.435	0.390
Estimate	-0.011	-0.006	0.037
	[-0.034, 0.005]	[-0.043, -0.013]	[0.009, 0.057]
N (Indiv.)	2,404	6,694	916
		Females only	
Mean	0.346	0.361	0.332
Estimate	-0.011	-0.011	0.028
	[-0.030, 0.005]	[-0.038, -0.004]	[0.006, 0.056]
N (Indiv.)	1,677	3,714	447
		Males only	
Mean	0.498	0.516	0.483
Estimate	-0.026	-0.017	0.006
	[-0.052, -0.011]	[0.036, 0.127]	[-0.017, 0.029]
N (Indiv.)	1,076	3,327	469

Notes: Table displays regression discontinuity estimates of the effect of Medicaid eligibility on the likelihood of repeating a grade from local linear regressions estimated with alternative bandwidth selection procedures on children in the fourth quartile only. Column 1 uses the procedure proposed by Calonico, Cattaneo, and Titiunik (2014). Column 2 uses the procedure proposed by Imbens and Kalyaranaman (2012). Column 3 uses the procedure proposed by Ludwig and Miller (2007). Reported means are kernel-weighted averages for children in the fourth quartile born within the chosen bandwidth before October 1983.