Essays on the Economics of Anti-Poverty Programs

by

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ABSTRACT

The economic circumstances of a child's upbringing play an outsize role in her development. Children raised in poverty experience significantly worse outcomes, on average, than those of their economically advantaged peers. This dissertation makes a modest contribution to understanding the determinants of children's outcomes, long a goal of social science research. Specifically, it studies the impact of public health insurance and housing subsidies on the lives of low-income children in Chicago.

The first chapter estimates the effect of a public health insurance expansion on students' high school graduation rates. A potential reason why low-income children are typically in worse health is the barrier to obtaining care posed by a lack of health insurance. If being in worse health hinders a child's ability to attend school or concentrate in class, then improving her health by providing insurance may increase her educational attainment.

An act of Congress in the early 1990s broadened the eligibility criteria for Medicaid, a large public health insurance program, to encompass a greater number of low-income children. In doing so, lawmakers also created a natural experiment: because only children born after a certain date gained eligibility under the law, those born before this date can serve as a control group to estimate its effect. Using data on low-income students in Chicago Public Schools, I compare children born on either side of this date and find that raising the likelihood of a child becoming eligible for Medicaid increases her chances of graduating high school. This finding is consistent with recent work testing the same hypothesis using a different research design, and

with studies demonstrating durable health and mortality improvements from gaining Medicaid eligibility during childhood.

The second and third chapters examine the impact and take-up of housing vouchers distributed via lottery in Chicago in 1997. In addition to providing an opportunity to relocate to a better unit and a less distressed neighborhood, vouchers reduce the financial burden on low-income families devoting a substantial portion of their income toward rent. This attribute of vouchers makes them particularly worthy of study: they are an in-kind housing subsidy with a large income transfer component. For the average household studied, the value of the voucher is two-thirds its income.

In the second chapter, co-authored with Brian Jacob and Jens Ludwig, we study the long-term impact on children of being in a family that was offered a voucher. The randomized lottery through which vouchers were distributed makes this possible: comparing families that win to those that do not isolates the causal effect of being offered a voucher. Using administrative data on children's test scores, graduation rates, arrests, earnings, public assistance receipt, and health up to 14 years after the lottery, we find that voucher receipt has little, if any, impact on the outcomes we measure. Although consistent with the findings of the one previous randomized study of housing vouchers, this result stands in contrast to recent work demonstrating large, positive effects of income transfer programs. One possible explanation for this discrepancy is that the additional income parents receive as the result of a voucher offer is spent differently—and in less developmentally productive ways—than additional income received through transfer programs, which often require parents to work more and substitute for their time with paid childcare.

In the third chapter, co-authored with Eric Chyn and Joshua Hyman, we provide descriptive evidence on the low take-up rate of housing vouchers. Among households knowledgeable and motivated enough to enter the lottery, only half of those offered a voucher ultimately use it to rent an apartment, despite the generous subsidy it provides. Understanding why this take-up rate is so low is important because those families with the most to gain from housing assistance may also be among those that find it most difficult to lease up successfully.

Using many of the same administrative data sources as in the second chapter, we detect a non-monotonic relationship between disadvantage and take-up: the unemployed, as well as the employed with relatively high incomes, do not lease up as often as those employed but earning relatively little. We also find that characteristics expected to strongly predict take-up on theoretical grounds, such as local crime rates, have no detectable relationship after controlling for other factors. Finally, we consider the effect that efforts to increase voucher take-up would have on labor supply. Using a reweighting procedure to extend the analysis of Jacob and Ludwig (2012), we find that increasing take-up would reduce participants' hours of work and increase their likelihood of receiving public assistance.

CHAPTER I

The Effect of Medicaid on Educational Attainment: Evidence From Chicago

Medicaid is a public health insurance program designed to shield the families of poor children from the economic insecurity of sickness and to improve children's 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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1.1 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.

1.2 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.

1.2.1 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 (Table B.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

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[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 1.1).

1.2.2 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 Table B.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).

1.3 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.

1.3.1 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

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[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.

1.3.2 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.

1.4 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).

1.4.1 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]

1.4.2 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 1.1).

[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 1.2). 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.

1.4.3 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 (Figure B.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 (Figure B.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 (Figure B.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 1.1. I adjust for non-independence of these characteristics within birth cohort by clustering standard errors at that level. See Table 1.1 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 (Figure B.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.

1.5 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:

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

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:

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

[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.

1.6 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.

1.6.1 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:

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

The coefficients δ_j, plotted in Figure 1.3, 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 1.3 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 1.3).[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.

1.6.2 How Much Additional Medicaid Coverage Did Affected Children Gain?

Table 1.2 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 1.4.

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 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.

1.6.3 Does Additional Medicaid Eligibility Affect Educational Outcomes?

Table 1.3 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

[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.
[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 (Table B.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

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 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 (Table B.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 1.4 and 1.5 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.

[31] Absence data from earlier grades are unavailable.

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. 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.

1.7 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?

1.7.1 Why Might Males Respond More 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.

1.7.2 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 1.1), 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.

[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.

1.8 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 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.

Table 1.1: 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 1.2: 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 1.3: 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 1.4: 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 [-0.3, 1.9]	0.0 [-1.4, 1.4]	-1.0 [-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, 2.0]	0.7 [-1.6, 3.1]	0.9 [0.3, 2.1]
N (Indiv.)	45,211	10,720	976
	Males only		
Mean	24.3	24.3	19.5
Estimate	0.8 [-0.7, 2.4]	-0.5 [-2.9, 1.9]	-0.7 [-1.6, -0.2]
N (Indiv.)	44,242	11,607	1,438

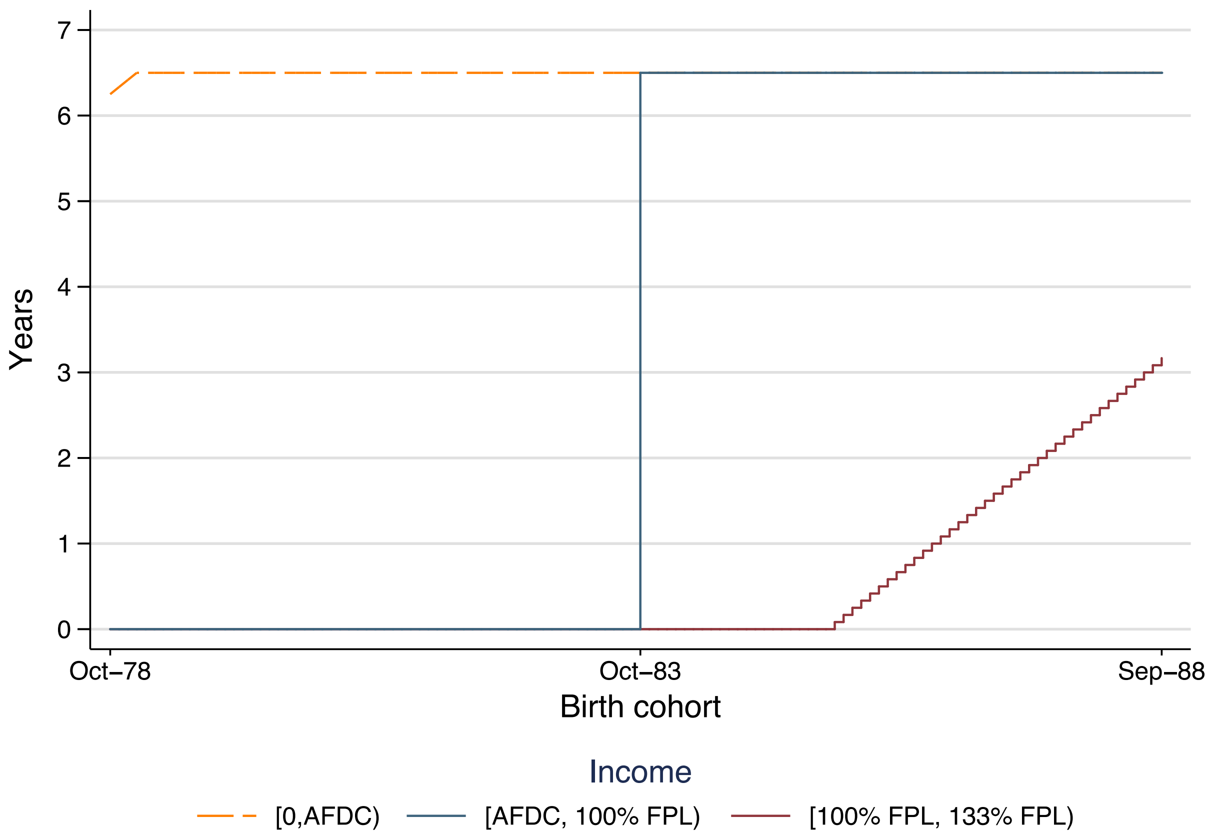
Notes: Table displays regression discontinuity estimates of the effect of Medicaid eligibility on average absences per year in high school. Column 1 is estimated using equation (1) on the full sample of children. Column 2 is estimated using equation (2) on children in the fourth quartile only. Column 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 1.5: 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

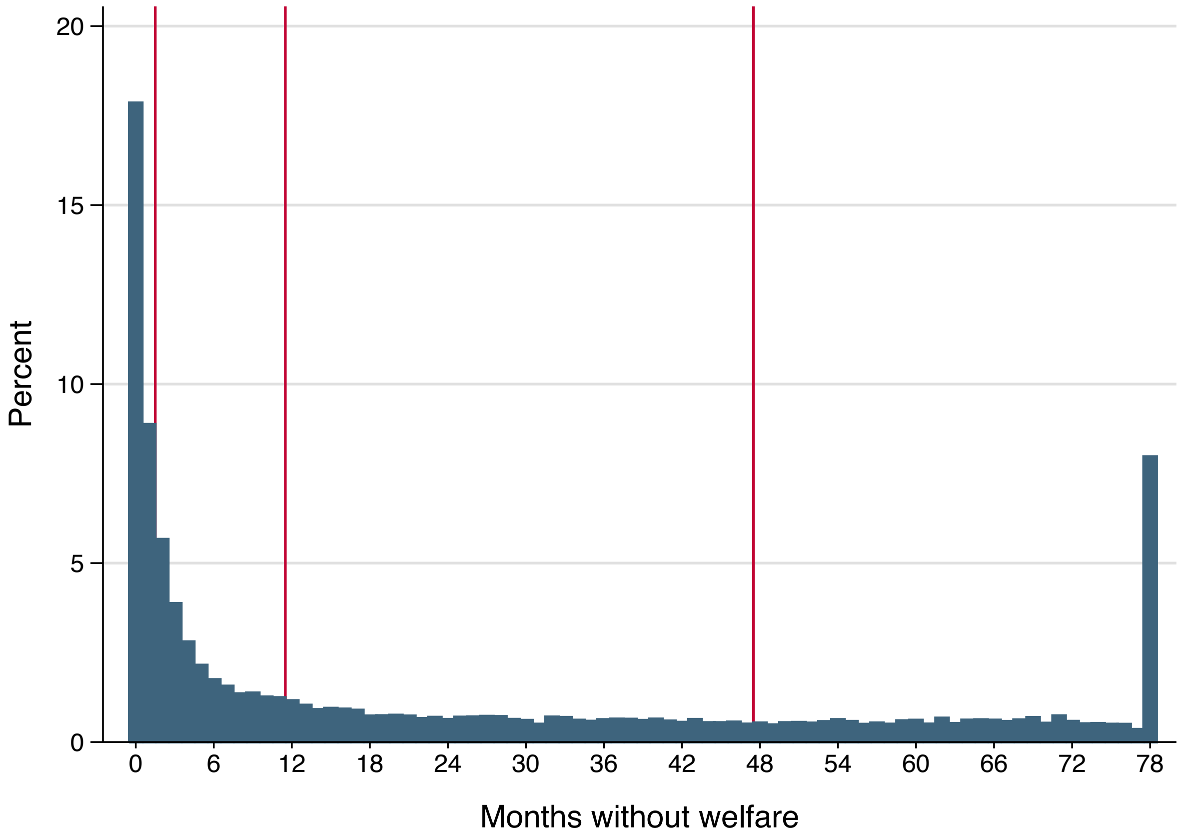
Notes: Table displays regression discontinuity estimates of the effect of Medicaid eligibility on the likelihood of repeating a grade. Column 1 is estimated using equation (1) on the full sample of children. Column 2 is estimated using equation (2) on children in the fourth quartile only. Column 3 is 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.

Figure 1.1: Years of Medicaid Eligibility During OBRA90 Discontinuity Period (Jul. 91 – Dec. 97)



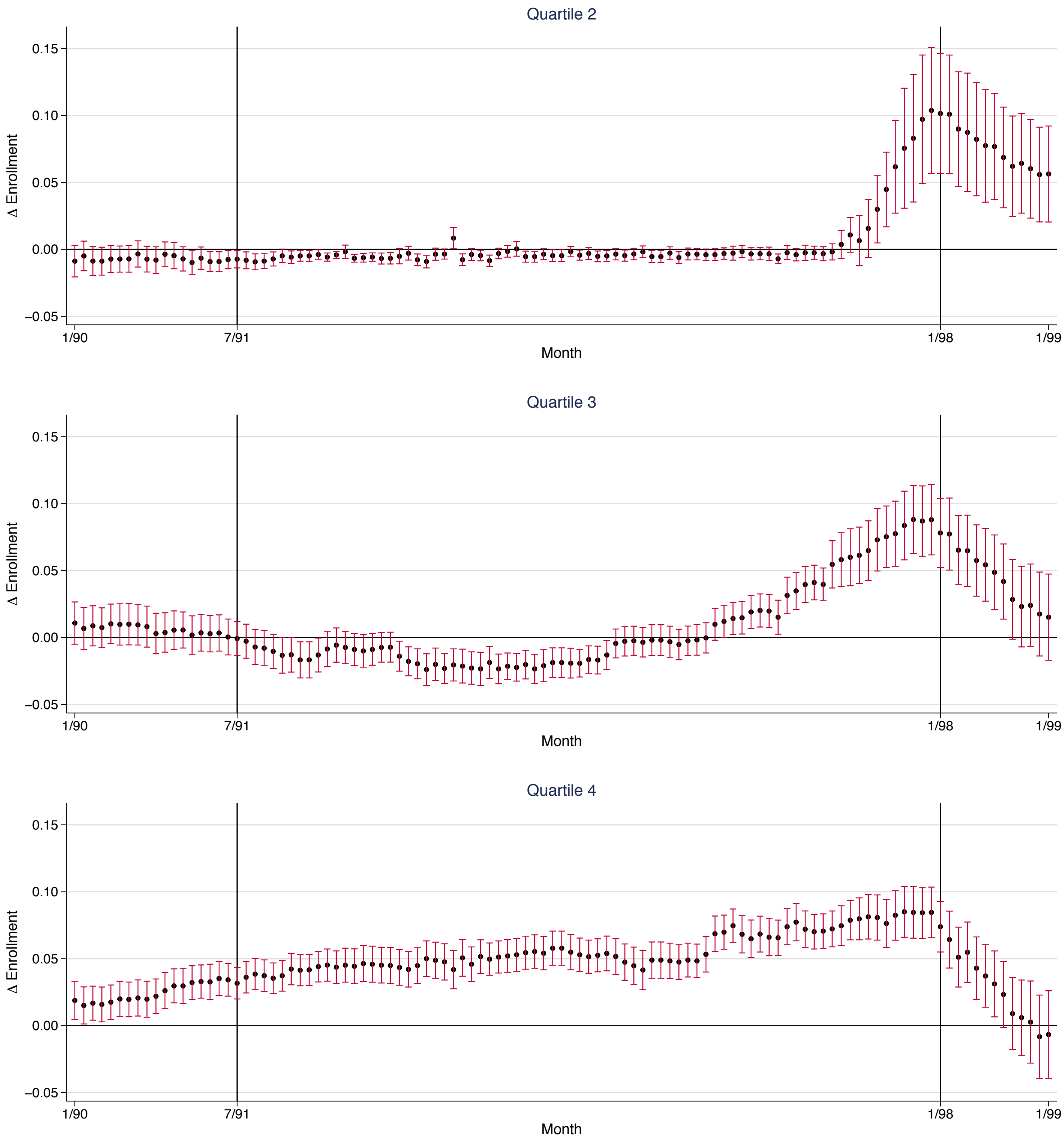
Notes: Figure presents the total number of years (up to 6.5) that a child could be eligible for Medicaid between July 1991 and December 1997, the period during which the OBRA90 discontinuity existed in Illinois, by her birth cohort and family income. (This assumes that family income remains fixed during 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 Table B.1 for details. The large discontinuity at October 1983 for children between the AFDC threshold and the poverty level is due to OBRA90.

Figure 1.2: Months Without Welfare During OBRA90 Discontinuity Period (Jul. 91 – Dec. 97)



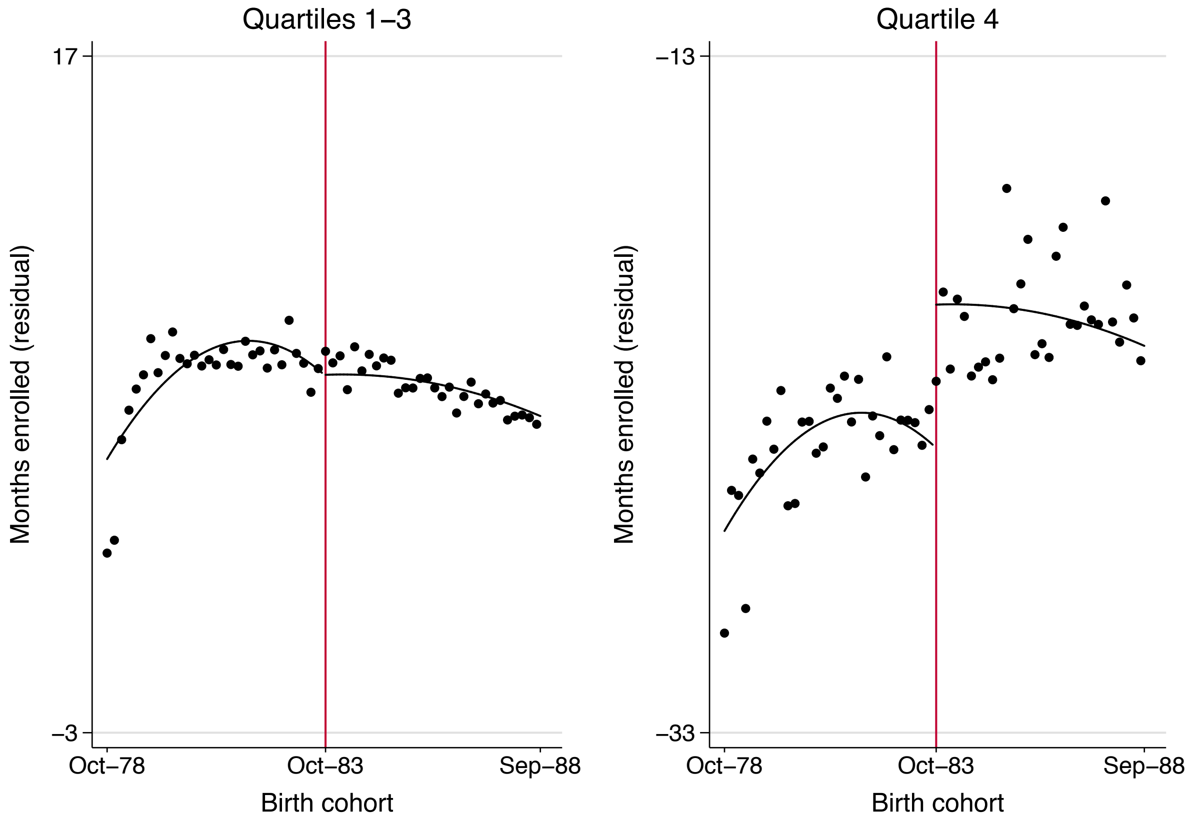
Notes: Figure presents the distribution of sample children by months without welfare receipt during the OBRA90 discontinuity period. Red lines indicate quartiles of the sample: 0-1 month (quartile 1), 2-11 months (quartile 2), 12-47 months (quartile 3), and 48-78 months (quartile 4).

Figure 1.3: Difference-in-Differences Estimates of Monthly Medicaid Enrollment



Notes: 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 1.4: Months Enrolled in Medicaid (Jul. 91 – Dec. 97)



Notes: 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.

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CHAPTER II

The Impact of Housing Assistance on Child Outcomes: Evidence From a Randomized Housing Lottery

(joint with Brian A. Jacob and Jens Ludwig)

One longstanding motivation for low-income housing programs is the possibility that housing affordability and housing conditions generate externalities, including on children's behavior and long-term life outcomes. We take advantage of a randomized housing voucher lottery in Chicago in 1997 to examine the long-term impact of housing assistance on a wide variety of child outcomes, including schooling, health and criminal involvement. In contrast to most prior work that focuses on families in public housing, we focus on families living in unsubsidized private housing at baseline, for whom voucher receipt generates large changes in both housing and non-

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housing consumption. We find that the receipt of housing assistance has little, if any, impact on neighborhood or school quality, or on a wide range of important child outcomes

2.1 Introduction

The U.S. federal government devotes roughly $40 billion each year to low-income housing programs, more than twice what is spent on cash welfare or the Title I program in education, four times what is spent on the children's health insurance fund (Falk 2012), and five times what is spent on Head Start.[1] In-kind housing programs are motivated by concerns about lack of affordable housing and by concerns about possible externalities of housing consumption, such as effects on behaviors like delinquency or dropout that contribute to what Rosen (1985) called the "social cost of slums." Senator Robert Wagner, co-sponsor of the Housing Act of 1937, argued "bad housing leaves its permanent scars upon the minds and bodies of the young, and thus is transmitted as a social liability from generation to generation" (Mitchell 1985, p. 245). Over the past several decades, housing vouchers have become the largest means-tested program through which the government provides housing assistance to low-income families.[2]

Despite its importance as part of the social safety net, there is surprisingly little evidence on how housing vouchers affect children's behavior and life chances. The well-known Moving to Opportunity (MTO) demonstration randomly offered housing vouchers to public housing residents, enabling families to move into less disadvantaged neighborhoods.[3] Because the rules for public housing and housing vouchers are identical in terms of income eligibility and required

[1] https://eclkc.ohs.acf.hhs.gov/hslc/standards/pdf/PDF_PIs/PI2013/ACF-PI-HS-13-03.pdf

[2] We use "housing voucher" as shorthand for tenant-based rental subsidies. See the Appendix A in the Online Appendix for details. All appendices referenced in this paper are located in the Online Appendix.

[3] Results of the 5-year MTO study are in Kling, Ludwig, and Katz (2005), Sanbonmatsu et al. (2006), and Kling, Liebman, and Katz (2007); long-term results are in Sanbonmatsu et al. (2011) and Ludwig et al. (2011, 2012). Similar issues are addressed by Rubinowitz and Rosenbaum (2000), Oreopoulos (2003), Sampson, Sharkey, and Raudenbush (2008), and Schwartz (2012).

rent contributions, MTO represented a change in the *form* rather than the *amount* of a family's housing assistance. This paper addresses an important policy question that MTO cannot answer: What are the effects on poor children from expanding the housing voucher program and reducing the share of low-income families who consume housing without a government subsidy?

Vouchers substantially increase housing consumption, but they also allow families to consume more of other goods by greatly reducing the fraction of income they must devote to rent. The net effect on children is theoretically ambiguous. Crowded housing conditions and poverty generally are negatively correlated with children's outcomes (Brooks-Gunn and Duncan 1997; Leventhal and Newman 2010). What remains unclear is the degree to which these correlations are due to low-income, credit-constrained parents being unable to adequately invest in their children's well-being, versus being due to parent attributes that affect their ability to both succeed in the labor market and promote their children's development (Mayer 1997).

This is an important question since nearly one in five U.S. households (21 million total) is "severely rent burdened," defined as spending over half their income on housing (JCHS 2014). Nearly as many households—around 17 million—have problems with the condition of their housing unit, such as pests, a leaky roof, broken windows, exposed wires, plumbing problems, cracks in the walls, or holes in the floor.[4] Yet only 23 percent of all low-income renters receive help from means-tested housing programs (Fischer and Sard 2013).

There has been only one previous randomized study of this question (Mills et al. 2006). About five years after baseline, the evaluation found no statistically significant effects on measures of child behavior, and mixed effects on school outcomes—specifically, voucher children were less likely than controls to miss school because of health, financial, or disciplinary problems, but were more likely to repeat a grade. However, the analysis relied on parent reports

[4] https://www.census.gov/library/publications/2013/demo/p70-136.html

of child outcomes and had only a modest sample size, so many of the null findings are imprecisely estimated (see Appendix D for additional discussion).

In this paper, we take advantage of a large housing voucher lottery carried out in 1997 in Chicago to estimate the impact of housing assistance on important child outcomes. A total of 82,607 eligible people applied, representing a large share of the roughly 300,000 households in poverty in Chicago at the time.[5] Applications greatly exceeded available vouchers so applicants were randomly assigned to a wait list. We show that this assignment was indeed random and greatly affected the chance a family was offered a voucher. We are able to link applicants to a wide range of local, state, and federal administrative databases that allow us to measure outcomes for children in these families up to 14 years after the voucher lottery, including standardized test scores, high school graduation, arrests, earnings, and social welfare receipt as adults, as well as health outcomes from Medicaid claims data. Our study focuses on the 90 percent of applicants who were living in unsubsidized private housing at the time of the lottery, for whom a housing voucher represents a large, in-kind transfer. We believe ours is the first large-scale study of the housing voucher program to use exogenous variation to examine such a wide range of children's outcomes over such a long follow-up period.

We find that receipt of a housing voucher had little if any impact on the education, crime, or health outcomes we are able to measure. Using randomized voucher offer as an instrumental variable (IV) for voucher use, our estimated effects on achievement test scores are 0.06 standard deviations (SD) for boys 0-6 at baseline (pair-wise error rate p~.05), but only 0.003 SD for girls age 0-6 at baseline (standard error of 0.03), and just 0.01 and 0.03 SD for boys and girls who were of school age at baseline (standard errors of about 0.03). Our IV estimate for the effects of

[5] In 2000 there were ~2.9 million people in Chicago, with an average of 2.67 people per household and a poverty rate in the city equal to 28.5 percent. http://www.brookings.edu/research/reports/2003/11/livingcities-chicago

vouchers on inpatient or emergency room visits is never higher than about 1 percentage point (versus a control complier mean, or CCM, of 25 percent), and for high school graduation is about 2 or 3 points (compared to CCMs of 41 and 58 percent for boys and girls, respectively). Once we account for multiple hypothesis testing, we find no statistically significant effects for our measured outcomes overall or in any of the pre-specified subgroups.

The main threat to internal validity with these results is from a slight treatment-control difference in migration out of the Chicago Public Schools that could bias our estimates of the education outcomes. However, as we show, the amount of differential attrition is extremely small and a variety of sensitivity analyses suggest that any bias is likely to be negligible. Moreover, we find no differential attrition from Illinois, implying that our crime and health outcomes (which come from state data) should not suffer from any such bias.

The lack of large, statistically significant effects is particularly surprising given the generosity of the program. For the average household in our sample, the subsidy associated with a housing voucher is over $12,000, equal to roughly two-thirds the average baseline income of sample households ($19,000).[6] We show that these effects do not change notably over time, which suggests that they are not merely due to temporary transition difficulties. Looking at mediating mechanisms, we find that receipt of a housing voucher does not seem to improve neighborhood or school inputs, which is consistent with the lack of longer-run child outcomes.

The null results we find for housing vouchers contrast sharply with the large, positive impacts of cash transfer programs documented in a number of recent studies (e.g., Dahl and Lochner 2012; Duncan, Morris, and Rodrigues 2011; Milligan and Stabile 2011et al. 2010). This dramatic difference is puzzling given that housing vouchers, while an in-kind transfer, provide recipients with substantial resources that can be taken in the form of cash by reducing out-of-

[6] Unless otherwise noted, all dollar amounts reported in this paper are in 2013 inflation-adjusted dollars.

pocket spending on rent. We explore a number of candidate explanations for the difference in results. One candidate explanation is that most studies of the effect of income on child outcomes rely on research designs vulnerable to selection bias, although several do rely on randomized experiments. Another possible explanation for why our results differ is that many recent studies examine cash transfers that are structured in ways that increase parent work, which may moderate how additional income is spent. For example, parents required to work more might devote resources to purchasing especially productive child "inputs" like center-based care. In contrast, housing vouchers tend to reduce parental labor supply (Jacob and Ludwig 2012).

The next section discusses the program rules for housing vouchers and the candidate mechanisms through which receipt of a housing voucher might affect children's outcomes. Section III provides background on the 1997 housing voucher lottery that serves as the basis for our empirical analysis. Sections IV and V discuss our data and empirical strategy. Our results are in Section VI, while the limitations and implications of our results are in Section VII.

2.2 Conceptual Framework

Concerns about the effects of housing conditions on children's life chances date back to at least the 1890s when Jacob Riis described tenement conditions in New York City (Riis/Warner, 1890/1970). These concerns helped motivate the start of federal low-income housing policy in the 1930s and continue today. In this section, we describe the current housing voucher program, and then discuss how the program might influence childhood outcomes.

2.2.1 Housing Program Rules

Housing vouchers subsidize low-income families to live in private-market housing.[7] Eligibility limits for housing programs are a function of family size and income, and prioritize what the US Department of Housing and Urban Development (HUD) terms "very low-income households," with incomes for a family of four below 50 percent of the local median.

The maximum subsidy available to families is governed by the Fair Market Rent (FMR), which is partly a function of family size (larger families get a higher FMR to lease a larger rental unit). The FMR is also linked to the local metropolitan area's private-market rent distribution, usually set at the 40th or 50th percentile, and so varies over time and across areas.[8]

Families receiving vouchers are required to contribute towards rent 30 percent of their adjusted income, which under program rules can be substantially less than total income. The voucher covers the difference between the family's rent contribution and the lesser of the FMR or the unit rent. Voucher recipients can keep the subsidy for as long as they meet income and other eligibility requirements. Most families in our study sample have average incomes that are far below the phase-out level, and so under any realistic view of their likely earnings growth would view these as very long-term subsidies. (For additional details about housing voucher rules, and how they interact with participation in other social programs, see Appendix C.)

2.2.2 Mechanisms Through Which Housing Vouchers Might Affect Child Outcomes

Receipt of a housing voucher could in principle affect children's long-term outcomes in several possible ways: 1) by improving the quality of the housing conditions in which children reside; 2) by allowing parents to invest more in non-housing goods that may be developmentally

[7] This discussion is based on the excellent summary in Olsen (2003).
[8] For example, the FMR for a two-bedroom apartment in the Chicago area, in nominal dollars, equaled $699 in 1994, $732 in 1997, and $762 in 2000.

productive for children; 3) by changing parent behavior due to the conditions of the housing program; 4) by reducing parental labor supply; and 5) increasing the number of residential moves families make. The first three mechanisms should improve children's outcomes, while the effects of the fourth mechanism (changes in parent labor supply) are theoretically ambiguous. The last mechanism (extra residential mobility) could in principle harm children's outcomes, although in practice we find vouchers wind up having little effect on a family's total number of moves – partly because low-income American households tend to be very mobile anyway.[9]

Figure 2.1 shows the budget constraint facing eligible households, and consumption choices with and without a housing voucher, as a way to help illustrate the first two mechanisms described above. The family must decide how to allocate income I between the consumption of housing (H) and other (non-housing) goods (C), both normalized so that $P_H = P_C = 1$. Without a housing voucher, the family's budget constraint is given by DJ, with initial consumption bundle B. After receiving a voucher subsidy with a cost to the government of S (in our sample, on average S = \$12,501), their new budget constraint is given by $DUVL$, where $D - C_V$ is the rent contribution required by the voucher program. If the family leases a unit with rent up to the FMR, their new consumption bundle is at point V.

The most obvious change for a family receiving a housing voucher is that their housing consumption increases substantially, from H_B to H_V. For families in our study, average annual rent at baseline is \$9,372 (Table 2.1), while the FMR for these families is on average \$16,220, so the maximum change in housing consumption from using a voucher is on average $H_V - H_B$ = \$6,849. This represents a 73 percent increase in housing consumption, or equal to about 36

[9] In Jacob and Ludwig (2012, Table 5), we find that the average family who is not offered a voucher but would move if given one makes 3.18 moves over that study's 8 year follow-up period. Voucher receipt causes families to move a bit earlier than they would have otherwise, but the IV effect on number of moves for voucher users is 0.119, or about 4 percent of the CCM.

percent of the average baseline income of our families ($18,978). With baseline rent being so much lower, on average, than the FMR, it is not surprising that only 7 percent of voucher users in the treatment group remain in the same housing unit.

Figure 2.2 gives some sense for the distribution of $H_V - H_B$ across families that leased up with a voucher, estimated as the rent recorded by the government the first time a household uses a voucher minus our estimate for their baseline (pre-lottery) rent.[10] The distribution includes some negative changes in rent after first use of a housing voucher, which could occur in the short run (as we are examining here) because of time constraints on searching for an eligible unit but should dissipate in the long run as housing consumption rises for all voucher recipients. If housing markets function at all well, we would expect higher-rent units to be either higher quality or located in more desirable neighborhoods. We show below that in practice families do not move to notably "better" neighborhoods, so most of the increase in housing consumption presumably comes from improved housing units.[11]

A large correlational literature has found that at least some specific features of housing units, like presence of toxins or crowding, are associated with outcomes such as respiratory problems in children (Leventhal and Newman 2010). However, few studies are able to control for unobserved family attributes that may confound estimates of housing effects on children.

Receipt of a housing voucher also allows a family to greatly increase their spending on non-housing goods (from C_B to C_V in Figure 2.1) by reducing out-of-pocket spending on rent. Our sample spends on average $9,372 on rent at baseline, over half their total income. Receipt of

[10] Actual baseline rent is unobserved in our data. Instead, we assign to each family the average rent paid by demographically-similar households in their baseline census tract using a special tabulation of 2000 Census data from Chicago conducted for us by the Census Bureau. See Appendix D for details.

[11] Some observers have noted landlords are aware of the rent limits in the voucher program and some artificially raise the rent of a unit to meet the tenant's new ability to pay (Mallach 2007; Collinson and Ganong 2013). To the extent that this is the case, the estimates described above may overstate the increase in housing consumption. Mills et al. (2006) suggest the net effect of housing voucher receipt may be an increase in unit quality or size.

a voucher would let the average family in our sample reduce out-of-pocket spending on housing to $D - C_V$ = \$3,719 (the average required rent contribution by the voucher program). This increases average consumption of other (non-housing) goods by $C_V - C_B$ = \$5,653, which equals 45 percent of the total voucher subsidy cost to the government and 29 percent of average baseline income for families. This represents a 59 percent gain in non-housing consumption. Whether this extra consumption improves children's outcomes obviously depends on how this large infusion of additional income is spent.

While a large body of research has studied the relationship between income and children's outcomes (e.g., Brooks-Gunn and Duncan 1997; Mayer 1997; see also Appendix D), credibly identified estimates are rare. However, several recent quasi-experimental studies find income transfer programs have large, positive impacts on child outcomes. For example, Dahl and Lochner (2012) examine the effects of expanding the Earned Income Tax Credit (EITC) during the 1990s and estimate that an extra \$1,000 in family income (in 2013 dollars) raises children's test scores by 0.045 SD overall, by 0.06 for black or Hispanic youth, and by 0.065 for males.[12] Duncan, Morris, and Rodrigues (2011) estimate similar impacts using data from several welfare-to-work experiments. Milligan and Stabile (2011) find even larger impacts in Canada, where \$1,000 in extra child-care benefits increases math scores by 0.05 SD overall and 0.177 for boys, and by 0.28 for boys on a vocabulary test (the PPVT). Akee et al. (2010) study the effects of income received by low-income Native American families from the opening of a casino on tribal land in North Carolina. Their reduced-form effect corresponds to an income change of \$4,000, and implies that an extra \$1,000 increases high school graduation by about 6 percentage points in the poorest families. These studies, if correct, would imply gains in children's outcomes from

[12] The estimates we report in the text are slightly different from those reported in the original papers we cite because we have re-scaled their estimates to reflect the effect per \$1,000 in constant 2013 dollars. In the case of Milligan and Stabile (2011), we also adjust for the fact that their estimates are reported in Canadian dollars.

cash transfers that are larger on a per-dollar basis than what we see from educational interventions like Head Start, class size reduction, or whole-school reforms.[13]

A third mechanism through which housing voucher receipt could change children's outcomes is through changes in parental behavior due to housing program rules. For example some public housing agencies require the voucher applicant and all children 18 and older in the home to pass a criminal background check.[14] Yet in practice the level of enforcement of these behavioral conditions may be modest. For example, in Jacob and Ludwig (2012, Table 1), we find that a sizable share of adults in our Chicago housing voucher lottery sample had a prior arrest at baseline. In addition, we find that among voucher recipients in our present study sample, being arrested does not affect the likelihood of staying in the voucher program.

A fourth mechanism through which housing vouchers may affect child outcomes is by reducing parental labor supply through both income effects (given the large resource transfer) and substitution effects (from the fact that they require families to contribute 30 percent of adjusted income towards rent). In our previous work examining data through 8 years after the voucher lottery, we find voucher receipt reduced parents' work rates by 3.6 percentage points compared to a CCM of 61 percent (Jacob and Ludwig 2012). Over the 14-year follow-up period that we examine in the present paper, we estimate that voucher receipt reduces work rates by parents of our sample of children by a statistically insignificant 1 percentage point. How increased parental time at home affects child outcomes depends on the relative developmental productivity of parental time versus the alternative way children would have spent their time.

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[13] In Ludwig and Phillips (2008), Table 1, the median effect of participation in Head Start is about 0.016 SD per $1,000 in 2013 spending. Data from Tennessee STAR suggest that for each $1,000 in 2013 dollars test scores increase for African-American children by about 0.018 SD (Schanzenbach 2007). And Borman and Hewes (2002) estimate the effects per $1,000 in 2013 dollars on math scores equal to 0.027 SD.

[14] This was the Chicago Housing Authority's policy up through a 2010 court decision; see for example: http://povertylaw.org/communication/advocacy-stories/tran-leung-landers.

2.3 The Chicago Housing Voucher Lottery

In July 1997, Chicago Housing Authority Corporation (CHAC) opened the city's voucher wait list for the first time in 12 years, and received a total of 82,607 applications from income-eligible people. CHAC hired Abt Associates to randomly assign applicants to a waiting list in August 1997, and notified those in the top 35,000 positions of their wait list number. CHAC told these families on the "active wait list" that they would be offered a voucher within three years. CHAC informed the remaining applicants (lottery numbers 35,001 to 82,607) that they would not receive vouchers.[15] By May 2003, after offering vouchers to 18,110 families from this wait list, CHAC was "over-leased," that is, had issued as many or more vouchers than it had funding to pay for, and essentially stopped offering any new vouchers.[16]

In the analysis that follows, we define our "*treatment group*" to be families offered vouchers by May 2003 (lottery numbers 1 to 18,110). The "*control group*" consists of applicants with lottery numbers above 35,000 who were told that they were not on the active wait list and would not get a voucher. We exclude families with lottery numbers between 18,110 and 35,000 from our primary sample because of their ambiguous treatment status.[17]

2.4 Data and Summary Statistics

This section briefly describes the key data sources used in our analysis. For more detail on the data, including variable construction and matching, see Appendix F. The starting point for constructing our sample are the application forms for the 1997 wait list, which provide baseline

[15] Service of the July 1997 wait list was interrupted in August 1998, as CHAC was required to provide vouchers to a set of Latino families in response to a discrimination lawsuit. CHAC began to serve the 1997 wait list again in 2000.
[16] The number of families offered vouchers per year (and the voucher utilization rate) was 1,540 (50.3 percent) in 1997; 3,085 (50.1 percent) in 1998; 2,631 (43.6 percent) in 2000; 5,733 (44.5 percent) in 2001; 4,674 (49.7 percent) in 2002; 446 (42.7 percent) in 2003.
[17] Although these families may have expected to receive a voucher, our results are not sensitive to including them.

information on the 82,607 adults and nearly 8,700 spouses who applied to CHAC for a housing voucher. The baseline application forms do not include the names of other household residents, so we use data from the Illinois Department of Human Services (IDHS) to determine who lived with the CHAC applicants in the period immediately *before* the wait list was opened.

Data on voucher utilization comes from HUD 50058 records, which families complete annually to verify program eligibility. Several methods were used to track residential locations for both treatment and control group families, which are then linked to census tract-level data.

2.4.1 Measurement

To measure behavioral outcomes, we use longitudinal administrative data from a number of different government agencies. All of our administrative data matching uses *only information from pre-randomization sources* to preserve the strength of the experimental design. From the Chicago Public Schools we obtained student-level school records for the academic years 1994-5 through 2010-11 that include test scores, grades, and enrollment or graduation status. We measure labor market involvement for youth and their parents using quarterly earnings data from the state unemployment insurance (UI) system through 2011:Q4. We measure social program participation of youth and parents from IDHS records through 2013:Q1. We measure criminal behavior using data from the Illinois State Police (ISP) that capture arrests through 2012:Q1.

Finally, we measure health outcomes using Medicaid claims data from the Center for Medicare and Medicaid Services (CMS) for the period from 1999:Q1 through 2008:Q4. One limitation of these data is that most but not all of the households in our sample use Medicaid. A second limitation is that we measure health outcomes only when a fee-for-service claim is filed, and some children in our sample receive benefits from a managed care organization (MCO) that does not generate such claims (although there is no treatment-control difference in propensity to receive benefits from an MCO). All results derived from claims that we present below are conditioned on being enrolled in fee-for-service care for six or more months during the academic year. Using this definition of enrollment, approximately 75 percent of our sample is ever enrolled at some point during 2000-2008, and 45 percent are enrolled at a point in time, with at most a 1.4 percentage point treatment-control difference in enrollment rates as we show below. A third limitation is that claims data could confound access to care with health outcomes. We try to mitigate this concern by focusing on usage of the most urgent types of care (e.g. inpatient and emergency); though still a course measure of health outcomes, it may at least capture dramatic changes in health status among sample members.

2.4.2 Sample

Table 2.1 presents summary baseline statistics for our main analysis sample—children of CHAC applicants living in private-market housing when they applied to the voucher lottery, separately for the 48,263 control children (whose families were not offered vouchers) and 18,347 treatment children (offered vouchers during 1997-2003) in our sample. We restrict our attention to children who were age 0-18 at the time of the 1997 lottery, and so do not include any children born subsequently since fertility could be affected by voucher receipt.

Our program population is quite disadvantaged at baseline. Almost all families are headed by an unmarried, African-American woman, with nearly four out of five receiving some form of social-program assistance. The year before the lottery children have an average GPA of 1.5 on a 4 point scale, and attend schools that are overwhelmingly attended by other minority students who are eligible for free or reduced price lunch.

Comparing the baseline average characteristics of the control group (column 1 of Table 2.1) with the treatment group (column 2) provides some evidence to confirm that the voucher lottery was indeed random. A few pair-wise comparisons are statistically significant, but an omnibus test of the null hypothesis that all of the treatment-control differences in baseline characteristics are jointly zero yields a p-value of .49.[18]

2.5 Empirical Strategy

Given that the voucher lottery was random, a simple comparison of means between those offered vouchers and those who were not will provide an unbiased estimate of the effects of being offered a voucher, known as the "intention to treat" (ITT) effect. We discuss here how we estimate the ITT and the effects of actually using a voucher, and how we handle statistical inference with so many different outcomes.

2.5.1 The Effect of Receiving a Voucher Offer

Our data consist of a balanced panel where the unit of observation is the child-year. To facilitate comparison between education and crime data, we use academic years that span from Q3 of one year to Q2 of the following year. Our analysis period runs from 1997-98 through 2010-11, the last year for which we have most of our data sources. For child *i* in year *t,* we use OLS to estimate the ITT effect on outcome y_{it} as:

(1) $$y_{it} = \alpha + \beta_1(PostOffer_{it}) + \beta_2(PreOffer_{it}) + X\Gamma + \gamma_t + \varepsilon_{it}$$

[18] We use the `suest` command in Stata to conduct an F-test for the joint significance of the treatment indicator, adjusting for the non-independence of baseline characteristics within households. This test essentially consists of regressing lottery numbers against all of the baseline characteristics shown in Table 2.1 in a way that accounts for the correlation among these baseline variables. An alternative approach is to cluster standard errors on baseline census tract rather than household ID; when we do this, the p-value is 0.44.

PostOffer$_{it}$ equals 1 if the family of child *i* has been offered a housing voucher through the CHAC 1997 lottery in any period up to or including *t*, and zero otherwise. We also control for year effects, γ_t, and to increase precision we control for a set of baseline characteristics (see Appendix F). Standard errors are clustered by household (Bertrand et al. 2004). Identification of the ITT effect β_1 comes from a within-period comparison of the average outcomes of those offered vouchers versus the control group.[19] We also include an indicator, *PreOffer*$_{it}$, equal to 1 for people who were on the active wait list but had not been offered vouchers yet by year *t*, and equal to 0 otherwise. The coefficient β_2 indicates whether families change their behavior in anticipation of getting a voucher; this is *not* a "randomization check," since it is estimated off of post-lottery treatment-control differences (our panel only includes post-lottery quarters).

The standard "education production function" in economics assumes children's outcomes are affected by the accumulated inputs they have experienced up to that point (Hanushek 1979), which suggests that the effects of additional resources could grow over time. To examine how voucher effects might change with the duration of voucher receipt we use OLS to estimate the per-period ITT effect using the following event study-style specification:

(2) $$y_{it} = \alpha + \sum_k D_{it}^k \delta_k + X\Gamma + \gamma_t + \varepsilon_{it},$$

The key explanatory variables in this case are indicators (D_{it}^k) equal to 1 if, in period *t*, individual *i* is *k* years from when they were offered a voucher through the lottery (*k* can take on positive and negative values). We present figures tracing out the time path of these effects below.

[19] If there is heterogeneity in the effects of a voucher offer across people, time, or duration of voucher receipt, then our ITT estimate is an average of the ITT effects across all post-voucher-offer person-years in our panel.

2.5.2 *The Effects of Using a Voucher*

The ITT estimate will not equal the effect of using a voucher because not all treatment-group families who were offered a voucher used them, and a small share of controls received a housing voucher through some other special allocation during our study period (between 5 and 8 percent, as shown below).[20] Under the assumption that the voucher offer does not have an impact on those who choose not to take it, we can use two-stage least squares with randomized voucher offers as an instrument to estimate the effects of using a voucher with equations (3) and (4). The dependent variable in equation (3) is an indicator for whether household *i* utilized a voucher provided by any source (the CHAC lottery or some other allocation) by or in period *t*.[21]

$$(3) \qquad Leased_{it} = \alpha + \theta_1 PostOffer_{it} + \theta_2 PreOffer_{it} + X\Gamma + \gamma_t + \varepsilon_{it}$$

$$(4) \qquad y_{it} = \eta + \pi_1 Leased_{it} + X\Pi + \mu_t + v_{it}$$

Our estimate for π_1 captures the local average treatment effect on those induced to use a voucher by their CHAC wait list position (Angrist, Imbens, and Rubin 1996).[22] As a benchmark for judging the size of our IV estimates, we present what Katz, Kling, and Liebman (2001) call the control complier mean (CCM), or the average outcome for controls who would have used vouchers had they been assigned to the treatment group. We calculate this using the formula from Heller et al. (2013) to account for the presence of control crossovers.

[20] For example, the HOPE VI program helped demolish several notorious Chicago housing projects; some displaced families were given vouchers through a special allocation. Other families on the wait-list could have received vouchers from another program because they contained a disabled member, or were at risk for having parents separated from children without a change in housing status, or were Latino and so received vouchers as a result of litigation by Latinos United against the CHA that temporarily interrupted service of the 1997 wait-list.

[21] Under this definition a family that uses but then gives up their voucher does not become "untreated," under the assumption that a child's outcomes are a function of current and past investments. In practice, over half of households who lease up with a CHAC voucher remain leased-up after eight years (Figure G.1). The results do not change much if we instead define the treatment as "using a housing voucher in period *t*."

[22] If voucher effects instead vary by how long a family is leased up, then π_1 captures the LATE for those who lease up for a longer period of time due to treatment group assignment, so long as we are willing to assume that control group cross-overs would have been leased up for at least as much time had they been assigned to treatment. If we instead calculated our IV estimate using a more conservative assumption that all treatment group voucher users lease up for a longer period of time than if they had been assigned to the control group, our IV estimate will capture the effects of treatment-on-the-treated (TOT).

Table 2.2 shows there is a large "first-stage" relationship between being offered a housing voucher through the CHAC lottery and whether the child's family used a voucher. Our estimate for θ_1 is nonetheless less than 1; despite the long wait list for housing vouchers, many families offered vouchers do not wind up using them. Reasons include the fact that many apartments have rents above the FMR limit, some landlords may avoid renting to voucher families, and families offered vouchers have a limited time (usually 2 to 4 months) to use the voucher to lease a unit. In the top panel, which presents results for our full analysis sample, column 1 shows the coefficient on the *PostOffer* indicator from estimating equation (3). Around 7 percent of controls used a voucher, and assignment of a wait list number below 18,110 increased voucher lease-up rates by 48 percentage points. The F-test statistic equals 5,835. The voucher take-up rate we report here is consistent with those reported in previous studies (Olsen 2003). The results are qualitatively similar if we estimate a cross-section regression for whether a child's family *ever* uses a voucher while CHAC was issuing vouchers (1997-2003), as in column 2, or if we focus on using a voucher from the 1997 CHAC lottery specifically (columns 3 and 4).

2.5.3 Multiple Hypothesis Testing

The final issue we discuss is how to manage the risks of false positives and false negatives given the large number of outcomes we examine. Our approach follows what we believe is best practice (Kling, Liebman, and Katz 2007; Schochet et al. 2008) although studies in social science often fail to make such adjustments (Anderson 2008).

First, we pre-specify a limited set of outcomes and subgroups for a main, confirmatory analysis. We focus on four outcomes: (i) high school graduation; (ii) a composite of math and reading achievement scores; (iii) the social cost of crimes committed by youth, essentially an "importance-weighted" index that assigns a dollar value representing the cost to society to each youth arrest based on estimates from the literature;[23] and (iv) emergency department and inpatient hospital admissions. Given prior evidence that social policy effects may differ by gender (Kling, Liebman, and Katz 2007; Anderson 2008; Milligan and Stabile 2011), we examined impacts separately by gender. We also look separately at children 0-6 versus 6-18 years of age at the time of the lottery, given the possibility of declining developmental plasticity by age (Shonkoff and Phillips 2000; Knudsen et al. 2006) and the findings of Morris et al. (2004) and Duncan et al. (2011) that income only affects achievement in young children.

Second, in addition to reporting per comparison p-values from standard t-tests, we also control for the false discovery rate (FDR), or the proportion of null-hypothesis rejections that are Type I errors or "false positives," using the two-step procedure from Benjamini, Krieger, and Yekutieli (2006). Because the TOT is basically just a re-scaled version of the ITT point estimate and standard error, with a similar t-statistic, we report the FDR-adjusted p-values for the ITT. Because our assessment of the housing vouchers versus the status quo alternative depends on the set of outcomes being compared and not on the significance of any single outcome, we think the FDR is the most appropriate adjustment for multiple comparisons. For completeness, we also control for the family-wise error rate (FWER), or the probability of making *any* Type I error, calculated using the bootstrap re-sampling technique discussed in Westfall and Young (1993; see also Anderson 2008). The FWER is the more conservative of the two adjustments, so the fact that we find few statistically significant impacts even with our focus on the FDR strengthens our conclusions about the limited effect of even large resource transfers on children's outcomes.

[23] A discussion of how we calculate the social costs of crime (following Kling, Ludwig, and Katz 2005) and results examining vouchers' effect on arrests for different types of offenses are in Appendix D and E, respectively.

2.6 Results

In this section, we present our findings on how housing vouchers affect child outcomes, and explore a variety of mediating mechanisms through which vouchers might operate.

2.6.1 Effects of Housing Vouchers on Children's Outcomes

Table 2.3 presents the impact estimates for our primary outcome measures.[24] Even if we initially ignore multiple testing issues and focus on pairwise error rates, just one out of twelve ITT estimates is significant at the usual 5 percent threshold (social costs of crime committed by females), and another is significant at the 10 percent cutoff (achievement test scores for males age 0-6 at baseline). If we account for multiple testing by controlling for either the FDR or FWER (see Appendix G), none of these estimates is significant at conventional levels.

Most of these estimates are also quite small in magnitude. For example, the IV estimates for the effects of voucher use on standardized achievement scores for children who were ages 6-18 at the time of the lottery equal 0.01 and 0.03 SD for boys and girls respectively, with standard errors of about 0.027 SD. The IV estimates for inpatient or emergency room claims are smaller than 1 percentage point in absolute value for all age-gender groups relative to CCMs of roughly 25 percent, with standard errors of about 1 percentage point.

For other outcomes, it appears that we have less statistical power. For example, the 95 percent confidence interval for the IV estimate of high school graduation for males ranges from -0.6 to +6.3 percentage points, relative to a CCM of 41 percent. The IV estimate for social costs of crime committed by boys is -$344, or about 10 percent of the CCM of $3,482, with a confidence interval that ranges from about -21 to +2 percent of the CCM. However, as we

[24] The sample sizes for different outcomes vary because of age restrictions. Not all children will have reached an age to be capable of graduating high school by the end of our panel, arrests are only measured for children aged 13 and older, and the Chicago Public Schools only administer achievement tests to students in grades 3-11.

discuss below, given the magnitude of the in-kind transfer from housing voucher receipt, even these moderately-sized reduced form impacts seem small on a per dollar basis.

Given our reliance on mostly city- or state-level administrative records, one threat to the internal validity of our estimates comes from the possibility of differential attrition. Table 2.4 shows that there is no treatment-control difference in the fraction of quarters living outside of IL between 1997 and 2005, which suggests there should be little bias with the data we get from state agencies on arrests, public assistance receipt, earnings, and Medicaid claims. A recent update of these address data allows us to get information on the location of households in 2012. Again, we see no detectable difference in the chance of living in Illinois.

However, we do find that younger children in our treatment group are slightly more likely to be in the Chicago Public School system in any given academic year. The ITT is 3 (2) percentage points for boys (girls) age 0-6 at baseline. This might bias our test score estimates for young children, but we think any bias is likely to be negligible (see Appendix G).

The results do not differ qualitatively for those children whose families received vouchers when they were most developmentally "plastic." When we re-calculate our estimates for children who were 0-3 at the time of the lottery and whose families were offered vouchers within a year of applying (through 1998), the results remain largely unchanged.

These results appear to generalize to a broader set of outcomes as well. Estimates of voucher effects on a variety of additional outcomes in each of our domains (schooling, health, criminal involvement) yield few detectable impacts. Nor do we see different effects at different points in the ability distribution (see Appendix G). While Jacob and Ludwig (2012) found some evidence for "anticipation effects" on parental labor supply, we see few signs of anticipation effects on the child outcomes we examine in this paper.

It is possible that any voucher effect increases with duration of voucher receipt, which could be missed by our main estimates. But we do not see much trend over time in any of these outcomes or across analytic samples (Figure G.2). We note that evolution in the behavioral response to leasing up with a voucher is only one reason the ITT effect could change over time. The estimates could also change with the fraction or composition of families that have leased up with a voucher, or because of changes in economic, policy, or other social conditions. But given the flat trends in the ITT estimate, these sources of confounding would need to act in the opposite direction with about the same magnitude each period to mask a behavioral response.

2.6.2 Mediating Mechanisms

Why do large resource transfers such as those generated by our housing voucher lottery not generate larger gains in children's outcomes? One possibility is that parents dedicate additional resources to goods other than those widely thought to improve children's outcomes. Mayer (1997, p. 99) shows that, in general, when low-income families get extra income they tend to spend it on things like food, shelter, clothes, health care, and transportation, which are weakly correlated with child outcomes. We do not have detailed consumption data for our sample of families, but with the administrative data sources we do have available we can try to narrow down how families are allocating their additional resources.

Table 2.4 showed that families do not seem to be "spending" extra resources moving to neighborhoods with features that some previous studies suggest may be developmentally productive for children (less poverty, racial segregation, or crime). This table reports ITT and TOT effects of voucher receipt on measures of neighborhood of residence for the 10 percent random sub-sample of CHAC applicants for whom we have passive tracking address data from

1997 through 2005 and in 2012.[25] Few of the effects are statistically significant and the point estimates are always small in relation to the control means. Table 2.5 shows that families do not seem to devote much of the additional voucher resources on improved school quality for their children or reduced school mobility.[26] Because neighborhood "quality" is capitalized into housing prices, our results imply families must be taking most of their increased housing consumption in the form of better housing units rather than "better" neighborhoods.

Another mechanism through which vouchers may affect children is via their involvement in the formal labor market. For example, Wilson (1996) argues that formal work can provide structure for daily routines or help develop social-cognitive skills. If voucher receipt reduces youth labor supply, it could potentially offset the beneficial effects of extra income on outcomes among the adolescents in our sample. But we see no statistically significant voucher effects on youth employment rates in quarterly UI earnings data (see Appendix G).

2.7 Reconciling Our Results With Those of Other Transfer Programs

The results described above suggest that even large resource transfers to families through housing vouchers do not generate many detectable changes in children's outcomes, which contrasts with recent quasi-experimental work on income transfer programs. In this section, we rescale our estimates so they are more comparable to the income transfer literature, which typically reports impacts per $1,000 of additional income. We then explore several possible reasons for our discrepant results.

[25] The results are similar when using address data from public assistance program records; see Appendix E.
[26] Frequent changes of a child's school attended are a major problem in urban districts; see NRC/IOM 2010

2.7.1 Rescaling Our Estimates

To compare our estimates to those from the income transfer literature, we need to determine the cash equivalent of a housing voucher from the perspective of affecting children's outcomes. For the average household in our sample, the total voucher subsidy (*S*) equals \$12,501, consisting of \$6,849 in additional housing consumption (ΔH) and \$5,653 in extra non-housing consumption (ΔC).[27] For the moment, we ignore the other channels through which vouchers might affect children's outcomes; we discuss these alternative pathways and their potential effects in the next sub-section. If we initially consider the value of the voucher to be this total subsidy amount, we would calculate the impact per \$1,000 by dividing the TOT estimates reported in Table 2.3 by *S*, i.e., $\pi_{income} \approx \frac{\pi_1}{S} \times 1{,}000$. In practice we estimate this by applying 2SLS to a variant of equations (3) and (4) shown earlier:

(5) $Leased_{it} * S_i = \alpha + \theta_1 PostOffer_{it} + \theta_2 PreOffer_{it} + X\Gamma + \gamma_t + \varepsilon_{it}$

(6) $y_{it} = \eta + \pi_{income} Leased_{it} * S_i + X\Pi + \mu_t + v_{it}$

The estimate of π_{income} based on the voucher value *S* is shown in column 3 of Table 2.6. The total subsidy *S* can be thought of as an upper bound on the value of the housing voucher to families insofar as it assumes that families lease units with the maximum permissible rent (i.e., the FMR) and that every dollar of additional housing consumption is equally productive for children's outcomes as each additional dollar of consumption on other goods. By using an *upper* bound estimate of the voucher's value for children's development, this approach implicitly yields a *lower* bound estimate for the effects of income on child outcomes.

[27] Recall that ΔH reflects the maximum change in housing consumption from using a voucher, and is calculated as the average difference between the FMR and a family's annual baseline rent. ΔC reflects the change in non-housing consumption due to decreased out-of-pocket spending on rent, and is calculated as the average difference between annual baseline rent and the rent contribution required by the voucher program.

To obtain an upper bound estimate for the impact of income, we can assume that extra housing consumption has *no* developmentally beneficial effect on children. In this case, any effect from receiving a voucher is assumed to be entirely due to increased non-housing consumption. Because the income elasticity of housing is non-zero, a family receiving cash would spend some of it on housing. To calculate the size of the cash subsidy S^* (see Figure 2.1) needed to generate the same increase in non-housing consumption ΔC as the housing voucher given baseline income I, rent H_B, and elasticity of housing consumption $e_{H,I}$, we solve:

$$(7) \qquad \Delta C = C_V - C_B = S^* - [\tfrac{S^*}{I} \times e_{H,I} \times H_B]$$

As our measure of I we use the CHAC applicant's estimated baseline income based on UI records, income received (owed) due to tax refunds (liabilities), TANF, and the monetary value of food stamps benefits received (see Appendix F). We assume an income elasticity of housing consumption of 0.35 (Mayo 1981; Polinsky and Ellwood 1979). We then substitute our estimate of S^* for S in estimating equations (5) and (6) above. These estimates are shown in column 4 of Table 2.6.

Finally, we create an even more conservative estimate by assuming the income elasticity of housing consumption is zero. In this case, the value of the voucher to families is simply the increase in available income generated by the reduction in rent payments with a voucher, i.e., ΔC. Since $\Delta C < S^*$, this yields an even larger upper bound for the estimated effects of income on children's outcomes compared to our second approach. These estimates are shown in column 5 of Table 2.6.

The results shown in Table 2.6 indicate that even the top of the confidence intervals around our largest upper-bound estimates are much smaller than the impacts on children's outcomes per $1,000 reported in the recent literature. For example, the largest estimate implied by our data for the impact of an extra $1,000 on young boys' achievement test scores is roughly 0.011 SD with a standard error of 0.006. Our confidence intervals enable us to rule out an effect of cash on test scores that is any larger than 0.022 SD. As Figure 2.3 shows, this is about one-third the estimated effect for boys from Dahl and Lochner (2012), although their confidence interval is fairly wide. Our estimate is about one-eighth the estimated effect on math scores for boys in Milligan and Stabile (2011). The same pattern is true if we look at high school graduation rates. As noted above, Akee et al. (2010) use data from a casino opening on an Indian reservation and estimate that an extra $1,000 of income increases high school graduation rates by about 6 percentage points. Our largest IV estimates, from column 5 of Table 2.6, suggest an effect per $1,000 of extra income on high school graduation rates for boys equal to 0.6 percentage points, with a standard error of 0.4 percentage points; for girls, the point estimate and standard error both equal about 0.4 percentage points. Our results are qualitatively similar when we focus just on infra-marginal families with baseline rents close to what is essentially the voucher program's rent cap, for whom a housing voucher changes mostly non-housing consumption (Appendix G).

2.7.2 Reconciling Differential Effects for Housing and Other Income-Transfer Programs

Why are our results so different from what might have been expected based on the results of previous studies of the income-child outcome relationship? Most of these studies rely on quasi-experimental sources of identifying variation in observational datasets, so there is inevitably some chance those estimates suffer from selection bias. But that cannot be the whole explanation since studies such as Duncan et al. (2011) rely on data from a pooled sample of randomized welfare-to-work experiments.

Alternative explanations for why our results differ from those of previous studies of income transfers include program rules that might reduce the apparent benefit of a housing voucher, or differences across studies in target populations and outcome measures, which might limit the generalizability of our results to the income-transfer programs discussed above. However, we believe there is little evidence that voucher program rules are likely to explain the difference, since (as noted earlier) voucher receipt has little effect on total residential mobility for this sample and other program rules (like prohibitions on voucher receipt by those with criminal records) seem to have been inconsistently enforced. Differences in outcome measures seem unlikely to explain the difference in results since there are overlaps in key measures such as test scores and graduation, and since our follow-up period is at least as long as those of other studies. And while our study sample is somewhat more disadvantaged than those in most other studies, this fact, together with the expectation of diminishing returns to household resources, would lead us to expect the effects of a transfer program to be *larger*, not smaller. In addition, the OLS relationship between income and child outcomes in our study is similar to the OLS relationship found in other studies, which also suggests that sample differences alone are unlikely to fully explain the discrepant results (see Appendix G for details).

One plausible candidate explanation for the difference in our results versus these other studies is how the different transfer programs change parental labor supply and how that in turn affects how parents spend their money. In our study, the transfer (housing voucher) *reduces* labor supply by 3.6 percentage points in the first 8 years after the voucher lottery (Jacob and Ludwig 2012) and a statistically insignificant 1 point drop in labor supply over the full 14-year follow-up period we examine in this paper. In contrast for example in Duncan et al. (2011) extra income always comes within the context of welfare-to-work programs that *require* women to work more.

We do not think the issue is the “main effect” of parental labor supply on children’s outcomes, but rather the way in which parental labor supply moderates the effects of extra income and changes how it is spent, particularly spending on child care in households with young children. Parent labor supply should moderate the way that income gets spent within the home under the standard Becker (1965) model in which parents combine parental time and market goods to produce children’s human capital (see Appendix E). For example, Mayer (1997) finds that most low-income parents devote extra income to things like food, housing, clothes, health care, and transportation. In contrast, Duncan et al. (2011) find in their welfare-to-work experiments that mothers of preschool-age children, required by these programs to work far more hours, wind up devoting a sizable share of their extra income to buying center-based care.

In fact, Morris et al. (2005) find that much of the relationship between income and child outcomes in the experiments studied by Duncan et al. (2011) is explained away after controlling for use of early childhood center care. Indeed, only pre-school-age children show gains in outcomes from extra income in those welfare-to-work experiments (Morris, Duncan, and Rodriguez 2004; Morris, Duncan, and Clark-Kauffman 2005). The fact that these experiments find no test score effects on school-age children (and, if anything, *negative* effects on test scores for teens) would seem to argue against other explanations for differences in results across studies. This finding is also consistent with the large body of evidence about the benefits for children from high-quality early childhood programs (Almond and Currie 2011).

In sum, it is possible that the most important explanation for why we get different results from these other studies, even more important perhaps than the distinction between in-kind and cash benefits, is that we are examining different “treatments” with respect to parent labor supply. Our study answers the question of what happens when households get more resources and *more* parental time. Studies of welfare-to-work experiments, the EITC, or childcare subsidies provide children with more income and *less* parental time, which may change the way that resources are spent, particularly on key “inputs” like early childhood education or care.

2.8 Conclusion

In this paper, we take advantage of a large housing voucher lottery carried out in 1997 in Chicago to estimate the impacts of housing assistance on children’s life chances. We find that receipt of a housing voucher had little if any impact on the education, crime, or health outcomes we measure over a 14-year follow-up period. The findings are surprising given the generosity of the voucher program, but are nonetheless broadly consistent with prior research by Mills et al. (2006). One way to reconcile our results with those of recent studies that estimate large effects of cash transfers on children's outcomes is that, unlike with housing vouchers, most of these other transfer programs increase parental labor supply in ways that may increase the share of extra cash spent on developmentally productive inputs for children, particularly childcare.

Our findings do not imply that cash transfers or other anti-poverty programs like housing vouchers are not worth supporting. These programs surely improve the well-being of families in a variety of critical ways. Nor do our results imply that eliminating the existing social safety net in the U.S. would not harm children’s outcomes, since the effect of income on children’s outcomes is almost certainly non-linear, and our data come from estimating the effects of adding income to existing safety net supports.

However, our results do suggest that housing vouchers may not be the most efficient way to improve the long-term outcomes of poor children, and that a tradeoff exists between alleviating some of families’ short-term material needs and bolstering long-term life outcomes for children. Our results imply that each $1,000 (in 2013 dollars) spent on the housing voucher program increases children's test scores by not more than 0.02 SD, much less than the estimated effects per dollar spent on a number of educational interventions. As suggested by Currie (2006), a more promising means of improving the long-term outcomes of poor children may be to invest in interventions designed to target them directly.

Table 2.1: Baseline Characteristics of Treatment and Control Group Households and Children

	Control Group	Treatment Group				
		All	*p*-value		Compliers	Non-Compliers
	(1)	(2)	(3)		(4)	(5)
Household Level						
Household head: Male	0.035	0.040	0.068	*	0.024	0.062
Household head: Black	0.942	0.944	0.501		0.959	0.924
Household head: Hispanic	0.035	0.032	0.224		0.025	0.041
Household head: White	0.020	0.022	0.425		0.014	0.033
Household head: Other race	0.003	0.002	0.342		0.002	0.002
Household head: Has spouse	0.082	0.084	0.695		0.070	0.101
# Adults in household (based on CHAC file)	1.4	1.4	0.800		1.4	1.5
# of kids 0-18 in household (based on CHAC file)	3.0	2.9	0.400		3.0	2.8
Age of household head	31.6	31.6	0.600		30.9	32.5
Indicated interest in certificate as well as voucher program	0.799	0.801	0.786		0.799	0.803
Reported receiving Supplemental Security Income (SSI) benefits	0.172	0.178	0.320		0.189	0.164
Time (in days) of application since application opened	9.3	9.3	0.800		9.0	9.7
Total household income (2013 $) 1996:III to 1997:II[1]	18,938	19,085	0.000	***	18,461	19,925
Household head earnings (2013 $) 1997:II	1,935	2,008	0.000	***	1,719	2,398
Household head employed 1997:II	0.462	0.469	0.350		0.456	0.486
Household head receiving TANF 1997:II	0.625	0.606	0.007	***	0.669	0.522
Household head receiving TANF, Med, or FS 1997:II	0.782	0.769	0.025	**	0.831	0.686
Household head: # of prior violent crime arrests	0.149	0.144	0.539		0.149	0.137
Household head: # of prior property crime arrests	0.271	0.228	0.011	**	0.235	0.219
Household head: # of prior drug crime arrests	0.128	0.126	0.793		0.124	0.129
Household head: # of prior other crime arrests	0.192	0.178	0.229		0.178	0.179
Census tract % black	0.822	0.824	0.694		0.849	0.791
Census tract poverty rate	0.302	0.301	0.499		0.310	0.288
Property crime rate (beat-level, per 1,000) in 1997	74.4	74.6	0.700		75.1	74.0
Violent crime rate (beat-level, per 1,000) in 1997	38.6	38.7	0.800		39.7	37.2
Monthy rent (2013 $)	782	778	0.000	***	777	780
Monthly fair market rent (2013 $)	1,316	1,314	1.000		1,320	1,306
Child Level						
Male	0.500	0.505	0.234		0.506	0.504
Black	0.942	0.945	0.340		0.959	0.926
Hispanic	0.035	0.032	0.208		0.025	0.041
Age	8.5	8.5	0.200		8.2	8.9
# of prior violent crime arrests	0.010	0.009	0.654		0.008	0.011
# of prior property crime arrests	0.005	0.005	0.895		0.004	0.005
# of prior drug crime arrests	0.015	0.018	0.097	*	0.016	0.021
# of prior other crime arrests	0.011	0.012	0.762		0.009	0.015
Enrolled in the Chicago Public Schools pre-lottery	0.598	0.599	0.824		0.604	0.592
Math test score in year prior to lottery	-0.244	-0.215	0.068	*	-0.243	-0.177
Reading test score in year prior to lottery	-0.213	-0.189	0.126		-0.198	-0.177
GPA in year prior to lottery	1.518	1.563	0.129		1.531	1.601
# of absences prior to lottery	28.9	28.6	0.700		28.8	28.4
Fraction nlack in child's school	0.848	0.853	0.220		0.871	0.829
Fraction Latino in child's school	0.108	0.103	0.151		0.092	0.119
Fraction eligible for free-lunch in child's school	0.855	0.854	0.654		0.861	0.846
Average test score in child's school	-0.182	-0.178	0.445		-0.187	-0.166
N (Children)	48,263	18,347			10,530	7,817
N (Households)	22,447	8,560			4,787	3,773
Joint test, all coefficients (including missing indicators)						
Chi-squared statistic (clustering at household level)	51.629					
p-value	0.488					

Notes: Unit of analysis in the top panel is the household; in the bottom panel, the child.

[1] Household income includes earnings of all household members (adults and children); estimated tax gain/loss; and TANF and Food Stamps benefits. Household members' earnings average approximately 55% of total household income.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table 2.2: Housing Voucher Effect on Lease-Up

	Leased Using			
	Any Voucher		1997 CHAC Voucher	
	Current Period	Ever	Current Period	Ever
	(1)	(2)	(3)	(4)
Full Sample				
Treatment group		0.4910***		0.5432***
		(0.0064)		(0.0062)
Offered voucher in current or prior year	0.4774***		0.5199***	
	(0.0062)		(0.0060)	
Control Mean	0.0705	0.0860	0.0000	0.0000
# observations	932,540	66,610	932,540	66,610
Males age 0-6 at baseline				
Treatment group		0.5090***		0.5759***
		(0.0103)		(0.0098)
Offered voucher in current or prior year	0.4964***		0.5515***	
	(0.0100)		(0.0095)	
Control Mean	0.0852	0.1030	0.0000	0.0000
# observations	172,032	12,288	172,032	12,288
Males age 6-18 at baseline				
Treatment group		0.4824***		0.5255***
		(0.0087)		(0.0083)
Offered voucher in current or prior year	0.4692***		0.5036***	
	(0.0084)		(0.0081)	
Control Mean	0.0625	0.0763	0.0000	0.0000
# observations	295,568	21,112	295,568	21,112
Females age 0-6 at baseline				
Treatment group		0.5088***		0.5693***
		(0.0105)		(0.0101)
Offered voucher in current or prior year	0.4971***		0.5471***	
	(0.0101)		(0.0097)	
Control Mean	0.0788	0.0966	0.0000	0.0000
# observations	167,790	11,985	167,790	11,985
Females age 6-18 at baseline				
Treatment group		0.4800***		0.5284***
		(0.0087)		(0.0083)
Offered voucher in current or prior year	0.4645***		0.5039***	
	(0.0084)		(0.0081)	
Control Mean	0.0653	0.0799	0.0000	0.0000
# observations	297,150	21,225	297,150	21,225

Notes: Columns 1 and 3 are ITT estimates from panel data observations. Columns 2 and 4 are ITT estimates from cross-sectional observations. Standard errors are reported in parentheses and are clustered at the household level.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table 2.3: Housing Voucher Effects on Education, Criminal Behavior, and Health

Baseline Age	Outcome	Children/ Obs.	CM	ITT	IV	CCM	ITT *p*-value Pair-wise	ITT *p*-value FDR
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
Male								
0-6	Test score	8,659	-0.3339	0.0369*	0.0634*	-0.3774	0.052	0.311
		[51,339]		(0.0190)	(0.0325)			
6-18	Test score	14,348	-0.3248	0.0068	0.0126	-0.3641	0.655	0.873
		[68,787]		(0.0152)	(0.0273)			
6-18	High school graduation	13,183	0.3940	0.0150	0.0286	0.4124	0.109	0.328
		[13,183]		(0.0094)	(0.0178)			
All	Soc. costs, most conservative	33,400	3,084	-161	-344*	3,482	0.102	0.328
		[283,091]		(98)	(206)			
0-6	Inpatient or emergency claim	9,538	0.2449	-0.0012	-0.0014	0.2421	0.852	0.920
		[52,378]		(0.0063)	(0.0114)			
6-18	Inpatient or emergency claim	12,526	0.2471	-0.0059	-0.0105	0.2547	0.324	0.556
		[56,480]		(0.0060)	(0.0112)			
Female								
0-6	Test score	8,488	-0.1446	0.0019	0.0029	-0.1511	0.919	0.920
		[52,107]		(0.0183)	(0.0316)			
6-18	Test score	14,855	-0.1479	0.0168	0.0300	-0.2082	0.240	0.556
		[73,389]		(0.0143)	(0.0273)			
6-18	High school graduation	13,792	0.5766	0.0101	0.0190	0.5846	0.279	0.556
		[13,792]		(0.0094)	(0.0176)			
All	Soc. costs, most conservative	33,210	574	61**	121*	635	0.043	0.311
		[284,057]		(30)	(63)			
0-6	Inpatient or emergency claim	9,379	0.2119	0.0018	0.0032	0.2202	0.767	0.920
		[50,549]		(0.0062)	(0.0113)			
6-18	Inpatient or emergency claim	16,050	0.3702	0.0025	0.0047	0.3823	0.653	0.873
		[75,526]		(0.0056)	(0.0108)			

Notes: Unit of observation is the person-year for all outcomes, except high school graduation which is a person-level cross-section. CM = control mean. ITT = intent-to-treat. IV = instrumental variables. CCM = control complier mean. FDR = false discovery rate. See text for discussion of these estimates. Standard errors are reported in parentheses and are clustered at the household level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 2.4: Housing Voucher Effect on Geographic Outcomes (10% Sample)

	1997-2005 Addresses				2012 Address[1]			
	CM	ITT	IV	CCM	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Has address on file	0.897	0.0067	0.0141	0.891	0.863	-0.0117	-0.0241	0.896
		(0.0082)	(0.0173)			(0.0168)	(0.0346)	
Miles from baseline address					63.243	9.1904	18.4862	34.647
						(11.6729)	(23.5552)	
Living in IL	0.956	0.0041	0.0085	0.972	0.862	0.0085	0.0171	0.909
		(0.0072)	(0.0151)			(0.0169)	(0.0339)	
Fraction of quarters outside IL	0.0471	-0.0064	-0.0132	0.0307				
		(0.0071)	(0.0146)					
Living in Cook County, IL					0.796	0.0145	0.0292	0.852
						(0.0193)	(0.0387)	
Poverty rate > 20%[2,3]	0.655	-0.0088	-0.0184	0.712	0.688	-0.0374	-0.0698	0.703
		(0.0176)	(0.0362)			(0.0248)	(0.0461)	
Poverty rate[2,3]	0.273	0.0039	0.0075	0.274	0.289	-0.0076	-0.0142	0.292
		(0.0055)	(0.0112)			(0.0080)	(0.0150)	
Fraction black[2,3]	0.794	0.0012	0.0023	0.837	0.760	-0.0011	-0.0020	0.789
		(0.0084)	(0.0172)			(0.0155)	(0.0290)	
Social capital[2,4]	3.495	-0.0056	-0.0109	3.501	3.776	0.0187	0.0345	3.769
		(0.0057)	(0.0114)			(0.0137)	(0.0253)	
Collective efficacy[2,4]	3.761	-0.0158**	-0.0312**	3.772	3.502	0.0177*	0.0326*	3.491
		(0.0078)	(0.0155)			(0.0096)	(0.0177)	
Violent crime rate (per 1,000)[5]	17.633	-0.0896	-0.1920	17.865	25.142	0.1358	0.2508	24.964
		(0.3026)	(0.5998)			(0.6984)	(1.2904)	
Property crime rate (per 1,000)[5]	75.479	-3.1948***	-6.2988***	77.120	60.185	0.5016	0.9263	59.530
		(0.9911)	(1.9753)			(1.3479)	(2.4904)	

Notes: Unit of observation in columns 1-4 (with the exception of "Fraction of quarters outside IL") is the person-quarter. Unit of observation in columns 5-8 is the person. Standard errors are reported in parentheses and are clustered at the household level.

[1] Outcome measures based on the American Community Surveys for 2005-09.

[2] Measured at the Census tract level.

[3] Data from the decennial 1990 and 2000 censuses and the American Community Surveys for 2005-09 (interpolating values for inter-censal years).

[4] Data from the Project on Human Development in Chicago Neighborhoods (PHCDN) Community Survey.

[5] Data from annual beat-level crime panel from the Chicago Police Department.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table 2.5: Housing Voucher Effects on Child's School Characteristics and Moving

Outcome	Children/ Obs.	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Males age 0-6 at baseline					
Fraction minority	10,341	0.9668	0.0014	0.0025	0.9716
	[90,561]		(0.0018)	(0.0031)	
Fraction with subsidized lunch	10,341	0.8698	0.0009	0.0016	0.8637
	[90,561]		(0.0018)	(0.0031)	
Average test score	10,341	-0.1981	0.0035	0.0062	-0.2274
	[90,561]		(0.0058)	(0.0100)	
School moves	9,888	0.26	0.0074	0.0132*	0.26
	[80,983]		(0.0045)	(0.0078)	
Miles from baseline address to school	9,730	2.91	0.2053**	0.3609**	2.86
	[86,748]		(0.0827)	(0.1437)	
Females age 0-6 at baseline					
Fraction minority	10,053	0.9662	0.0025	0.0044	0.9690
	[88,883]		(0.0018)	(0.0032)	
Fraction with subsidized lunch	10,053	0.8677	0.0006	0.0010	0.8609
	[88,883]		(0.0019)	(0.0034)	
Average test score	10,053	-0.1800	-0.0000	-0.0002	-0.1955
	[88,883]		(0.0061)	(0.0108)	
School moves	9,575	0.25	0.0108**	0.0196**	0.25
	[79,257]		(0.0045)	(0.0079)	
Miles from baseline address to school	9,472	2.86	0.2469***	0.4419***	2.96
	[85,273]		(0.0789)	(0.1380)	

Notes: Unit of observation is the person-year for all outcomes. Standard errors are reported in parentheses and are clustered at the household level.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 2.6: Estimated Effects of Cash Transfers on Education, Criminal Behavior, and Health

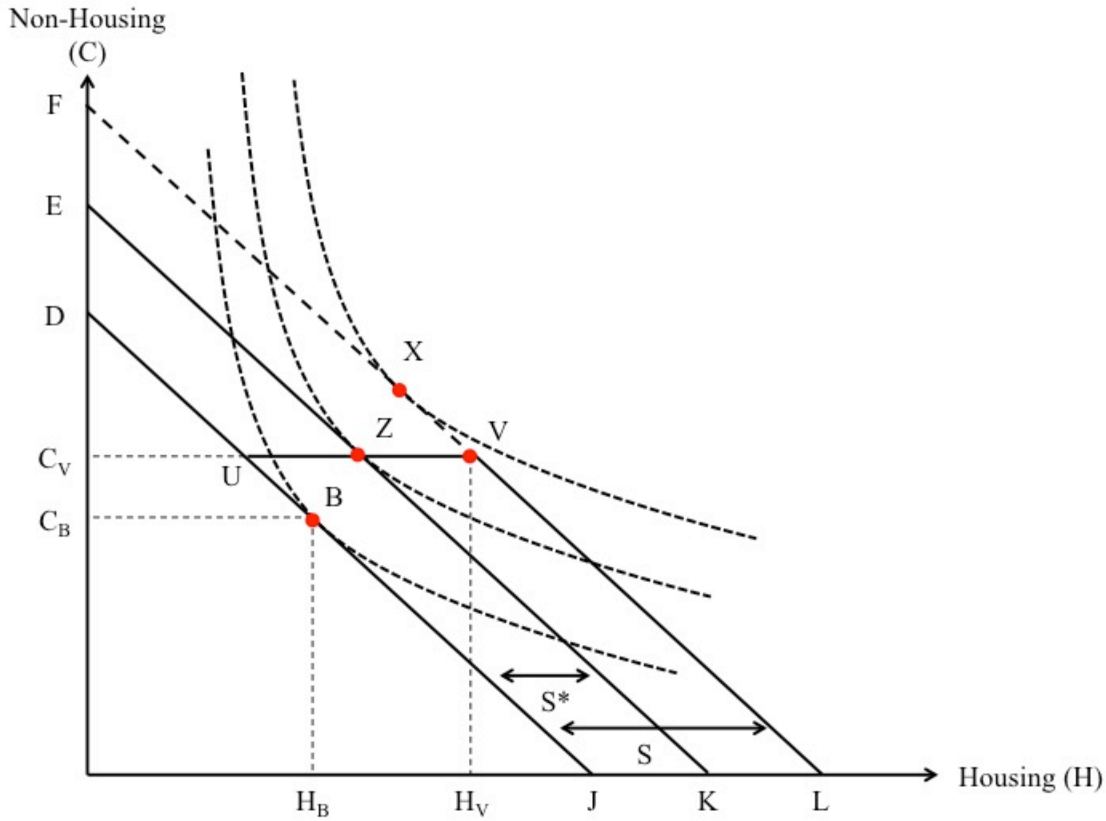
Baseline Age	Outcome	Children/ Obs.	CM	IV Rescaled by Implied Voucher Value *S*	*S**	*ΔC*
		(1)	(2)	(3)	(4)	(5)
Male						
0-6	Test score	8,659	-0.3339	0.0050*	0.0084*	0.0107*
		[51,339]		(0.0026)	(0.0043)	(0.0055)
6-18	Test score	14,348	-0.3248	0.0010	0.0019	0.0021
		[68,787]		(0.0022)	(0.0042)	(0.0047)
6-18	High school graduation	13,183	0.3940	0.0029	0.0065	0.0064
		[13,183]		(0.0018)	(0.0041)	(0.0040)
All	Soc. costs, most conservative	33,400	3,084	-27*	-59*	-60*
		[283,091]		(16)	(36)	(37)
0-6	Inpatient or emergency claim	9,538	0.2449	-0.0001	-0.0002	-0.0003
		[52,378]		(0.0009)	(0.0015)	(0.0019)
6-18	Inpatient or emergency claim	12,526	0.2471	-0.0009	-0.0017	-0.0019
		[56,480]		(0.0009)	(0.0018)	(0.0020)
Female						
0-6	Test score	8,488	-0.1446	0.0003	0.0004	0.0007
		[52,107]		(0.0025)	(0.0042)	(0.0054)
6-18	Test score	14,855	-0.1479	0.0024	0.0046	0.0052
		[73,389]		(0.0021)	(0.0042)	(0.0047)
6-18	High school graduation	13,792	0.5766	0.0020	0.0045	0.0044
		[13,792]		(0.0018)	(0.0042)	(0.0041)
All	Soc. costs, most conservative	33,210	574	10**	22**	22**
		[284,057]		(5)	(11)	(11)
0-6	Inpatient or emergency claim	9,379	0.2119	0.0003	0.0004	0.0006
		[50,549]		(0.0009)	(0.0015)	(0.0019)
6-18	Inpatient or emergency claim	16,050	0.3702	0.0004	0.0008	0.0008
		[75,526]		(0.0009)	(0.0019)	(0.0019)

Notes: Unit of observation is the person-year for all outcomes, except high school graduation which is a person-level cross-section. IV estimates shown are re-scaled by the implied value of the voucher--S, S*, or ΔC--in thousands of 2013 $. S is the total cost to the government of the housing voucher subsidy, equal to $12,501 on average for our study sample. S* is the cash transfer that would generate the same increase in non-housing consumption as does a housing voucher, equal to $6,377 on average for our study sample; see text for calculation. ΔC is the increase in non-housing consumption from receiving a housing voucher, equal to $5,653 on average for our study sample. Standard errors are reported in parentheses and are clustered at the household level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

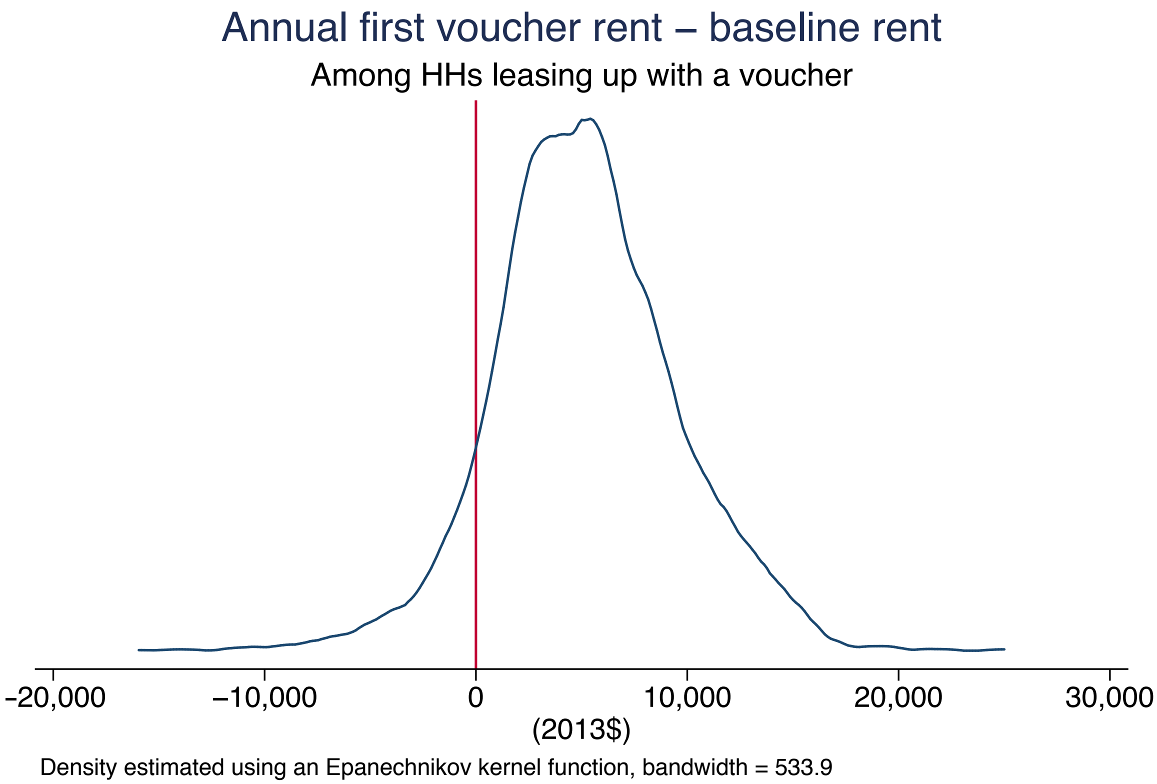
Figure 2.1: Budget Constraint and Consumption With and Without Housing Voucher



Notes: Without a housing voucher, the family's budget constraint is given by *DJ* with initial consumption bundle *B*. With a housing voucher, the family's new budget constraint is *DUVL*, and the new consumption bundle, assuming the family chooses to lease a unit at the maximum allowable level, is *V*. (H_V is essentially the maximum rent allowable under the program, the Fair Market Rent. In some versions of the program, families can lease units with higher rents, but for simplicity we assume here that the FMR is the maximum rent.) Note that the voucher consumption bundle *V* results in more housing consumption than if the family was given a cash transfer with the same cost to the government (*S*), represented by consumption bundle *X*.

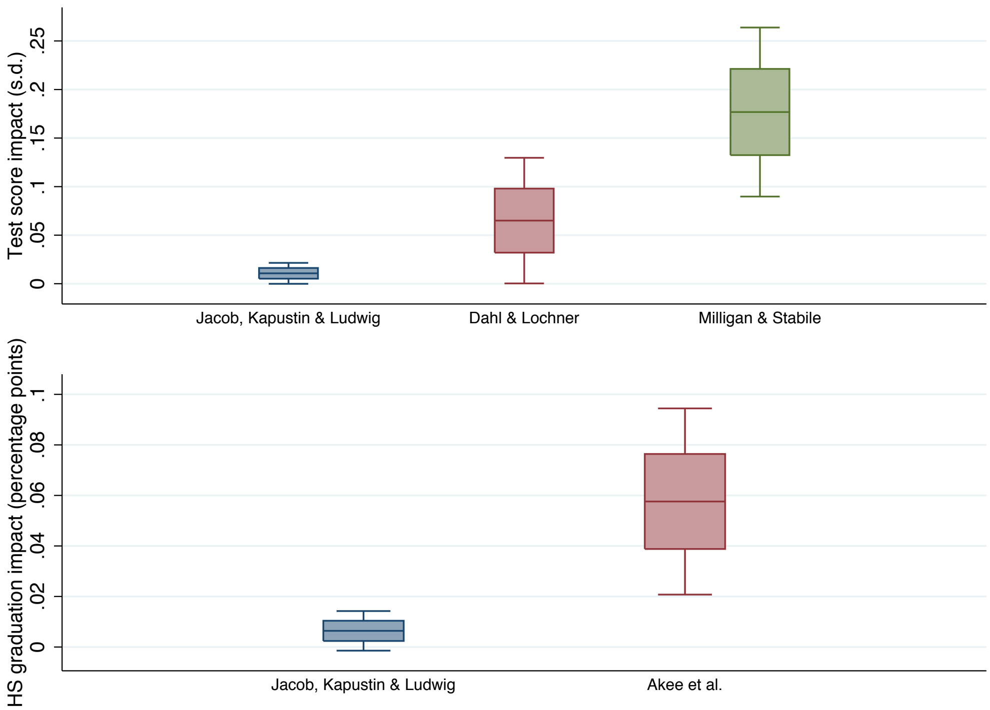
A cash transfer of S^* will result in the same change to consumption of non-housing goods ($C_V - C_B$) as a family experiences when it receives a housing voucher worth *S* to the government. One of our model specifications in the tables below assumes (conservatively) that housing consumption has no effect on children's outcomes, and uses an indicator for randomly-assigned voucher offer as an instrument for non-housing consumption (S^*) to estimate the change in children's outcomes for a $1,000 gain in family income.

Figure 2.2: Distribution of Change in Housing Consumption Among Leased-Up Sample



Notes: Figure shows the distribution of the change in housing consumption from receipt of a housing voucher. Baseline rent is estimated using a special tabulation of 2000 Census data from Chicago and assumes that families in our study sample have the same average rents as other, demographically-similar households in the same baseline census tracts (see Appendix F). First voucher rent is measured using HUD 50058 forms, which all families in means-tested housing programs are required to complete each year (or whenever they relocate). All figures converted to constant 2013 dollars.

Figure 2.3: Effects of Cash Transfers on Educational Outcomes of Males Across Studies



Notes: Figure reports the effects on children's achievement test scores (top panel) and high school graduation rates (bottom panel) per $1,000 change in family income (in 2013 dollars). The estimates from Jacob, Kapustin, and Ludwig are taken from Table 2.6, column 5, using as the dependent variables (1) an average of reading and math achievement test scores for males 0-6 at baseline and (2) an indicator for whether the youth graduated from high school during our study period for males 6-18 at baseline, from Chicago Public Schools student-level school records. The estimate from Dahl and Lochner (2012) is also for an average of reading and math test scores, taken from their Table 6 for males (equal to 0.088 SD in their paper reported in 2000 constant dollars, and equal to 0.065 when we update to 2013 dollars). The estimate from Milligan and Stabile (2011) is for math scores for males, taken from their Table 3, equal to 0.23 SD in their paper for a $1,000 change in Canadian 2004 dollars, and equal to 0.177 when we update to 2013 US dollars. The estimate from Akee et al. (2010) is for males is based on their Table 5, column 2; the marginal effect here corresponds to a 32 percentage point change in high school graduation rates from a $4,000 change in family income in 1996-2002 dollars, or 8 percentage points per $1,000. The effect equals 5.8 percentage points per $1,000 when we update to 2013 dollars.

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CHAPTER III

Predictors of Successful Housing Voucher Lease Up and Implications for Estimated Labor Market Responses

(joint with Eric Chyn and Joshua Hyman)

Low participation rates in government assistance programs are a major policy concern in the United States. We estimate the predictors of successful take-up of Section 8 housing vouchers, a program in which take-up rates among interested and eligible households are often as low as 50%. We examine 18,109 households in Chicago that were offered a voucher during 1997-2003. We link household members to administrative datasets on employment, public assistance usage, arrests, residential location, and children's academic performance. Our results suggest a non-monotonic relationship between disadvantage and take-up: unemployed residents and employed residents with relatively high incomes do not lease up as often as residents who are employed but earn relatively little. Other factors that predict voucher use suggest that the perceived benefit of the voucher is particularly important in explaining take-up. Based on our analysis of the predictors of voucher take-up, we use a reweighting procedure that generalizes the estimates of the impact of vouchers on labor supply presented in Jacob and Ludwig (2012). We find that policies to increase take-up rates could exacerbate intensive margin labor market reductions among voucher recipients.

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3.1 Introduction

Low participation rates in government assistance and benefit programs are a major policy concern in the United States. Estimates of take-up rates range from 8-14% for the State Children's Health Insurance Program (LoSasso and Buchmueller 2004), 69% for Food Stamps (Currie 2006), and 75-85% for the Earned Income Tax Credit (Bhargava and Manoli 2015; IRS 2002; Scholz 1994). Low take-up rates undermine the effectiveness of government policies by decreasing the likelihood that benefits reach the households for whom they are intended.

One large social program with historically low rates of take-up is tenant-based rental housing assistance, commonly referred to as the Section 8 housing voucher program. In 2011, the federal government provided 2 million families with Section 8 assistance at a cost of over $18 billion, approximately equal to expenditures on TANF (NCSHA 2011; Falk 2012). Despite this large investment, voucher take-up, particularly in large cities, tends to be low, often near 50% (Finkel and Buron 2001; Mills et al. 2006; Sanbonmatsu 2011).

This low take-up rate is surprising for two reasons. First, unlike many other means-tested social programs, housing assistance is not an entitlement; thus, the low take-up rate for Section 8 is among those eligible families aware of and sufficiently motivated to apply for assistance, not among all eligible families. This contrasts with take-up rates for other programs which are calculated over all eligible individuals, including those unaware or uninterested in the programs. Second, housing assistance is among the more generous social programs in the US. For example, a family with one child and an annual income of $10,000 living in an area with the program's average payment standard would receive an annual housing subsidy of $6,600 (Olsen 2008).

In this paper, we answer two research questions. First, what are the predictors of successful lease up when a household is offered a housing voucher? The answer is of practical importance to the department of Housing and Urban Development (HUD) and local Public Housing Authorities (PHAs), which administer the Section 8 program. HUD prioritizes assistance for what it terms "very low-income households," such as a family of four of with less than 50% of the local area median income.[1] If families that successfully lease up are the most advantaged among those offered a voucher, then the program may not be reaching the most vulnerable households that could benefit most from the assistance.

Our second research question asks how the labor market effects of housing assistance would change if voucher take-up rates improved. Jacob and Ludwig (2012) provide evidence that vouchers reduce employment and earnings while increasing participation in social programs. However, their estimates provide the causal effect of vouchers among the roughly half of their sample that "comply" by successfully leasing up (Angrist, Imbens, and Rubin 1996). If effects on labor supply among complier households differ from those that would occur among non-leasing households, potential efforts to bolster take-up may either exacerbate or diminish the average impact of vouchers.

To address these questions, we study housing voucher take-up in Chicago, where 18,109 households were offered Section 8 vouchers during 1997 to 2003. This setting provides several advantages relative to previous studies of voucher take-up (Finkel and Buron 2001; Mills et al. 2006; Shroder 2002). First, our sample size is considerably larger than those in Finkel and Buron (2001), Mills et al. (2006), and Shroder (2002) (N = 2,609, N = 4,650, and N = 1,308, respectively), and thus we may be able to detect additional results with greater statistical precision. Second, we are able to link household members to administrative data on income, arrests, public assistance usage, residential location, and children's academic performance. This allows us to examine lease up behavior in greater detail than was previously possible.

[1] The poverty line is usually about 30 percent of local median income (Olsen 2003).

A third benefit to our study, and one that distinguishes it from the study most relevant to ours, is that the families in our sample primarily live in private, market-rate housing at baseline. By contrast, Shroder (2002) uses data from the Moving to Opportunity (MTO) demonstration, a sample entirely comprising families living in public housing when offered a voucher. The distinction is meaningful for two reasons: one, the value of a housing voucher to a family varies considerably based on whether the family is living in private or public housing at the time of offer,[2] and so the predictors of successful lease up may vary across the two groups; and two, the experience of Chicago, where the overwhelming majority of families applying for housing vouchers live in private housing, is increasingly the norm. The affordability of rental housing, particularly for very low-income and minority families, has diminished in recent years, and a larger share of renters are devoting a greater share of their budgets to housing (JCHS 2008). Understanding what drives take-up in this group, and the consequences of take-up for their labor market outcomes, is of primary importance to policymakers.

We find mixed evidence on whether the most disadvantaged households are the least likely to lease up with a voucher. On the one hand, being employed prior to receipt of a voucher offer is extremely predictive of leasing up in our sample, with those who are employed having a 24 percentage point higher probability of lease up after controlling for other observed characteristics. However, among the approximately 60% of the sample who were employed prior to the voucher offer, those with higher earnings were less likely to lease up. Further, unmarried household heads, who are less likely to have stable financial conditions relative to married household heads, have a greater probability of leasing up.

[2] Families in private housing are offered a subsidy to offset their living costs, whereas families in public housing are offered a chance to move residences and thereby change the form—but not the amount—of their existing housing subsidy.

We find a number of other factors that predict voucher take-up, many of which suggest that a family's perception of how beneficial the voucher will be to their lives is an important driver of successful lease up. For example, poor academic performance by children in the household and living further from amenities such as a public transit stop, are both associated with greater lease up. Other characteristics, such as local crime rates, have a strong theoretical relationship with lease up, but have no detectable relationship after controlling for our rich set of covariates.

For the characteristics examined in the existing literature (e.g., basic household demographics, employment, metropolitan area vacancy rates), our results are similar. Further, we find a similar pattern of results among households living in private and public housing at baseline, suggesting that results estimated using the MTO sample in Shroder (2002) can likely be extrapolated to households living in private housing, despite differences in the benefits of vouchers between these samples.

Finally, based on the analysis of lease up, we consider how efforts to increase voucher use would affect household labor market outcomes. Specifically, we extend the analysis of voucher effects from Jacob and Ludwig (2012) by providing new estimates using weights based on observed compliance patterns. Our approach scales up the treatment effects for compliers who are observationally similar to non-compliers so that our estimates represent the effect of vouchers on the entire population of voucher-seeking households. We find that increasing take-up would have no impact on the employment effects found in Jacob and Ludwig (2012) but would substantially exacerbate the negative effects on earnings and the positive effects on public assistance receipt. Thus, policies to increase take-up rates could result in larger intensive margin labor market reductions among voucher recipients.

This paper is organized as follows. The next section presents details on the Section 8 housing voucher program and reviews the related literature. Section 2 describes our data and presents summary statistics. Section 3 presents the outline of a theoretical model of how various factors affect the likelihood of leasing up. Section 4 presents our results for predictors of take-up. Section 5 details our empirical approach to, and results of, re-weighting the labor supply effects of increased take-up. Section 6 concludes.

3.2 Background

3.2.1 Program Details

Vouchers subsidize low-income families to live in private, market-rate housing. Eligibility for the program is a function of family size and income. The subsidy amount is based on the local Fair Market Rent (FMR), which in Chicago during 1997 to 2003 was set at approximately the 45th percentile of the metropolitan area rent distribution. A household using a voucher must contribute 30% of its adjusted income[3] toward rent, with the voucher making up the difference between the family's contribution and the lesser of FMR or the unit rent. Since 1987, families can use vouchers offered by one PHA to live in the jurisdiction of other PHAs ("porting out"), though most do not.

A family offered a voucher has a limited amount of time in which to find and lease a unit, which during Chicago for this time period was 60 days.[4] If the household fails to do so, it loses the opportunity to receive housing assistance and the PHA rescinds its offer. To be able to

[3] Adjusted income reflects dependents ($480 deduction per child), disability ($400 deduction per disabled household member), childcare expenses, and medical care expenses exceeding 3% of annual income. Although some forms of public assistance, such as TANF, are considered income, EITC and the value of in-kind benefits, such as Food Stamps and Medicaid, are not.

[4] Extensions of an additional 60 days could be granted for families in need of large units or who have documented medical problems.

successfully lease up with a voucher, a landlord must first be willing to have Section 8 participants as tenants.[5] In addition, lease up also requires that the desired unit must meet minimum housing quality standards, including the requirement that there must be a bedroom for every two persons, and that, excepting young children, children of the opposite sex may not be required to share a bedroom (HUD 2001, Ch. 10.3). In terms of timeline, a family interested in a unit must file a Request for Lease Approval (RFLA) with the PHA, await inspection, and, once approved, sign a lease.

Prior to voucher issuance, the PHA is required to brief families about the housing voucher program's details and requirements. As part of this briefing, the PHA must distribute packets containing, among other items, information on the voucher's term, policies regarding extensions, and a list of landlords or real estate agents who can assist families in finding a unit. Although HUD's guidebook describing the voucher program mentions several ways that PHAs can assist families in their search (e.g., providing transportation, counseling, childcare, or a list of available units), it does not require them to provide any of these.[6] For additional details about housing voucher rules, see HUD (2001) and Olsen (2003).

3.2.2 Previous Literature

What we know about housing voucher lease up success rates and the characteristics of those households that successfully lease up comes from a series of HUD-commissioned reports (Leger and Kennedy 1990; Kennedy and Finkel 1994; Finkel and Buron 2001; Mills et al. 2006) and work by HUD economist Mark Shroder (2002). While the older HUD reports offer context and historical information, the work by Finkel and Buron (2001), Mills et al. (2006), and Shroder

[5] While it is illegal for landlords to discriminate on the basis of income in Chicago and other large cities, it has been nevertheless been widely reported to occur (Popkin and Cunningham 1999).
[6] Unfortunately, we have been unable to determine which if any of these were provided in the setting studied here.

(2002) are most relevant to this paper since they study a similar time period. Table 3.1 summarizes results from these three papers.

Finkel and Buron (2001) wrote the most recent HUD-commissioned report. It examines the success rate and predictors of lease up for 2,609 households in 48 large PHAs across the country in the spring and summer of 2000.[7] The authors find several characteristics associated with lease up. Elderly-headed households, households with five or more members, those with no children, and those with relatively high or zero household income were all less likely to lease up with a voucher. Households with a disabled household head were more likely to lease up. Metropolitan area level vacancy rates also had a positive effect on leasing, while success rates did not differ by the household head's race, gender, or source of income.

Mills et al. (2006) study a Welfare-to-Work program across six PHAs providing vouchers to 4,650 households that were receiving, had received, or were eligible to receive TANF benefits. Voucher offers were made in 2000-2001, and participants were surveyed in 2004-2005. Among the authors' findings are that higher earnings and participation in job training were both positive predictors of lease up. Having been previously employed, having dependent children, receiving TANF, and not receiving supplemental security income (SSI) were also positively associated with lease up.

Shroder (2002) examines data from the MTO experiment conducted in Baltimore, Boston, Chicago, Los Angeles, and New York during 1994-1998. In this experiment, Section 8 housing vouchers were randomly offered to 1,308 households living in public housing.[8] Shroder finds that metropolitan area vacancy rates and the number of preschool-aged children in the

[7] 70% of the sample was offered vouchers in May and June.

[8] Vouchers were offered to an additional 1,740 households with the requirement that they be used to move to census tracts in which fewer than 10% of households are poor. Shroder finds that the predictors of successful lease up differ among households offered vouchers with the location constraint relative to those offered unconstrained vouchers, the group most similar to our study.

household are positively associated with the voucher success rate. Household size is negatively related to take-up, as is receiving SSI/SSDI/SS Survivor benefits. Additionally, several variables from the baseline survey in the MTO experiment predicted lease up. For example, the household head's subjective probability of having a successful search, dissatisfaction with current neighborhood, and self-reported level of comfort with change are all positively associated with successful lease up. Belonging to a church nearby, having many friends in the neighborhood, current housing condition, and years living in the metropolitan area are all negatively related to successfully leasing up with a voucher.

In addition to these HUD-affiliated reports, qualitative evidence on the lease up process from focus group participants sheds light on where individuals encounter difficulty when searching for a unit to lease. For example, almost 90% of survey participants interviewed in Popkin and Cunningham (1999) who failed to lease a unit did so without submitting a single RFLA, implying that finding a suitable unit and a willing landlord, rather than passing inspection, posed the greatest challenge to applicants.

3.3 Data

The Chicago Housing Authority Corporation (CHAC), a private entity tasked by HUD to administer Chicago's Section 8 program, reopened the city's voucher wait list in July 1997 for the first time in over a decade. A total of 82,607 income-eligible household heads applied before the list was closed just a few weeks after it opened. In August 1997, CHAC randomly assigned each applicant a lottery number from 1 to 82,607 and informed those families with the 35,000 best (lowest) lottery numbers of their position and told them that they would receive a voucher within three years. CHAC told the remaining families (numbers 35,001 – 82,607) that they would not receive a voucher. CHAC then began to offer vouchers to households beginning with the lowest lottery numbers.[9] By May 2003, 18,109 families from the wait list had been offered housing vouchers. At this point, CHAC was "over-leased" and stopped offering new vouchers.

We focus on these 18,109 families that were offered a voucher off the CHAC wait list by May 2003. Figure 3.1(A) displays the number of these families that were offered vouchers by CHAC each quarter from 1997:III to 2003:II, as well as the number that used them. As shown in the bottom panel, the take-up rate fluctuated in a narrow range, averaging 46% across the sample period. Annual vacancy rates for the Chicago metropolitan area during this period are included in Figure 3.1(B); there does not appear to be a stark relationship between vacancy rates and lease up, nor between time on the wait-list and lease up.

The data on these 18,109 households are an extension of the data used in Jacob and Ludwig (2012). We obtain baseline address information, lottery number, basic household demographics, and information for the household head and spouse from the CHAC wait list application forms. These forms do not include identifying information for other members of the household, so we determine who else was living with the household head at baseline using data from the Illinois Department of Human Services (IDHS). This means that throughout our period of analysis, we have information on only those household members living with the household head at the time of the lottery. We obtain voucher usage from HUD 50058 records, which families must complete annually to verify program eligibility.

We use data from the 2000 Census to determine tract-level characteristics of households' baseline neighborhoods, and we calculate annual local crime rates from beat-level Chicago Police Department data. We have access to Illinois Unemployment Insurance (UI) data that

[9] Service of the wait list was interrupted from August 1998 until early 2000 so that vouchers could be distributed to a set of Latino families in response to a discrimination lawsuit against the City.

provide quarterly employment and earnings information; IDHS records that provide quarterly indicators of AFDC/TANF, Food Stamps, and Medicaid usage; Illinois State Police arrest records for juveniles and adults; and Chicago Public Schools data for children's test scores. Finally, for approximately half of the households in our sample, we also have data on residential location at the time they received a voucher offer, obtained from their public assistance records. We use these address data to calculate the distances between households' baseline address and local amenities, as well as to determine whether households recently moved to a new address.

Figure 3.2 displays the locations of households in our sample at the time they applied to the lottery. Households are concentrated in the historically low-income South and West sides of Chicago. Figure 3.3 shows the variation in voucher take-up rates across census tracts in Chicago. Darker areas represent neighborhoods with the highest rates. Take-up rates do not vary considerably across tracts. The interquartile range for the fraction of a baseline tract's households that lease up is 43% to 58%.

Table 3.2 provides sample means by baseline housing status. Among the 90% of the sample who lived in private housing at baseline (column 1), the average age at voucher offer of the household head was 38, with the majority being unmarried African-American women. Among the 49% of households with children at the time of voucher offer, the average number of children was two and the average child age was 10.5. Nearly 60% of household heads were employed during the year prior to voucher offer, earning on average $17,700 annually, and nearly the same percentage received TANF, Food Stamps, or Medicaid. During the two years prior to voucher offer, about 10% of household heads and children were arrested at least once.

The 10% of households living in public housing at baseline (column 5) were even more likely to be headed by an unmarried African-American woman with children. The household head was slightly less likely to be employed, earned substantially less conditional on working ($13,800 annually), and was considerably more likely to receive public assistance. The likelihood of the household head or children having been recently arrested was also greater among the public housing sample, particularly so for children.

Table 3.2 also displays differences in characteristics between households that successfully lease up with their voucher (compliers) and those who do not (non-compliers). The raw differences in means presented in columns 4 and 8 suggest that household heads who lease up are more likely to be black, female, unmarried, younger, employed, have children, use public assistance, and live in more impoverished, racially homogenous neighborhoods with higher violent crime rates than their counterparts who do not lease up. Nearly all of these differences are of the same sign among the households living in public housing at baseline, with some differences in magnitude. We discuss in detail below why these and other factors may predict lease up, and which are statistically significant in a multivariate regression model.

3.4 Model

To motivate our empirical analysis, we developed a simple, one-period model of the lease up process.[10] As discussed previously, program rules dictate that a voucher recipient must find a unit with a participating landlord within 60 days that meets minimum housing standards (verified by inspection).[11]

With this in mind, a household receiving a voucher offer must undertake a search for suitable housing. Let V represent the net benefit of living in the next rental property that the

[10] Our discussion is similar in spirit to Shroder (2002).
[11] Note that this design does not preclude households from attempting to apply their voucher offer to their pre-program residence. In our sample, this happens infrequently, with only 6.8% of households that use the voucher deciding to “lease in place.”

household considers. Denote the probability that this particular unit can be successfully leased—that is, pass inspection and gain the landlord's approval—as P. Finally, let C represent the costs (monetary or otherwise) associated with finding and applying for this particular unit. In this case, the household will continue searching for housing as long as:

$$PV - C > 0$$

That is, we assume that households search as long as the expected benefit outweighs the cost. Households will fail to lease if costs are high, expected value is low, or their perceived probability of finding a suitable unit is low.[12]

With this model in mind, we can relate each of the variables in our data to the probability of acceptance, expected benefits of leasing, and the costs associated with search. Some variables clearly relate to the probably of lease up through only one of the channels, while other variables may relate to lease up through multiple channels.

3.4.1 Probability of Finding a Unit

The probability of lease up is related to factors that influence the supply of both units suitable for the family to lease and landlords willing to lease them. In our data, we examine the following variables:

- Number of children – Larger units are more difficult to find (Popkin and Cunningham 1999), so we expect that larger households will have a reduced likelihood of finding a unit in time.
- Metropolitan vacancy rate – The greater the overall supply of rental units, the more likely it is that a household will find one to lease using the voucher.

[12] Households that fail to lease up do so because they stop or fail to search. This is because the probability of successfully finding a rental unit to use with the voucher approaches one as the number of applications goes to infinity. But, infinite search is not rational when costs are non-zero.

- Season – Offers arrive throughout the year, but rental markets are seasonal, being most active in Chicago during the summer months and least active during the winter.[13]

3.4.2 Net Benefit of Lease Up

The net benefit of leasing up with a voucher can vary substantially across individuals depending on their family's current and recent life circumstances and current neighborhood quality. Several variables we examine that could affect the benefit of leasing up include:

- Recently moved – A household that recently moved may be less willing to deal with the burden of moving again if offered a voucher.
- Children's academic performance and criminal activity – Families with children whose academic or social performance has suffered prior to receipt of a voucher may be more likely to lease up in order to move to a better neighborhood or change schools.[14]
- Neighborhood quality – Rates of crime and poverty, the percent of households that are black, and distance to amenities such as schools and hospitals are measures of neighborhood quality. Individuals in worse current neighborhoods should value vouchers more than those living in neighborhoods that are relatively better.

3.4.3 Costs Associated With Search

Searching for a unit is a costly endeavor for many applicants. Several factors that may affect the cost of search that we study include:

- Age – Elderly recipients are more likely than their younger peers to find searching difficult.
- Disability – Like the elderly, disabled recipients may face an increased cost for search.

[13] https://www.rentalutions.com/education/chicago-rental-market-stats-ordinances-resources/

[14] Though recent research suggests this may not be the case, with voucher recipients living near worse schools than low-income non-voucher recipients (Horn, Ellen, and Schwartz 2012).

- Receiving public assistance – Families who receive public assistance may be relatively more familiar with the social service system, which may aide them in making use of the resources available to facilitate housing search.

3.4.4 Variables Potentially Related to Lease Up Through Multiple Channels

Inevitably, many characteristics can reasonably be thought to affect the probability of finding a unit, the benefit of a voucher, or the cost of search.

- Children – Households with children may be less appealing to landlords due to noise and other spillovers (probability of lease up). Alternatively, the perceived benefit of leasing up may be higher for a household with children (net benefit).
- Average age of children – Similarly, households with older children may be less appealing to landlords than households with younger children due to noise and other spillovers (probability of lease up). Alternatively, perceived benefit of leasing up may decline with the number of years a family's children benefit from the housing (net benefit). Finally, young children impose a cost to search if childcare is needed (search cost).
- Distance from nearest transit stop – Access to mass transit is an amenity (net benefit); however, that access could also allow for less costly search.
- Prior arrests – An individual with recent prior arrests may be incarcerated at the time of offer (search cost) or viewed by landlords as an undesirable tenant (probability of lease up). Alternatively, these individuals may desire a clean slate and want to move away from a neighborhood they believe plays a role in their criminal behavior (net benefit).
- Summer interacted with has children – For those households with children, receiving the voucher offer during the summer could increase search costs due to the need for childcare,

but it could also increase the net benefit of the voucher because disrupting the school year with a move is costly.

- Employment and earnings – Being employed (and having higher earnings conditional on employment) should raise the opportunity cost of time and thus increase the cost of searching.[15] However, landlords may view employed (or higher-earning) tenants as desirable, and there may also be unobserved characteristics (e.g., motivation, perseverance) associated with being employed (or earning more) that affect the probability of successful lease up.
- Recent change in employment – An individual who recently began employment may view moving as a risky disruption. Conversely, having recently lost a job may add value to a voucher due to the ability to move closer to a new (potential) employer. However, recently beginning employment makes searching for an apartment costlier, and recently ending employment makes searching easier.

3.5 Predicting Voucher Use

Our primary empirical strategy is to predict whether a household leases up, y, using a rich set of household-level covariates, X. To do this, we estimate the following linear probability model for households offered a voucher through the lottery:[16]

$$y = \alpha + X\beta + \varepsilon, \qquad (1)$$

where our estimates of the coefficients in the β vector reveal which characteristics are predictive of lease up. We report heteroskedasticity-robust standard errors.

[15] Although we cannot separate wage from hours worked, whether a job pays a high hourly wage, requires more hours, or both, the opportunity cost of taking time off is higher relative to a job with a lower wage or reduced time requirements.

[16] Estimating this model using logit does not substantively alter any of our findings. See Tables I.1 and I.2.

In addition to our baseline specification, we also exploit the detailed panel nature of our data to examine how time-varying factors during the period leading up to a voucher offer predict lease up. For example, a child being arrested may influence a parent's decision to use a voucher arriving a few weeks after the arrest more so than a voucher arriving several years later. For these predictors, we estimate the following specification using OLS:

$$y = \alpha + X\beta + \pi_t x_t + \varepsilon, \qquad (2)$$

where t represents the quarter-year relative to voucher offer and x_t is a time-varying characteristic. We perform this estimation separately for several time-varying predictors (employment, public assistance receipt, household head's and child's arrest) and for up to nine quarters prior to voucher offer (t = -9, …, 0). Our estimates of π_t capture the time-varying association of x with voucher take-up.

Table 3.3 presents the results from our estimation of equation (1) for the households in our sample living in private housing at baseline. Controlling for all covariates, we find that basic household head demographics are important and statistically significant predictors of lease up. Male heads of household are 5.7 percentage points less likely than women to lease up when offered. This could reflect either landlord preferences for women tenants or reduced willingness by men to undergo costly search efforts. Black household heads are 3.8 percentage points more likely than whites to lease up, suggesting that in the majority-black neighborhoods of Chicago in which these households are searching for housing, either black household heads may have access to networks easing the search process or landlords prefer to lease to black tenants. Finally, non-married household heads are 7.3 percentage points more likely to lease up than those with a spouse.

3.5.1 Probability of Finding a Unit

The first variables we examine provide insight into whether supply-side constraints, such as the availability of units that fit their needs, influence households' lease up decisions. We find a precisely estimated zero association between lease up and the number of either adults or children in the household. This result stands counter to prior evidence and suggests that larger apartments may be no more difficult to lease than smaller apartments.

Our only direct measure of the supply of available units, the metropolitan area vacancy rate, has the predicted sign, with higher lease up rates when the vacancy rate is higher. Note that this relationship exists after controlling for other covariates even though there is no visual relationship apparent between vacancy and lease up rates in Figure 3.1(B). Our estimates imply that a one standard deviation increase in the vacancy rate (1.7 percentage points) leads to a 1.7 percentage point increase in the likelihood of leasing up.

Finally, the season in which the voucher offer occurred is related to lease up, with offers during the summer and especially the fall resulting in a higher lease up rate relative to the spring, which has the lowest lease up rate. One possible explanation for this result is that landlords attempt to rent new properties coming on the market during the spring to non-voucher holders, but as the seasons pass and landlords face the possibility of not renting their property before the next winter lull, they increasingly offer properties to voucher holders (whom they may view as less ideal tenants).

3.5.2 Net Benefit of Lease Up

Now we examine variables that measure the net benefit to families of leasing up with a voucher. As mentioned previously, families that may experience a greater benefit include those living in distressed neighborhoods, with at-risk children, or whose voucher offer comes at a time when they could benefit most from it.[17]

We find no evidence that recently moving affects a household's probability of lease up. This stands in contrast to our hypothesis that households having recently moved face a reduction in net benefit of leasing up.

The perceived benefit of a voucher may be greater if households are unhappy with the current neighborhood and educational situation for their children. We find mixed evidence of this being the case. Households are somewhat more likely to lease up if their children's recent academic performance is lower, with a standard deviation lower average test score associated with a 2 percentage point higher lease up rate.[18] We find no statistically significant relationship between lease up and whether a household's children were recently arrested. This is surprising, given that household heads participating in the MTO demonstration listed moving away from gangs and drugs as the first or second most important reason for participating in the program (Sanbonmatsu et al. 2011).

Finally, we find little evidence that individuals living in more disadvantaged neighborhoods are more likely to use a voucher if offered. Once we control for other factors, rates of crime in an individual's baseline neighborhood do not predict lease up, despite the difference in baseline neighborhood crime rates between compliers and non-compliers observed in Table 3.2. Living closer to a school and to a hospital is predictive of *more* voucher take-up, opposite of the sign we expected, suggesting that these may not be seen as amenities, or that the

[17] In an alternative specification, we exclude the year fixed effects, and instead include time on the waitlist to test whether households are less likely to lease up after a longer amount of time on the wait list. As in Figure 3.1(B), we find no relationship and prefer to include year fixed effects, which are collinear with time on the waitlist.
[18] We define children's average test scores as the average across subjects of the scores standardized to mean zero and standard deviation of 1 before taking the average. We assess test scores and being arrested during the two years prior to voucher offer.

areas surrounding them may be otherwise undesirable along unobserved dimensions. Interestingly, an increase of one standard deviation in the percentage of an individual's baseline neighborhood that is black is associated with a 2 percentage point increase in the probability of take-up.

3.5.3 Costs Associated With Search

We next examine measures of the cost of searching for a unit. As found in prior work, the household head's age at the time of voucher offer is negatively predictive of lease up, suggesting that older individuals face more difficulties searching. Household heads who are 10 years older lease up at a 2 percentage point lower rate. Disabled household heads are 4 percentage points more likely to lease up, counter to our expectation based on search costs, but similar to results from prior work. We do not find that receipt of social programs such as Food Stamps, TANF, or Medicaid is predictive of lease up, suggesting that familiarity with public assistance does not help reduce the cost of searching. This stands in contrast to the almost 30 percentage point difference observed in Table 3.2; after controlling for our rich set of covariates, this relationship between public assistance usage and lease up completely vanishes.

3.5.4 Variables Potentially Related to Lease Up Through Multiple Channels

Many characteristics can reasonably be thought to affect lease up through multiple channels. We present such characteristics here, and only predict the sign of their relationship with lease up if all channels predict the same sign. For characteristics where the multiple channels have oppositely signed predictions, the observed sign can reveal which channel is the dominant constraint to leasing up.

Households with children are 4.9 percentage points more likely to lease up than households without children, suggesting that any difficulty in finding an apartment due to landlords preferring households without children is outweighed by the increased net benefit to the voucher from having children.

Families with older children have a lower probability of lease up: having children who are, on average, ten years older reduces the probability of successful lease up by 3 percentage points, suggesting either that landlords prefer to lease to families with relatively younger children, that the perceived benefit to leasing up is inversely related to the number of years a family's children will be able to benefit from the housing, or that moving is more disruptive once children are older. This result suggests that the probability of lease up and net benefit channels outweigh higher search costs resulting from finding childcare for younger children. Households with children are no more or less likely than households without children to lease up if their offer occurs during the summer months.

Distance between baseline address and a rail or bus stop has a positive relationship with lease up. Living one mile farther away from a transit stop is associated with a 7.8 percentage point larger probability of lease up. This suggests that the amenity value of the transit stop (net benefit) outweighs the search cost channel.

The household head having been arrested in the two years prior to receiving a voucher offer is associated with a 7 percentage point reduction in voucher use, suggesting that either increased search costs due to incarceration or a reduced probability of lease up due to disapproving landlords outweighs the increased net benefit from wanting to move away.

There is no relationship between lease up and whether a household head recently ended employment.[19] However, household heads who recently began employment are 10.2 percentage points less likely to lease up, suggesting some combination of these households' preference to avoid disrupting recently improving life-circumstances (net benefit) and the recent increase in the opportunity cost of time.

Finally, whether a household head is employed is a very large, positive predictor of lease up, suggesting that those individuals who are able to secure work are more appealing as tenants and prove more capable at navigating the lease up process.[20] Employed household heads are 24.2 percentage points more likely to lease up. However, conditional on being employed, those with higher earnings are less likely to lease up, suggesting the importance of the time-value of search.[21] A one standard deviation increase in an individual's earnings, for example, is associated with a 7.7 percentage point decrease in their probability of using a voucher.

These results depict an inverted U-shaped relationship between advantage and lease up: those households that are the least advantaged have the lowest time-cost of search but either face discrimination by landlords or have trouble leasing up for the same reasons they struggle to secure employment – due to some omitted measure of ability, motivation, or persistence. Those households that can secure employment, but earn relatively little, have a relatively low time-cost of search and either face approving landlords or can successfully navigate the search process.

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[19] We define recently beginning employment as starting an employment spell during the four quarters prior to voucher offer after a spell of non-employment lasting at least one year. Recently ending employment is defined in an analogous manner.

[20] Employment is defined as having positive earnings during at least one of the four quarters prior to voucher offer. This result is robust to alternative definitions of employment, such as having earnings in excess of $100 during one of the preceding four quarters.

[21] An alternative explanation for the negative relationship between earnings and the likelihood of leasing up is that some household heads meet the voucher income criteria at the time they apply but exceed the criteria when an offer is made. Excluding household heads whose nominal earnings in the year prior to offer exceed HUD's income limits does not materially change this relationship.

The most advantaged individuals face a high time-cost of search that trumps any preference received by landlords and high ability at navigating the search process.

3.5.5 Results for Households Living in Public Housing at Baseline

Given the smaller sample size, our results estimated for households living in public housing at baseline reveal fewer statistically significant predictors of successful lease up. However, the sign of the significant point estimates nearly uniformly match those for the private sample, suggesting that the predictors of successful lease up are similar between households living in private and public housing at baseline.

3.5.6 Dynamic Predictors of Lease Up

Using equation (2), we examine how several pre-offer characteristics—employment, public assistance usage, and household arrests—dynamically predict lease up. Figure 3.4 plots the regression coefficients associated with each characteristic over time.[22]

Employed household heads are consistently more likely to lease up, but this relationship tends to be strongest in the five quarters prior to voucher offer relative to the sixth through ninth quarters prior to offer. The exception is the quarter the offer is received, in which there is a relatively weaker relationship. Like the negative effect for recently beginning employment, this seems to suggest that while working individuals are more likely to lease up, actually being employed at the time of the offer may make searching more difficult or decrease the net benefit of moving.

[22] Recall that, in this context, the regression coefficients are differences in means since each measure we consider is a dummy variable.

In contrast to the null relationship observed in Table 3.3, Figure 3.4 shows that households receiving Food Stamps, TANF, or Medicaid more than one year before their voucher offer are more likely to lease up. It is in the year prior to offer that this relationship attenuates toward zero, perhaps suggesting that these households have not been on other forms of public assistance for long enough to become familiar with the system.

Household heads arrested prior to receiving a voucher offer are consistently less likely to lease up, with no dynamic relationship between the timing of arrest and voucher offer. Similarly, there appears to be no relationship between lease up and previous child's arrest, regardless of the timing relative to voucher offer.

3.6 Revisiting the Labor Market Effects of Housing Assistance

3.6.1 Methodology: Reweighting the Effects of Voucher Use

We next consider the role of compliance in estimating the impact of voucher assistance on household behavior. Jacob and Ludwig (2012) (henceforth JL) exploit the exogenous variation in housing assistance created by the CHAC 1997 lottery to estimate the effect of means-tested housing benefits on labor supply and public assistance usage. They find that voucher receipt reduces employment and earnings while increasing participation in social programs. Their estimates represent local average treatment effects (LATEs) that are valid for the set of housing voucher compliers (i.e., the approximately 50% of households offered a voucher who lease up (Imbens and Angrist 1994; Angrist, Imbens, and Rubin 1996)). Although the estimate of the LATE is credibly identified through the lottery design, it is widely acknowledged in the applied literature that the LATE may differ from another parameter of interest: the average treatment effect (ATE).[23] The latter would be useful for evaluating what effects would occur if compliance rates increased (e.g., if additional search assistance was provided). Estimating the ATE requires moving beyond the purely experimental framework provided by the housing voucher lottery.

We revisit the impact of housing assistance on labor supply and public assistance usage by estimating ATEs using inverse compliance score weighting based on the extrapolation framework provided by Angrist and Fernandez-Val (2010) and Aronow and Carnegie (2013). Specifically, we weight each member of our sample by the inverse of his or her lease up compliance score, which is the predicted probability of being a complier based on observed characteristics. This has the effect of re-weighting the complier sample to reflect the covariate distribution in the entire population selected for treatment.

Intuitively, the thought experiment behind our approach is as follows: how would the effects of vouchers change if take-up were improved? The difficulty in answering this question stems from the fact that some types of households may be underrepresented among the group that leases up with a voucher. The weighting procedure solves this problem by scaling up the labor market impacts for complying households that are less likely to lease up.[24] These low-probability compliers represent the type of household head that will be affected if voucher take-up is improved.

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[23] A large literature discusses the distinction between LATE and ATE estimates. A general overview is provided by DiNardo and Lee (2011).

[24] Consider the following simplified example based on Aronow and Carnegie (2013) to see how re-weighting the sample recovers the average treatment effect (ATE). Suppose that gender is the only determinant of compliance and that 50 and 25 percent of men and women comply with treatment, respectively. Assume that the treatment effect is always 0 for men and 2 for women. In this case, the average treatment effect is 1 assuming men and women have equal proportion in the experiment. However, the LATE is equal to the ratio of the ITT and the compliance rate in the treatment group. In this case, the LATE is equal to 2/3 since the ITT is equal to 1/4 (=0.5*0.05*0 + 0.5*0.25*2), and the average compliance rate is 0.375.

We implement the estimation procedure in two steps.[25] First, we estimate the predicted probability of a household being a "complier" using the covariates specified in equation (1). This probability, referred to as a compliance score, reflects the likelihood that a household uses a voucher when offered one through the lottery.[26] Then, the inverse of this compliance score serves as the weight for each household in a weighted IV estimation, as outlined in JL:

$$Leased_{it} = \alpha + \theta_1 PostOffer_{it} + \theta_2 PreOffer_{it} + X\Gamma + \gamma_t + \epsilon_{it} \qquad (3)$$

$$y_{it} = \alpha + \pi_1 Leased_{it} + X\Gamma + \gamma_t + \epsilon_{it} \qquad (4)$$

For the first stage equation in (3), the outcome is an indicator for whether household *i* uses a housing voucher by period *t*. Here, a family that stops using its voucher does not become "untreated." The variable $PostOffer_{it}$ equals 1 if household *i* is offered a voucher in any period prior to (and including) *t* and is 0 otherwise. $PreOffer_{it}$ equals 1 if household *i* will eventually receive a voucher offer but has not yet as of period *t*. The vector γ_t controls for calendar-year effects. Finally, we include X in the regression to control for baseline characteristics. Standard errors are clustered by household (Bertrand, Duflo, and Mullainathan 2004). In addition, we bootstrap the entire estimation process to account for variance due to estimation of the compliance score.

As discussed in Angrist and Fernandez-Val (2010) and Aronow and Carnegie (2013), this weighting method consistently estimates the ATE under three assumptions. First, the ATE for all individuals with a given set of characteristics must equal the ATE for all compliers with the same covariate profile. This is similar to the selection-on-observables assumption used in matching

[25] Appendix H discusses the implementation in detail and provides a simple numerical example illustrating the above intuition.

[26] The compliance score is calculated by subtracting the likelihood of take-up if assigned to the control group, obtained from a probit model estimated on control group households, from the likelihood of take-up if assigned to the treatment group, obtained from a separate probit model estimated on treatment group households. See Appendix H for details.

estimators. As in matching, this assumption is more plausible given a rich set of covariates with which to estimate the model, which we use in the present analysis.

The second assumption is that we have specified the compliance score estimator properly, a concern we attempt to address by flexibly parameterizing our covariates and testing the sensitivity of the estimates to different functional forms. We test whether our results are sensitive to including either a quadratic or cubic in all of our continuous covariates, and find that our results are unchanged. The third assumption is that there must be non-zero compliance in *every* covariate profile. Intuitively, this assumption requires that the compliance score is strictly above zero (i.e., each unit has a non-zero probability of being a complier). Figure H.1 in Appendix H shows the density of compliance scores. Less than 1 percent of the sample has a compliance score of 0.05 or smaller.

3.6.2 Improved Housing Assistance Take-Up and Labor Supply

In this section, we examine whether the relationships between household characteristics and successful lease up that we have documented would cause differences in the effects of housing assistance on labor supply if lease up rates improved. To do this, we replicate the LATE estimates reported in JL in columns 2-4 of Table 3.5, using their sample of 42,358 families headed by a working-aged (younger than 65), able-bodied (not disabled, as self-reported) adult at the time they applied to the CHAC wait list. We maintain their presentation by also reporting the control group mean (CM) in column 1 and the control complier mean (CCM) in column 3, which is the average outcome for the controls who would have used vouchers had they been assigned to the treatment group (Katz, Kling, and Liebman 2001).[27]

[27] In practice, the CCM is calculated by subtracting the LATE estimate of π_1 from the mean outcome of housing voucher participants.

Our replication of JL produces results that are identical to those in their paper: Voucher use reduced quarterly employment by 3.6 percentage points and quarterly earnings by $329, declines of 6% and 11%, respectively, relative to the CCM. Further, vouchers increased participation in social assistance programs by 6.7 percentage points (12%), with most of the increase being concentrated in Medicaid and Food Stamps.

Using the weighting method, we report two sets of ATE estimates in Table 3.5. The first, in column 5, uses weights derived from compliance scores estimated using the covariates in JL.[28] The second, in column 7, use weights derived from compliance scores estimated using the more comprehensive set of covariates used to predict lease up in equation (1).[29] Both sets of ATE estimates drop observations with estimated compliance scores less than 5%, per the recommendation of Aronow and Carnegie (2013).[30]

For both the JL and our preferred set of covariates, the ATE of voucher use on the probability of being employed (row 1) are similar to those presented in JL, all between 3 and 4 percentage points. This suggests that if the lease up rate improved, the new individuals leasing up would experience employment effects similar to those individuals currently leasing up. In contrast to the findings for employment, the ATE results for household head earnings consistently suggest that the negative impact of voucher use will be larger if take-up is increased. Using the JL covariates and covariates from the present paper, we find that voucher use reduces quarterly earnings by $459 and $492 (14% and 15%), respectively. These effects are notably larger than the $329 (10%) LATE estimated in JL. We find a similar pattern of results for the

[28] For a full list of the covariates used in Jacob and Ludwig (2012), see Appendix F.

[29] The covariates in Jacob and Ludwig (2012) and in the present paper are generally similar. The primary difference is that the present paper includes pre-offer measures of child academic performance; baseline address distance to public transit and schools; and indicators for recently or ending employment.

[30] Including these observations can skew the results, because the inverse weighting method assigns any observation with a low compliance score a high weight. We argue that it is sensible to leave aside those individuals who are the least likely to lease up, because even if lease up rates were substantially increased, unless the rate approached 100%, it is unlikely that these individuals would lease up.

other dependent variables measuring household head earnings.[31] These results suggest that the earnings of those individuals who are less likely to lease up are more sensitive to voucher receipt than the individuals currently leasing up.

Finally, there is little difference between the LATE estimated in JL and the ATE estimated here for any form of public assistance receipt when using the covariates from JL. However, when using the more comprehensive set of covariates from the present paper, the ATE on public assistance receipt is notably higher, revealing an increase of 10.0 percentage points (22%) relative to the LATE of 6.7 points (15%). This suggests that, as with earnings, the public assistance receipt behavior of individuals who would use a voucher if lease up rates were improved is particularly sensitive to voucher receipt.

3.7 Conclusion

Tenant-based rental housing assistance is a large and generous means-tested benefit program with historically low rates of take-up, particularly in large urban areas. In an attempt to understand the sources of this low lease up rate, we identify household characteristics that are predictive of successfully leasing when offered a housing voucher. Our sample is considerably larger than those used in prior studies, and we are able to link the sample to a host of administrative data sets to examine a richer set of household characteristics and alternative outcomes. Further, our study is distinct from the most similar previous work (Shroder 2002) because it focuses on individuals living in private, market-rate housing at the time they apply for a voucher, a group that is representative of most new voucher recipients today.

[31] These are: a) a dummy for whether the household head earns greater than \$3,220, which is equivalent to working full time at \$8/hour; b) household head earnings conditional on working; and c) household head log earnings conditional on working.

We find mixed evidence of whether the most disadvantaged households are those who struggle to lease up with a voucher. On the one hand, being employed prior to receipt of a voucher offer is extremely positively predictive of leasing up in our sample. However, among employed households, those with higher earnings were less likely to lease up. Further, unmarried household heads, who likely have less stable financial and family situations relative to married household heads, have an increased probability of leasing up.

In addition, we find a number of additional factors that are predictive of voucher take-up, many of which suggest that a family's perception of the benefits of vouchers is an important driver of successful lease up. For example, households living further from amenities such as a public transit stop or whose children have been recently performing poorly in school are more likely to lease up. Across characteristics available in the previous literature, our results tend to match those found in previous studies. Further, we find a similar pattern of results among households living in private and public housing at baseline.

Finally, based on this lease up analysis, we consider the consequences of increasing voucher take-up on household behavior. Extending the voucher analysis of Jacob and Ludwig (2012) by using a reweighting method, we estimate that increasing voucher take-up would not change the impact on employment, but would substantially exacerbate the (negative) effects on earnings. In addition, increasing voucher take-up would also increase reliance on public assistance programs. For policy-makers, this suggests that the objective of increasing lease up rates may come at the cost of reducing self-sufficiency for beneficiaries.

Table 3.1: Past Research on Predictors of Housing Voucher Lease-Up

	Finkel & Buron (2001)	Mills et al. (2006)	Shroder (2002)
	(1)	(2)	(3)
Voucher lease-up rates			
Overall	69%	71%	61%
Chicago	82%	n/a	67%
HH demographics & employment			
Black		+	
Elderly HHH	-	n/a	n/a
Household size	-	n/a	-
Has dependent children	+	+	n/a
Number of preschool-age children	n/a	n/a	+
Relatively high income	-	+	
Zero income	-	n/a	n/a
Source of income (SSI, welfare, etc.)			-
Disabled HHH	+	n/a	
Other characteristics			
Metropolitan area vacancy rates	+	n/a	+
Metropolitan area unemployment rate	n/a	+	n/a
HHH's subjective prob. of searching successfully	n/a	n/a	+
HHH's dissatisfaction w/ current nbhd.	n/a	n/a	+
HHH's comfort with change	n/a	n/a	+
Belonging to a church nearby	n/a	n/a	-
Having many friends in neighborhood	n/a	n/a	-
Current housing condition	n/a	n/a	-
Years living in metropolitan area	n/a	n/a	-
N (Households)	2,609	4,650	1,308
N, Chicago only	85	n/a	199

Notes: Finkel and Buron (2001) examine 2,609 households in 48 PHAs that were offered vouchers during the spring and summer of 2000. Mills et al. (2006) examine 4,650 households in 6 PHAs that were offered vouchers from 2000-2001. Shroder (2002) examines 1,308 households in 5 PHAs that were offered vouchers from 1994-1998. Chicago samples sizes in Finkel and Buron (2001) and Shroder (2002) are 85 and 199 households, respectively. + indicates a positive relationship, - indicates negative, an empty cell indicates no relationship, and n/a indicates that this characteristic is not available in this study. HHH=household head.

Table 3.2: Descriptive Statistics of Households, by Baseline Housing Status

	Private Housing at Baseline				Public Housing at Baseline			
	All	Leased Up	Did Not Lease	Difference	All	Leased Up	Did Not Lease	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Demographics (HHH)								
Male	0.164	0.117	0.211	-0.095***	0.125	0.072	0.193	-0.121***
Black	0.912	0.935	0.886	0.049***	0.942	0.971	0.903	0.069***
Hispanic	0.033	0.028	0.040	-0.012***	0.007	0.004	0.011	-0.008*
Age[1]	38.540	36.630	40.436	-3.806***	39.742	36.711	43.608	-6.897***
Disabled	0.258	0.261	0.255	0.006***	0.278	0.237	0.329	-0.092***
Has spouse	0.104	0.075	0.134	-0.059***	0.077	0.058	0.100	-0.041***
Household composition								
Number of adults (including HHH)[1]	1.462	1.438	1.486	-0.048***	1.540	1.566	1.506	0.060***
Has children[1]	0.494	0.565	0.424	0.141***	0.537	0.612	0.442	0.170***
Number of children[1]	2.031	2.079	1.968	0.111***	2.389	2.455	2.274	0.181**
Average age of children[1]	10.510	10.260	10.840	-0.579***	10.881	10.660	11.268	-0.608**
Average composite test scores of children[2,3]	-0.172	-0.196	-0.135	-0.061***	-0.294	-0.295	-0.292	-0.003***
Employment and public assistance usage (HHH)[4]								
Employed	0.585	0.663	0.508	0.155***	0.541	0.624	0.436	0.188***
Annual earnings (thousands of 2013 $)	17.712	14.547	21.795	-7.249***	13.802	12.422	16.307	-3.885***
Recently began employment	0.074	0.090	0.057	0.033***	0.084	0.095	0.071	0.024*
Recently ended employment	0.146	0.174	0.118	0.056***	0.131	0.158	0.098	0.060***
Received public assistance	0.582	0.724	0.442	0.282***	0.737	0.809	0.645	0.164***
Criminal activity[2]								
Household head arrested	0.096	0.092	0.100	-0.008*	0.111	0.091	0.137	-0.047***
Household children arrested[5]	0.102	0.101	0.103	-0.002***	0.135	0.133	0.137	-0.003***
Baseline neighborhood characteristics in 1997								
Poverty rate	0.283	0.295	0.270	0.025***	0.584	0.613	0.547	0.067***
Fraction black	0.791	0.826	0.756	0.070***	0.857	0.888	0.818	0.070***
Property crime rate (per 1,000)	74.458	74.850	74.048	0.802***	115.949	120.256	110.439	9.818**
Violent crime rate (per 1,000)	36.298	37.667	34.866	2.801***	61.498	66.079	55.636	10.443***
Distance to nearest:								
School	0.398	0.353	0.482	-0.130***	0.224	0.209	0.248	-0.039**
Hospital	1.373	1.314	1.483	-0.169***	1.207	1.175	1.261	-0.086***
Train or bus station	0.244	0.218	0.292	-0.075***	0.110	0.106	0.118	-0.012***
Fraction receiving voucher offer in								
Winter	0.246	0.245	0.248	-0.003***	0.244	0.240	0.248	-0.008***
Spring	0.259	0.246	0.272	-0.026***	0.260	0.244	0.281	-0.037*
Summer	0.255	0.258	0.251	0.007*	0.247	0.254	0.239	0.015*
Fall	0.240	0.251	0.229	0.022***	0.249	0.262	0.233	0.030***
N (Households)	16,179	8,042	8,137		1,930	1,079	851	

Notes: The sample is households that applied for and received Section 8 vouchers during 1997 to 2003. See text for details. HHH=household head. 1 = At voucher offer. 2 = During two years prior to voucher offer. 3 = Conditional on having at least one child tested in Chicago Public schools during pre-offer period. 4 = During year prior to voucher offer. 5 = Conditional on having at least one child during pre-offer period. *** Significant at the 1% level. ** 5% level. * 10% level.

Table 3.3: Predictors of Lease Up Among Households in Private Housing at Baseline

	Predicted Sign	Coefficient	Std. Error
	(1)	(2)	(3)
Demographics (household head)			
Male		-0.057***	(0.011)
Black		0.038**	(0.017)
Hispanic		0.01	(0.025)
Has spouse		-0.073***	(0.013)
Probability of finding and leasing suitable unit[1]			
Number of adults (including household head)	-	-0.004	(0.005)
Number of children	-	-0.001	(0.004)
MSA vacancy rate	+	1.078**	(0.502)
Offer arrived in winter		0.017	(0.010)
Offer arrived in fall		0.042***	(0.011)
Offer arrived in summer		0.029**	(0.013)
Expected net benefit of a voucher			
Recently moved[2]	-	-0.018	(0.011)
Average composite test scores of children[3]	-	-0.020**	(0.009)
Household children arrested[3]	+	-0.016	(0.018)
Poverty rate[4]	+	0.035	(0.032)
Fraction black[4]		0.077***	(0.016)
Property crime rate (per 1,000)[4]	+	0.000	(0.000)
Violent crime rate (per 1,000)[4]	+	0.000	(0.000)
Distance to nearest school (miles)[1]	+	-0.075***	(0.020)
Distance to nearest hospital (miles)[1]	+	-0.017**	(0.007)
Cost of finding a unit			
Age[1]	-	-0.002***	(0.000)
Disabled	-	0.039***	(0.009)
Received public assistance[2]	+	0.019	(0.030)
Difficult to categorize			
Has children		0.049**	(0.020)
Average age of children	-	-0.003**	(0.001)
Distance to nearest rail or bus stop (miles)[1]		0.078***	(0.021)
Household head arrested[3]		-0.069***	(0.013)
Offer arrived in summer X Has children		-0.009	(0.017)
Recently ended employment[2]	+	-0.018	(0.012)
Recently began employment[2]	-	-0.102***	(0.016)
Employed[2]		0.242***	(0.012)
Annual earnings (thousands of 2013 $)[2]		-0.006***	(0.000)
N (Households)		16,179	
R-squared		0.169	

Notes: The sample is Chicago households living in private market housing at baseline (July 1997) and offered a housing voucher between 1997 and 2003. Each point estimate and standard error are from a single Linear Probability Model regression of an indicator equal to one if the household leased up on the covariates. The regression also includes offer year fixed effects and indicators for missing values. Standard errors are heteroskedasticity-robust. 1 = At voucher offer. 2 = During year prior to voucher offer. 3 = During two years prior to voucher offer. 4 = At baseline (July 1997). *** Significant at the 1% level. ** 5% level. * 10% level.

Table 3.4: Predictors of Lease Up Among Households in Public Housing at Baseline

	Predicted Sign	Coefficient	Std. Error
	(1)	(2)	(3)
Demographics (household head)			
Male		-0.114***	(0.035)
Black		0.078	(0.055)
Hispanic		-0.115	(0.125)
Has spouse		-0.024	(0.043)
Probability of finding and leasing suitable unit[1]			
Number of adults (including household head)	-	0.022*	(0.012)
Number of children	-	0.005	(0.010)
MSA vacancy rate	+	2.308	(1.441)
Offer arrived in winter		0.043	(0.030)
Offer arrived in fall		0.089***	(0.032)
Offer arrived in summer		0.051	(0.039)
Expected net benefit of a voucher			
Recently moved[2]	-	0.039	(0.029)
Average composite test scores of children[3]	-	0.007	(0.028)
Household children arrested[3]	+	0.008	(0.046)
Poverty rate[4]	+	0.107	(0.080)
Fraction black[4]		0.026	(0.059)
Property crime rate (per 1,000)[4]	+	0.000	(0.000)
Violent crime rate (per 1,000)[4]	+	0.000	(0.000)
Distance to nearest school (miles)[1]	+	-0.078	(0.082)
Distance to nearest hospital (miles)[1]	+	-0.026	(0.018)
Cost of finding a unit			
Age[1]	-	-0.006***	(0.001)
Disabled	-	-0.009	(0.026)
Received public assistance[2]	+	0.032	(0.112)
Difficult to categorize			
Has children	-	0.029	(0.060)
Average age of children	-	-0.004	(0.004)
Distance to nearest rail or bus stop (miles)[1]		0.048	(0.102)
Household head arrested[3]		-0.154***	(0.035)
Offer arrived in summer X Has children		0.031	(0.048)
Recently ended employment[2]	+	-0.015	(0.035)
Recently began employment[2]	-	-0.090**	(0.044)
Employed[2]		0.168***	(0.036)
Annual earnings (thousands of 2013 $)[2]		-0.004**	(0.001)
N (Households)		1,930	
R-squared		0.167	

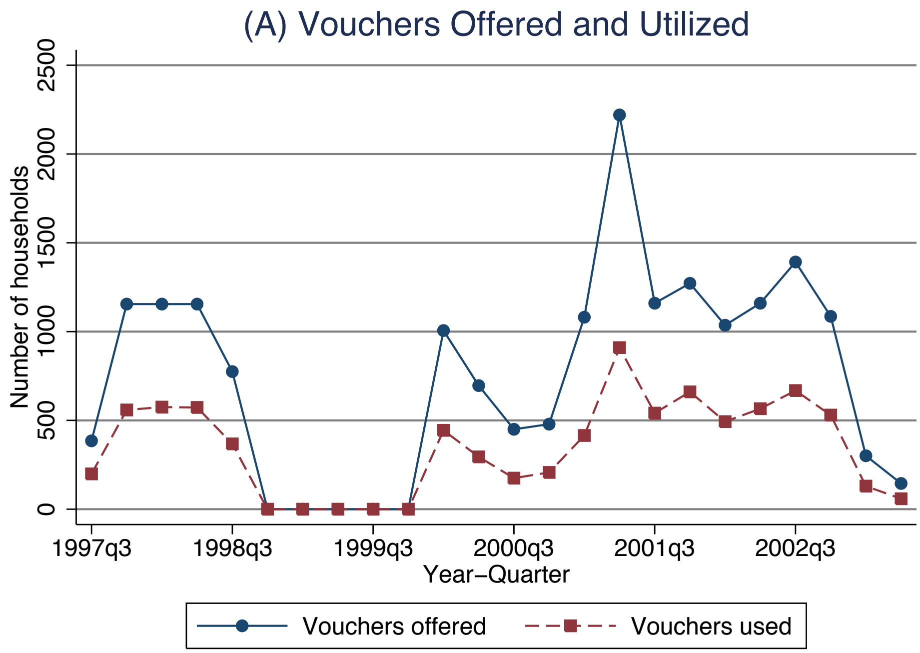
Notes: The sample is Chicago households living in public housing at baseline (July 1997) and offered a housing voucher between 1997 and 2003. Each point estimate and standard error are from a single Linear Probability Model regression of an indicator equal to one if the household leased up on the covariates. The regression also includes offer year fixed effects and indicators for missing values. Standard errors are heteroskedasticity-robust. 1 = At voucher offer. 2 = During year prior to voucher offer. 3 = During two years prior to voucher offer. 4 = At baseline (July 1997). *** Significant at the 1% level. ** 5% level. * 10% level.

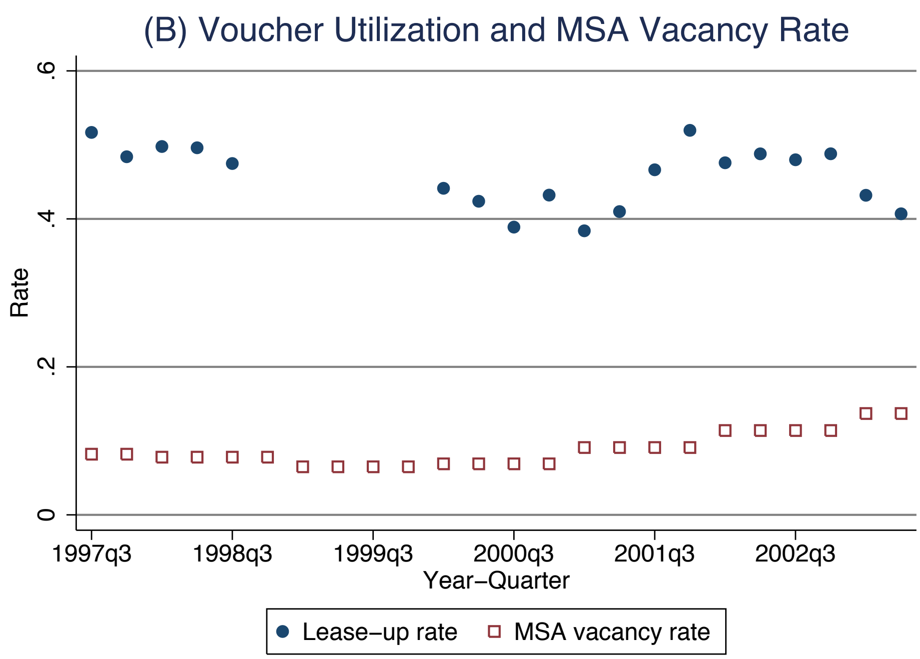
Table 3.5: Effects of Housing Vouchers on Labor Supply and Public Assistance Receipt (LATE vs. ATE)

		Jacob and Ludwig (2012)			CHK (JL covariates)		CHK (CHK covariates)	
	CM	IV	CCM	Obs.	IV	Obs.	IV	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HHH Employed	0.592	-0.036**	0.605	42,358	-0.040**	42,052	-0.031**	42,105
		(0.009)			(0.013)		(0.013)	
HHH Earnings	3291.016	-328.949**	3113.896	42,358	-458.726**	42,052	-491.534**	42,105
		(74.560)			(129.218)		(142.391)	
HHH Earnings>$3220 (FT@$8/hr)	0.404	-0.045**	0.403	42,358	-0.056**	42,052	-0.059**	42,105
		(0.009)			(0.013)		(0.013)	
HHH Earnings conditional on working	5557.984	-227.544**	5128.369	38,628	-327.428**	38,363	-532.053**	38,393
		(80.221)			(129.546)		(151.311)	
HHH Log earnings conditional on working	8.279	-0.073**	8.220	38,628	-0.093**	38,363	-0.142**	38,393
		(0.018)			(0.027)		(0.030)	
HHH Received public assistance	0.460	0.067**	0.552	42,358	0.070**	42,052	0.100**	42,105
		(0.009)			(0.012)		(0.012)	
HHH Received TANF	0.146	0.017**	0.110	42,358	0.013**	42,052	0.022**	42,105
		(0.004)			(0.005)		(0.005)	
HHH Received Medicaid	0.400	0.058**	0.484	42,358	0.059**	42,052	0.083**	42,105
		(0.009)			(0.011)		(0.011)	
HHH Received Food Stamps	0.375	0.076**	0.449	42,358	0.076**	42,052	0.102**	42,105
		(0.008)			(0.011)		(0.011)	

Notes: Columns 2-4 replicate the results from Table 3 of Jacob and Ludwig (2012) (henceforth JL). Columns 5-7 are re-weighted estimates, using the covariates in JL to estimate voucher compliance, and trimming observations with less than a 5% predicted probability of compliance. Columns 8-10 are re-weighted estimates, using the covariates in Table 3 to estimate voucher compliance, and trimming observations with less than a 5% predicted probability of compliance. HHH = household head. CM = control mean. IV = instrumental variables. CCM = control complier mean. See text for discussion of these estimates. We construct standard errors (clustered at the household level) by bootstraping (N = 200) the entire estimation process to account for variance in the estimated compliance score. All earnings measured in 2007 dollars. *** Significant at the 1% level. ** 5% level. * 10% level.

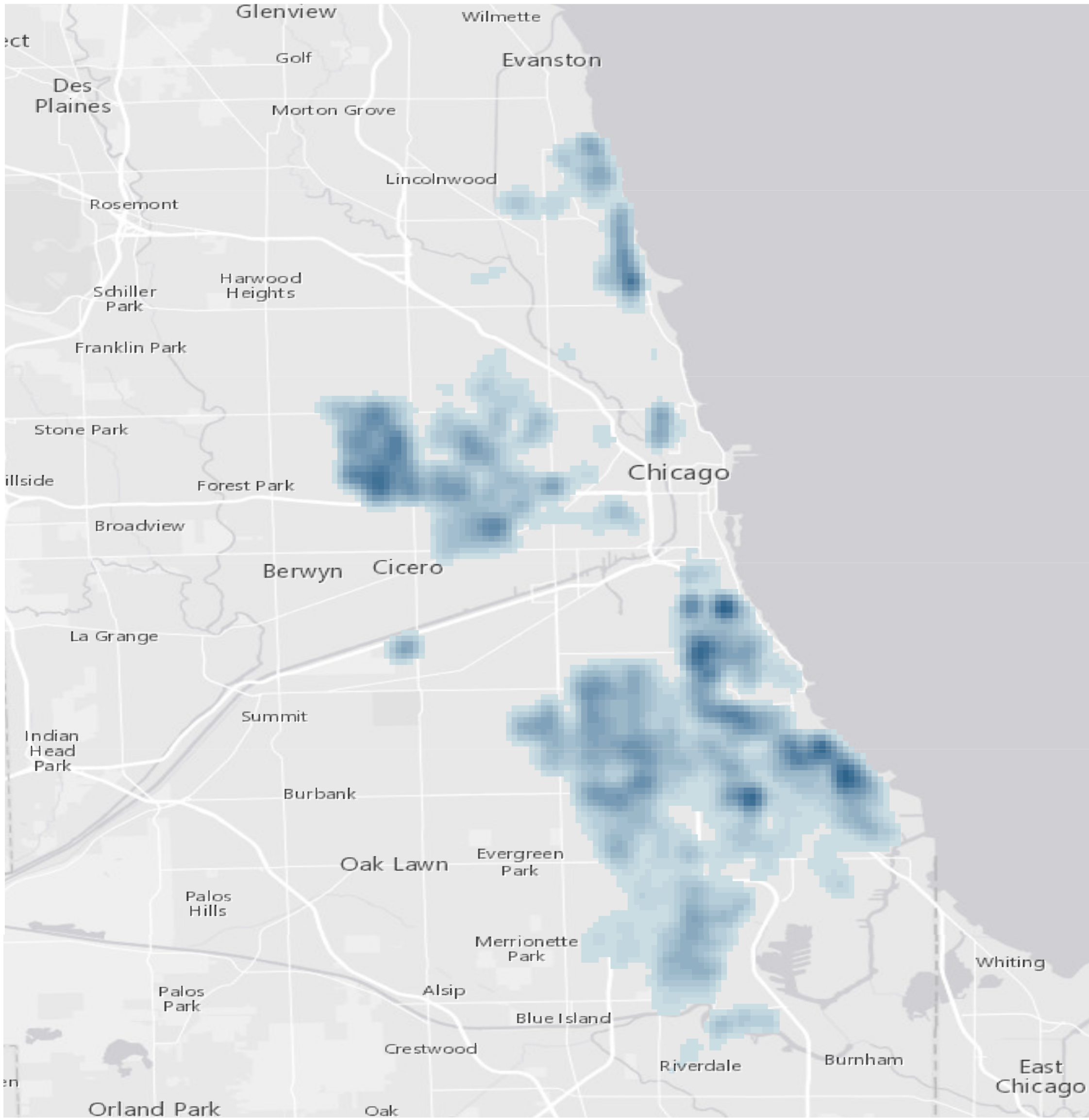
Figure 3.1: Voucher Offers, Utilization, and MSA Vacancy Rate





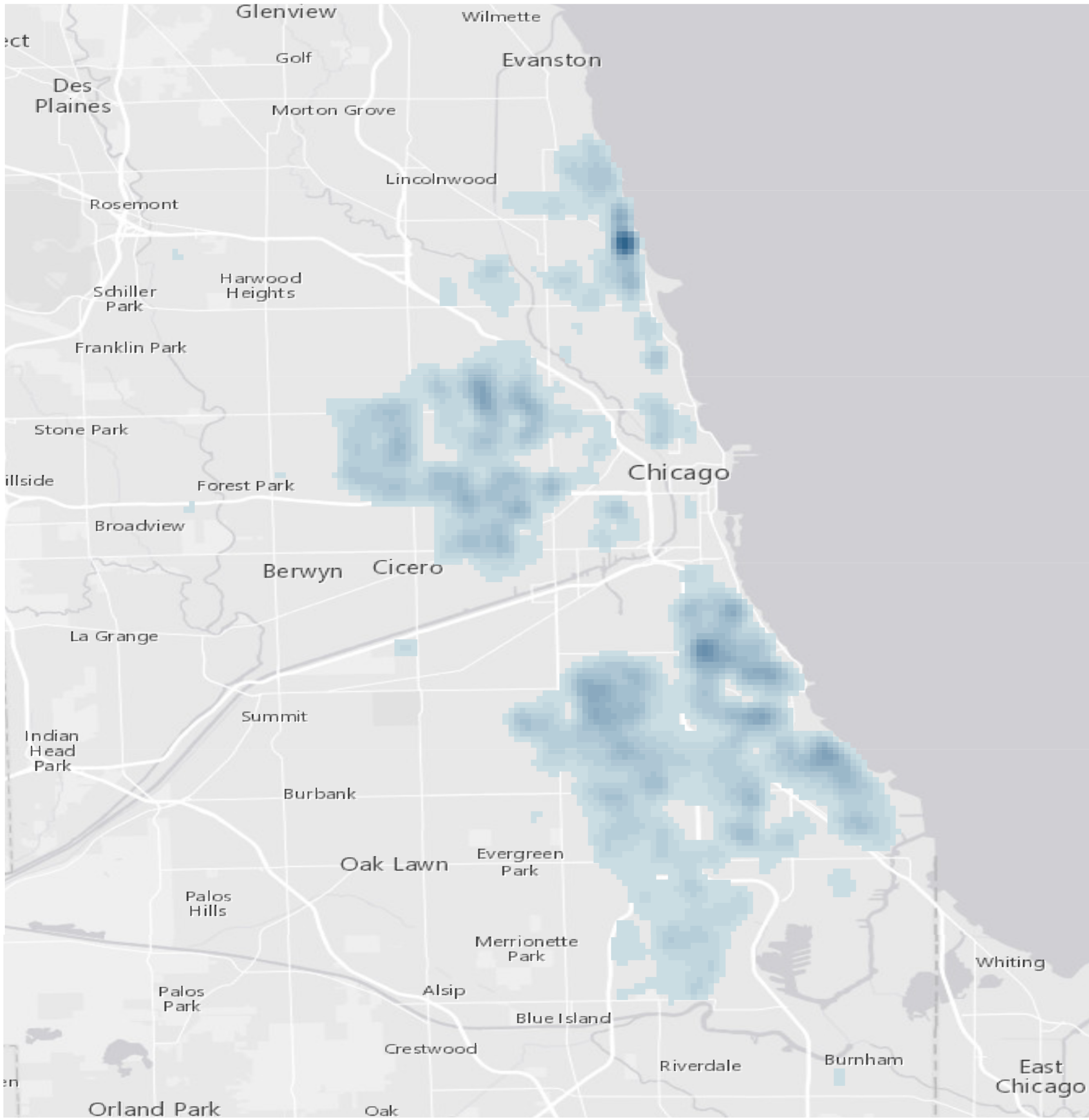
Notes: Top panel reports the number of CHAC vouchers issued to, and the number subsequently used by, families on the waiting list from 1997:III to 2003:II. No vouchers were issued from September 1998 until January 2000 in response to a discrimination lawsuit against the City of Chicago. Bottom panel reports the fraction of voucher offers made each quarter that were subsequently used (lease-up rate) and the annual, MSA-level vacancy rate.

Figure 3.2: Household Locations at Time of Voucher Lottery



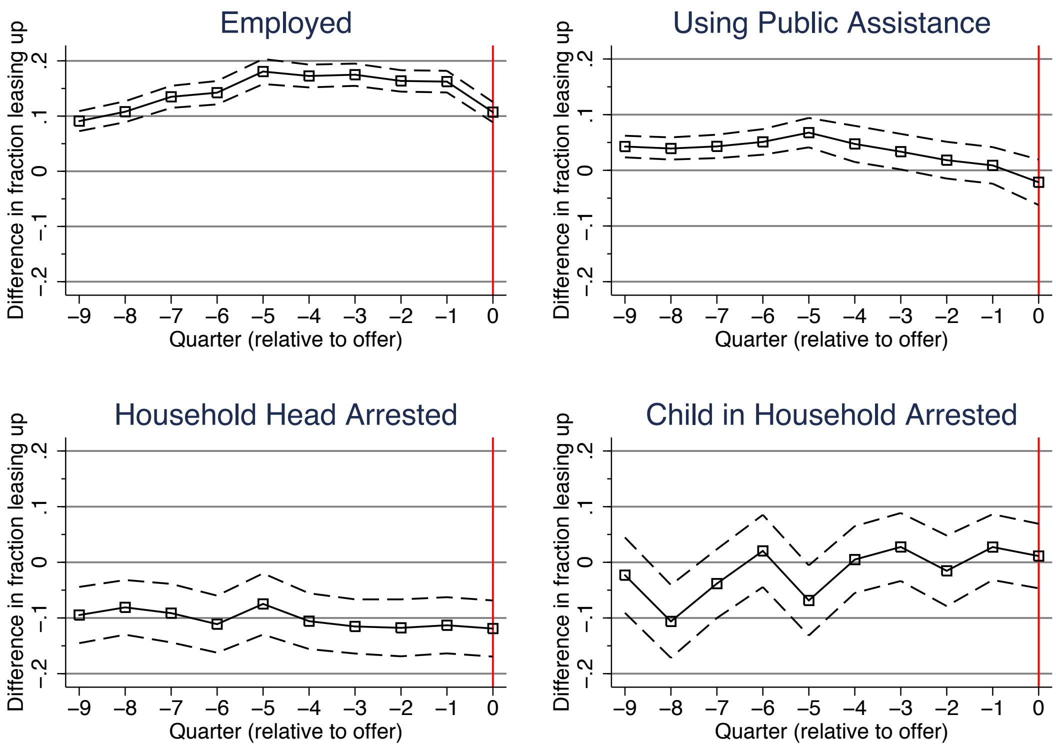
Notes: Map displays the density of sample households throughout the Chicago area based on their baseline address. The highest concentrations of households, represented by the dark blue regions, are located in the historically low-income South and West sides of the city.

Figure 3.3: Heat Map of Voucher Take-Up



Notes: Map displays variation in voucher take-up rates across census tracts in the Chicago area with voucher recipients, based on their baseline address. Higher rates of voucher use are shaded dark blue.

Figure 3.4: Dynamic Predictors of Voucher Take-Up



Notes: Estimates come from equation (2) in the text. Point estimates and 95% confidence intervals represent the change in the probability of successful lease-up associated with x_t being equal to one in the indicated quarter relative to offer. All regressions include controls for demographics, household composition, baseline neighborhood characteristics, MSA vacancy rate, year and season of offer arrival, distances to local amenities, and missing indicators. In addition, each regression includes pre-offer indicators for the characteristics *not* pictured; for example, the regressions in the top left panel controls for public assistance usage, household head's and children's arrest in the period prior to offer, in addition to controls for employment in each quarter relative to offer.

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APPENDIX A

Changes to AFDC in Illinois

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.[1] 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.[2]

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.[3] 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

[1] 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>

[2] 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.

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

concerning income disregards and child support enforcement, most elements of the waiver took effect October 1995.

A.1 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.[4] 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.

A.2 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 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.

[4] 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.

A.3 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.

A.4 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.[5]

A.5 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

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

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.

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APPENDIX B

Additional Results From Chapter I

This appendix contains additional results referenced in Chapter I.

Table B.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.

Table B.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.

Table B.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 [-0.007, 0.025]	0.001 [-0.011, 0.035]	0.085 [0.017, 0.070]
N (Indiv.)	4,298	9,366	916
		Females only	
Mean	0.541	0.553	0.511
Estimate	-0.030 [-0.048, -0.017]	-0.034 [-0.057, -0.027]	-0.009 [-0.029, 0.025]
N (Indiv.)	1,325	3,004	976
		Males only	
Mean	0.348	0.355	0.206
Estimate	0.062 [0.035, 0.086]	0.057 [0.044, 0.086]	0.205 [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.

Table B.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

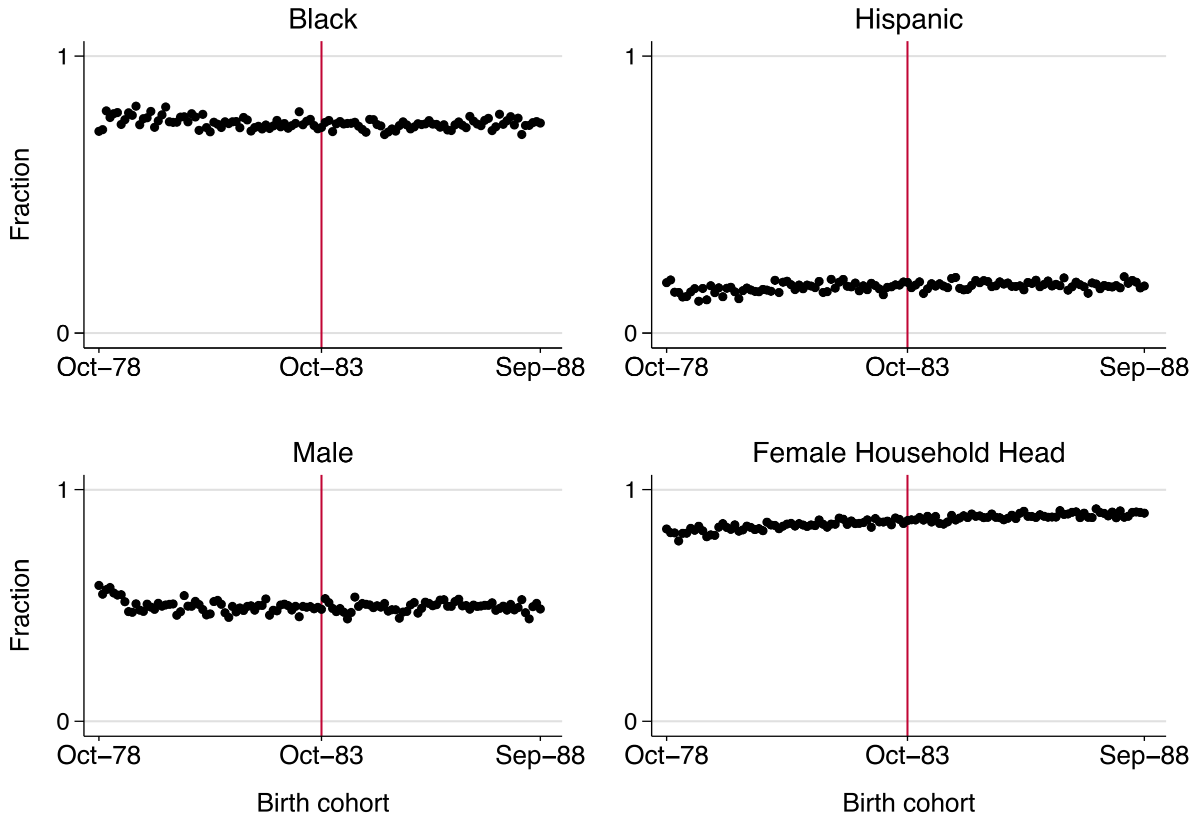
Notes: Table displays regression discontinuity estimates of the effect of Medicaid eligibility on average absences per year in high school from local linear regressions estimated with alternative bandwidth selection procedures on children in the fourth quartile only. Column 1 uses the procedure proposed 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.

Table B.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

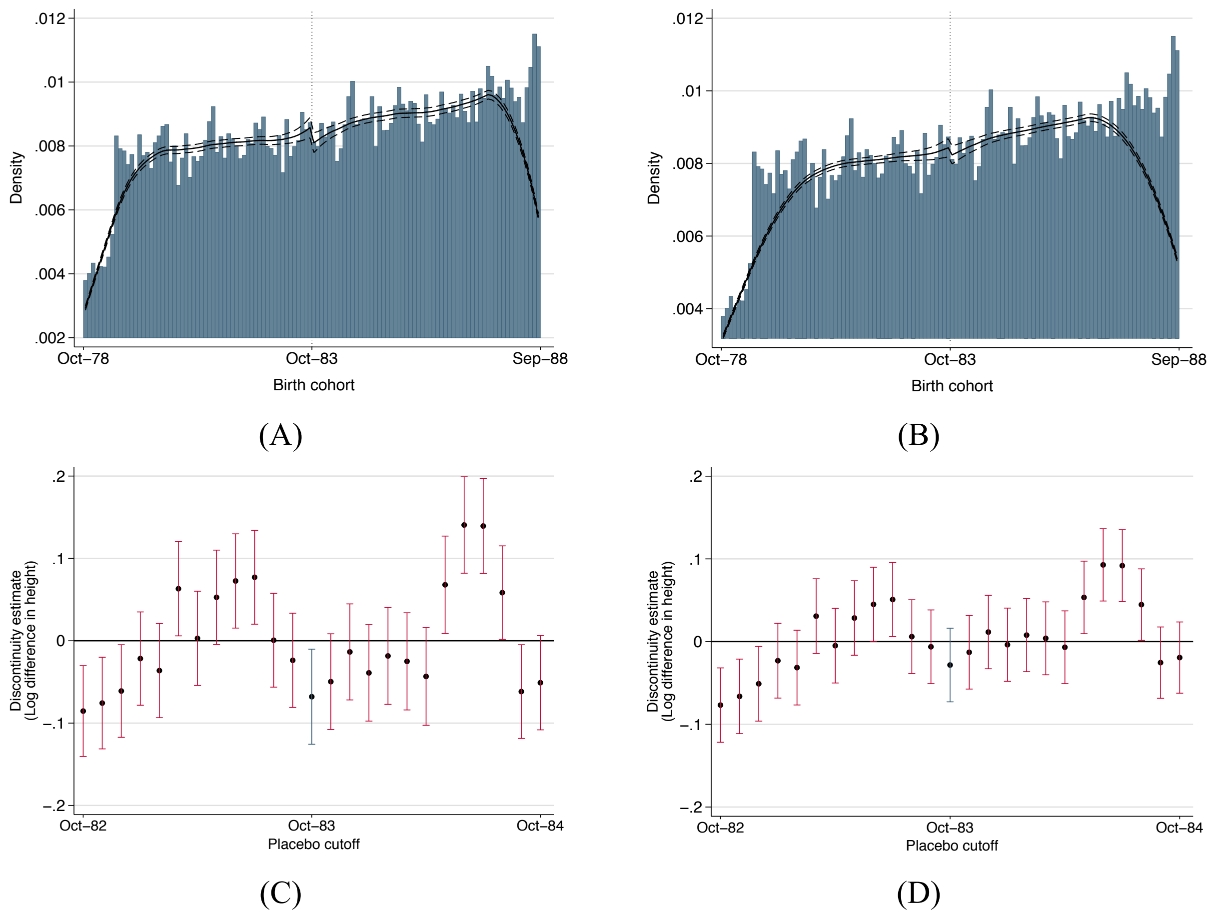
Notes: Table displays regression discontinuity estimates of the effect of Medicaid eligibility on the likelihood of repeating a grade from local linear regressions estimated with alternative bandwidth selection procedures on children in the fourth quartile only. Column 1 uses the procedure proposed by 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.

Figure B.1: Sample Members' Baseline Characteristics by Birth Cohort



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

Figure B.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.

APPENDIX C

Program Rules For Housing Vouchers, TANF, and Food Stamps

This appendix discusses the program rules for housing vouchers, the main means-tested housing program that we study here. We also discuss the rules for two other programs in which a sizable share of families in our study sample were participating at baseline, namely the cash welfare program (Temporary Assistance to Needy Families, or TANF) and what was called the Food Stamp program during most of our study period (now called the Supplemental Nutrition Assistance Program, or SNAP). We discuss these other programs to help readers understand the degree to which benefit and eligibility levels for the three programs do or do not interact, which is relevant for understanding the degree to which participation in these other programs might moderate the effects of housing vouchers on parent labor supply or total family resources.

C.1 Housing Vouchers

Throughout the paper we use the term "housing voucher" as shorthand for tenant-based rental subsidies. These programs have changed somewhat over the course of our study period. At the time of the wait-list lottery that we study here, tenant-based subsidies came in the form of either *Section 8 housing vouchers* or *Section 8 housing certificates*, which differed slightly along some dimensions such as whether families were able to lease a unit with rent that is above the usual program limit, the fair market rent (FMR), by increasing their own out-of-pocket contribution towards rent. Since the wait-list lottery was conducted, the federal government has consolidated these two programs into a single program, *Housing Choice Vouchers (HCV).* In what follows we focus on the core, shared elements of these programs that are central to our study, and note important differences across the program variants when they are relevant.

Housing vouchers subsidize low-income families to live in private-market housing.[1] Eligibility limits for housing programs are a function of family size and income, and have been changing over time. Since 1975 an increasing share of housing assistance has been devoted to what HUD terms "very low-income households," with incomes for a family of four that would be not more than 50 percent of the local median. (The federal poverty line is usually around 30 percent of the local median). Some families with incomes up to 80 percent of the local median income may be eligible, including those who are in Section 8 project-based units when the private-market landlord opts out of the government program, as well as those who are displaced as a result of HUD's Hope VI public housing demolition program. In 1998, the year after the housing voucher lottery we study was conducted), HUD began to prioritize for assistance "extremely" low-income households, who make less than 30 percent of local median income.

The eligibility limits for families of different sizes equal the following percentages of the four-person limit (taken from Olsen 2003, p. 379).

[1] This discussion is based on the excellent, detailed and highly readable summary in Olsen (2003).

Table C.1: Housing Voucher Eligibility by Family Size (Relative to Four-Person Limit)

Family Size	Percentage Adjustment
1	70%
2	80%
3	90%
4	100%
5	108%
6	116%
7	124%
8	132%

The maximum subsidy available to families is governed by the "payment standard," which for the old Section 8 housing certificate program (which was still in operation at the time what we are calling the CHAC "voucher lottery" occurred) was equal to the Fair Market Rent (FMR). The FMR was equal to the 45th percentile of the local rent distribution for a unit of a given size up through 1995. It was then lowered to the 40th percentile in 1995, and beginning in 2001 specific metropolitan areas, including Cook County, Illinois (in which Chicago is located), were allowed to set the FMR equal to the 50th percentile. Since 2012 the FMR for Cook County has been set at the 40th percentile again. For example the FMR for a two-bedroom apartment in Chicago (in nominal terms, not adjusted for inflation) equaled $699 in 1994, $732 in 1997, and $762 in 2000.

In Chicago, the FMR has the same value throughout the entire Metropolitan Statistical Area (MSA) – that is, over the course of our study period there are no adjustments for neighborhood-by-neighborhood variation in cost of living. Since we expect both housing-unit quality and neighborhood quality to be capitalized into rents, families with housing vouchers who try to lease units with rent as close to the FMR as possible will face a tradeoff between "spending" the subsidy on higher unit quality versus higher neighborhood "quality."

For the old Section 8 voucher program that was in operation at the time of the CHAC lottery, the payment standard could not exceed the FMR, but housing agencies had the option of setting the payment standard below the FMR. The new housing voucher program that was phased in towards the end of our study period enabled families to lease units with rents above the FMR, but the payment standard was capped at the FMR, and housing consumption is capped by limiting the family's contribution towards rent to be no more than 40 percent of adjusted income (see Olsen, 2003, pp. 376-86, 401-4 for details). The FMR also varies according to the number of bedrooms to which a family is entitled as a function of the family's size and gender composition (for example, male and female children are not asked to share a bedroom). In our calculations we use publicly available HUD data on FMR by housing unit size.[2]

For simplicity, we describe just the rule for the Section 8 voucher program for a jurisdiction that sets the payment standard equal to FMR. Families receiving a voucher have a maximum subsidy value equal to:

Maximum Subsidy = [FMR – S]

S = Family's monthly rent payment

$= \max\{.3\times Y_{ah}, .1\times Y_{gh}\}$

Y_{ah} = Adjusted Income under housing program rules

= Earnings + TANF – (\$480×Children) – (\$400×Disabled) – Child Care Expenses – [Unreimbursed Medical Care Expenses Over 3% of Annual Income] – [Unreimbursed Attendant Care or Auxiliary Apparatus Expenses to Disabled Family Members That Support Work by Other Family Members, Over 3% of Annual Income]

[2] See http://www.huduser.org/portal/datasets/fmr.html.

Y_{gh} = Gross household income

= Earnings + TANF

That is, families receiving vouchers are required to pay 30 percent of their adjusted income toward rent. Adjusted income is calculated by subtracting from a family's (reported) gross income deductions of $480 per child, $400 per disabled member of the household, child care expenses, and medical care expenses over 3% of annual income. TANF assistance is counted toward the calculation of gross income, but EITC benefits and the value of Food Stamps, Medicaid and other in-kind benefits (and income by household members under 18, or payments received for the care of foster children) are not counted. The voucher covers the difference between the family's rent contribution and the lesser of the FMR or the unit rent.

Note also that families offered housing vouchers have a limited time to lease up a unit from when they are offered the voucher (usually 3 to 6 months; while they may request an extension, there is still ultimately a finite search period). Families can also only use vouchers in private-market units that meet HUD's minimum quality standards. Landlords may prefer tenants paying with cash to those with vouchers because of these quality standards and other HUD paperwork involved with the program. The combination of these three factors helps explain why many families who are already living in private-market housing fail to use a voucher when it is offered to them – they fail within the specified time period to successfully find and lease up a new unit that has a landlord willing to rent to them and meets the quality standard.

Another important feature of means-tested housing assistance is that it is not an entitlement. Currently only around one-quarter of renters who are income eligible for federal government means-tested housing programs actually participate (Rice and Sard 2009; see also Olsen 2003). In Chicago, as in other big cities, there are generally extremely long waiting lists to receive housing assistance, especially for vouchers.

Unlike other social programs, once an individual qualifies for a housing voucher, the person is not removed from the program if his or her income exceeds the eligibility limit. However, since voucher recipients are required to pay 30 percent of their income toward their rent, the actual amount of their subsidy will decrease. Essentially, this means that there is no "notch" in the budget constraint with housing vouchers – there is simply a smooth phase-out. Since the average earnings of families in our CHAC applicant sample is so far below the phase-out range, most families probably expect to receive some sort of subsidy for a very extended period of time (perhaps permanently) if they are offered a housing voucher.

Starting in 1987, the government made these tenant-based subsidies "portable," meaning that families could use them to live in a municipality different from the one that issued them the subsidy. That is, a family living in Chicago who is offered a voucher by CHAC as part of the 1997 could if they wish use the voucher to move outside of Chicago to Hawaii (or anywhere).

C.2 Temporary Assistance for Needy Families (TANF)

The TANF program in Illinois replaced its cash-welfare predecessor (Aid to Families with Dependent Children, or AFDC) on July 1, 1997, at almost exactly the same time as the CHAC housing voucher program. Thus all of the post-lottery data analyzed in this paper were generated in a social policy environment governed by TANF rules.

TANF provides cash assistance to: (1) families with children but without any employed members, and with assets low enough to be eligible; (2) families with children and at least one employed member, but with incomes and assets low enough to be eligible; and (3) children whose parents have incomes and assets low enough to be eligible for TANF, but are not because they are not U.S. citizens or eligible non-citizens, or receive some other form of cash assistance such as SSI or SSA disability. Asset limits under the TANF program are equal to $2,000 for one-person TANF filing units, $3,000 for 2-person filing units, and increase by $50 for each additional person in the filing unit. The TANF benefit per month is essentially equal to:

TANF benefit = P - $.3 \times Y_{at}$

Y_{at} = Adjusted income under TANF program rules

= Earnings – Workers Deduction ($90) – Child Care[3]

Note that the maximum payment, P, varies by family size, type of TANF case, and year. [4] The dollar values for the income disregards and deductions did not change from 1997-2010. In July 2010 the first income disregard in the formula above (the worker deduction per person whose income is non-exempt) now varied, equal to the difference between 50 percent of the current Federal Poverty Level for the applicant's family size and their TANF payment level. In addition, in July 2010 the "tax rate" on adjusted income declined from 0.3 to 0.25.

Under the TANF program in Illinois, income in these formulas *does not* include benefits from housing vouchers, nor does it include benefits from Food Stamps, the EITC, or government programs such as VISTA or the Job Corps, nor does it include earnings through college work-

[3] The childcare deduction is $175 maximum per child for children over 12 where the care is not because of a physical or psychological condition or court-ordered supervision, and $0 for children under 12. See IDHS Program Manual, 08-01-02-d, http://www.dhs.state.il.us/page.aspx?item=15234

[4] The data for P come from this website prior to 2003: http://www.dhs.state.il.us/page.aspx?item=19811. After 2003, we obtain benefit levels from the 2004 Green Book (http://www.gpoaccess.gov/wmprints/green/2004.html) for households of size 1-6. We don't have data on larger households, and so we apply the same increment increases for larger households as existed pre-2003.

study or those earned by dependent children. If families reduce their work without prior permission from the Illinois Department of Human Services or they failed to report their earnings (and then those earnings are discovered), they are taxed at a 100 percent rate.

C.3 Food Stamps (FS)

The formula that determines Food Stamp (FS) benefits interacts with whether families receive TANF or not, so the marginal tax rate on earnings that families face depends on whether families are on one or both or neither, but the FS formula *does not* interact with participation in the housing voucher program. Specifically, Food Stamp benefits are set as:

Y_g = Gross Income

= Earnings + TANF

Y_n = Net Income

= Y_g – Standard Deduction– .2×Earnings – Child Care[5] - min{\$250, R}

R = Rent – .5×[Y_g – Standard Deduction– .2×Earnings – Child Care]

Food Stamp benefit = max{P-.3*Y_n, Minimum Allotment}

Note that gross income under the Food Stamp program includes the household's total cash income, including earnings and TANF benefits, minus some excluded sources (such as

[5] Currie (2003, p. 207) reports that the dependent care expenses for those in work activities or training equal up to \$175 per month per child (or \$200 for children under age 2).

earnings from dependent children and payments from the EITC). Also, note that P, the standard deduction (in some years), and the minimum allotment vary by family size. [6]

[6] The time-varying data for P and the standard deduction come from the following websites: http://www.dhs.state.il.us/page.aspx?item=21871#a_toc3 and http://www.dhs.state.il.us/page.aspx?item=21863.

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APPENDIX D

Previous Research on Housing Vouchers, Income, and Education Programs

This appendix briefly reviews the existing research literatures on the effects of housing programs and family income supports. As a way to help gauge the size of the effects on children's outcomes per dollar spent, we also provide a brief review of some of some of the more successful school-based educational interventions that have been studied. We show that there is considerable overlap in the implied effects per dollar spent across the latter two literatures, with several particularly prominent studies of family income changes suggesting effects that are at least as large as what most education studies find. If these family-income studies are correct, the implication would be that cash transfers are among the most effective ways to help improve outcomes of poor children – or, put differently, that there is no tradeoff between the social policy goal of alleviating the short-term material needs of poor families versus the goal of increasing the long-term human capital and life outcomes of children. That helps motivate the comparison we make in the main paper for the magnitudes of extra resources on child outcomes in our data versus previous cash-transfer studies, and our search for explanations for why the estimated magnitudes implied by our data are much smaller.

D.1 Effects of Housing Programs on Child Outcomes

A large observational literature has examined the relationship between children's outcomes and participation in subsidized housing programs, as well as with different measures of housing consumption directly, such as physical housing quality, crowding, residential mobility, homeownership, and affordability (Leventhal and Newman 2010). The literature tends to find mixed associations between subsidized housing-program participation and child outcomes. Studies also find that higher rates of residential mobility are correlated with adverse educational and behavioral outcomes for children, while certain measures of adverse housing quality (like toxins) or crowding are correlated with adverse health outcomes for children.

One particularly good observational study in this literature is by Currie and Yelowitz (2000), who find no effect of public housing occupancy on grade repetition for whites but find a 19 percent reduction for blacks. Their study controls for the endogeneity of participation in the public housing program by exploiting the fact that the families that are eligible for larger rental units under public housing rules (because of the gender composition of children in the home) are more likely to participate in the program. That is, both the public housing and housing voucher program require children of the same gender to share a bedroom but not children of different genders, so that a family with (say) one boy and one girl will be eligible for a larger apartment than a family with two boys or two girls. We note that their study examines a different means-tested housing program than the one examined in our own study (public housing, not housing vouchers). Their findings are nonetheless broadly relevant to our question, given that data from the HUD Moving to Opportunity (MTO) experiment finds little overall difference in average achievement test scores for children in families offered housing vouchers versus public housing.[1]

[1] As a reminder, the question examined in the present paper – of transferring additional resources to families in the form of housing vouchers (expanding the scope of the voucher program) – is different from the one examined by the

The one previous randomized experimental study that addresses the same question examined here is the evaluation of the HUD-sponsored Welfare-to-Work (WtW) voucher study by Mills et al. (2006). Their paper examines the effects of housing vouchers on families living in unsubsidized housing and for whom the voucher is deemed to be important in helping them secure employment, a somewhat unusual study sample. About five years after baseline the evaluation found no statistically significant effects on children's behavior problems, delinquency, or risky behavior. Their study finds mixed effects on school outcomes – voucher children were less likely than controls to miss school because of health, financial, or disciplinary problems, but more likely to repeat a grade.

However, the inferences that can be drawn from this study are somewhat limited by the modest sample size (2,481 parent surveys), so that many of the null findings reported in that study are fairly imprecisely estimated. For example the 95% confidence intervals do not allow them to reject effects of treatment on the treated (TOT) from voucher utilization any smaller than about 8% of a standard deviation (SD) for highest grade completed in school, about 25% of a SD for whether the child has ever been suspended or expelled in school, and about 30% of a SD for the widely-used behavior problems index (Mills et al. 2006, Exhibits 6.3 and 6.4).

Another limitation of their study is that children's outcome measures come from parent survey reports, which in other applications seem to be subject to substantial amounts of measurement error – or at least substantial amounts of disagreement with what the children

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HUD-sponsored Moving to Opportunity (MTO) experiment. MTO provided housing vouchers to families living in public housing, and so provides information about the effects of changing the mix of existing housing subsidies from project-based to voucher-based (or "tenant-based") subsidies. In the MTO study voucher receipt has no detectable effect on achievement test scores for children overall but leads to improvements in other behaviors for female youth, and on balance detrimental impacts for male youth. Results from the interim (4-7 year) MTO follow up are in Kling, Ludwig, and Katz (2005), Sanbonmatsu et al. (2006), and Kling, Liebman, and Katz (2007). Results for children in the long-term (10-15 year) MTO follow-up are in Sanbonmatsu et al. (2011), while long-term MTO results for adults are presented in Ludwig et al. (2011, 2012). Lessons from the MTO studies are discussed in Ludwig (2012).

themselves report. For example, Duncan, Morris, and Rodrigues (2011) report that the correlation of parent reports on children's achievement with teacher reports is .37, while the correlation of teacher reports with student performance on achievement test scores ranges from .49 to .54. Theunissen et al. (1998) compare child and parent reports about the child's physical health, cognitive functioning, social functioning and emotions and find correlations of 0.44 to 0.61.

D.2 Effects of Family Income on Child Outcomes

In contrast to the very limited number of studies examining the effects of housing vouchers on children's outcomes, a vast literature has shown that family income is correlated with a wide range of important child outcomes (for example Haveman and Wolfe 1995; Brooks-Gunn and Duncan 1997). What remains unclear is the degree to which these correlations reflect causal relationships.

One of the first large-scale studies to use exogenous variation to examine the effects of cash transfers on children's outcomes is the multi-site Negative Income Tax (NIT) experiments that were carried out in the 1970s (for an excellent overview see Munnell 1986). Four NIT experiments were carried out: the first was in New Jersey and Pennsylvania from 1968-72 with 1,357 low-income couples in urban areas; a second rural NIT experiment in Iowa and North Carolina from 1969-73, with 809 low-income families in rural areas; the third was in Gary, Indiana from 1971-74 with 1,780 low-income African-American households, a majority of which were headed by single women; and the experiment with the most generous benefits, in Seattle and Denver experiments (SIME/DIME) from 1971-82 with 4,800 families. The first three experiments provided families with income supplements for three years, while SIME/DIME provided families with benefits for up to five years. On balance assignment to the treatment condition in these experiments reduced labor force participation rates, by about 17 percent for women and 7 percent for men (Munnell 1986). In that sense both the design of the work incentive in the NIT and the net effects on labor supply are similar to housing vouchers.

In the three sites that measured years of schooling completed by youth (New Jersey, Seattle and Denver) there was some beneficial effect of the NIT (Hanushek 1986), while all of the other outcomes examined for various age groups of children were much more mixed. Among the sites that measured academic outcomes of youth, there were no statistically significant effects in Iowa, North Carolina, Seattle, or Denver; in the Gary site there were if anything *negative* effects of the NIT treatment on the GPAs of 9th and 10th graders and an increase in school absences of 9th graders (Maynard 1977; Maynard and Murnane 1979; SRI International 1983). The rural NIT in Iowa and North Carolina also collected data on school aspirations, delinquency and mental health (psychological well-being), which showed no statistically significant treatment-control differences (HEW 1976). Five sites collected data on academic achievement outcomes for elementary school-age children; two of these sites (Gary and North Carolina) found statistically significant beneficial effects, while three did not (Iowa, Seattle, Denver) (Maynard 1977; SRI International 1983). It should be said that the number of children for whom schooling or other outcome data is available in these different NIT experiments is usually small, and so the proper interpretation of this mixed pattern of results is somewhat unclear.

Given the unclear picture from the NIT studies, Mayer (1997) conducted a variety of empirical tests that improve upon correlational evidence and found much smaller effects of family income on children's outcomes compared to what is reported in previous correlational studies. For instance, she shows that trends in family income over time across different parts of the income distribution are not mirrored by differential changes in children's outcomes. And the gap in outcomes for children living in single parent versus two-parent households does not appear to be much different in states with generous versus less-generous AFDC benefits, which would affect the gap in income between one- and two-parent households. Mayer also shows that when low-income parents receive additional income, they tend to spend it largely on housing, transportation, and food consumed at home, which are not strongly correlated with child outcomes. The investments most highly correlated with children's outcomes – such as books in the home, or trips to museums – depend more on parent time and interest than on money.

Several studies account for unmeasured family attributes associated with both income and children's outcomes (i.e., family fixed effects) by comparing test scores across siblings, or by taking advantage of variation over time in family income. These studies generally report stronger effects of income on children's outcomes than those reported in Mayer's work (Duncan et al. 1998; Levy and Duncan 2000; Blau 1999). While these studies control for bias from unmeasured features of the shared family environment, this design is still vulnerable to bias from unmeasured family-level characteristics that change over time, or unmeasured characteristics that differ among children within the same family.

A number of recent well-done and influential studies have also found large effects of family income on children's outcomes, and are summarized in Table D.1. We report the effect on schooling outcomes per $1,000 in constant 2013 dollars so that impacts per dollar spent can be compared across studies and to those findings reported in the present paper.[2]

[2] There are also a number of other excellent papers in this literature that are not included in our summary table for different reasons. For example Oreopolous, Page, and Stevens (2008) use data from Canada and focus on the long-term life outcomes of children whose fathers did versus did not experience job displacement. They show that their treatment and comparison groups have very similar earnings trajectories during the period prior to job displacement for the treatment group, but following displacement the incomes of the treatment group families are 13 percent below those of the control group, and even 8 years later family income is around 15 percent lower than what it would have been otherwise. Children in these families that experience job displacement wind up with adult earnings

The first row of Table D.1 summarizes the research design and findings of Dahl and Lochner (2012) [hereafter D&L], who exploit the fact that families with some exogenous characteristics (defined by mother's age, race, educational attainment and her own achievement test scores) experienced relatively larger changes in family income over the 1990s than did other families due to changes in the EITC schedule. Presumably most families think of the EITC expansions as a change in permanent income. Their estimates are reported in terms of effects per extra $1,000 in family income in 2000 dollars, and suggest gains in test scores of 0.061 standard deviations overall (standard error=0.023) (D&L Table 3), 0.080 SD for blacks (SE=0.030) and 0.088 SD for males (SE=0.045) (D&L Table 6). Given that $1 in 2000 is the equivalent of $1.35 in 2013, the effect of $1,000 in 2013 dollars implied by their study equals 0.045 (SE=0.017) for the full sample, 0.059 (SE=0.022) for blacks, and 0.065 (SE=0.033) for males.

Because transfer programs change the incentives for work through standard income and substitution effects, with details (and hence incentives) that vary across transfer programs, it is worth understanding the degree to which changes in labor supply influence effects on child outcomes. Changes in parental work could in principle be a mediator (mechanism) through which transfer programs change child outcomes, since parent time with children may be a positive input into child development relative to alternatives like unsupervised time or time spent in informal child-care arrangements. Changes in parental work could also be a moderator for income effects on child outcomes (that is, interact with family income) by changing the way that

levels that are about 9 percent below those of their comparison group counterparts. We do not include that in our review in Table D.1 because the outcome they examine (earnings during adulthood) is different from what we examine here, and so we cannot make a direct comparison of the magnitude of the effects across studies. Shea (2000) uses father's union status and industry as instruments for family income and finds little effect on children's outcomes, although whether these instruments meet the exclusion restriction for valid IV estimation is unclear. Løken (2010) uses data from the Norwegian oil boom of the early 1970s and finds little effect of family income on schooling attainment of children, by comparing differences across pre- and post-boom birth cohorts in areas where oil was versus was not discovered. Her research design may be susceptible to bias from endogenous in-migration of families into places where oil was discovered.

families spend their money (see for example the model presented in Appendix E below). For example, families that receive income within the context of programs that incent them to work more may spend relatively more of the additional cash on work-related expenses such as transportation, work clothes, or child care compared to families that receive extra income through programs that do not push them to work more. D&L note that studies typically find that the EITC has generally modest effects on labor force participation and hours-worked decisions, with small negative effects on hours among women who already work and some positive effect on labor force participation by single women. They find in their analysis that controlling for parental labor supply does not change the estimated effect of income on child outcomes very much – that is, parent labor supply does not seem to be an important mediator behind their effect, although they do not explicitly test for moderation. Their analysis also suggests that current (rather than lagged) income seems to matter most for child outcomes.

Milligan and Stabile (2011) use variation in child tax benefits across Canadian provinces over time and by number of children. Variation in benefit generosity by family size enables them to condition on province-by-year fixed effects in their analyses, which allows them to control for some of the more obvious sources of potential confounding in a difference-in-differences type design. Their identifying assumption is that changes in benefit levels for families of different sizes within a province are unrelated to whatever else might be going on that differentially affects larger versus smaller families. Their study does not seem to focus much on the role of changes in parental labor supply as a mediator or moderator for effects on child outcomes.

Their estimates focus on children age 10 and under and suggest that a $1,000 increase in family income (measured in 2004 Canadian dollars, p. 187) has little effect on children's educational outcomes for the full sample, other than to *increase* rates of grade repetition by 2.7

percentage points (SE=0.3 percentage points). Among disadvantaged families (in which parents have a high school education or less), an extra $1,000 in benefits increases math scores by 0.069 SD (SE=0.015). Among boys, the effect is even larger, equal to 0.231 SD in math (SE=0.058) and 0.365 SD on the PPVT (SE=0.151). Given that $1 CAD in 2004 is the equivalent of $1.19 CAD in 2013, and that $1 USD was worth approximately $1.1 CAD throughout 2004-2013, the effect per $1,000 in 2013 US dollars on math scores implied by their study equals 0.053 (SE=0.012) for the full sample and 0.177 (SE=0.044) for boys, and 0.279 (SE=0.116) for boys' scores on the PPVT.

Akee et al. (2010) use variation in family income generated by the sharing of revenue among members of an Indian tribe from opening a new casino. The amount of revenue sharing that occurred was quite substantial, equal to $4,000 per adult tribe member (in 1998 dollars),[3] which families presumably expected to be permanent. Their research design uses a standard difference-in-differences approach, comparing trends over time in child outcomes for members of the relevant Indian tribe with those of families that were not eligible for transfers. They show that these transfers have almost no detectable effect on parent labor supply. They find that each $1,000 in additional transfer income increased high school graduation rates by from 7.45 percentage points (SE=3.5 points) to 9.78 percentage points (SE=3.38 points), depending on which birth cohort they examine in their study sample (with the slightly larger effects showing up for children who were two years younger at baseline relative to the other cohort).[4] The effect per $1,000 in 2013 dollars on high school graduation equals 5.2 to 6.9 percentage points (SE of 2.46 to 2.37 points). It is worth noting that these are effects on receiving an actual high school

[3] Base year for dollars comes from personal communication of Jens Ludwig with Randall Akee, July 8, 2013.
[4] These estimates come from dividing the point estimates and standard errors in column 2 of Table 5 of their paper (which show the effect per $4,000 transferred per adult) by 4.

diploma, which is important because most of the educational interventions that have been found to work (discussed below) mix together receipt of a high school diploma with GED receipt.

Akee et al. also find mixed effects on criminal activity due to higher family income, with increased arrests to “treated” children who were relatively younger at baseline but reduced arrests to those two years older at baseline (Akee et al., Table 8).

Aside from the NIT studies of the 1970s discussed above, to the best of our knowledge there is only one previous study that uses data from randomized experiments to examine this question. Duncan, Morris, and Rodrigues (2011) pool data from several randomized welfare-to-work experiments and compare the impacts of programs that increase income and maternal work together with those of programs that just increase maternal work. Child outcomes were measured up to five years from baseline. Their estimates suggest that each $1,000 in additional income in this context (in 2001 dollars) increases test scores (average of math, vocabulary and reading) for young children (2-5 years old) by 0.052 SD (SE=0.017), very similar in magnitude to D&L’s estimates. Their estimates imply an effect in terms of each additional $1,000 in 2013 dollars equal to 0.04 SD (SE=0.013). However as reported in the earlier working draft of their paper (Morris, Duncan, and Rodrigues 2004), income changes have few detectable effects on the outcomes of children who are 6-9 years old, and may have if anything *deleterious* impacts on children 10-15 years of age.

Much of the beneficial impact of family income on the young children in these welfare-to-work experiments seems to come from parents spending extra income on center-based care. Using data from the same set of welfare-to-work experiments examined by Morris and colleagues, Gennetian et al. (2007) show that the IV estimate for the effect of family income on children’s outcomes is reduced by 75 percent after controlling for use of center-based child care and is no longer statistically significant. This finding helps explain why the benefits of increased family income are concentrated among pre-school age children, who would be the ones to benefit from utilization of center-based care services.

Whether the findings of Duncan et al. for preschool-age children should generalize to cases where the income transfers are *not* associated with increased maternal work is not clear, since Mayer (1997) suggests that in general families spend their extra income on things like better housing or eating out, which seem to be less developmentally productive than center-based child care (Blau and Currie 2006). Unfortunately our hypothesis about the interactive effects of increased family income and increased maternal labor supply cannot be directly tested by the data from Morris et al., since all of the welfare-to-work programs increase maternal work. Appendix E below devotes some additional discussion within the context of a simple model about how the design of a transfer program and its effects on parent labor supply may moderate the effects of income on child outcomes.

D.3 Promising Educational Interventions

Table D.2 reviews studies of the effects of several influential or commonly proposed educational interventions on child outcomes. The first row presents the results of Head Start, from the recent randomized experimental study of that program, the National Head Start Impact Study (NHSIS). Note that the technical report prepared for the federal government by Westat focused on presenting intention-to-treat (ITT) effects, or the effects of offering children the chance to participate in Head Start, which will differ from the effects of actually participating in Head Start because not all children assigned to the treatment group and offered Head Start participated, while some children randomized to the control group in the experiment wound up getting into a Head Start program on their own. To facilitate comparison to other studies and measure impacts on a per-thousand-dollars-spent basis, we draw on estimates for the effects on Head Start participants (the local average treatment effect) from Ludwig and Phillips (2008; see also Gibbs, Ludwig, and Miller 2013). The short-term effects on reading and math achievement test scores per $1,000 spent (in 2013 dollars) is on the order of about 0.016 standard deviations (standard error of about 0.01 SD) – much smaller than the test score gains that would come from an extra $1,000 family income as implied by the studies in Table D.1.[5]

Another influential and widely cited educational intervention is class size reduction. The Tennessee STAR experiment showed that reducing class sizes from 22 to 15 in early elementary school improved test scores at the end of 3rd grade by 0.152 SD overall (SE=0.030; see grade 3 result from Table 4 of Schanzenbach 2007), and by 0.242 SD for black students (SE=0.060), at a cost of about $4,400 per year in 2000 dollars. The average student assigned to the "treatment" group is in a smaller classroom for about 2.3 years, so that the average cost per student of this intervention is $10,120 in 2000 dollars. The effect on achievement test scores per $1,000 of 2013 spending then is equal to 0.011 SD for the full sample (SE=0.002) and equal to 0.018 SD for blacks (SE=0.004).

A third educational intervention, Success for All (SFA), is a comprehensive, school-wide reform that has been implemented in over 1,200 Title I schools in the US (Borman et al. 2007). A recent study of the implementation of SFA in Baltimore public schools found that students in schools adopting the reform experienced improvements of 0.29 SD and 0.11 SD on reading and math test scores (SE=0.053 and 0.052, respectively), at a cost per pupil of $3,054 in 2000 dollars

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[5] This is the median point estimate and standard error for the TOT effect across test subjects for 4 year olds in the NHSIS, reported in Ludwig and Phillips (2008) Table 1.

(Borman and Hewes 2002). The implied effect per $1,000 in 2013 dollars on reading and math scores is equal to 0.07 SD (SE=0.013) and 0.027 SD (SE=0.013), respectively.

Put differently, the effects on children's test scores per $1,000 spent as reported in D&L, Duncan et al., and Milligan and Stabile tend to be at least as large, and in some cases substantially larger, than what we find from influential educational interventions like Head Start, class-size reduction or Success for All.[6]

Some readers might wonder about the issue of fade-out and how that compares between studies of educational interventions versus of income transfers. One reading of the literature suggests that fade-out is not all that different for educational interventions versus cash transfers. For example in the Tennessee STAR class size reduction study, by fourth grade (one year after the class size reduction ended) the gains for students were one-third to one-half as large. By comparison, D&L imply that one year after a similarly-costly cash transfer to children, effects would also be about one-third to one-half as large as the contemporaneous effects of the cash transfer – plus poor families would also have benefited directly from increased consumption. For both types of interventions – cash transfers and educational programs – fade out of test score impacts does not mean that there are no long-term benefits to participants. In fact the results presented in Chetty et al. (2011) suggest that even very rapid fade-out of initial test-score impacts is not inconsistent with long-term effects on earnings and other outcomes.

A different way to think about the long-term effects implied by previous research about providing poor children with educational interventions versus cash transfers is to look at impacts

[6] The only educational intervention we know of that clearly dominates the effects on children's test scores of just providing cash is the My Teaching Partner (MTP) professional development intervention for K-12 teachers, as reported by Allen et al. (2011). MTP is a program designed to aid the professional development of teachers and improve the quality of interaction with students. The program reported a cost of roughly $4,000 (2013 dollars) per *teacher;* the average gain in test scores per *student* was 0.220 SD (0.096). The study treatment included 76 teachers who were responsible for 1,267 students. When per teacher spending is amortized over all of the students, the per-student cost may be as little as $40 to $50. If this intervention could be replicated at scale the gains in test scores would be over 4 SD per $1,000 per student.

on high school graduation rates. As noted above, the study by Akee et al. suggests each extra $1,000 in cash transfers increases high school graduation likelihood by 5.2 to 6.9 percentage points. This estimated impact is at least as large or larger than any of the effective educational interventions reviewed by the cost-effectiveness analysis of Levin et al. (2012). The Levin study reviewed those interventions deemed proven or promising by the US Department of Education's What Works Clearinghouse (WWC) for graduation, with effects summarized in Table D.3. (Levin et al. only report point estimates, not standard errors, so that is what we include in our table.)[7] The most cost-effective intervention cited by Levin, Talent Search, yields an estimated increase in high school graduation rates per $1,000 that is only half as large as that estimated by Akee et al. from offering $1,000 in extra cash to families.

[7] Unfortunately most of these studies mix together effects on GED receipt and high school diplomas, even though previous research suggests that the effects of a GED on long-term life outcomes are not the same as the effects of an actual high school diploma. Moreover while a number of the WWC-endorsed interventions are evaluated using randomized experiments, several are studied using less reliable approaches such as propensity score matching.

Table D.1: Estimated Effect of Additional Family Income on Children's Outcomes

Study	Effect of $1,000 of Additional Family Income (2013 Dollars)		
Dahl and Lochner (2012)	**Overall Test Scores**	**0.045**	**(0.017)**
	Blacks	0.059	(0.022)
	Males	0.065	(0.033)
Milligan and Stabile (2011)	**Math Scores**	**0.053**	**(0.012)**
	Boys	0.177	(0.044)
	PPVT Scores	**0.114**	**(0.100)**
	Boys	0.279	(0.116)
Duncan, Morris, and Rodrigues (2011)	**Average Test Scores (2-5 Years Old)**		
	0.039	(0.013)	
Akee et al. (2010)	**Change in High School Graduation Probability (Percentage Points)**		
	5.24 to 6.87		
	(2.46) (2.37)		

Notes. All results are reported in terms of outcome per $1,000 (in 2013 dollars) received.

Table D.2: Estimated Effect of Additional Per Student Spending on Children's Outcomes

Study	Effect of $1,000 of Additional Per Student Spending (2013 Dollars)		
Ludwig and Phillips Head Start (2008)	**Reading and Math Test Scores**		
		0.016	(0.010)
Schanzenbach Tennessee STAR (2007)	**Overall Test Scores**	**0.011**	**(0.002)**
	Blacks	0.017	(0.004)
Borman and Hewes Success for All (2002)	**Reading Test Scores**	**0.070**	**(0.013)**
	Math Test Scores	**0.027**	**(0.013)**

Notes. All results are reported in terms of outcome per $1,000 (in 2013 dollars) received.

Table D.3: Estimated Effect on High School Graduation Rates from Educational Interventions Deemed Promising or Proven by What Works Clearinghouse (Levin et al. 2012)

Program	Effect of $1,000 Program Spending on High School Graduation Rate, percentage points (2013 Dollars)
Talent Search	3.26
National Guard Youth Challenge	1.40
JOBSTART	1.44
Chicago Child-Parent Centers	0.75
Perry Preschool	0.60
New Chance	0.51

Notes. All results are reported in terms of percentage point change in high school graduation rates per $1,000 (in 2013 dollars) received. Levin et al. (2012) do not report standard errors, so we just report point estimates.

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APPENDIX E

Conceptual Model of Transfer Effects on Labor Supply, Income, and Child Outcomes

Our paper is motivated primarily by the desire to understand the value for children of policy efforts to transfer additional resources to low-income families. Any transfer program may change the labor supply of household adults, which may have independent effects on child outcomes since parental time is itself an important input to children's development. The transfer programs studied in our paper and the previous literature all differ slightly in their design, and so in how they affect parental labor supply. This provides one candidate explanation for the different results across studies. Since our goal is to understand how differences in the design of transfer programs might lead to different effects on child outcomes, rather than to develop a complete structural model of how housing vouchers or other transfer programs affect children, we abstract from many of the details of the actual housing voucher program in what follows.

E.1 Baseline Model

To understand how the changes in material resources and parent time in our study relate to what has been examined in previous papers, we use a simple version of Becker's (1965) model of household production. Let parent utility be a function of two commodities, parent consumption (C_1) and child outcomes (C_2), which are produced with market goods (X_i) and parental time (H_i).[1] For simplicity we normalize the units of market goods so P_1=P_2=1.

(1) $U(C_1, C_2)$

(2) $C_i = f^i(H_i, X_i)\ for\ i = 1,2$

Parents seek to maximize utility by choosing how much time to allocate to work (L) subject to the constraints (where V equals non-earned income), and X_1, X_2, H_1 and H_2 are non-negative.

(3) $X_1 +\ X_2 \leq V + WL$

(4) $L +\ H_1 +\ H_2 \leq T$

This yields the Lagrangian:

(5) $\mathcal{L} = U(f^1(H_1, X_1), f^2(H_2, X_2)) - \lambda[\ H_1W{+}H_2W + X_1 + X_2 - V - TW]$

The first-order conditions then equal:

(6) $\frac{\partial \mathcal{L}}{\partial X_1} = \frac{\partial U}{\partial f^1}\frac{\partial f^1}{\partial X_1} - \lambda = 0$

(7) $\frac{\partial \mathcal{L}}{\partial X_2} = \frac{\partial U}{\partial f^2}\frac{\partial f^2}{\partial X_2} - \lambda = 0$

(8) $\frac{\partial \mathcal{L}}{\partial H_1} = \frac{\partial U}{\partial f^1}\frac{\partial f^1}{\partial H_1} - \lambda[W] = 0$

(9) $\frac{\partial \mathcal{L}}{\partial H_2} = \frac{\partial U}{\partial f^2}\frac{\partial f^2}{\partial H_2} - \lambda[\ W] = 0$

(10) $H_1W{+}H_2W + X_1 + X_2 - V - TW = 0$

[1] Our simple set-up ignores two other potential mechanisms. First, it is in principle possible that increased resources could help "buy" reduced parental stress. That is, low income may cause parents stress, contributing to deteriorated mental health outcomes and lower-quality parenting, so that increased resources could change the production function $f^2(X_2,H_2)$. However Mayer (1997) finds little evidence for any detectable effect of family income on parent mental health outcomes. An alternative "role model" theory argues that "because of their position at the bottom of the social hierarchy, low-income parents develop values, norms, and behaviors that cause them to be 'bad' role models for their children" (Mayer 1997, p. 7). This idea seems closely related to William Julius Wilson's argument that it is the income-generating activities themselves – work – that may be developmentally productive for children, since work may "provide a framework for daily behavior because it readily imposes discipline and regularity" (Wilson 1996, p. 21, 75). That is, work may help structure and organize family life, which may in turn be conductive to children's learning and socialization.

It is easy to see from (6) and (7) that:

$$(11)\quad \frac{\partial U}{\partial f^1}\frac{\partial f^1}{\partial X_1} = \frac{\partial U}{\partial f^2}\frac{\partial f^2}{\partial X_2} = \lambda$$

We can also re-arrange (8) and (9) to see that:

$$(12)\qquad \frac{\partial U}{\partial f^1}\frac{\partial f^1}{\partial H_1} = \frac{\partial U}{\partial f^2}\frac{\partial f^2}{\partial H_2} = \lambda W$$

It is also easy to see within this setup that families may vary in their initial investments in their children's human capital because they are differentially good at turning resources or parental time into child learning, or because they differ in how they value a unit change in their child's learning. Parents with higher wage rates will also rely relatively more intensively on market-purchased inputs to child development rather than parent time, all else equal.

This model also helps us think through the potential effects of providing families with a housing voucher. For simplicity, we abstract from most of the housing-voucher program details and initially simply think of a voucher as increasing unearned income, V. It is easy to see that if C_1 and C_2 are both normal goods, a household will increase consumption of X_1 and X_2 and reduce time at work in order to increase H_1 and H_2. This simple setup predicts that increased V should translate into improved child outcomes, although without imposing a great deal of additional structure on the problem the model has nothing to say about whether such gains should be large or small.

E.2 Extending the Model to Welfare-to-Work

Perhaps the most striking difference in results across studies is between our own findings and those from Duncan, Morris, and Rodrigues (2011). Their study pools together data from multiple randomized welfare-to-work experiments, some of which change maternal labor supply only, and some provide income supplements as well as maternal work requirements, and then use treatment assignment to a program that includes income supplements as an instrument for family income. Selection bias (differences across studies in internal validity) seems an unlikely way to reconcile their findings with ours given their design. It is possible that the difference across studies could be due to different study samples responding differently to cash transfers (external validity issues). But another explanation, which we explore further here, is the way that the work requirements in the experiments studied by Duncan, Morris, and Rodrigues may condition (moderate) the effects of cash transfers on child outcomes.

The welfare-to-work programs studied by Duncan and colleagues differs from our housing voucher application, in that the former provide families with additional income (V) but now also impose a work requirement, so that:

(13) $$L \geq L^{*}$$

Or equivalently:

(14) $$H_1 + H_2 \leq (T - L^{*}) = H^{*}$$

For the sub-set of families in the welfare-to-work experiments for whom the work requirement are binding, they will produce both C_1 and C_2 using more market goods and less parental time than they would absent the work requirement. *That is, work requirements change the way that parents deploy income*. The change will be most pronounced for those consumption goods that had previously been most time-intensive in their production. Work requirements will increase the beneficial effects of receiving additional income on child outcomes if the market goods that parents purchase are more developmentally productive for children compared to the developmental benefits of time with parents themselves.

This simple setup could help explain why Duncan, Morris, and Rodrigues (2011) find that additional income paired with work requirements produces larger outcomes for preschool-age children than what we find in our study of housing vouchers, and why in their earlier working paper (2005) they do not find similarly large gains in outcomes for school-age children. Parents (especially mothers) spend much more time with children under five (21 hours per week) than with children over five (just 9.4 hours per week, implied by Table 1 in Guryan, Hurst, and Kearney 2008). So the effect of the work requirement on how parents "produce" outcomes will be most pronounced for pre-school age children than for school-age children. And for preschool-age children, the market good that substitutes for parental time is childcare. A large body of research suggests that for mothers with low levels of educational attainment, time in center-based care, especially early childhood education, is on average more developmentally productive than is time with parents (see for example Currie 2001). Indeed most of the relationship between receiving additional income and improved outcomes for preschool age children in the welfare-to-work experiments studied by Duncan et al. is attenuated (explained away) by controlling for use of early childhood center care (Morris, Gennetian, and Duncan 2005).

In reality the housing voucher program rules are more complicated than simply providing families with additional unearned income (V). A more realistic incorporation of the voucher program rules into our simple model just strengthens the argument that parental labor supply moderates the effect of additional income on child outcomes. Specifically, a key aspect of the housing-voucher program is that families must contribute 30% of their incomes towards rent, which effectively reduces the net hourly wage families receive from working. Incorporating this feature into our model enhances the effect of voucher receipt on reducing parental labor supply in our application (see Jacob and Ludwig 2012), further strengthening the contrast to the Duncan et al. sample where additional income comes in the context of increased parental labor supply.

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APPENDIX F

Data Appendix

Baseline information on the 82,607 adults and nearly 8,700 spouses that applied to CHAC for a housing voucher in 1997 comes from the lottery application forms. These files include information on address, lottery number and household demographics such as the number and gender of other children and adults in the household, as well as identifying information (names, date of birth, and social security number) for the household heads and spouses. These data are then linked to information from the Illinois Department of Human Services (IDHS) Client Data Base (CDB) to learn the identity of others in the home, as well as to measure participation in social programs. The combined dataset is then merged to our other sources of longitudinal information from the Chicago Public Schools (CPS) student-level records and Illinois State Police (ISP) arrest records ("rap sheets"). We discuss these different data sources and merging procedures in this appendix.

Because the CHAC voucher application forms do not include identifying information on children in the home, we must use longitudinal administrative data on social-program spells to identify children in our study sample. We discuss those procedures below. We also discuss our procedures for imputing baseline rent and baseline total income for families, since these measures are not included on the CHAC application forms.

F.1 Rules for Cleaning and Processing Data

We impute certain demographic variables that are either incomplete or not included on the CHAC voucher application forms using information from the Illinois Department of Human Services (IDHS) Client Data Base (CDB). Note that we have non-missing data for virtually all observations, and that we only impute demographic data for a small fraction of our sample. Moreover, it is important to realize that the imputation we do generally involves prioritizing one data set over another.

Gender - Household head gender is not included on the CHAC application form, so we use gender from the CDB. For household heads who do not appear in the CDB, we impute gender by comparing their first name with lists of names of known gender using four data sources: Census data, Social Security Administration data, two websites with lists of names; and finally using a gender-assigning algorithm. For spouses with missing gender, we assign them the opposite gender of the household head. Children's gender comes from the CDB as well.

Race - We start with the CDB race variable and then impute missing values using the less complete lottery application information. For those observations that are missing, we check to see whether the "multiple races" box is checked on the CHAC application. To determine the coding of these multiple races, we create an empirical link by looking at those individuals with multiple races on the CHAC application forms *and* who also have race information in the CDB. For each combination of multiple races we choose the modal race that is indicated by the CDB. For example, if those who are listed as both white and Hispanic on the CHAC forms are listed most often as Hispanic in the CDB, then we assume that all people marked both white and Hispanic in the CHAC forms are Hispanic.

Age - We use information from both the CHAC application forms and the CDB. The age variables we create indicate age during 1997 when the CHAC lottery application takes place. For the household heads, if the CHAC age is missing but we have CDB age, then we use CDB age. If he or she indicates age less than 16 on the application form and we have no CDB information, then we set age equal to missing. If the CHAC age is less than 18 or greater than 70, and the difference between that age and the CDB age is greater than one, then we use the CDB age. For spouse age, we use date of birth information if available and when missing we use CDB age as long as it is a reasonable value (i.e., not less than 16). For children and other household members we first check for members age 0 to 18 that are a household head or spouse somewhere else in the sample (e.g., a 17 year who applied as a head and is also the child of a parent who applied separately as a head). For those that we find, we make sure their age is consistently reported across observations. There are a small number of observations that have age greater than 100; we set these to missing.

Household Size and Composition – See discussion below.

Voucher Utilization - Data on voucher utilization until the beginning of 2006 comes from HUD 50058 records, which families must complete at least once a year to verify eligibility and also when they exit or enter housing programs or when household composition or income changes. These HUD 50058 forms provide complete longitudinal information on housing assistance administered by CHAC (i.e., all tenant-based rental assistance such as Section 8 vouchers and certificates, but excluding public housing), including when the household started and stopped receiving assistance and the different addresses where the household lived while on a Section 8 voucher. We merge the application data to CHAC files on voucher utilization using CHAC tenant identification numbers coupled with name, social security number and date of birth. We use a probabilistic match that is robust to misspellings, typos and other minor inconsistencies across data sets. These files also provide information on the type of apartment leased, and the number of members in the household.

Residential Location - To track residential locations for both the treatment and control groups, we rely on passive tracking sources such as the National Change of Address (NCOA) registry and national credit bureau checks. Because of resource constraints, we tracked a random ten percent sub-sample of all CHAC applicants. We have confirmed that this subset matches the overall applicant pool on a variety of baseline characteristics, and that the impact estimates on labor supply for this *10 percent random sub-sample* are virtually identical to the impact estimates for the full sample. We are also able to (at least partially) verify the accuracy of the passive tracking techniques using the subset of families that received housing vouchers. In the vast majority of these cases, the location information obtained through passive tracking matches the information found in the administrative 50058 records.

Because of the limitations on this residential tracking data, we focus most of our analyses on addresses for CHAC families measured at two points in time: 2005 and 2012. Using these addresses along with data from the census and other sources, we can characterize each household's residential neighborhood down to the tract level and, in the case of our crime measures, to the police beat level.

As a sensitivity check, we also take advantage of the fact that we have address information in the IDHS data system for families participating in social programs. We generally prefer the address data for our 10 percent random sub-sample; even though the number of observations is smaller compared to the IDHS address records, the sample is representative of our entire analysis sample rather than just those who are participating in social programs.

<u>Neighborhood Characteristics</u> – Census tract characteristics come from the 1990, 2000 and 2010 decennial censuses. Values for tract characteristics during the inter-censal years are imputed. Tract level social capital and collective efficacy scores come from the 1995 Project on Human Development in Chicago Neighborhoods (PHCDN) Community Survey. Although PHCDN used 1990 tract boundaries, we assign the scores based on 2000 census tract boundaries because there were extremely few Chicago tracts that changed boundaries between 1990 and 2000. Beat level property and violent crime rates come from annual beat-level crime information from the Chicago Police Department. We estimate beat-level population figures to convert beat crime data into rates, using the census data.

If anyone has a missing value for census tract, then all of the above neighborhood characteristics will be missing. Some individuals have a non-missing census tract but a missing beat (e.g. those who live in Cook County but outside the city of Chicago), or have a census tract without matching PHCDN data, in which case just those characteristics that we fail to match are missing, and appropriate indicators are included to reflect this.

Baseline Housing Status - We determine whether a family was living in public housing or a project-based Section 8 housing at the time of the lottery by merging baseline addresses from the CHAC application files to lists of subsidized units maintained by the Chicago Housing Authority and HUD. We use baseline housing status because housing arrangements may be influenced by the outcome of the voucher lottery. This means the group identified as living in a housing project at baseline may include some families who are in private-market housing by the time they are actually offered a housing voucher by CHAC. This occurs in part because of the natural transition of families out of project-based housing units over time, and in part because the city of Chicago was demolishing thousands of units of public housing during the course of the 1990's (see Jacob 2004).

Baseline Rent – See discussion below.

Labor Market Outcomes - To measure labor market participation and earnings, we have obtained quarterly earnings data from the Illinois unemployment insurance (UI) program, maintained by the Illinois Department of Employment Security (IDES). If an individual works for more than one employer in a given calendar quarter, we aggregate up earnings from all employers. People in our sample are counted as working in a given quarter if they report having any earnings at all in the UI data in a quarter. Household-level employment is defined as having anyone in the CHAC baseline household with positive earnings in a given quarter. We set to missing those person-quarter observations where quarterly earnings are reported to be less than $5 in nominal terms. We set equal to the 99th percentile of the distribution those outlier observations greater than the 99th percentile. Earnings figures are then converted into constant 2013 dollars. These data are available from 1995:Q1 through 2011:Q4.

Social Program Participation - We obtain our welfare information from the IDHS administrative databases. They provide us with start and end dates of AFDC/TANF, Food Stamp and Medicaid spells for every household member of those households that we match to the CDB. From these start and end dates we then create, for each of the welfare programs, a variable indicating the number of days during the current quarter a person was receiving assistance and separate binary indicators for whether the person received assistance during the current quarter, the first quarter of 1997, and second quarter of 1997. We also create binary indicators for whether the household head received assistance of any type during the current quarter, the first quarter of 1997, and the second quarter of 1997. These IDHS data are available for the period 1989:Q2 through 2013:Q1.

Criminal behavior: We have obtained data from the Illinois State Police (ISP) that capture all arrests made in the state of Illinois. These arrest histories include information on the date and criminal charges associated with each arrest event. Revisions made to the Illinois Juvenile Court Act that allowed for the submission of juvenile misdemeanor arrests into the ISP database, coupled with improvements in fingerprinting technology, resulted in more complete coverage of juvenile arrests from 1998 onward. (Prior to this, the arrest data for juveniles is limited to serious felonies.)[1] We use these ISP arrest histories to create indicators for the number of pre-randomization arrests that CHAC applicants have experienced for different offense types (violent, property, drug, other). These ISP arrest records capture all arrests up through 2012:Q1.

[1] Personal communication between Jens Ludwig and Christine Devitt Westley, May 6, 2014.

The measure for the social cost of crime committed by each youth is essentially an importance-weighted index for all the crimes committed by a youth – that is, we multiply each arrest that a youth experiences by the estimated cost to society from that particular type of crime, using previous estimates from the literature. We use the approach from Kling, Ludwig, and Katz (2005, p. 205) adopting both the original and modified versions of the cost-of-crime estimates presented in Miller, Cohen, and Wiersema (1996). The modified versions address two conceptual and empirical challenges in constructing this type of dollar index for the social costs of crime. One issue is that the social costs of homicide are much higher than any other crime type, and so will exert disproportionate leverage on any dollar-weighted index; we address this issue by exploring the sensitivity of our estimates to "trimming" the dollar value associated with the costs of homicide to equal two times the next-most-socially-costly crime type. The other issue is that thinking about the social costs of arrests for drug possession offenses is conceptually complicated, so we also explore how our estimates change if we assign zero costs to such crimes.

Schooling outcomes: We obtained student-level school records from the Chicago Public Schools (CPS) that includes information at the level of the student-year. These school records include information on the specific school (or schools) a student attended in a given academic year, the number of days the student attended, how many absences were excused versus unexcused, course grades, student misconducts, and scores on standardized achievement tests. For most of our study period students in CPS in grades 3-8 take the Iowa Test of Basic Skills (ITBS), and in some years also take a state assessment as well. High school students towards the later part of our panel take the Explore and other achievement tests that are part of the set of tests leading up to the ACT. These CPS records are available from 1995 through 2011.

F.2 Covariates Included in Baseline Regression Specifications

Because of randomization of families to the voucher program wait list in Chicago, our estimates would be unbiased even without any control for baseline covariates. However in our analysis we include controls for a variety of baseline characteristics in order to help account for residual variation in our outcomes of interest and so improve the precision of our estimates. (Our results are qualitatively similar without these baseline controls).

Unless otherwise noted, the baseline covariates in our models include the following:

- binary indicators for child's and household head's race: black, Hispanic, white, other
- binary indicators for child's and household head's gender
- binary indicator for disabled household head
- binary indicator for spouse present
- age of household head, and age bins of child
- continuous measures of the number of adults in the household and the number of children in the household, and an indicator for being an only child
- continuous measure of the number of days after the opening of the waiting list that the family submitted an application
- binary indicator based on self-reported information from the CHAC application form of whether the household head was willing to accept a certificate as well as voucher
- binary indicators from baseline CHAC applications about whether the household was currently receiving any earned income, currently receiving any SSI benefits, currently receiving AFDC/TANF
- a series of measures drawn from Illinois administrative databases describing the household head's public assistance receipt and employment in the eight quarters prior

to the CHAC lottery, including up to a cubic in fraction of quarters employed, received TANF, Food Stamps, or Medicaid, and total earnings during the period

- up to a quadratic in child's standardized number of prior arrests for different crimes (e.g. violent, property, drug, or other)
- a series of 12 binary indicators of household head total prior arrests for different crimes (1,2,3+ prior arrests for a violent crime, property crime, drug crime or other crime)
- a series of measures drawn from Chicago Public Schools data describing whether the child was enrolled or had left CPS pre-lottery (and, if so, the reason for their leaving, if given); their special education and lunch status; whether they were ever old for their grade; average demographics, lunch status, and test scores of their pre-lottery schools; up to a quadratic in math and reading scores, GPA, and number of days absent in each pre-lottery year; interaction of math and reading scores in each pre-lottery year; siblings' average math and reading scores, GPA, and number of days absent in each pre-lottery year
- measures of the applicant's baseline neighborhood, including percent minority, percent black, poverty rate, collective efficacy, and social capital (at the tract level), and violent and property crime rates (at the beat level)
- the household's imputed fair market rent and baseline rent.

Where appropriate, missing values are coded as zero and indicators included as covariates in the models.

F.3 Procedure for Identifying Other CHAC Household Members

The CHAC application forms ask household heads for information on the total number of male and female adults, and male and female children, living within the home, but only ask for individual identifying information (name, date of birth, and social security) for the head and his or her spouse (if applicable). Only when families with sufficiently good lottery numbers were offered housing vouchers by CHAC did the organization ask household heads to provide individual identifying information on *all* household members.

In order to preserve the strength of our research design – random assignment of households to the voucher waiting list – *we must identify household members for all families across the entire CHAC waiting list (treatment and control group families) using the exact same method*. To do this, we subcontracted with Chapin Hall at the University of Chicago to match the individual identifying information available for all CHAC applicants and their spouses to administrative data on social program participation from the Illinois Department of Human Services (IDHS). The essence of our approach is to identify any other individuals who were listed as a member of the CHAC applicant's household (based on the IDHS data) *during the pre-CHAC lottery period*. The imputation strategy we follow means that our estimates involving other household members will be representative of the subset of CHAC applicants who appear on the IDHS files prior to July 1997, because they themselves or someone in their household was receiving AFDC/TANF, Food Stamps or Medicaid during this period. However, because approximately 94 percent of the 82,607 CHAC applicants appear on the IDHS files prior to the lottery, our estimates reflect the vast majority of housing applicants. Roughly 93 percent of those families that would be likely to have children (working-age, able-bodied adults) appear in the IDHS files prior to the voucher lottery.

In this sub-section we summarize the procedures we use to impute the identity of other members of the households that applied to CHAC for vouchers, and then discuss how well these procedures appear to work.

F.3.1 Household Member Imputation Procedure

Chapin Hall was able to match around 94 percent of CHAC applicant households to the IDHS client data base (CDB) using probabilistic matching techniques that use a combination of name (converted to Soundex), dates of birth, and Social Security numbers. For each CHAC applicant (or spouse) who matched to the IDHS CDB, Chapin Hall identified their spell of social program participation that was closest in time prior to the date of the CHAC lottery (7/1/97), which we call the "target case." We then identified the other members of the CHAC applicant household through the following multiple-step process:

1. Identify everyone who was in the CHAC applicant's (or spouse's) target case.
2. Then determine the target case for everyone identified in step (1). Note that some members of the CHAC applicant's target case could have a different target case if, for example, the daughter of a welfare recipient left her mother's household before the time of the CHAC lottery and started her own household and then also received welfare benefits on her own for this new household.
3. For individuals whose target case is the same as that of the CHAC applicant, we count these people as members of the CHAC applicant's household.
4. For individuals whose target case is different from that of the CHAC applicant, we count these people as members of the CHAC applicant's household (as well as anyone else

listed as part of the household in this target case) *only if* the address of this other household member's target case is equal to the address of the CHAC applicant's target case. This scenario could occur if, for example, the daughter of a CHAC applicant has started her own welfare spell but continues to live with her mother.

Note that our procedure counts everyone who we believe was living in the CHAC applicant's household at the time of the voucher lottery as being part of the study sample. It is possible that some people living in these baseline households might start their own households during the post-lottery period, particularly if the CHAC applicant receives a voucher. Under our definition everyone in the baseline household at the time of the voucher application is counted as "treated," even household members who do not move, since they still experience some "treatment" from a reduction in crowding within the housing unit.

F.3.2 How Well Does This Imputation Procedure Work?

Our process for identifying household members is necessarily imperfect and will introduce some measurement error into our measures of household composition. To explore the extent of measurement error, we examine the subset of CHAC applicant households who matched to the IDHS files pre-lottery. Starting with this set of 77,666 households, we drop roughly 2,400 households with missing data on gender for any household member and 84 households that report more than 10 household members on the CHAC application forms (which we believe is most likely due to a data entry errors). Our final sample thus includes 75,145 households. Note that including cases with missing gender or large number of household members yields nearly identical results to those reported below.

Our imputation procedure and the CHAC baseline applications identify the exact same number of total household members in 47.4 percent of cases (the CHAC applications reported more in 38.7 percent of cases); the same number of adult females in 70.8 percent of cases (the CHAC applications reported more in 6.9 percent of cases); the same number of male adults in 71.9 percent of cases (the CHAC applications reported more adult males in 19.4 percent of cases); and the same number of children for over half (56.5 percent) of applications (the CHAC forms reported more children in 36.7 percent of cases). Table F.1 presents a more thorough breakdown of whether our IDHS estimation procedure and the CHAC applications are identifying the same number of household members.

Table F.1: To What Extent Does the IDHS Estimation Procedure Over or Underestimate Household Size? (N=75,145)

	Fraction of the cases in which:				
	CHAC and IDHS equal	CHAC greater than IDHS by:		IDHS greater than CHAC by:	
		One	More than one	One	More than one
Number of Female Adults	0.71	0.06	0.01	0.20	0.03
Number of Male Adults	0.72	0.17	0.02	0.08	0.01
Number of Female Children	0.71	0.16	0.06	0.05	0.02
Number of Male Children	0.67	0.19	0.08	0.04	0.01
Number of Total Adults	0.70	0.10	0.03	0.13	0.04
Number of Total Children	0.57	0.21	0.15	0.04	0.03
Total Household Size	0.48	0.21	0.17	0.08	0.06

Table F.2 presents comparisons for the average household size and compositions implied by the CHAC applications and our imputation procedure.

Table F.2: Comparisons of Average Household Size as Reported on CHAC Application Forms Versus the IDHS Estimation Procedure (N=75,145)

	CHAC Applications	IDHS Estimates
Number of Female Adults	0.86	1.04
Number of Male Adults	0.45	0.33
Number of Female Children	0.79	0.59
Number of Male Children	0.92	0.60
Number of Total Adults	1.31	1.37
Number of Total Children	1.72	1.19
Total Household Size	3.03	2.56

One reason the IDHS data may understate household size is that some welfare target cases may end before 7/1/97, and so we might miss household members who enter between the end of that target spell and the time of the CHAC voucher lottery. To test this hypothesis, we replicated the above tables using only those households where the household head's target case was active at the time of the CHAC voucher application period (that is, the household head's most recent social program spell prior to 7/1/97 was still active on that date), and find results similar to those from the full sample – that is, entry into the household by members between the last welfare spell and the time of the CHAC application period does not seem to be an important explanation for why the IDHS data understate household size. It is possible that some households might overstate on the CHAC application form the number of children living in the household in order to receive a larger unit, although we have no way to directly test this hypothesis.

The key question for identification in our study is whether any error in the identification of household members is systematically related to a family's position in the CHAC housing-voucher lottery. Given the procedure we used to impute household members (namely the fact that it relies entirely on pre-lottery information), there should be no such relationship. To address this question empirically, we create the following variables to characterize disagreements between the CHAC applications and our IDHS estimation procedure for each household in our analytic sample: a dummy variable equal to 1 if the CHAC application reports more people in the household than does our IDHS estimation procedure, and equal to 0 otherwise; a dummy variable equal to 1 if the IDHS data report more people in the household than does the CHAC data, and equal to 0 otherwise; a variable equal to the difference between the total number of household members reported on the CHAC application and the total number of household members suggested by our IDHS estimates; and similar variables for specific sub-groups of household members (female adults, male adults, total adults, female children, male children and total children).

First, we regress each of these outcome measures against each household's actual lottery number. Out of the 21 total regressions that we estimate, only one yields a coefficient on the household lottery variable that is statistically significant at the 5 percent level, about what we would expect by chance alone.[2] Of course these 21 regression coefficients for comparing measures from the IDHS and CHAC applications are not truly independent; if we focus on the four independent measures of household size (actual difference between the two data sources for female adults, male adults, female children, male children), none of these are statistically significant. Nor do we find any evidence of a non-linear relationship between wait-list position and measurement error in our IDHS household identification procedure.[3]

[2] The one significant coefficient suggests that households with higher lottery numbers are somewhat more likely to have more male adults reported by our IDHS estimation procedure than on the CHAC baseline application, with p=.047, although the measure for the actual difference in the number of male adults between the two datasets, as opposed to a dummy variable indicating that there is a discrepancy, is not significant.

[3] Some non-linear relationship between wait list position and this measurement error could arise if for example families who are offered vouchers immediately are more likely to be captured by the IDHS records for some reason. To explore this possibility, we create a set of indicator variables that divide families up into groups of 5,000 based

F.3.3 Who Gets Missed By Our Household Member Identification Procedure?

While it is reassuring that there is no systematic relationship between CHAC lottery numbers and measurement error in household composition, the question of who gets missed by our IDHS estimation approach to household composition is still of some interest to our study.

We cannot directly determine who is included in the household count on the CHAC application forms because the former includes total counts of other household members but not individual identifying information. We instead take advantage of the fact that households who lease up with a voucher are required to fill out what are called HUD 50058 forms, which capture individual identifying information for everyone in the household that is leasing up. So we can try to learn more about who is missed by our IDHS household identification procedure by comparing the results of our IDHS procedure with who is listed on the HUD 50058 forms, at least for those households who lease up.

There are several limitations to this approach. First, those families who lease-up are different in some observable and likely unobservable ways from those families who were offered a voucher but do not lease up (as reported in the body of our paper itself). Second, household composition could change between the time when a family applies to CHAC and when they are actually offered a voucher and lease up (members could in principle be either lost or added in the interim). For this reason, we focus this analysis on those households who were offered a voucher by the end of 1998 (within the first 16 months following the start of the program) and who lease

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on each household's CHAC lottery number, and regress each of the outcome measures described above against these lottery number indicators. Of the 315 total regression coefficients that we generate, only five are statistically significant at the 5 percent level, about what we would expect based on chance alone. If we focus only on the raw difference in household members between the two data sources for the four independent groups (female adults, male adults, female children, male children), only one of these sixty regression coefficients is statistically significant.

up. Tables F.3 and F.4 indicate that the patterns in Tables F.1 and F.2 are also apparent in this subsample.[4]

Table F.3: To What Extent Does the IDHS Estimation Procedure Over or Underestimate Household Size for Those Households Who Were Offered a Voucher by 1998 and Leased Up? (N=2,164)

	CHAC and IDHS equal	CHAC greater than IDHS by:		IDHS greater than CHAC by:	
		One	More than one	One	More than one
Number of Female Adults	0.70	0.06	0.01	0.20	0.02
Number of Male Adults	0.72	0.18	0.02	0.07	0.01
Number of Female Children	0.71	0.16	0.05	0.05	0.02
Number of Male Children	0.67	0.19	0.08	0.04	0.02
Number of Total Adults	0.73	0.10	0.02	0.12	0.03
Number of Total Children	0.57	0.21	0.15	0.04	0.03
Total Household Size	0.49	0.21	0.17	0.07	0.05

[4] Note that the sample of 2,164 households included in this analysis meet the following sample criteria: (1) the household head (or spouse) appeared in the IDHS files prior to the voucher lottery; (1) the household was offered a voucher by 1998; (3) the household utilized the voucher and leased an apartment; (4) the household reported at most 10 total household members on the voucher application form.

Table F.4: Comparisons of Average Household Size as Reported on CHAC Application Forms Versus IDHS Estimation Procedure for Those Households Who Were Offered a Voucher by 1998 and Leased Up? (N=2,164)

	CHAC Applications	IDHS Estimates
Number of Female Adults	0.86	1.03
Number of Male Adults	0.42	0.27
Number of Female Children	0.81	0.64
Number of Male Children	0.98	0.66
Number of Total Adults	1.28	1.30
Number of Total Children	1.79	1.30
Total Household Size	3.06	2.60

Our next step is to try to figure out who exactly is in the 50058 data but not identified by our IDHS procedure, and who is identified by our IDHS procedure but does not show up in the HUD 50058 forms. We do this by attempting to match specific individuals through some combination of name, DOB and SSN. We restrict this sample to non-household heads because the goal of this analysis is to compare who shows up in the 50058 data to who is identified using our IDHS procedure, and all household heads will show up in the 50058 data by definition. As above, we limit this analysis to the set of 2,164 households who were offered vouchers in 1997 or 1998 and who utilized these vouchers to lease up.

Comparing the 50058 records to either the IDHS or CHAC application records for this set of households, we find the 50058 records contain a larger number of people. Specifically, the average number of children (non-head adults) in the 50058 records is 2.15 (0.29) compared with 1.79 (0.28) in the CHAC application files and 1.30 (0.30) in the IDHS records. This suggests that individuals may have "joined" successful CHAC applicants in starting a new household, which is consistent with evidence that voucher receipt is often accompanied by changes in household composition (see, for example, Gubits et al. 2006). It may also be the case that families have a greater incentive to accurately and fully account for all household members on 50058 forms. Individuals had no incentive to accurately report household size or composition on the CHAC application form. And we know that the IDHS records may not contain information on individuals who do count toward the benefits calculation for the family, as in the case of other adults and AFDC/TANF benefits.

Table F.5 shows that roughly 77 percent of the 3,417 non-household heads who appear in our IDHS sample show up in the 50058 data. However, the match rates for young children in our IDHS sample are much higher – approximately 90 percent for those children under the age of 11. Among children age 11-15 that we identify in our IDHS sample, 83 percent also appear in the 50058 records, while the match rate for 16-17 year olds are noticeably lower (i.e., 70 percent). Interestingly, very few of the adult family members we identify in the IDHS files appear in the 50058 data. This pattern is consistent with a situation in which young children are very likely to accompany their parent or guardian to a new residence, but that the receipt of a housing voucher allows adults who had previously been living together to form their own households.

Table F.5: Fraction of Non-Household Heads Who Appear in IDHS records (N=3,417) and Also Matched to 50058 Records, Separately by Age

Age as of 7/1/97	Fraction of the total sample of 3,417 individuals (1)	Fraction of individuals that match to the 50058 records (2)
All ages	1.00	0.77
0-3	0.20	0.91
4-6	0.19	0.88
7-10	0.21	0.90
11-15	0.18	0.83
16-17	0.05	0.71
18-25	0.07	0.30
25-45	0.07	0.20
45-65	0.03	0.32
65 or older	0.01	0.35

F.3.4 Summary

Because the CHAC application forms list the total number of adults and children in the home but do not provide individual identifying information about household members other than the household head and his or her spouse (if applicable), we use IDHS data on pre-CHAC-lottery social program spells to identify other household members using the procedure described above. Our IDHS procedure suggests household sizes that are about one-half child smaller than what is suggested by the CHAC application files. However, a comparison of the individuals who appear in our IDHS data and those who later appear on official HUD 50058 forms among those families who utilized a housing voucher suggests our IDHS imputation procedure correctly identifies nearly all of the young children (below the age of 15) in a household and a fairly high (70 percent) fraction of older children. On the other hand, it appears that our IDHS estimation may not reliably identify other adults associated with the household. Finally, and quite importantly, the analysis reported here confirms that the measurement error in identifying household members is unrelated to the randomly assigned CHAC voucher wait list position.

F.4 Calculation of Baseline Income, Rent, and Implied Voucher Benefits

At several points in the analysis, we rely on estimates of income, rent and taxes in our sample. Because this information is not reported directly or fully in any single data set, we must estimate these values for families in our sample using data from a variety of different administrative data sources. Using our estimates of baseline income and rent, we are able to estimate the value of the housing voucher for each family.

F.4.1 Estimating Fair Market Rents for CHAC Applicants

In order to calculate the housing benefit available to each family that is offered a voucher, we must first determine the maximum value of the apartment for which the voucher can be used. This value is known as the Fair Market Rent (FMR). The FMR is a function of the number and gender composition of the adults and children in the household, the metropolitan area the family is living in, and the calendar year. CHAC applicants must report all the relevant information for household size and gender composition, and HUD publishes the FMR for different-sized housing units in each local metro area each year at www.huduser.org/datasets/fmr.html. We estimate the FMR for each CHAC family for 1997 using the baseline information on household composition that they report to CHAC on their voucher application to identify the largest apartment the family is entitled to, and then assign them the FMR for that size unit using the FMR reported by HUD. The average 1997 FMR for CHAC applicant households in our dataset was around $1,352 per month, or $16,220 per year.[5]

[5] This FMR calculation uses the household size and gender composition that we estimate using the Illinois Department of Human Services (IDHS) data and estimation procedure described above for households that ever show up in the IDHS data system; for those who do not show up in the IDHS system, we use the household composition and gender composition reported directly on the CHAC application forms. We prioritize the estimates for household composition obtained from the IDHS data using our procedure because we can only calculate earnings and total income for people we can specifically identify through that IDHS procedure, and so the FMR calculation will be conceptually consistent with the income figures we estimate for each families.

F.4.2 Estimating Baseline Rent for CHAC Applicants

For our calculations we require a way of determining each CHAC applicant's baseline rent *that we can apply consistently for all families across the entire voucher wait list*. Unfortunately direct data on baseline rents are only available for families in our treatment group who were offered vouchers by CHAC, and then use their voucher to lease up in their same baseline apartment. The HUD 50058 forms that these families will be required to fill out as a condition of their voucher receipt will include complete information on their unit's rent.

To estimate baseline rents for our entire sample of CHAC applicants (treatment and control families), we use data from a special tabulation conducted for us by the Census Bureau using 2000 Census data for Chicago. We basically assign each CHAC applicant the average rent paid by households with similar basic demographic characteristics living in the CHAC applicant's same baseline census tract. We define household "types" or categories on the basis of the census tract of residence, race of the household head, number of adults in the home, and number of children in the home. The Census Bureau suppresses rent figures in cases where there are too few households of a given type in a given census tract. In these cases, we assign CHAC applicants the average rent for households with the same number of adults and children in the same census tract (regardless of race). In cases where the relevant rent figures for a given household type in a tract are also suppressed by Census confidentiality requirements, we assign the average rent from households in the same tract with the same number of children (ignoring race and number of adults).[6]

[6] Around 20 percent of our CHAC sample are assigned baseline rents for families of the same race, number of adults, and number of children in the same tract; around 75 percent of the CHAC sample are assigned rents based on households in the Census with the same number of adults and children in the same tract (pooling all races together); and the remaining 5 percent or so of CHAC applicants are assigned baseline rents of households with the same number of children in the same tract.

A final complication in estimating baseline rents for CHAC applicants from the Census 2000 special tabulation is that we are interested in rents paid by families living in private-market housing, yet the 2000 Census questionnaire does not ask families whether they are living in public- or private-market housing. It is not clear what a family living in public housing would actually answer to a Census question about unit rent; would they, or should they, report their own out-of-pocket rent contribution, equal to 30 percent of adjusted income just as in the housing voucher program? Or would a family in public housing instead report some guess about the true market-equivalent "rent" for their public housing unit? (How a family would even begin to make such an assessment if they tried is not clear). We try to deal with this problem by estimating baseline rents under three different procedures: (a) using the mean rent reported by families in the 2000 Census, with no adjustments; (b) using median rent; (c) using an adjusted mean rent, where the adjustment assumes a truncated normal distribution for rents and truncates the rent distribution at the minimum rent cutoff used by HUD in their own calculations of the FMR (to weed out what HUD believes are likely to believe either public housing rents reported in the Census, or sub-standard private-market units).[7] The results under each of these approaches are quite similar. We have also asked the Census Research Data Center at the University of Michigan to do some tabulations with restricted-use individual-level Census data excluding households with rents below the cutoff HUD uses; those mean rent figures across family types and tracts are generally similar to what the Census has estimated for us without any adjustment for low rents. The average baseline rent in our sample is estimated to be on the order of $781 per month, or $9,372 per year.

[7] For the truncated mean adjustment we try this once using a common standard deviation calculated for households of all sizes citywide, and once trying to calculate tract-specific standard deviations for the rent distribution Here the data become quite limited given Census bureau data suppression at the tract level. In any case, both procedures yield similar results.

F.4.3 Estimating Baseline Income for CHAC Applicants

In reality, families in our sample may receive income from a variety of different sources. Due to data limitations, we only consider earned income that appears on UI records, income received (owed) due to legislated tax refunds (liabilities), TANF, and the monetary value of food stamps benefits received.

Earned Income: We sum all quarterly UI earnings reported for all household members for the four quarters prior to the CHAC application period (from 1996:Q3 through 1997:Q2).

Legislated federal, state, and FICA tax refund or liability levels (including EITC): These were obtained using TAXSIM. [8] We do not have data on who actually filed a tax return. Our baseline specification assumes that all individuals with positive earnings file a tax return.[9] Note that this assumes that individuals automatically receive all EITC benefits for which they qualify based on their earned income and household characteristics. Individuals with zero earnings are assigned zero tax liability. While we know whether an individual claims a "spouse" on their CHAC application form, we do not know whether the CHAC household head and listed "spouse" are married or merely cohabiting, and even if the couple is legally married, whether the household head filed jointly with his or her spouse. The baseline specification assumes that all

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[8] An overview of TAXSIM can be found in Feenberg and Coutts (1993). The calculations were done using the Stata program `taxsim9`. These tax rates include state and federal EITC programs. We assume that individuals file for the child tax credit if eligible. FICA tax rates include the employee portion only.

[9] We are aware that not all low-income individuals file. For example, Scholz (1994) estimates that 80-86 percent of EITC eligible families file their taxes. As he points out, this could be either for legal or illegal reasons. Legally, individuals below a certain gross income threshold are not required to file. In 2005, the thresholds were $8,200 for single filers, $16,400 for married filers filing jointly, and $10,500 for head of household filers. At the same time, Scholz (1994) shows that 32.3 percent of individuals claiming the EITC were in fact ineligible in 1988. This is roughly 4-5 times larger than noncompliance rates for other social programs such as TANF and food stamps. We also consider an alternative, which assumes that all individuals who were not legally required to file in a given year choose to not file.

household heads with listed "spouses" are married and file jointly.[10] Lastly, to accurately calculate tax refund (liability) levels, we need a measure for dependents. For the purpose of calculating baseline income, we use the information on dependents listed on the CHAC application form and the administrative records of the Illinois Department of Human Services.[11]

TANF benefit levels: In our data, we know who was on TANF in each quarter, but not the level of benefits they were receiving. As noted in Appendix C, benefit levels are a function of earned income, household size, and childcare. We do not have data on childcare used, so this does not enter into our calculations. In our baseline specification, earned income includes income of all individuals in the household age 18 and older.[12] We also consider an alternative specification, in which earned income includes income of all individuals in the household. If we conclude that an individual receives no benefit given our measures of earned income and household size, the tax rate and benefit levels are set to zero.[13]

Food stamp benefit levels: In our data, we know whether or not an individual was on food stamps, but not the benefit level. As noted in Appendix C, benefit levels are a function of earned income, household size, childcare, and rent.[14] We do not have data on childcare or rent, so these values do not enter into our calculations. The appropriate household unit for food stamps is vaguely defined. We assume that the household unit consists of all household members at

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[10] We also construct an alternative in which all individuals with "spouses" are cohabiting (or file separately). In this alternative specification, all dependents are assigned to the household head.

[11] Our baseline specification takes the number of dependents as given. We estimate an alternative specification that caps the number of dependents at six.

[12] Technically, the appropriate definition of earned income should be income of parents and siblings. Because we do not know which children in the household are siblings and which adults are parents of the qualifying children, we simply include earned income for all individuals age 18 or older.

[13] In roughly 3 percent of household-quarter observations during 1996:Q3-1997:Q2 in which our records indicate that the household head was receiving some TANF benefits, we estimate zero benefit levels.

[14] We assume household size is one plus the number of other members under the age of 18.

baseline regardless of age. If we conclude that an individual receives no benefit given our measures of earned income and household size, the tax rate is set to zero and the benefit level is set to the minimum (we assume this is $10 per month for all individuals). [15]

Summary Statistics on Baseline Income: This table shows the mean, median, and standard deviation of baseline income for the whole sample and our main analysis sample, which includes all households living in private housing at baseline with children.

	Mean	Median	Std. Dev.
Whole Sample	14,077.53	11,657.93	12,423.79
Main Analysis Sample	18,978.47	16,897.79	11,336.20

F.4.4 Housing Voucher Benefits

After calculating total family baseline income, we then tabulate the adjusted income value that is used under housing voucher program rules to determine the family's rent contribution. We first subtract from total household income those sources that are not counted as income by the voucher program, namely tax refunds (liabilities), food stamp receipt, and earnings by household members under the age of 18. We then also subtract allowable deductions that we can identify with the data available to us, namely the $480 per child deduction under voucher program rules. Mean *adjusted* income for our sample of households in private housing at baseline with children is $12,520.

As discussed in Appendix C, the maximum value of a family's housing voucher or certificate subsidy is equal to the payment standard minus the family's obligated rent payment.

[15] In roughly 3 percent of household-quarter observations during 1996:Q3-1997:Q2 in which our records indicate that the household head was receiving some food stamp benefits, we estimate that the household receives the minimum benefit allocation or no benefit at all.

We assume the payment standard is the Fair Market Rent (FMR)[16] and the obligated rent payment as .3 times net income.[17]

One can think of the total value of the housing voucher as the sum of two components: (1) the increase in housing consumption that the individual receives by moving into a more expensive apartment and (2) the increase in disposable income the family receives as a result of devoting a smaller fraction of its income to rent.

Most families in our sample will have baseline rents that are far below the FMR, and will be spending far more on rent than 30 percent of their adjusted income. (Recall from Appendix C that adjusted income is less than total income because the housing voucher program rules exclude certain sources of income, and allow families deductions for dependents and other reasons). For these families, the amount of the voucher subsidy that they can take as cash is equal to the difference between their baseline rent and 30 percent of their adjusted income. The increase in housing consumption for a family that leases a unit with rent equal to the FMR is equal to the difference between the FMR and the family's baseline rent.[18]

Our estimation procedure will unavoidably add some error to our measures of baseline rent and income values. But since our estimation procedure for baseline rent and income relies entirely on pre-baseline administrative records, this measurement error should be orthogonal to each family's randomly assigned position on the CHAC voucher wait list.

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[16] The payment standard differs for the old Section 8 certificate program, the old voucher program, and the new voucher program, but that we will assume is equal to the FMR for simplicity.

[17] In some cases, the rent payment is defined as .1 times gross income (or the welfare rent payment – that is, the minimum amount of a family's welfare contribution towards rent). For the purposes of the calculations described above, we only use .3 times net income as the obligated rent payment.

[18] Leger and Kennedy (1990) provide some evidence suggesting that most families will choose units with rents equal to the relevant FMR. To simplify things our discussion abstracts from the differences in program rules for the old Section 8 certificate program, the old Section 8 voucher program, and the new voucher program (all of which were in operation during our study period) that impact how the housing voucher influences consumption patterns among families. For example the old Section 8 certificate program prevented families from leasing units with rents above FMR, which means that a family with baseline rent above the FMR would receive no change in consumption of either housing or other goods without moving to a new unit with rent at or below the FMR.

Given our estimates for able-bodied, working-age adult CHAC applicants of average baseline total household income of \$18,978, adjusted income (under housing program rules) of \$12,520, baseline rent of \$9,372 per year, and average FMR of \$16,220, then the average maximum voucher subsidy value (cost to the government) will equal \$12,464. Since Reeder (1985) estimates the ratio of benefit to the recipient to cost to the government for vouchers to be around .83, this implies an average equivalent variation for a voucher on the order of \$10,345. Our calculations imply that on average, the extra cash a family can take out of a voucher will be around (\$9,372 - \$3,756) = \$5,616 per year, while the family will increase their housing consumption (\$16,220 - \$9,372) = \$6,848. Put differently, the fact that families spend such a large amount of their baseline income on rent, and can then substantially reduce their spending on housing upon receipt of a voucher, means that the typical CHAC applicant is able to take almost half of the dollar value of the housing voucher subsidy in the form of cash.

In the main paper we compare the size of our impacts to what we would expect based on existing studies of cash transfer effects on child outcomes, which requires us to scale our reduced-form estimate by some measure of the cash equivalent of a voucher from the perspective of impact on child development. We first do this scaling using the total voucher subsidy value, S (\$12,501), and use two-stage least squares to estimate equations (5) and (6) in Chapter II:

(5) $$Leased_{it} * S_i = \alpha + \theta_1 PostOffer_{it} + \theta_2 PreOffer_{it} + X\Gamma + \gamma_t + \varepsilon_{it}$$

(6) $$y_{it} = \eta + \pi_{income} Leased_{it} * S_i + X\Pi + \mu_t + v_{it}$$

The total subsidy S can be thought of as an upper bound on the value of the housing voucher to families insofar as it assumes that families lease units with the maximum permissible rent (i.e., the FMR) and that every dollar of additional housing consumption is equally productive for children's outcomes as each additional dollar of consumption on other goods. By using an *upper* bound estimate of the voucher's value for children's development, this approach implicitly yields a *lower* bound estimate for the effects of income on child outcomes.

To obtain an upper bound estimate for the impact of income, we can assume that extra housing consumption has *no* developmentally beneficial effect on children. In this case, any effect from receiving a voucher is assumed to be entirely due to increased non-housing consumption. Because the income elasticity of housing is non-zero, a family receiving cash would spend some of it on housing. To calculate the size of the cash subsidy S^* (see Figure 2.1) needed to generate the same increase in non-housing consumption ΔC as the housing voucher given baseline income I, rent H_B, and elasticity of housing consumption $e_{H,I}$, we solve equation (7) in Chapter II as follows:

(7) $$\Delta C = C_V - C_B = S^* - [\frac{S^*}{I} \times e_{H,I} \times H_B]$$

As our measure of I we use the CHAC applicant's estimated baseline income based on UI records, income received (owed) due to legislated tax refunds (liabilities), TANF, and the monetary value of food stamps benefits received. We assume an income elasticity of housing consumption of 0.35 (Mayo, 1981; Polinsky and Ellwood, 1979). We then substitute our estimate of S^* for S in estimating equations (5) and (6) above.

Finally, we create an even more conservative estimate by assuming the income elasticity of housing consumption is zero. In this case, the value of the voucher to families is simply the increase in available income generated by the reduction in rent payments made possible with a voucher, i.e., ΔC. Since $\Delta C < S^*$, this yields an even larger upper bound for the estimated effects of income on children's outcomes compared to our second approach.

We calculate the three scaling factors that we use —S, S^*, and ΔC—at the household level. However, because S^* is undefined for certain values of I and H_B, and is not well-behaved when its denominator approaches zero, we use in its place the average value of S^* within a cell defined by a household head's sex, race, and age, the presence of a spouse, and the number of adults and children in the household at baseline. For consistency, though it doesn't affect our estimates, we perform the same averaging for S and ΔC. The resulting implied voucher values in our sample are, on average, S = \$12,501, S^* = \$6,377, and ΔC = \$5,653.

F.5 Address Tracking

To track residential locations for both the treatment and control groups, we rely on two different data sources that have complementary strengths and weaknesses. First, we had a commercial vendor track a random 10 percent sub-sample of our study participants using passive tracking sources such as the National Change of Address (NCOA) registry and national credit bureau checks. These addresses are representative of our study sample but the sample size is modest and the addresses are available for just two points in time (2005 and 2012). Second, we use longitudinal IDHS data that contain residential addresses for families participating in social programs like TANF, SNAP, or Medicaid. This dataset provides more frequent address coverage for a large sample, but one that is not representative of our overall study sample.

We geocoded both sets of addresses and linked them to census tract-level neighborhood characteristic data from the decennial 1990 and 2000 censuses and the American Community Surveys for 2005-9 (interpolating values for inter-censal years), tract-level social capital and collective efficacy scores come from the 1995 Project on Human Development in Chicago Neighborhoods (PHCDN) Community Survey, and to annual beat-level crime data from the Chicago Police Department.

F.6 Medicaid Claims Data

To measure individuals' health outcomes, we rely on administrative Medicaid claims records of health-care service utilization. These data come from the Center for Medicare and Medicaid Services (CMS) and span the period from 1999:Q1 through 2008:Q4, covering sample members living in Illinois, Indiana, Michigan, or Wisconsin. They include monthly indicators of Medicaid enrollment (regardless of usage), and fee-for-service claims for outpatient care, emergency department (ED) use,[19] inpatient hospitalizations, and pharmacy use.

Each claim includes primary and secondary diagnostic codes (using the ICD-9 system) that allow us to identify the condition an individual was diagnosed with, along with the dollar amount paid by Medicaid for the claim.[20] The diagnosis-derived outcomes we focus on in the exploratory analysis (Tables G.6 through G.9) include injury, asthma, and routine medical exams. (Although the latter is a procedure rather than a diagnosis, it too is captured by a set of supplementary ICD-9 codes meant to record the nature of contact with health services.) Injury claims, which we limit to those seen in an inpatient or emergency setting, are those where any diagnostic code associated with the claim matches one (or more) of the following ICD-9 codes:

ICD-9 Code	Description
8XX	Fractures; dislocations; sprains and strains of joints and adjacent muscles; intracranial injury; internal injury of thorax, abdomen, or pelvis; open wound
90X	Injury to blood vessels, and late effects of injuries, poisonings, toxic effects, and other external causes
91X	Superficial injury
92X	Contusions and crushing injuries
93X	Effects of foreign body entering through body orifice
94X	Burns
95X	Injury to nerves and spinal cord, traumatic complications, and unspecified injuries
994	Effects of other external causes
995	Adverse effects not elsewhere classified

[19] Emergency department use is recorded using outpatient claims with a "place of service" code indicating an urgent care facility, hospital emergency room, or ambulance.

[20] Inpatient claims include up to nine diagnostic codes and information on length of stay in the hospital. Pharmacy claims include the National Drug Code (NDC) identifier of the prescription.

Asthma claims are those where only the primary diagnostic code is 493, regardless of setting. Routine medical exams are identified by outpatient claims where any diagnostic code associated with the claim matches one (or more) of the following ICD-9 codes:

ICD-9 Code	Description
V20X	Health supervision of infant or child
V700	Routine general medical examination
V703	Other general medical examination
V705	Health examination of defined subpopulations
V706	Health examination in population surveys
V708	Other specified general medical examination
V709	Unspecified general medical examination
V720	Examination of eyes and vision
V721	Examination of ears and hearing
V722	Dental examination

We focus on ED and inpatient claims in our main analysis, as they presumably capture serious conditions for which most people would seek and receive treatment to minimize confounding the effects of vouchers on health status with effects on access to, or utilization of, health services. Beyond the fact that these Medicaid claims data cover just a sub-set of our sample (those on Medicaid), another limitation is that children in families offered a voucher (what we call our "treatment group") have slightly higher Medicaid use rates than controls. Our bounding exercises suggest this small difference matters little in practice for our estimates.

A further limitation involves the use of managed care organizations (MCOs) to provide medical services to Medicaid beneficiaries. Unlike traditional Medicaid, where beneficiaries seek care that is reimbursed by the state, MCOs are paid a monthly lump-sum premium ("capitated payment") for each beneficiary and must bear the risk of providing all required care. A Medicaid enrollee receiving coverage through an MCO does *not* generate fee-for-service claims, and we are therefore unable to learn anything about their healthcare usage. On average, between 20-35%

of children’s monthly Medicaid enrollment is through an MCO. All Medicaid-derived results presented in Chapter II are conditioned on being enrolled in fee-for-service care for six or more months during the academic year.

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APPENDIX G

Additional Results From Chapter II

G.1 Additional Results for Selective Attrition

Given our reliance on mostly city- or state-level administrative records, one threat to the internal validity of our estimates comes from the possibility of differential attrition. Table 2.4 shows that there is no difference in the treatment versus control group in the fraction of quarters living outside of IL between 1997 and 2005, which suggests there should be little bias with the data we get from state agencies on arrests, public assistance receipt, earnings and Medicaid claims. A recent update of these address data allowed us to get information on the location of households in 2012. (Unfortunately we were not able to get these data for the period from 2005 through 2012). Again, we see no detectable difference in the chance of living in state.

However, analyses reported in Table G.1 show that treatment children are slightly more likely to be enrolled in Medicaid over our sample period, which is consistent with earlier evidence that the voucher offer led to a small increase in social program participation (see Jacob and Ludwig 2012). We also find that younger children in our treatment group are slightly more likely to be in the Chicago Public School (CPS) system in any given academic year. The ITT is 3 (2) percentage points for boys (girls) age 0-6 at baseline.

In theory, this differential attrition might bias our achievement estimates for the young children in our sample. In practice, we think that any bias is likely to be negligible. First, even after attrition, children in the treatment and control groups are nearly identical on the comprehensive set of baseline observables (see Table G.2). Second, because there is little difference between treatment and control children in the rates with which they *ever* appear in the CPS system, we re-estimate our ITT model using a very simple imputation method (replacing any missing student test scores with a student's last observed score) and find estimates very similar to those reported in Table 2.3. We also calculate bounds using the approach from Lee (2009), which suggest that our results for graduation impacts are robust to differential attrition, although these bounds are very wide for test scores (Tables G.3 and G.4). A final point to note is that differential treatment-control attrition is identical for boys and girls (Table G.1). Yet vouchers appear to have (if anything) a more positive impact on boys, suggesting differential attrition is unlikely to drive the result. While it is possible that the attrition process works differently across genders despite identical rates of attrition, this seems unlikely.

G.2 Additional Sensitivity Analyses for Main Results

Our main results in Table 2.3 account for the risk of "false positives" in carrying out a large number of impact estimates by controlling for the false discovery rate (FDR). Table G.5 shows that the results are qualitatively similar if we control instead for the family-wise error rate (FWER); see Chapter II for additional discussion of the FDR versus FWER.

One general way we have tried to control for the risk of false positives in our tables is to focus on a small number of pre-specified outcomes in our main results. In the spirit of completeness, Tables G.6 through G.9 present the results of looking at the full range of individual outcome measures that we can construct with our administrative data for each of our four key analysis groups (boys 0-6 at baseline, boys 6-18 at baseline, girls 0-6 at baseline, and girls 6-18 at baseline).

While most of our main analyses focus on average effects, one might imagine that the intervention had different impacts across different points in the ability distribution. To explore this possibility we estimated quantile regressions of the test score. To simplify the estimation, we collapse the panel to a single observation per student containing their last observed test score and their average test score across all post-lottery periods. We then estimate a cross-sectional regression where the key independent variable is an indicator for being in the treatment group. We show estimates of the effect of being offered a voucher (i.e., ITT estimates) on the 10^{th}, 25^{th}, 50^{th}, 75^{th} and 90^{th} quantiles of the test score distribution. The results presented in Table G.10 suggests that, to the extent that there is any effect on math test scores for pre-school age boys, the results may be concentrated at the higher end. While the estimates are not very precise, we see no significant effects below the 75^{th} percentile, where the impact is 0.0526 SD.

The Chapter II tables present estimates for voucher effects on neighborhood characteristics using data from a commercial passive-tracking source that, for cost reasons, is available for just a random 10% sub-sample of our overall study sample and for just a sub-set of years in our follow-up period (1998-2005 and then again in 2012). Table G.11 shows that the results are qualitatively similar when we use our other source of address data, from Illinois Department of Human Services (IDHS) social-program participation records. Relative to our passive-tracking data, the IDHS addresses have the advantage of being available for a larger number of study subjects, but the disadvantage of being available only for those person-years in which a sample household is receiving some sort of IDHS social program service (such as cash welfare, Food Stamps, or Medicaid).

Table G.12 replicates our table of main results but now for households whose baseline rents are close to the FMR (which as a reminder is loosely speaking essentially the maximum rent allowable under the voucher program). These are the “infra-marginal” families for whom a voucher receipt is closer to a pure cash transfer relative to other voucher applicants whose baseline rents are far below the FMR.

Table G.13 presents the estimated effects of voucher use on the CPS school characteristics of older children in our study sample (those who were ages 6-18 at baseline), as a complement to our main tables that focus on results for children who were 0-6 at baseline. Table G.14 shows the results for all of our analysis samples for school moves.

Our main results exclude households with lottery numbers between 18,110 and 35,000, because these families may have expected to receive a voucher in the future and so their behavior could in principle be changed as a result of “anticipation effects.” Table G.15 shows that the results are qualitatively similar when we exclude this group of families whose treatment status is ambiguous into our control group.

G.3 Reconciling Our Results with Other Studies of Cash Transfer Effects on Children

Why are the results we find so different from what we might have expected based on the results of previous studies of income effects on children’s outcomes? Differences in research designs across studies inevitably mean that differences in the internal validity of different studies could be one explanation. In this section, we discuss several other potential explanations, including (1) program rules that might reduce the apparent benefit of a housing voucher, (2) differences in population and outcome measures, which might limit the generalizability of our results to the income-transfer programs discussed above, and (3) how different transfer programs change parental labor supply and how that, in turn, affects how parents spend the additional income. We believe the final explanation is the most compelling.

In theory, certain rules governing the voucher program could influence child outcomes. For example, voucher households are required to contribute one-third of their income toward rent. If families are homeless or living with others and not paying rent ("doubled up"), then receipt of a voucher could actually increase their out-of-pocket spending on rent and reduce their non-housing consumption, leading us to understate the effect of income on child outcomes.

In practice, we believe it is extremely unlikely for the share of such households in our sample to be high enough to explain the differences in results across studies. The Homelessness Research Institute estimates that 800,000 families with incomes 125 percent of the poverty line or less are doubled up,[1] while the Census Bureau estimates that in 2012 there were 12.5 million families with incomes at or below 125 percent of the poverty line. These two figures imply that about 6 percent of poor and near-poor families are doubled up. Presumably at least some of those families are contributing to rent, though we are not aware of any reliable estimates of rent contributions by this population. While there are higher estimates in the literature for the number of people who are doubled up in America, these higher estimates include large numbers of adult children living with their parents, or grandparents living with their children and grandchildren, which are not relevant for our applicant pool of low-income families with children. For families who are spending nothing on housing at baseline, receipt of a voucher would require them to begin paying rent and so reduce their non-housing consumption by the amount of the required voucher rent contribution, which for our full sample is on average about $3,700. If we make the extreme assumption that every doubled-up family pays zero rent, and if 6 percent of our study sample was doubled up at baseline, then the scaling factor for our IV would go from $5,653 to

[1] http://b.3cdn.net/naeh/97569cfc8f6ecf741f_vhm6bhzcg.pdf

$5,091 [.94x$5,653 + .06x(-$3,700)], which would increase the size of our IV estimate by about 10 percent. Even if the share of applicants paying no rent at baseline was 30 percent, it would only double our IV point estimate, while the Dahl & Lochner and Milligan & Stabile estimates are between 3 and 8 times the upper bound of the estimates we present in column 5 of Table 2.6.

The voucher program also requires families to take certain steps in order to utilize the benefit – e.g., finding a unit that passes inspection in a limited time window. As far as we know, there are no good data on the share of units that pass inspection. Finkel and Buron (2001) report instead on the experiences of those who successfully lease up with a voucher, missing what happens among those who fail to lease up. Among those who use a voucher, they find that 68 percent lease the first unit for which they requested an inspection after it passed on the first try, 28 percent lease the first unit after it passed a subsequent inspection, and 4 percent leased the second or third unit inspected. It is possible, albeit counterintuitive, that households able to lease up successfully are different in ways that attenuate the voucher's beneficial effects on children, although lease-up does not seem to be strongly related to the observable characteristics of households. In Table 2 of Jacob and Ludwig (2012), we find the R^2 in regressions of lease-up against baseline household characteristics using data from the treatment group range from 0.01 to 0.16, suggesting that observable characteristics explain little of the variation in lease-up success. Other program rules, like criminal background checks, would if anything seem more likely to improve rather than harm the developmental quality of children's home environments, and in any case do not seem to have been very stringently enforced for our sample as noted in Section 2.2.

Another possibility is that the residential mobility generated by voucher use suppresses child outcomes, given that 93% of voucher users in our sample move to a different housing unit. However as discussed in Section 2.2, in practice our data show that vouchers simply cause families to move somewhat earlier than they would have otherwise and do not appear to affect the total number of moves. As shown in Figure G.2, our estimated effects of voucher offers on children's outcomes do not seem to grow with time since voucher receipt, which also argues against any early disruption effect.

Another potential set of explanations involves differences in study populations and outcome measures, which might limit the generalizability of our results to recent studies of income-transfer programs. However, we think these factors are unlikely to explain the discrepancy in results across studies. While some outcomes are not consistently examined across all studies (e.g., criminal behavior, child mental health), most studies include common outcomes like high school graduation and cognitive achievement. If our study's time frame was shorter than those of other studies, that could potentially explain the results if we think that the effects of household resources on children's outcomes grow over time. But our study actually has a substantially longer follow-up period than these other studies. For example Dahl-Lochner measure outcomes in children from 1988 through 2000, but the variation they use to identify the effect of income on children's outcomes primarily comes from EITC expansions between 1993 and 1995, effectively yielding a 5-7 year follow-up period. Milligan-Stabile utilize variation from the introduction and modification of child benefit programs in Canadian provinces in the late 1990s and early 2000s, and have outcome data through 2004-05, yielding a similar follow-up period. Akee et al. use data from a survey initiated in 1993 focusing on children aged 9, 11, and 13 years at intake, who are interviewed until age 21. The treatment they study begins in 1996, with the initial disbursement of casino profits, yielding a follow-up period of 5-9 years.

Our sample is somewhat more disadvantaged than those in the other studies. The average income in our sample is lower than that in Akee et al. (2010), but roughly equivalent to the subsidized families in Dahl and Lochner (2012) and Milligan and Stabile (2011), although the households in our sample are more disadvantaged on other dimensions (e.g., household heads are nearly all African-American, single parents). As shown in Table 2.1, the average household income (in 2013 US dollars) for our study sample at baseline was about $18,900, and the vast majority of our households is African-American and headed by an unmarried woman. By way of comparison, the average baseline household income (also reported in 2013 US dollars) equaled about $29,900 for the set of Indian families that received extra payments in the study of the North Carolina reservation that opened a casino by Akee et al. (2010). Table 1 in Dahl and Lochner (2012) implies that the average incomes of the EITC recipients in their study (in 2013 dollars) are about $15,200 to $19,050, very similar to that of our own study sample, but their EITC sample is nationally representative rather than Chicago-specific and consists of just 47 percent black households and 20 percent Hispanic households, with the remainder presumably mostly white or Asian (see their Table A1, p. 1953), and 37 percent of households in their EITC sample were headed by a married couple. The change in child care benefits in Milligan and Stabile (2011) was concentrated among families with incomes in 2013 US dollars of $13,000 to $33,000, but their Canadian sample contains far more white households than ours.

Our study sample is most similar to the single-parent households in the welfare-to-work experiments examined by Duncan et al. (2011). If anything, the disadvantage of our sample relative to those in other studies would lead us to expect the measured effects of a transfer program to be larger, not smaller.

In addition the similarity of OLS estimates across studies also suggests that sample differences alone are unlikely to fully explain the discrepant results. For example, our OLS estimate for the effects of extra income on children's test scores falls between that of Duncan et al. (2011) and Dahl and Lochner (2012). The OLS relationship between household income (thousands of 2013 dollars) and child outcomes (test scores in SD units) in our dataset, using data from 1996-1998 and controlling for race/ethnicity, child and household head age, and household composition, is 0.005 to 0.006 (standard error = 0.0006), depending on whether we use family income data from 1 year or averaged over 3 years to reduce measurement error. This is similar to or even slightly larger than the OLS coefficient reported in Duncan et al. (2011) of 0.003 to 0.005 (standard errors ~ 0.002). Dahl and Lochner (2012) report an OLS estimate of income effects on test scores of 0.004 for the full sample and a somewhat imprecisely estimated 0.019 for households in the bottom income quartile in their dataset (the group most comparable to our sample), with a 95 percent confidence interval that ranges from 0.006 to 0.033.

The OLS relationship in our data between extra income and high school graduation rates is very similar to that in the data used by Akee et al. (2011). In unpublished estimates that Randall Akee graciously generated for us, the OLS relationship between family income and rates of high school graduation by age 19 (controlling for child's gender and parent education) is about 0.0036 (just over a third of a percentage point) per $1,000 in 2013 dollars. The relationship in our dataset (using an "ever graduated" measure) is 0.0043 to 0.0049 (standard error 0.0003).

We think a plausible explanation for the difference in our results versus these other studies is how the different transfer programs change parental labor supply and how that in turn affects how parents spend their money. In our study, the transfer (housing voucher) *reduces* labor supply by 3.6 percentage points in the first 8 years after the voucher lottery (Jacob and Ludwig 2012) and a statistically insignificant 1 point drop in labor supply over the full 14-year follow-up period we examine in this paper. The EITC expansions examined by Dahl and Lochner (2012) tend to *increase* labor force participation rates among the most disadvantaged households (those headed by single mothers), while in the Duncan et al. (2011) paper extra income always comes within the context of welfare-to-work programs that *require* women to work more. In Milligan and Stabile (2011), the four largest provinces in their sample had tax credits that depended at least partly on earnings and so may have increased work rates. The casino openings in Akee et al. (2011) may have improved job prospects on the reservation they study, though their estimates for labor force participation effects among parents are not statistically significant.[2]

We do not think the issue is the "main effect" of parental labor supply on children's outcomes, but rather the way in which parental labor supply moderates the effects of extra income and changes how it is spent, particularly spending on child care in households with young children. Neither the estimates of Dahl and Lochner (2012) nor Duncan et al. (2011) change much when they condition on maternal employment. Our results also do not change very much when we focus on sub-groups for whom voucher receipt generates little change in labor supply. Specifically, in Jacob and Ludwig (2012) we show that households with 3+ children had a maternal labor supply effect less than half the size in absolute magnitude as for the full sample. Yet, estimates for children's outcomes in this subsample are not so different from the full sample results reported here. Our results are also similar when we restrict the sample to 1998-2005, the period for which the effects of voucher receipt on labor supply are larger in absolute value (see Jacob and Ludwig 2012) compared to the longer study period examined here.

Parent labor supply should moderate the way that income gets spent within the home under the standard Becker (1965) model in which parents combine parental time and market goods to produce children's human capital (see Appendix E). For example, Mayer (1997) finds that most low-income parents devote extra income to things like food, housing, clothes, health

[2] We are very grateful to one of the referees for making this observation about the sample examined in Milligan and Stabile. The average marginal effect on labor force participation for mothers in Akee et al. (2011) is about 2 percentage points with a standard error of 2.6 percentage points.

care, and transportation. In contrast, Duncan et al. (2011) find in their welfare-to-work experiments that mothers of preschool-age children, required by these programs to work far more hours, wind up devoting a sizable share of their extra income to buying center-based care. Morris et al. (2005) note that much of the relationship between income and child outcomes in the welfare-to-work experiments is explained away after controlling for use of early childhood center care. Indeed, only pre-school-age children show gains in outcomes from extra income in those welfare-to-work experiments (Morris, Duncan, and Rodriguez 2004; Morris, Duncan, and Clark-Kauffman 2005). The fact that these experiments find no test score effects on school-age children (and if anything negative effects on test scores for teens) would seem to argue against other explanations for differences in results across studies. This finding is also consistent with the large body of evidence about the benefits for children from high-quality early childhood programs (Almond and Currie 2011).

In sum, it is possible that the most important explanation for why we get different results from these other studies, even more important perhaps than the distinction between in-kind and cash benefits, is that we are examining different "treatments" with respect to parent labor supply. Our study answers the question of what happens when households get more resources and *more* parental time. Studies of welfare-to-work experiments, the EITC or child care subsidies provide children with more income together with *less* parental time, which may change the way that resources are spent, particularly on key "inputs" like early childhood education or care.[3]

[3] An alternative hypothesis for the Duncan et al. (2011) effects could be that extra income reduces parent stress, or what Yeung et al. (2002) calls the "family process" or "family stress" perspective. But as Duncan et al. (2011, p. 1276) note, the earnings supplements they study do not seem to have had much in the way of detectable effects on parent harshness, depression, warmth, monitoring, or provision of learning experiences in the home. Another hypothesis for differences across transfer programs is that some programs provide resources as lump-sum payments rather than monthly. But many of the earnings supplements examined by Duncan et al. (2011) are essentially paid out monthly as in the housing voucher program, by for example letting families keep more of their earnings.

Table G.1: Housing Voucher Effect on Enrollment in Chicago Public Schools and Medicaid

	Effect of Voucher Offer (ITT)			
	Males		Females	
	Age 0-6	Age 6-18	Age 0-6	Age 6-18
	(1)	(2)	(3)	(4)
Ever Enrolled in CPS during 1998-2011	-0.0003	-0.0003	0.0004	0.0014
	(0.0017)	(0.0020)	(0.0014)	(0.0019)
	[0.845]	[0.812]	[0.843]	[0.820]
	12,288	21,112	11,985	21,225
Ever Left CPS (Moved or Enrolled in Private)	-0.0399***	-0.0115*	-0.0225**	-0.0117**
	(0.0092)	(0.0061)	(0.0095)	(0.0059)
	[0.278]	[0.223]	[0.266]	[0.201]
	12,288	21,112	11,985	21,225
Enrolled in CPS in Current Academic Year	0.0328***	0.0068**	0.0203***	0.0151***
	(0.0067)	(0.0032)	(0.0068)	(0.0031)
	[0.505]	[0.324]	[0.509]	[0.334]
	172,032	295,568	167,790	297,150
Tested in Current Academic Year	0.0277***	0.0052*	0.0239***	0.0130***
	(0.0056)	(0.0027)	(0.0057)	(0.0027)
	[0.299]	[0.235]	[0.311]	[0.250]
	172,032	295,568	167,790	297,150
Ever Enrolled in Medicaid during 2000-2008	0.0008	0.0077	0.0025	0.0096
	(0.0091)	(0.0076)	(0.0094)	(0.0070)
	[0.776]	[0.591]	[0.783]	[0.754]
	12,288	21,112	11,985	21,225
Enrolled in Medicaid in Current Academic Year	0.0108	0.0113**	0.0145*	0.0112*
	(0.0086)	(0.0052)	(0.0088)	(0.0058)
	[0.471]	[0.295]	[0.466]	[0.394]
	110,592	190,008	107,865	191,025

Notes: Order of results: ITT estimate; standard error (parentheses); control mean (brackets); number of observations. Standard errors are clustered at the household level.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table G.2: Baseline Characteristics of Treatment and Control Group Households and Children: CPS and IL Attrition

	Never Left CPS (Moved or Enrolled in Private)		Never Missed Test During Ages 8-17: Person-Year Obs.		Had Illinois Address in 1997, 2005, 2012[1]	
	Cont.	Treat.	Cont.	Treat.	Cont.	Treat.
	(1)	(2)	(3)	(4)	(5)	(6)
Household Level						
Household head: Male	0.036	0.042	0.027	0.032	0.035	0.036
Household head: Black	0.947	0.948	0.949	0.954	0.941	0.953
Household head: Hispanic	0.033	0.029	0.035	0.031	0.037	0.030
Household head: White	0.018	0.020	0.013	0.013	0.020	0.015
Household head: Other race	0.003	0.002	0.003	0.002	0.002	0.002
Household head: Has spouse	0.081	0.085	0.076	0.078	0.076	0.070
# Adults in household (based on CHAC file)	1.4	1.4	1.4	1.4	1.4	1.4
# of kids 0-18 in household (based on CHAC file)	2.9	2.9	3.0	3.0	2.8	2.9
Age of household head	32.0	31.9	30.7	30.4	31.7	31.5
Indicated interest in certificate as well as voucher program	0.802	0.802	0.802	0.805	0.763	0.811
Reported receiving Supplemental Security Income (SSI) benefits	0.176	0.183	0.154	0.165	0.151	0.198
Time (in days) of application since application opened	9.3	9.3	9.0	9.0	9.2	9.1
Total household income (2013 $) 1996:III to 1997:II[2]	19,063	19,273	18,634	18,718	19,579	18,677
Household head earnings (2013 $) 1997:II	1,951	2,023	1,832	1,872	2,169	1,978
Household head employed 1997:II	0.462	0.471	0.449	0.456	0.487	0.476
Household head receiving TANF 1997:II	0.619	0.605	0.673	0.661	0.613	0.652
Household head receiving TANF, Med, or FS 1997:II	0.778	0.770	0.819	0.815	0.777	0.804
Household head: # of prior violent crime arrests	0.151	0.152	0.140	0.146	0.126	0.131
Household head: # of prior property crime arrests	0.277	0.236	0.246	0.212	0.250	0.258
Household head: # of prior drug crime arrests	0.131	0.130	0.125	0.130	0.096	0.143
Household head: # of prior other crime arrests	0.196	0.182	0.177	0.157	0.134	0.184
Census tract % black	0.827	0.830	0.839	0.840	0.829	0.826
Census tract poverty rate	0.305	0.305	0.316	0.310	0.300	0.309
Property crime rate (beat-level, per 1,000) in 1997	74.6	75.0	74.8	75.1	75.5	73.4
Violent crime rate (beat-level, per 1,000) in 1997	39.0	39.2	39.2	39.1	38.8	38.1
Monthy rent (2013 $)	779	774	775	774	777	751
Monthly fair market rent (2013 $)	1,315	1,315	1,325	1,324	1,315	1,327
Child Level						
Male	0.494	0.499	0.486	0.494	0.504	0.505
Black	0.947	0.949	0.949	0.953	0.942	0.952
Hispanic	0.033	0.029	0.035	0.031	0.037	0.031
Age	8.9	8.9	6.8	6.8	8.6	8.6
# of prior violent crime arrests	0.011	0.011	0.000	0.000	0.009	0.014
# of prior property crime arrests	0.006	0.006	0.000	0.000	0.002	0.005
# of prior drug crime arrests	0.019	0.022	0.000	0.000	0.018	0.017
# of prior other crime arrests	0.013	0.014	0.000	0.000	0.009	0.015
Enrolled in the Chicago Public Schools pre-lottery	0.596	0.596	0.625	0.620	0.595	0.607
Math test score in year prior to lottery	-0.246	-0.217	-0.186	-0.156	-0.234	-0.174
Reading test score in year prior to lottery	-0.221	-0.183	-0.161	-0.132	-0.179	-0.147
GPA in year prior to lottery	1.541	1.586	1.860	1.878	1.514	1.612
# of absences prior to lottery	28.7	28.0	19.3	19.3	29.0	27.0
Fraction nlack in child's school	0.853	0.857	0.864	0.868	0.841	0.856
Fraction Latino in child's school	0.104	0.101	0.100	0.096	0.114	0.099
Fraction eligible for free-lunch in child's school	0.851	0.852	0.886	0.886	0.848	0.855
Average test score in child's school	-0.190	-0.187	-0.177	-0.177	-0.179	-0.177
N (Children or Observations)	36,983	14,447	174,210	68,976	3,290	1,291
Joint test, all coefficients (including missing indicators)						
Chi-squared statistic (clustering at household level)	51.571		41.177		43.153	
p-value	0.451		0.711		0.743	

Notes: Columns 1, 2, 5, and 6, unit of analysis in the top panel is the household; in the bottom panel, the child. Columns 3 and 4, unit of analysis is the person-year.
[1] 10% sample.
[2] Household income includes earnings of all household members (adults and children); estimated tax gain/loss; and TANF and Food Stamps benefits.
Household members' earnings average approximately 55% of total household income.

Table G.3: Lee Bounds: High School Graduation

		Lee Bounds		
	Preferred Estimate	OLS: Tightening Group FEs	Lower Bound	Upper Bound
	(1)	(2)	(3)	(4)
Males age 6-18 at baseline				
ITT estimate	0.0150	0.0153	0.0102	0.0305**
Normal std. error	(0.0088)	(0.0088)	(0.0163)	(0.0147)
Clustered std. error	(0.0094)	(0.0093)		
Control mean	0.3940	0.3940		
Number of individuals	13,183	13,183		
Females age 6-18 at baseline				
ITT estimate	0.0101	0.0113	0.0057	0.0189
Normal std. error	(0.0088)	(0.0087)	(0.0143)	(0.0162)
Clustered std. error	(0.0094)	(0.0093)		
Control mean	0.5766	0.5766		
Number of individuals	13,792	13,792		

Notes: Lee bounds estimation (col 2-4) restricted to children aged 6-18 at baseline, who attended Chicago Public Schools during the post-lottery period (academic years 1998-2011) and have a non-missing exit status. Estimates use deciles of a student's predicted probability high school graduation as the tightening groups. The predicted probabilities are obtained from a regression of high school graduation on all the typical covariates, excluding treatment indicators and pre/post-offer indicators. For comparison, column 2 reports estimates of high school graduation on the treatment X years after offer indicator with FEs for these ten deciles but no other covariates.

** Significant at the 5 percent level.

Table G.4: Lee Bounds: Test Scores

	Preferred Estimate				Lee Bounds	
	Full Sample	Ages 8-17	Last Non-Missing Score[1]	OLS: Tightening Group FEs	Lower Bound	Upper Bound
	(1)	(2)	(3)	(4)	(5)	(6)
Males age 0-6 at baseline						
ITT estimate	0.0369*	0.0351*	0.0309*	0.0355*	-0.3482***	0.3983***
Normal std. error	(0.0077)	(0.0078)	(0.0065)	(0.0077)	(0.0133)	(0.0124)
Clustered std. error	(0.0190)	(0.0191)	(0.0181)			
Control mean	-0.3339	-0.3315	-0.3555	-0.3315		
Number of individuals	8,659	8,596	8,659	8,596		
Number of observations	51,339	49,980	73,294	49,980		
Males age 6-18 at baseline						
ITT estimate	0.0068	0.0061	-0.0087	0.0105	-0.2375***	0.2725***
Normal std. error	(0.0069)	(0.0070)	(0.0035)	(0.0069)	(0.0118)	(0.0114)
Clustered std. error	(0.0152)	(0.0154)	(0.0112)			
Control mean	-0.3248	-0.3179	-0.4082	-0.3179		
Number of individuals	14,348	14,153	14,348	14,153		
Number of observations	68,787	66,792	190,751	66,792		
Females age 0-6 at baseline						
ITT estimate	0.0019	0.0024	-0.0124	0.0009	-0.3415***	0.3256***
Normal std. error	(0.0074)	(0.0074)	(0.0063)	(0.0074)	(0.0118)	(0.0112)
Clustered std. error	(0.0183)	(0.0184)	(0.0173)			
Control mean	-0.1446	-0.1415	-0.1613	-0.1415		
Number of individuals	8,488	8,416	8,488	8,416		
Number of observations	52,107	50,721	72,512	50,721		
Females age 6-18 at baseline						
ITT estimate	0.0168	0.0157	0.0222**	0.0110	-0.2100***	0.2464***
Normal std. error	(0.0064)	(0.0065)	(0.0033)	(0.0064)	(0.0109)	(0.0108)
Clustered std. error	(0.0143)	(0.0145)	(0.0105)			
Control mean	-0.1479	-0.1404	-0.2391	-0.1404		
Number of individuals	14,855	14,701	14,855	14,701		
Number of observations	73,389	71,715	198,509	71,715		

Notes: Lee bounds estimation (col 4-6) restricted to observations where a student would normally have been tested (current age 8-17). Estimates use deciles of a predicted test score as the tightening groups. The predicted test scores are obtained from a regression of test score on all the typical covariates, excluding treatment indicators and pre/post-offer indicators. For comparison, column 4 reports estimates of test score on the treatment X years after offer indicator with FEs for these ten deciles but no other covariates.

[1] Missing test score is replaced with an individual's last non-missing test score.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table G.5: Multiple Hypothesis Testing

Baseline Age	Outcome	Children/ Obs.	CM	ITT	ITT *p*-value Pair-wise	FWER	FDR
		(1)	(2)	(3)	(4)	(5)	(6)
Male							
0-6	Test score	8,659	-0.3339	0.0369*	0.052	0.449	0.311
		[51,339]		(0.0190)			
6-18	Test score	14,348	-0.3248	0.0068	0.655	0.919	0.873
		[68,787]		(0.0152)			
6-18	High school graduation	13,183	0.3940	0.0150	0.109	0.650	0.328
		[13,183]		(0.0094)			
All	Soc. costs, most conservative	33,400	3,084	-161	0.102	0.650	0.328
		[283,091]		(98)			
0-6	Inpatient or emergency claim	9,538	0.2449	-0.0012	0.852	0.919	0.920
		[52,378]		(0.0063)			
6-18	Inpatient or emergency claim	12,526	0.2471	-0.0059	0.324	0.896	0.556
		[56,480]		(0.0060)			
Female							
0-6	Test score	8,488	-0.1446	0.0019	0.919	0.919	0.920
		[52,107]		(0.0183)			
6-18	Test score	14,855	-0.1479	0.0168	0.240	0.880	0.556
		[73,389]		(0.0143)			
6-18	High school graduation	13,792	0.5766	0.0101	0.279	0.892	0.556
		[13,792]		(0.0094)			
All	Soc. costs, most conservative	33,210	574	61**	0.043	0.425	0.311
		[284,057]		(30)			
0-6	Inpatient or emergency claim	9,379	0.2119	0.0018	0.767	0.919	0.920
		[50,549]		(0.0062)			
6-18	Inpatient or emergency claim	16,050	0.3702	0.0025	0.653	0.919	0.873
		[75,526]		(0.0056)			

Notes: Unit of observation is the person-year for all outcomes, except high school graduation which is a person-level cross-section. CM = control mean. ITT = intent-to-treat. See text for discussion of these estimates. FWER = family-wise error rate. FDR = false discovery rate. Standard errors are reported in parentheses and are clustered at the household level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table G.6: Voucher Effects for Males, Age 0-6 at Baseline

Education, Cross-Sectional Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Enrolled in CPS (1998-2011)	12,288	0.845	-0.0003	-0.0005	0.903
			(0.0017)	(0.0033)	
Cumulative GPA (Final Year)	4,332	1.656	-0.0208	-0.0353	1.647
			(0.0311)	(0.0526)	
Cumulative Credits (Final Year)	4,332	12.385	0.2587	0.4377	12.023
			(0.2576)	(0.4360)	
Final Non-missing Math Score	8,655	-0.411	0.0033	0.0057	-0.419
			(0.0203)	(0.0354)	
Final Non-missing Reading Score	8,660	-0.431	0.0429**	0.0750**	-0.473
			(0.0198)	(0.0347)	
Average Math Score (1998-2011)	8,731	-0.353	0.0213	0.0371	-0.367
			(0.0191)	(0.0333)	
Average Reading Score (1998-2011)	8,736	-0.348	0.0334*	0.0583*	-0.374
			(0.0189)	(0.0331)	
Non-missing Final Status	10,374	0.999	-0.0005	-0.0010	1.000
			(0.0009)	(0.0016)	
Attrited (Moved or Enrolled in Private)	10,360	0.329	-0.0464***	-0.0850***	0.324
			(0.0109)	(0.0198)	
Graduated	7,085	0.067	0.0085	0.0149	0.062
			(0.0060)	(0.0104)	
Enrolled 2-year school (public or private)	483	0.261	0.0006	0.0011	0.283
			(0.0496)	(0.0818)	
Enrolled 4-year public school	483	0.264	-0.0093	-0.0153	0.215
			(0.0473)	(0.0779)	
Enrolled 4-year private school	483	0.133	0.0376	0.0620	0.095
			(0.0398)	(0.0658)	

Education, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Enrolled in CPS in Academic Year	12,288	0.505	0.0328***	0.0658***	0.584
			(0.0067)	(0.0132)	
Grade 1 - 12	9,980	0.867	0.0019	0.0031	0.941
			(0.0017)	(0.0031)	
Old for Grade	9,533	0.228	-0.0079	-0.0139	0.254
			(0.0087)	(0.0150)	
Repeat	9,980	0.049	-0.0033*	-0.0059*	0.060
			(0.0017)	(0.0031)	
# Absences	4,699	36.123	-0.2258	-0.3796	37.383
			(0.8843)	(1.4873)	
# Credits	4,699	5.163	0.0381	0.0641	5.108
			(0.0619)	(0.1042)	
GPA in Current Year	4,699	1.677	0.0099	0.0166	1.638
			(0.0296)	(0.0497)	
Tested	8,784	0.942	0.0039	0.0067	0.939
			(0.0032)	(0.0054)	
Composite Test Score	8,659	-0.334	0.0369*	0.0634*	-0.377
			(0.0190)	(0.0325)	
Math Test Score	8,654	-0.330	0.0319	0.0549	-0.371
			(0.0200)	(0.0343)	
Reading Test Score	8,659	-0.334	0.0413**	0.0710**	-0.380
			(0.0198)	(0.0339)	

Crime, Cross-Sectional Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Arrested (1998-2011)	12,288	0.2809	0.0026	0.0051	0.3017
			(0.0092)	(0.0181)	
Sum of social costs (most conservative)	3,453	33,866	-2,641	-4,765	37,146
			(2,117)	(3,827)	
Sum of social costs (least conservative)	3,453	113,244	-26,692	-48,165	152,320
			(22,219)	(40,143)	
Total Violent Crime Arrests	3,453	1.0112	-0.0476	-0.0859	1.1014
			(0.0528)	(0.0954)	
Total Property Crime Arrests	3,453	0.5901	-0.0196	-0.0353	0.5725
			(0.0416)	(0.0751)	
Total Drug Crime Arrests	3,453	0.8849	0.0269	0.0486	0.8560
			(0.0681)	(0.1229)	
Total Other Crimes Arrests	3,453	1.8314	0.0026	0.0047	1.7058
			(0.1050)	(0.1895)	

Crime, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Arrested in Academic Year	12,288	0.1537	-0.0050	-0.0099	0.1740
			(0.0060)	(0.0118)	
Social costs (most conservative)	3,381	15,694	-798	-1,410	17,083
			(929)	(1,645)	
Social costs (least conservative)	3,381	53,926	-10,618	-18,773	70,230
			(10,770)	(19,067)	
# Violent Crime Arrests	3,381	0.4643	-0.0186	-0.0330	0.5139
			(0.0212)	(0.0376)	
# Property Crime Arrests	3,381	0.2745	-0.0044	-0.0078	0.2730
			(0.0188)	(0.0332)	
# Drug Crime Arrests	3,381	0.4219	0.0227	0.0402	0.4017
			(0.0286)	(0.0506)	
# Other Crime Arrests	3,381	0.8620	0.0133	0.0235	0.7965
			(0.0401)	(0.0710)	

Labor, Public Assistance, and Household Composition, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Total Earnings (2013 $)	7,013	517.110	-29.4496	-57.6955	564.831
			(62.5716)	(122.6556)	
Fraction of Year: Employed	7,013	0.068	0.0035	0.0069	0.068
			(0.0052)	(0.0101)	
Fraction of Year: Any Public Assistance	12,288	0.699	0.0184**	0.0368**	0.757
			(0.0075)	(0.0148)	
Fraction of Year: Foodstamps	12,288	0.516	0.0215***	0.0433***	0.563
			(0.0078)	(0.0154)	
Fraction of Year: AFDC/TANF	12,288	0.189	0.0048	0.0088	0.126
			(0.0042)	(0.0084)	
Fraction of Year: Medicaid	12,288	0.679	0.0174**	0.0348**	0.740
			(0.0076)	(0.0150)	
Fraction of Year: Address on File	12,288	0.696	0.0194***	0.0389***	0.751
			(0.0075)	(0.0148)	
# People in HH (annual average)	11,668	3.858	0.0087	0.0141	3.786
			(0.0294)	(0.0526)	
# Children in HH (annual average)	11,668	2.403	0.0294	0.0524	2.269
			(0.0185)	(0.0331)	
September: Any Public Assistance	12,288	0.704	0.0169**	0.0337**	0.761
			(0.0074)	(0.0146)	
September: Foodstamps	12,288	0.522	0.0217***	0.0437***	0.565
			(0.0078)	(0.0153)	
September: AFDC/TANF	12,288	0.205	0.0044	0.0080	0.137
			(0.0043)	(0.0086)	
September: Medicaid	12,288	0.685	0.0156**	0.0310**	0.746
			(0.0075)	(0.0148)	
September: Address on File	12,288	0.700	0.0178**	0.0356**	0.756
			(0.0074)	(0.0146)	
# People in HH (September)	11,614	3.916	0.0189	0.0344	3.821
			(0.0301)	(0.0535)	
# Children in HH (September)	11,614	2.470	0.0334*	0.0599*	2.334
			(0.0191)	(0.0339)	

Health, Cross-Sectional Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Enrolled in Medicaid (2000-2008)	12,288	0.776	0.0008	0.0015	0.826
			(0.0091)	(0.0179)	
Inpatient hospital claim	9,538	0.084	0.0044	0.0080	0.078
			(0.0065)	(0.0120)	
Emergency room claim	9,538	0.589	0.0157	0.0291	0.594
			(0.0119)	(0.0220)	
Inpatient or emergency claim	9,538	0.595	0.0132	0.0244	0.601
			(0.0119)	(0.0220)	
Outpatient claim	9,538	0.963	0.0034	0.0063	0.976
			(0.0044)	(0.0081)	
Injury, inpatient or emergency	9,538	0.395	0.0068	0.0125	0.409
			(0.0116)	(0.0215)	
Asthma	9,538	0.250	-0.0135	-0.0249	0.273
			(0.0101)	(0.0187)	
Routine medical exam	9,538	0.915	0.0027	0.0049	0.936
			(0.0066)	(0.0121)	

Health, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Enrolled in Medicaid in Academic Year	12,288	0.471	0.0108	0.0209	0.510
			(0.0086)	(0.0171)	
Inpatient hospital claim	9,538	0.022	0.0003	0.0009	0.020
			(0.0021)	(0.0039)	
Emergency room claim	9,538	0.240	0.0001	0.0010	0.237
			(0.0062)	(0.0113)	
Inpatient or emergency claim	9,538	0.245	-0.0012	-0.0014	0.242
			(0.0063)	(0.0114)	
Outpatient claim	9,538	0.880	0.0029	0.0046	0.888
			(0.0047)	(0.0085)	
Injury, inpatient or emergency	9,538	0.114	-0.0046	-0.0086	0.120
			(0.0039)	(0.0071)	
Asthma	9,538	0.132	0.0055	0.0106	0.147
			(0.0074)	(0.0135)	
Routine medical exam	9,538	0.679	0.0068	0.0119	0.707
			(0.0062)	(0.0114)	

Table G.7: Voucher Effects for Males, Age 6-18 Baseline

Education, Cross-Sectional Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Enrolled in CPS (1998-2011)	21,112	0.812	-0.0003	-0.0006	0.859
			(0.0020)	(0.0042)	
Cumulative GPA (Final Year)	9,383	1.425	0.0184	0.0345	1.431
			(0.0188)	(0.0351)	
Cumulative Credits (Final Year)	9,383	16.379	0.4019*	0.7529*	16.624
			(0.2201)	(0.4120)	
Final Non-missing Math Score	14,278	-0.444	-0.0126	-0.0240	-0.428
			(0.0133)	(0.0252)	
Final Non-missing Reading Score	14,313	-0.461	-0.0249**	-0.0474**	-0.450
			(0.0124)	(0.0236)	
Average Math Score (1998-2011)	14,513	-0.363	0.0022	0.0042	-0.342
			(0.0120)	(0.0229)	
Average Reading Score (1998-2011)	14,573	-0.363	-0.0103	-0.0196	-0.343
			(0.0113)	(0.0216)	
Non-missing Final Status	17,168	0.999	-0.0001	-0.0002	0.998
			(0.0005)	(0.0011)	
Attrited (Moved or Enrolled in Private)	17,151	0.235	-0.0152**	-0.0301**	0.219
			(0.0075)	(0.0147)	
Graduated	13,183	0.394	0.0150	0.0286	0.412
			(0.0094)	(0.0178)	
Enrolled 2-year school (public or private)	5,308	0.392	0.0056	0.0103	0.395
			(0.0149)	(0.0275)	
Enrolled 4-year public school	5,308	0.193	-0.0060	-0.0110	0.193
			(0.0116)	(0.0214)	
Enrolled 4-year private school	5,308	0.150	-0.0032	-0.0059	0.146
			(0.0105)	(0.0193)	

Education, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Enrolled in CPS in Academic Year	21,112	0.324	0.0068**	0.0140**	0.290
			(0.0032)	(0.0067)	
Grade 1 - 12	15,972	0.996	-0.0009*	-0.0016	0.999
			(0.0005)	(0.0010)	
Old for Grade	15,911	0.319	0.0016	0.0029	0.373
			(0.0083)	(0.0151)	
Repeat	15,972	0.077	-0.0035	-0.0066	0.092
			(0.0025)	(0.0046)	
# Absences	11,991	26.571	-0.0709	-0.1122	26.044
			(0.4294)	(0.7648)	
# Credits	11,991	4.982	0.0444	0.0803	5.107
			(0.0429)	(0.0763)	
GPA in Current Year	11,991	1.478	0.0032	0.0044	1.516
			(0.0193)	(0.0344)	
Tested	15,320	0.881	0.0096**	0.0178***	0.865
			(0.0038)	(0.0069)	
Composite Test Score	14,348	-0.325	0.0068	0.0126	-0.364
			(0.0152)	(0.0273)	
Math Test Score	14,268	-0.318	0.0076	0.0136	-0.353
			(0.0177)	(0.0319)	
Reading Test Score	14,302	-0.326	0.0019	0.0043	-0.372
			(0.0154)	(0.0278)	

Crime, Cross-Sectional Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Arrested (1998-2011)	21,112	0.5977	0.0072	0.0149	0.6083
			(0.0076)	(0.0158)	
Sum of social costs (most conservative)	12,638	60,791	-2,509	-5,009	61,595
			(1,873)	(3,743)	
Sum of social costs (least conservative)	12,638	276,352	-33,834	-67,549	255,389
			(21,569)	(43,068)	
Total Violent Crime Arrests	12,638	1.3493	-0.0369	-0.0737	1.3942
			(0.0351)	(0.0700)	
Total Property Crime Arrests	12,638	0.5367	0.0205	0.0410	0.5114
			(0.0234)	(0.0467)	
Total Drug Crime Arrests	12,638	2.6052	-0.1072	-0.2141	2.7018
			(0.0686)	(0.1371)	
Total Other Crimes Arrests	12,638	3.4704	-0.0046	-0.0092	3.5112
			(0.0960)	(0.1916)	

Crime, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Arrested in Academic Year	21,112	0.2223	0.0004	0.0008	0.2336
			(0.0042)	(0.0088)	
Social costs (most conservative)	12,617	14,473	-683	-1,397*	15,335
			(417)	(836)	
Social costs (least conservative)	12,617	66,011	-10,378*	-21,540**	66,497
			(5,311)	(10,670)	
# Violent Crime Arrests	12,617	0.3199	-0.0077	-0.0154	0.3300
			(0.0074)	(0.0148)	
# Property Crime Arrests	12,617	0.1266	0.0030	0.0051	0.1242
			(0.0055)	(0.0110)	
# Drug Crime Arrests	12,617	0.6231	-0.0167	-0.0313	0.6301
			(0.0129)	(0.0257)	
# Other Crime Arrests	12,617	0.8303	0.0007	0.0014	0.8839
			(0.0183)	(0.0367)	

Labor, Public Assistance, and Household Composition, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Total Earnings (2013 $)	21,112	3,889.7830	38.1019	91.7312	3,596.7602
			(117.3116)	(250.6524)	
Fraction of Year: Employed	21,112	0.2579	0.0042	0.0095	0.2488
			(0.0046)	(0.0099)	
Fraction of Year: Any Public Assistance	21,112	0.5143	0.0158***	0.0337***	0.5306
			(0.0054)	(0.0112)	
Fraction of Year: Foodstamps	21,112	0.3624	0.0163***	0.0353***	0.3580
			(0.0048)	(0.0101)	
Fraction of Year: AFDC/TANF	21,112	0.1295	-0.0022	-0.0037	0.0809
			(0.0022)	(0.0047)	
Fraction of Year: Medicaid	21,112	0.4349	0.0099**	0.0213**	0.4394
			(0.0051)	(0.0107)	
Fraction of Year: Address on File	21,112	0.5085	0.0159***	0.0340***	0.5246
			(0.0054)	(0.0112)	
# People in HH (annual average)	19,146	4.0612	0.0388	0.0718	3.8398
			(0.0504)	(0.0938)	
# Children in HH (annual average)	19,146	2.0499	-0.0396**	-0.0724**	1.7924
			(0.0170)	(0.0315)	
September: Any Public Assistance	21,112	0.5235	0.0153***	0.0325***	0.5408
			(0.0054)	(0.0113)	
September: Foodstamps	21,112	0.3741	0.0147***	0.0318***	0.3691
			(0.0049)	(0.0103)	
September: AFDC/TANF	21,112	0.1441	-0.0023	-0.0041	0.0915
			(0.0023)	(0.0050)	
September: Medicaid	21,112	0.4474	0.0098*	0.0208*	0.4532
			(0.0051)	(0.0107)	
September: Address on File	21,112	0.5180	0.0156***	0.0331***	0.5350
			(0.0054)	(0.0113)	
# People in HH (September)	18,937	4.1602	0.0572	0.1044	3.9145
			(0.0523)	(0.0964)	
# Children in HH (September)	18,937	2.2005	-0.0329*	-0.0598*	1.9394
			(0.0182)	(0.0335)	

Health, Cross-Sectional Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Enrolled in Medicaid (2000-2008)	21,112	0.591	0.0077	0.0160	0.642
			(0.0076)	(0.0157)	
Inpatient hospital claim	12,526	0.095	0.0008	0.0015	0.090
			(0.0058)	(0.0110)	
Emergency room claim	12,526	0.529	0.0160	0.0300	0.534
			(0.0103)	(0.0193)	
Inpatient or emergency claim	12,526	0.536	0.0178*	0.0336*	0.540
			(0.0103)	(0.0193)	
Outpatient claim	12,526	0.887	-0.0001	-0.0001	0.899
			(0.0062)	(0.0117)	
Injury, inpatient or emergency	12,526	0.378	0.0033	0.0061	0.384
			(0.0100)	(0.0189)	
Asthma	12,526	0.183	0.0026	0.0049	0.180
			(0.0080)	(0.0150)	
Routine medical exam	12,526	0.706	0.0006	0.0011	0.730
			(0.0083)	(0.0155)	

Health, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Enrolled in Medicaid in Academic Year	21,112	0.295	0.0113**	0.0237**	0.304
			(0.0052)	(0.0109)	
Inpatient hospital claim	12,526	0.031	-0.0003	-0.0003	0.031
			(0.0024)	(0.0044)	
Emergency room claim	12,526	0.241	-0.0061	-0.0110	0.249
			(0.0059)	(0.0111)	
Inpatient or emergency claim	12,526	0.247	-0.0059	-0.0105	0.255
			(0.0060)	(0.0112)	
Outpatient claim	12,526	0.759	0.0040	0.0070	0.742
			(0.0059)	(0.0112)	
Injury, inpatient or emergency	12,526	0.133	-0.0033	-0.0060	0.137
			(0.0042)	(0.0080)	
Asthma	12,526	0.104	-0.0065	-0.0117	0.106
			(0.0058)	(0.0108)	
Routine medical exam	12,526	0.456	0.0006	0.0001	0.450
			(0.0059)	(0.0112)	

Table G.8: Voucher Effects for Females, Age 0-6 Baseline

Education, Cross-Sectional Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Enrolled in CPS (1998-2011)	11,985	0.843	0.0004	0.0008	0.906
			(0.0014)	(0.0028)	
Cumulative GPA (Final Year)	4,529	2.084	0.0567*	0.1002*	2.062
			(0.0321)	(0.0568)	
Cumulative Credits (Final Year)	4,529	14.988	0.2336	0.4129	15.262
			(0.2481)	(0.4386)	
Final Non-missing Math Score	8,484	-0.311	-0.0192	-0.0339	-0.302
			(0.0188)	(0.0333)	
Final Non-missing Reading Score	8,488	-0.182	0.0037	0.0065	-0.178
			(0.0193)	(0.0343)	
Average Math Score (1998-2011)	8,573	-0.225	-0.0150	-0.0265	-0.204
			(0.0183)	(0.0324)	
Average Reading Score (1998-2011)	8,576	-0.096	0.0102	0.0180	-0.093
			(0.0184)	(0.0326)	
Non-missing Final Status	10,096	0.998	0.0005	0.0009	0.999
			(0.0008)	(0.0015)	
Attrited (Moved or Enrolled in Private)	10,081	0.315	-0.0274**	-0.0501**	0.303
			(0.0112)	(0.0205)	
Graduated	6,983	0.115	-0.0012	-0.0022	0.136
			(0.0067)	(0.0118)	
Enrolled 2-year school (public or private)	815	0.343	0.0024	0.0038	0.321
			(0.0400)	(0.0632)	
Enrolled 4-year public school	815	0.291	0.0002	0.0003	0.284
			(0.0378)	(0.0596)	
Enrolled 4-year private school	815	0.167	-0.0128	-0.0202	0.205
			(0.0312)	(0.0493)	

Education, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Enrolled in CPS in Academic Year	11,985	0.509	0.0203***	0.0398***	0.601
			(0.0068)	(0.0135)	
Grade 1 - 12	9,701	0.870	0.0031*	0.0056*	0.940
			(0.0017)	(0.0032)	
Old for Grade	9,218	0.156	0.0111	0.0197	0.156
			(0.0081)	(0.0141)	
Repeat	9,701	0.032	0.0022	0.0038	0.034
			(0.0015)	(0.0027)	
# Absences	4,844	33.399	-1.6719**	-2.9129**	33.606
			(0.7850)	(1.3721)	
# Credits	4,844	5.788	0.0559	0.0974	5.777
			(0.0494)	(0.0862)	
GPA in Current Year	4,844	2.122	0.0371	0.0647	2.103
			(0.0307)	(0.0536)	
Tested	8,571	0.958	0.0073***	0.0128***	0.955
			(0.0025)	(0.0044)	
Composite Test Score	8,488	-0.145	0.0019	0.0029	-0.151
			(0.0183)	(0.0316)	
Math Test Score	8,484	-0.206	-0.0094	-0.0165	-0.210
			(0.0193)	(0.0333)	
Reading Test Score	8,488	-0.081	0.0113	0.0190	-0.090
			(0.0192)	(0.0331)	

Crime, Cross-Sectional Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Arrested (1998-2011)	11,985	0.1466	0.0007	0.0013	0.1631
			(0.0075)	(0.0147)	
Sum of social costs (most conservative)	1,758	15,534	863	1,534	17,004
			(2,228)	(3,959)	
Sum of social costs (least conservative)	1,758	25,644	18,758	33,358	30,452
			(21,864)	(38,849)	
Total Violent Crime Arrests	1,758	0.7186	-0.0660	-0.1173	0.8055
			(0.0533)	(0.0950)	
Total Property Crime Arrests	1,758	0.4341	-0.0166	-0.0295	0.4649
			(0.0387)	(0.0689)	
Total Drug Crime Arrests	1,758	0.1044	-0.0097	-0.0172	0.0922
			(0.0283)	(0.0504)	
Total Other Crimes Arrests	1,758	0.6228	0.0126	0.0225	0.6402
			(0.0616)	(0.1096)	

Crime, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Arrested in Academic Year	11,985	0.0539	-0.0001	-0.0002	0.0612
			(0.0032)	(0.0063)	
Social costs (most conservative)	1,706	10,518	722	1,262	11,428
			(1,156)	(2,019)	
Social costs (least conservative)	1,706	17,830	11,673	20,401	24,393
			(13,110)	(22,871)	
# Violent Crime Arrests	1,706	0.4833	-0.0332	-0.0580	0.5211
			(0.0297)	(0.0519)	
# Property Crime Arrests	1,706	0.3060	0.0034	0.0060	0.2982
			(0.0252)	(0.0441)	
# Drug Crime Arrests	1,706	0.0734	-0.0063	-0.0111	0.0617
			(0.0157)	(0.0275)	
# Other Crime Arrests	1,706	0.4370	0.0257	0.0450	0.4133
			(0.0363)	(0.0634)	

Labor, Public Assistance, and Household Composition, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Total Earnings (2013 $)	6,788	712.718	-69.5465	-135.5828	842.742
			(62.0875)	(121.1878)	
Fraction of Year: Employed	6,788	0.107	-0.0055	-0.0107	0.117
			(0.0064)	(0.0125)	
Fraction of Year: Any Public Assistance	11,985	0.698	0.0248***	0.0498***	0.740
			(0.0076)	(0.0149)	
Fraction of Year: Foodstamps	11,985	0.520	0.0281***	0.0569***	0.549
			(0.0079)	(0.0155)	
Fraction of Year: AFDC/TANF	11,985	0.199	-0.0020	-0.0038	0.138
			(0.0042)	(0.0085)	
Fraction of Year: Medicaid	11,985	0.678	0.0251***	0.0502***	0.722
			(0.0077)	(0.0151)	
Fraction of Year: Address on File	11,985	0.696	0.0253***	0.0507***	0.739
			(0.0076)	(0.0149)	
# People in HH (annual average)	11,377	3.867	-0.0591**	-0.1069**	3.809
			(0.0297)	(0.0536)	
# Children in HH (annual average)	11,377	2.413	-0.0050	-0.0095	2.279
			(0.0186)	(0.0336)	
September: Any Public Assistance	11,985	0.703	0.0239***	0.0478***	0.745
			(0.0075)	(0.0148)	
September: Foodstamps	11,985	0.528	0.0270***	0.0544***	0.555
			(0.0079)	(0.0155)	
September: AFDC/TANF	11,985	0.215	-0.0020	-0.0040	0.150
			(0.0044)	(0.0087)	
September: Medicaid	11,985	0.683	0.0241***	0.0478***	0.727
			(0.0076)	(0.0150)	
September: Address on File	11,985	0.701	0.0244***	0.0488***	0.743
			(0.0075)	(0.0148)	
# People in HH (September)	11,331	3.926	-0.0552*	-0.0983*	3.858
			(0.0302)	(0.0542)	
# Children in HH (September)	11,331	2.479	0.0007	0.0013	2.346
			(0.0194)	(0.0349)	

Health, Cross-Sectional Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Enrolled in Medicaid (2000-2008)	11,985	0.783	0.0025	0.0048	0.825
			(0.0094)	(0.0185)	
Inpatient hospital claim	9,379	0.071	-0.0061	-0.0112	0.086
			(0.0060)	(0.0111)	
Emergency room claim	9,379	0.535	0.0234*	0.0434*	0.551
			(0.0121)	(0.0224)	
Inpatient or emergency claim	9,379	0.543	0.0226*	0.0419*	0.558
			(0.0121)	(0.0224)	
Outpatient claim	9,379	0.966	-0.0044	-0.0082	0.979
			(0.0048)	(0.0088)	
Injury, inpatient or emergency	9,379	0.296	0.0057	0.0106	0.315
			(0.0111)	(0.0205)	
Asthma	9,379	0.197	0.0065	0.0121	0.206
			(0.0097)	(0.0180)	
Routine medical exam	9,379	0.911	-0.0046	-0.0086	0.943
			(0.0070)	(0.0130)	

Health, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Enrolled in Medicaid in Academic Year	11,985	0.466	0.0145*	0.0303*	0.500
			(0.0088)	(0.0174)	
Inpatient hospital claim	9,379	0.018	-0.0013	-0.0022	0.022
			(0.0019)	(0.0035)	
Emergency room claim	9,379	0.208	0.0020	0.0034	0.215
			(0.0061)	(0.0111)	
Inpatient or emergency claim	9,379	0.212	0.0018	0.0032	0.220
			(0.0062)	(0.0113)	
Outpatient claim	9,379	0.876	-0.0049	-0.0099	0.899
			(0.0050)	(0.0090)	
Injury, inpatient or emergency	9,379	0.076	-0.0006	-0.0009	0.085
			(0.0033)	(0.0059)	
Asthma	9,379	0.098	0.0011	0.0017	0.112
			(0.0063)	(0.0113)	
Routine medical exam	9,379	0.674	-0.0075	-0.0151	0.710
			(0.0063)	(0.0114)	

Table G.9: Voucher Effects for Females, Age 6-18 Baseline

Education, Cross-Sectional Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Enrolled in CPS (1998-2011)	21,225	0.820	0.0014	0.0029	0.847
			(0.0019)	(0.0040)	
Cumulative GPA (Final Year)	11,074	1.880	0.0039	0.0073	1.857
			(0.0180)	(0.0337)	
Cumulative Credits (Final Year)	11,074	19.544	0.3041	0.5696	19.627
			(0.1867)	(0.3494)	
Final Non-missing Math Score	14,790	-0.349	0.0257**	0.0499**	-0.421
			(0.0124)	(0.0241)	
Final Non-missing Reading Score	14,837	-0.255	0.0215*	0.0418*	-0.319
			(0.0121)	(0.0236)	
Average Math Score (1998-2011)	14,948	-0.222	0.0338***	0.0659***	-0.291
			(0.0111)	(0.0218)	
Average Reading Score (1998-2011)	14,998	-0.148	0.0170	0.0331	-0.197
			(0.0109)	(0.0213)	
Non-missing Final Status	17,336	0.999	-0.0004	-0.0008	1.000
			(0.0005)	(0.0011)	
Attrited (Moved or Enrolled in Private)	17,323	0.208	-0.0154**	-0.0306**	0.174
			(0.0072)	(0.0142)	
Graduated	13,792	0.577	0.0101	0.0190	0.585
			(0.0094)	(0.0176)	
Enrolled 2-year school (public or private)	8,009	0.517	-0.0162	-0.0300	0.533
			(0.0127)	(0.0235)	
Enrolled 4-year public school	8,009	0.260	0.0005	0.0010	0.240
			(0.0107)	(0.0198)	
Enrolled 4-year private school	8,009	0.197	-0.0076	-0.0142	0.197
			(0.0097)	(0.0180)	

Education, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Enrolled in CPS in Academic Year	21,225	0.334	0.0151***	0.0322***	0.261
			(0.0031)	(0.0065)	
Grade 1 - 12	16,394	0.998	0.0001	0.0002	0.999
			(0.0003)	(0.0007)	
Old for Grade	16,357	0.225	-0.0115	-0.0213	0.266
			(0.0076)	(0.0146)	
Repeat	16,394	0.052	-0.0041**	-0.0079*	0.059
			(0.0021)	(0.0041)	
# Absences	13,160	24.352	0.0169	0.0114	24.721
			(0.3699)	(0.7018)	
# Credits	13,160	5.675	0.0197	0.0380	5.760
			(0.0330)	(0.0626)	
GPA in Current Year	13,160	1.920	0.0066	0.0121	1.912
			(0.0194)	(0.0368)	
Tested	15,599	0.909	0.0030	0.0052	0.902
			(0.0033)	(0.0065)	
Composite Test Score	14,855	-0.148	0.0168	0.0300	-0.208
			(0.0143)	(0.0273)	
Math Test Score	14,779	-0.183	0.0314*	0.0575*	-0.255
			(0.0163)	(0.0311)	
Reading Test Score	14,824	-0.112	0.0066	0.0116	-0.171
			(0.0151)	(0.0289)	

Crime, Cross-Sectional Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Arrested (1998-2011)	21,225	0.3479	0.0105	0.0218	0.3580
			(0.0077)	(0.0160)	
Sum of social costs (most conservative)	7,434	18,809	2,064**	4,037**	18,380
			(922)	(1,800)	
Sum of social costs (least conservative)	7,434	43,281	-1,596	-3,121	45,963
			(9,386)	(18,360)	
Total Violent Crime Arrests	7,434	0.7600	0.0704**	0.1378**	0.7365
			(0.0322)	(0.0629)	
Total Property Crime Arrests	7,434	0.6079	-0.0211	-0.0412	0.5996
			(0.0267)	(0.0522)	
Total Drug Crime Arrests	7,434	0.4050	0.0457	0.0894	0.3515
			(0.0352)	(0.0690)	
Total Other Crimes Arrests	7,434	1.0064	0.0334	0.0653	1.0377
			(0.0501)	(0.0980)	

Crime, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Arrested in Academic Year	21,225	0.0640	0.0045**	0.0096**	0.0658
			(0.0021)	(0.0044)	
Social costs (most conservative)	7,410	8,977	371	590	9,378
			(341)	(677)	
Social costs (least conservative)	7,410	20,803	-5,913	-12,875	28,339
			(4,025)	(8,100)	
# Violent Crime Arrests	7,410	0.3616	0.0261**	0.0515**	0.3447
			(0.0124)	(0.0242)	
# Property Crime Arrests	7,410	0.2909	-0.0177	-0.0345	0.2815
			(0.0118)	(0.0231)	
# Drug Crime Arrests	7,410	0.1955	0.0114	0.0206	0.1773
			(0.0139)	(0.0273)	
# Other Crime Arrests	7,410	0.4848	-0.0068	-0.0133	0.5353
			(0.0176)	(0.0347)	

Labor, Public Assistance, and Household Composition, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Total Earnings (2013 $)	21,225	5,463.2029	-71.9918	-145.4267	5,800.1344
			(117.8536)	(249.3975)	
Fraction of Year: Employed	21,225	0.3791	-0.0033	-0.0068	0.3951
			(0.0049)	(0.0103)	
Fraction of Year: Any Public Assistance	21,225	0.6273	0.0215***	0.0458***	0.6610
			(0.0056)	(0.0119)	
Fraction of Year: Foodstamps	21,225	0.4777	0.0214***	0.0468***	0.4995
			(0.0054)	(0.0114)	
Fraction of Year: AFDC/TANF	21,225	0.1757	0.0026	0.0062	0.1216
			(0.0028)	(0.0061)	
Fraction of Year: Medicaid	21,225	0.5690	0.0148***	0.0316***	0.5950
			(0.0057)	(0.0120)	
Fraction of Year: Address on File	21,225	0.6250	0.0216***	0.0462***	0.6580
			(0.0057)	(0.0119)	
# People in HH (annual average)	20,019	3.4677	0.0052	0.0086	3.2240
			(0.0265)	(0.0511)	
# Children in HH (annual average)	20,019	1.7297	0.0037	0.0073	1.4282
			(0.0150)	(0.0290)	
September: Any Public Assistance	21,225	0.6323	0.0203***	0.0434***	0.6661
			(0.0056)	(0.0119)	
September: Foodstamps	21,225	0.4842	0.0210***	0.0458***	0.5032
			(0.0055)	(0.0115)	
September: AFDC/TANF	21,225	0.1884	0.0023	0.0055	0.1306
			(0.0029)	(0.0063)	
September: Medicaid	21,225	0.5756	0.0142**	0.0301**	0.6016
			(0.0056)	(0.0119)	
September: Address on File	21,225	0.6298	0.0204***	0.0436***	0.6632
			(0.0056)	(0.0119)	
# People in HH (September)	19,899	3.5856	0.0141	0.0254	3.3237
			(0.0274)	(0.0523)	
# Children in HH (September)	19,899	1.8503	0.0126	0.0243	1.5354
			(0.0155)	(0.0298)	

Health, Cross-Sectional Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Enrolled in Medicaid (2000-2008)	21,225	0.754	0.0096	0.0200	0.792
			(0.0070)	(0.0146)	
Inpatient hospital claim	16,050	0.418	0.0153*	0.0299*	0.408
			(0.0082)	(0.0160)	
Emergency room claim	16,050	0.626	0.0138	0.0269	0.629
			(0.0088)	(0.0172)	
Inpatient or emergency claim	16,050	0.699	0.0239***	0.0467***	0.691
			(0.0081)	(0.0159)	
Outpatient claim	16,050	0.953	0.0003	0.0006	0.960
			(0.0039)	(0.0075)	
Injury, inpatient or emergency	16,050	0.310	0.0094	0.0184	0.306
			(0.0083)	(0.0163)	
Asthma	16,050	0.191	0.0022	0.0043	0.199
			(0.0072)	(0.0140)	
Routine medical exam	16,050	0.646	0.0020	0.0039	0.662
			(0.0080)	(0.0157)	

Health, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Enrolled in Medicaid in Academic Year	21,225	0.394	0.0112*	0.0239*	0.417
			(0.0058)	(0.0124)	
Inpatient hospital claim	16,050	0.137	0.0029	0.0053	0.140
			(0.0032)	(0.0063)	
Emergency room claim	16,050	0.311	0.0000	0.0001	0.324
			(0.0055)	(0.0108)	
Inpatient or emergency claim	16,050	0.370	0.0025	0.0047	0.382
			(0.0056)	(0.0108)	
Outpatient claim	16,050	0.861	0.0059	0.0117	0.859
			(0.0040)	(0.0079)	
Injury, inpatient or emergency	16,050	0.097	0.0001	-0.0002	0.099
			(0.0031)	(0.0060)	
Asthma	16,050	0.098	0.0018	0.0033	0.107
			(0.0049)	(0.0095)	
Routine medical exam	16,050	0.370	0.0055	0.0103	0.363
			(0.0046)	(0.0089)	

Table G.10: Housing Voucher Effect on Test Scores, Cross-Sectional Models

Baseline							ITT Quantile Treatment Effect					IV Quantile Treatment Effect[1]				
Age	Outcome	Children	CM	ITT	IV	CCM	10	25	50	75	90	10	25	50	75	90
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Male																
0-6	Last observed test score	8,659	-0.4192	0.0255	0.0448	-0.4435	0.0025	0.0086	0.0146	0.0526*	0.0406	0.0467	0.0381	0.0411	0.0555	0.0176
				(0.0187)	(0.0329)		(0.0254)	(0.0235)	(0.0215)	(0.0297)	(0.0339)	(0.0344)	(0.0302)	(0.0316)	(0.0438)	(0.0507)
	Average test score	8,659	-0.3490	0.0267	0.0469	-0.3678	-0.0018	0.0359*	0.0238	0.0392	0.0140	0.0276	0.0614*	0.0467	0.0616	0.0187
				(0.0185)	(0.0327)		(0.0277)	(0.0210)	(0.0209)	(0.0287)	(0.0327)	(0.0353)	(0.0333)	(0.0337)	(0.0466)	(0.0494)
	Has at least one test score	12,288	0.7022	0.0110	0.0218	0.7799										
				(0.0100)	(0.0197)											
6-18	Last observed test score	14,348	-0.4573	0.0066	0.0126	-0.4848	0.0239	0.0077	0.0029	0.0142	-0.0185	0.0426	0.0173	0.0020	-0.0021	-0.0187
				(0.0142)	(0.0270)		(0.0232)	(0.0128)	(0.0123)	(0.0202)	(0.0345)	(0.0302)	(0.0220)	(0.0202)	(0.0350)	(0.0432)
	Average test score	14,348	-0.3608	0.0185	0.0353	-0.3867	0.0282	0.0061	0.0107	0.0145	0.0306	0.0311	0.0334	0.0224	0.0198	0.0589*
				(0.0143)	(0.0273)		(0.0232)	(0.0162)	(0.0154)	(0.0208)	(0.0257)	(0.0296)	(0.0220)	(0.0265)	(0.0300)	(0.0354)
	Has at least one test score	21,112	0.6762	0.0141**	0.0294**	0.7234										
				(0.0060)	(0.0125)											
Female																
0-6	Last observed test score	8,488	-0.2419	-0.0037	-0.0065	-0.2421	0.0232	0.0244	0.0138	-0.0323	-0.0914***	0.0263	0.0126	-0.0045	-0.0082	-0.0573
				(0.0176)	(0.0314)		(0.0274)	(0.0180)	(0.0172)	(0.0258)	(0.0331)	(0.0332)	(0.0287)	(0.0300)	(0.0361)	(0.0522)
	Average test score	8,488	-0.1604	0.0047	0.0084	-0.1615	0.0382	0.0142	0.0161	-0.0127	-0.0321	0.0433	0.0316	0.0196	-0.0073	-0.0109
				(0.0176)	(0.0313)		(0.0290)	(0.0253)	(0.0206)	(0.0232)	(0.0295)	(0.0385)	(0.0305)	(0.0328)	(0.0443)	(0.0476)
	Has at least one test score	11,985	0.7023	0.0217**	0.0429**	0.7693										
				(0.0101)	(0.0198)											
6-18	Last observed test score	14,855	-0.3033	0.0205	0.0397	-0.3619	0.0441**	0.0193	0.0093	0.0099	0.0431	0.0499**	0.0355**	0.0294	0.0353	0.0367
				(0.0136)	(0.0264)		(0.0172)	(0.0134)	(0.0146)	(0.0198)	(0.0359)	(0.0236)	(0.0169)	(0.0227)	(0.0275)	(0.0477)
	Average test score	14,855	-0.1832	0.0206	0.0401	-0.2324	0.0220	0.0226*	0.0172	0.0239	0.0331	0.0467*	0.0454**	0.0366	0.0479	0.0368
				(0.0135)	(0.0263)		(0.0186)	(0.0136)	(0.0143)	(0.0179)	(0.0236)	(0.0278)	(0.0208)	(0.0245)	(0.0332)	(0.0423)
	Has at least one test score	21,225	0.7006	0.0099*	0.0206*	0.7308										
				(0.0058)	(0.0122)											

Notes: The five ITT quantile treatment effects for a given outcome and sample are estimated simultaneously using Stata's qreg command. Standard errors for the QTE are bootstrapped (100 reps). Stata does not readily incoporate a cluster boostrap approach with sqreg. Investigations comparing a simple bootstrap and clustered bootstrap for a given single quantile suggest the standard errors are very similar in this setting. All regressions include controls for HHH disability status and age at baseline; pre-lottery employment, public assistance receipt, and criminal activity; child's age and pre-lottery enrollment and school lunch status; baseline neighborhood poverty rate; and an indicator for being an only child.

[1] Abadie, Angrist, Imbens (2002) estimator. Standard errors are bootstrapped (100 reps).

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table G.11: Housing Voucher Effect on Geographic Outcomes (IDHS Data)

	Children/ Obs. (1)	CM (2)	ITT (3)	IV (4)	CCM (5)
Valid geocoded address on file	66,610	0.663	0.0189***	0.0393***	0.694
	[932,540]		(0.0040)	(0.0081)	
Miles from baseline address	60,305	8.060	-0.7012*	-1.2962*	8.221
	[602,553]		(0.4152)	(0.7644)	
Living in IL	62,182	0.998	0.0007**	0.0014**	0.998
	[621,023]		(0.0003)	(0.0006)	
Living in Cook County, IL	62,182	0.942	0.0109***	0.0203***	0.944
	[621,023]		(0.0028)	(0.0051)	
Poverty rate > 20%[1,2]	61,183	0.737	-0.0142***	-0.0261***	0.758
	[514,618]		(0.0046)	(0.0085)	
Poverty rate[1,2]	61,183	0.304	-0.0076***	-0.0140***	0.307
	[514,618]		(0.0015)	(0.0027)	
Fraction black[1,2]	61,183	0.812	0.0042	0.0079	0.836
	[514,618]		(0.0027)	(0.0050)	
Social capital[1,3]	58,357	3.756	0.0065**	0.0115**	3.759
	[517,224]		(0.0025)	(0.0045)	
Collective efficacy[1,3]	58,357	3.494	0.0034*	0.0061*	3.500
	[517,224]		(0.0019)	(0.0034)	
Violent crime rate (per 1,000)[4]	58,551	29.765	-0.2051	-0.3461	28.201
	[523,839]		(0.1466)	(0.2605)	
Property crime rate (per 1,000)[4]	58,551	65.913	-0.1685	-0.2524	64.637
	[523,839]		(0.2700)	(0.4791)	

Notes: Unit of observation is the person-year. Standard errors are reported in parentheses and are clustered at the household level.

[1] Measured at the Census tract level.

[2] Data from the decennial 1990 and 2000 censuses and the American Community Surveys for 2005-9 (interpolating values for inter-censal years).

[3] Data from the Project on Human Development in Chicago Neighborhoods (PHCDN) Community Survey.

[4] Data from annual beat-level crime panel from the Chicago Police Department.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table G.12: Estimated Effects of Cash Transfers on Education, Criminal Behavior, and Health for Inframarginal Households

Baseline Age	Outcome	Baseline Rent > FMR - $50			Baseline Rent > FMR		
		Individuals	CM	Subsidy = ΔC	Individuals	CM	Subsidy = ΔC
		(1)	(2)	(3)	(4)	(5)	(6)
Male							
0-6	Test score	490	-0.2424	0.0145	338	-0.2519	0.0166
				(0.0116)			(0.0147)
6-18	Test score	720	-0.2616	-0.0007	498	-0.2725	-0.0080
				(0.0096)			(0.0109)
6-18	High school graduation	661	0.4134	0.0013	461	0.4387	0.0050
				(0.0101)			(0.0125)
All	Soc. costs, most conservative	1,752	2,926	-79	1,221	2,859	-153**
				(67)			(69)
0-6	Inpatient or emergency claim	543	0.2613	-0.0018	382	0.2676	-0.0063
				(0.0040)			(0.0043)
6-18	Inpatient or emergency claim	633	0.2367	0.0008	441	0.2204	0.0075
				(0.0035)			(0.0046)
Female							
0-6	Test score	484	-0.0146	-0.0045	328	0.0490	-0.0255*
				(0.0119)			(0.0143)
6-18	Test score	755	-0.0668	-0.0158	543	-0.0849	-0.0240*
				(0.0127)			(0.0144)
6-18	High school graduation	702	0.6357	-0.0052	505	0.6589	-0.0065
				(0.0095)			(0.0108)
All	Soc. costs, most conservative	1,757	643	6	1,220	594	7
				(27)			(31)
0-6	Inpatient or emergency claim	532	0.2426	0.0010	360	0.2446	-0.0007
				(0.0035)			(0.0043)
6-18	Inpatient or emergency claim	776	0.3716	-0.0006	537	0.3824	-0.0069
				(0.0043)			(0.0045)

Notes: Unit of observation is the person-year for all outcomes, except high school graduation which is a person-level cross-section. IV estimates shown are re-scaled by the non-housing value of the voucher (ΔC) in thousands of 2013 $. Standard errors are reported in parentheses and are clustered at the household level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table G.13: Housing Voucher Effects on Older Child's School Characteristics

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Males age 6-18 at baseline					
Fraction minority	17,005	0.9556	-0.0006	-0.0012	0.9645
			(0.0017)	(0.0030)	
Fraction with subsidized lunch	17,005	0.8528	-0.0010	-0.0020	0.8485
			(0.0019)	(0.0034)	
Average test score	17,005	-0.2109	0.0006	0.0019	-0.2515
			(0.0052)	(0.0095)	
Females age 6-18 at baseline					
Fraction minority	17,274	0.9540	0.0017	0.0035	0.9581
			(0.0016)	(0.0031)	
Fraction with subsidized lunch	17,274	0.8564	0.0004	0.0010	0.8493
			(0.0017)	(0.0034)	
Average test score	17,274	-0.1789	-0.0045	-0.0089	-0.2128
			(0.0054)	(0.0105)	

Notes: Unit of observation is the person-year for all outcomes. Standard errors are reported in parentheses and are clustered at the household level.

Table G.14: Housing Voucher Effects on School Moving

Outcome	Individuals	CM	ITT	IV	CCM
	(1)	(2)	(3)	(4)	(5)
Males age 0-6 at baseline					
School moves	9,888	0.2585	0.0074	0.0132*	0.2633
			(0.0045)	(0.0078)	
School moves (involuntary)	9,888	0.1146	0.0014	0.0026	0.1088
			(0.0019)	(0.0033)	
School moves (voluntary)	9,888	0.1439	0.0060	0.0105	0.1545
			(0.0040)	(0.0070)	
Miles from baseline address to school	9,730	2.9078	0.2053**	0.3609**	2.8576
			(0.0827)	(0.1437)	
Miles from current address to school	8,243	2.7543	0.1204	0.2093	2.7041
			(0.0930)	(0.1596)	
Missing school move data	9,964	0.0223	0.0008	0.0015	0.0020
			(0.0006)	(0.0011)	
Missing baseline address to school data	10,374	0.0575	0.0015	0.0028	0.0501
			(0.0015)	(0.0027)	
Missing current address to school data	10,374	0.3414	0.0274***	0.0486***	0.3199
			(0.0099)	(0.0172)	
Males age 6-18 at baseline					
School moves	16,710	0.2740	0.0062	0.0115	0.2874
			(0.0040)	(0.0073)	
School moves (involuntary)	16,710	0.1289	0.0014	0.0021	0.1529
			(0.0020)	(0.0038)	
School moves (voluntary)	16,710	0.1451	0.0048	0.0094	0.1344
			(0.0036)	(0.0066)	
Miles from baseline address to school	16,110	2.6321	0.3251***	0.5820***	2.6727
			(0.0707)	(0.1286)	
Miles from current address to school	13,197	2.2226	0.3793***	0.6959***	2.1350
			(0.0882)	(0.1634)	
Missing school move data	16,834	0.0060	0.0001	0.0002	0.0014
			(0.0005)	(0.0010)	
Missing baseline address to school data	17,168	0.0648	0.0018	0.0033	0.0550
			(0.0022)	(0.0040)	
Missing current address to school data	17,168	0.3336	0.0080	0.0170	0.3456
			(0.0093)	(0.0169)	

Females age 0-6 at baseline					
School moves	9,575	0.2497	0.0108**	0.0196**	0.2514
			(0.0045)	(0.0079)	
School moves (involuntary)	9,575	0.1158	-0.0004	-0.0003	0.1085
			(0.0019)	(0.0034)	
School moves (voluntary)	9,575	0.1339	0.0112***	0.0199***	0.1429
			(0.0040)	(0.0070)	
Miles from baseline address to school	9,472	2.8560	0.2469***	0.4419***	2.9567
			(0.0789)	(0.1380)	
Miles from current address to school	8,105	2.7384	0.1675*	0.3129*	2.8259
			(0.0909)	(0.1640)	
Missing school move data	9,648	0.0223	0.0003	0.0006	0.0046
			(0.0006)	(0.0011)	
Missing baseline address to school data	10,096	0.0541	-0.0010	-0.0017	0.0446
			(0.0009)	(0.0015)	
Missing current address to school data	10,096	0.3455	0.0081	0.0152	0.3582
			(0.0096)	(0.0170)	
Females age 6-18 at baseline					
School moves	17,048	0.2633	0.0155***	0.0309***	0.2667
			(0.0040)	(0.0078)	
School moves (involuntary)	17,048	0.1262	0.0021	0.0037	0.1475
			(0.0019)	(0.0038)	
School moves (voluntary)	17,048	0.1372	0.0134***	0.0272***	0.1192
			(0.0037)	(0.0071)	
Miles from baseline address to school	16,450	2.6933	0.1637**	0.3252**	2.9493
			(0.0739)	(0.1426)	
Miles from current address to school	13,425	2.1993	0.2345***	0.4612***	2.3640
			(0.0850)	(0.1646)	
Missing school move data	17,143	0.0053	0.0001	0.0001	0.0009
			(0.0005)	(0.0011)	
Missing baseline address to school data	17,336	0.0489	-0.0009	-0.0020	0.0651
			(0.0013)	(0.0025)	
Missing current address to school data	17,336	0.3340	-0.0047	-0.0068	0.3616
			(0.0093)	(0.0178)	

Notes: Unit of observation is the person-year for all outcomes. Standard errors are reported in parentheses and are clustered at the household level.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

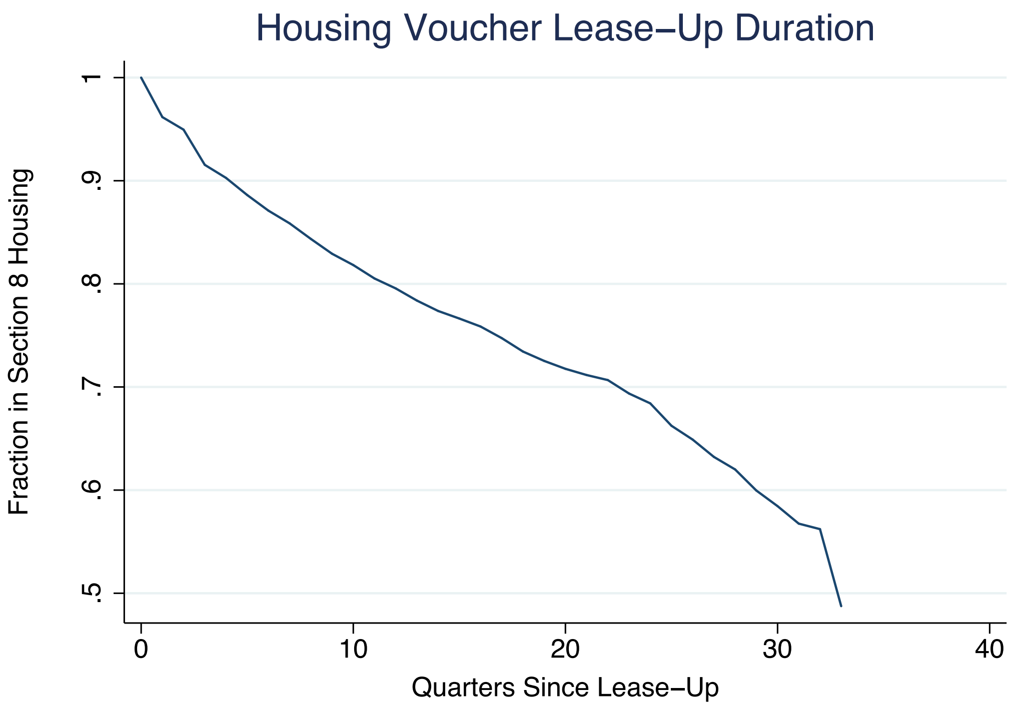
Table G.15: Housing Voucher Effects on Education, Criminal Behavior, and Health (Full Lottery Sample)

Baseline Age	Outcome	Children/ Obs.	CM	ITT	IV	CCM
		(1)	(2)	(3)	(4)	(5)
Male						
0-6	Test score	10,833	-0.3247	0.0273	0.0484	-0.3627
		[64,396]		(0.0184)	(0.0326)	
6-18	Test score	18,114	-0.3240	0.0016	0.0033	-0.3539
		[86,753]		(0.0147)	(0.0273)	
6-18	High school graduation	16,608	0.3998	0.0095	0.0188	0.4228
		[16,608]		(0.0090)	(0.0178)	
All	Soc. costs, most conservative	42,033	3,091	-177*	-391*	3,487
		[356,867]		(94)	(204)	
0-6	Inpatient or emergency claim	11,980	0.2445	-0.0010	-0.0011	0.2423
		[65,887]		(0.0061)	(0.0115)	
6-18	Inpatient or emergency claim	15,803	0.2447	-0.0032	-0.0058	0.2515
		[71,140]		(0.0057)	(0.0112)	
Female						
0-6	Test score	10,625	-0.1451	0.0011	0.0016	-0.1449
		[65,320]		(0.0176)	(0.0315)	
6-18	Test score	18,713	-0.1471	0.0149	0.0274	-0.2063
		[92,108]		(0.0139)	(0.0274)	
6-18	High school graduation	17,457	0.5785	0.0079	0.0154	0.5894
		[17,457]		(0.0090)	(0.0174)	
All	Soc. costs, most conservative	41,762	584	51*	105*	650
		[358,450]		(29)	(63)	
0-6	Inpatient or emergency claim	11,669	0.2138	0.0004	0.0006	0.2230
		[63,249]		(0.0060)	(0.0112)	
6-18	Inpatient or emergency claim	20,242	0.3735	-0.0000	-0.0002	0.3879
		[95,536]		(0.0054)	(0.0108)	

Notes: Unit of observation is the person-year for all outcomes, except high school graduation which is a person-level cross-section. CM = control mean. ITT = intent-to-treat. IV = instrumental variables. CCM = control complier mean. Standard errors are reported in parentheses and are clustered at the household level.

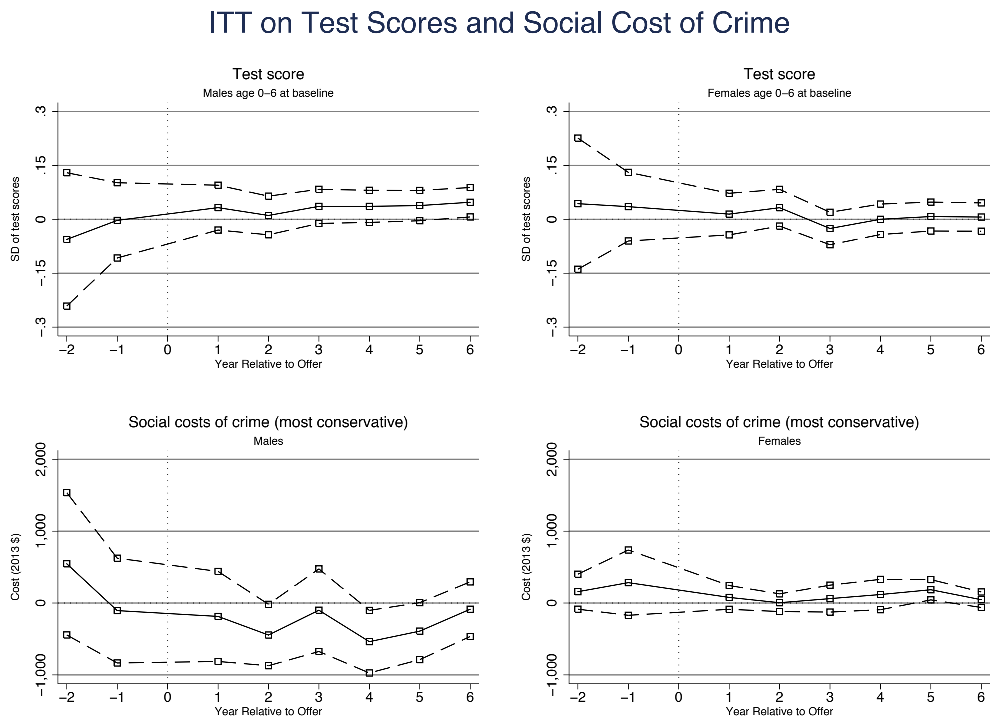
* Significant at the 10 percent level.

Figure G.1: Durations of Leases Among Users of CHAC Vouchers



Notes: Figure presents the fraction of households using vouchers obtained through the 1997 lottery that remained leased-up for various durations.

Figure G.2: Estimated ITT Effects on Children's Achievement Test Scores and Criminal Activity, by Year From Housing Voucher Offer



Notes: Each panel of the figure presents the results from estimating a separate regression where the dependent variable is either achievement test scores in math and reading from Chicago Public School student-level records (panels A and B) or the monetized value of the criminal activity youth commit (panels C and D). The explanatory variables of interest are a series of indicator variables for whether a given child-year observation is (k) years from when the child's family is offered a housing voucher, where (k) takes on both positive values (post-voucher offer) and negative values (pre-voucher offer). The figures present the regression coefficients on these indicator variables, together with the 95% confidence intervals. Note that because our dataset consists of child-year observations that are all after the randomized housing voucher lottery occurred, the coefficients on pre-voucher-offer years represent tests of whether there are behavioral "anticipation effects" rather than a test of whether randomization was carried out correctly.

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APPENDIX H

Details on Estimating Inverse Compliance Score Weights

As discussed Section 3.6.1, we use a weighting approach to generalize the local average treatment effect (LATE) estimates presented in Jacob and Ludwig (2012). Our weighting procedure follows the general framework discussed in Angrist and Fernandez-Val (2010) and Aronow and Carnegie (2013). Specifically, the compliance score c_i that we estimate is:

$$c_i(X) = \boldsymbol{E}(D|Z = 1, X) - \boldsymbol{E}(D|Z = 0, X)$$

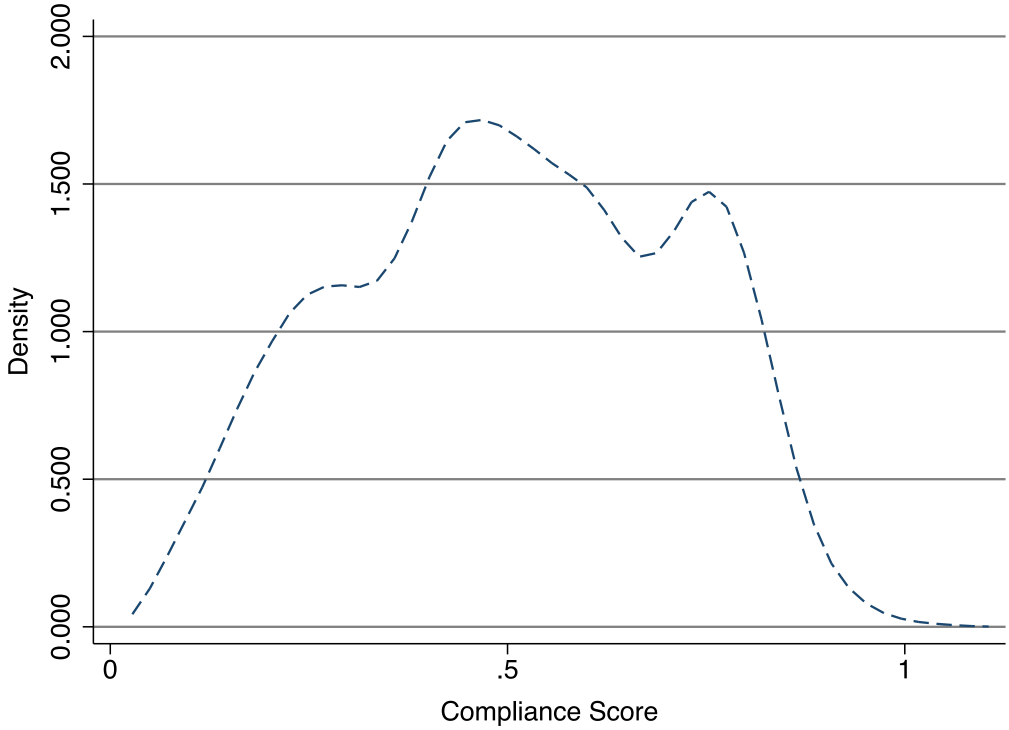
where D is an indicator for ever using a housing voucher, Z is a dummy variable instrument indicative of winning a voucher offer and X is a set of covariates. In words, the compliance score is the strength of the first stage conditional on observed covariates. Note that in the language of the potential outcomes framework of Angrist et al. (1996) this is the predicted probability of being a complier. Recall that a complier is an individual who accepts an experimental treatment when assigned to the treatment group, but does not obtain the treatment if assigned to the control group.

When no members of the control group are able to obtain the treatment the second term in the above expression is always zero and the probability of being a complier is just the likelihood of accepting the treatment given that the individual is a member of the treatment group.[1] In our case, we face "two-sided non-compliance" because a small fraction of CHAC 1997 lottery applicants are able to obtain a housing voucher through other means. Hence, the second term in the expression above is non-zero and we must model take-up in the control group. Note that in the language of the potential outcomes framework of Angrist et al. (1996), these control group individuals who obtain the treatment are called always-takers.

In our setting of two-sided non-compliance, we estimate the compliance score in two steps. First, we use the sample of treated households and estimate a probit model of leasing-up given the characteristics X. Using the parameters from this model, we predict $\boldsymbol{E}(D|Z = 1, X)$ for both treated and control units. Second, we use the sample of control households and estimate a probit model of leasing-up given the characteristics X. Using the parameters from this model, we predict $\boldsymbol{E}(D|Z = 0, X)$ for both treated and control units. Having calculated $\widehat{c_\iota}(X)$, we define our weights for each individual as $\widehat{w_\iota}(X) = 1 / \widehat{c_\iota}(X)$ and use these in our 2SLS estimates of the impact of voucher use on labor supply. For the interested reader, Figure H.1 shows the density $\widehat{c_\iota}(X)$ for the sample.

[1] The case in which there are no-always takers, but some members of the treatment group do not receive the treatment is referred to as "one-sided non-compliance." For an example of weighting in this context of one-sided compliance, see Follmann (2000).

Figure H.1: Density of Estimated Compliance Scores



Notes: The figure shows the kernel density estimate of the compliance scores estimated for our sample of 42,358 working-aged, able-bodied, CHAC 1997 lottery applicants.

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APPENDIX I

Additional Results From Chapter III

This appendix contains additional results referenced in Chapter III.

Table I.1: Logit Estimation of Lease Up for Households in Private Housing

	Predicted Sign	Coefficient	Std. Error	Marginal Effect[5]	Std. Error
	(1)	(2)	(3)	(4)	(5)
Demographics (household head)					
Male		-0.276***	(0.053)	-0.069***	(0.013)
Black		0.188**	(0.088)	0.047**	(0.022)
Hispanic		0.053	(0.126)	0.013	(0.032)
Has spouse		-0.366***	(0.065)	-0.092***	(0.016)
Probability of finding and leasing suitable unit[1]					
Number of adults (including household head)	-	-0.019	(0.023)	-0.005	(0.006)
Number of children	-	-0.004	(0.020)	-0.001	(0.005)
MSA vacancy rate	+	5.171**	(2.416)	1.293**	(0.604)
Offer arrived in winter		0.085*	(0.050)	0.021*	(0.013)
Offer arrived in fall		0.203***	(0.054)	0.051***	(0.013)
Offer arrived in summer		0.139**	(0.064)	0.035**	(0.016)
Expected net benefit of a voucher					
Recently moved[2]	-	-0.089*	(0.053)	-0.022*	(0.013)
Average composite test scores of children[3]	-	-0.093**	(0.045)	-0.023**	(0.011)
Household children arrested[3]	+	-0.081	(0.085)	-0.020	(0.021)
Poverty rate[4]	+	0.17	(0.152)	0.04	(0.038)
Fraction black[4]		0.373***	(0.076)	0.093***	(0.019)
Property crime rate (per 1,000)[4]	+	0.000	(0.001)	0.000	(0.000)
Violent crime rate (per 1,000)[4]	+	0.000	(0.001)	0.000	(0.000)
Distance to nearest school (miles)[1]	+	-0.350***	(0.099)	-0.087***	(0.025)
Distance to nearest hospital (miles)[1]	+	-0.083**	(0.035)	-0.021**	(0.009)
Cost of finding a unit					
Age[1]	-	-0.011***	(0.002)	-0.003***	(0.000)
Disabled	-	0.195***	(0.044)	0.049***	(0.011)
Received public assistance[2]	+	0.077	(0.136)	0.019	(0.034)
Difficult to categorize					
Has children		0.222**	(0.094)	0.055**	(0.024)
Average age of children	-	-0.014**	(0.007)	-0.003**	(0.002)
Distance to nearest rail or bus stop (miles)[1]		0.376***	(0.109)	0.094***	(0.027)
Household head arrested[3]		-0.323***	(0.062)	-0.081***	(0.015)
Offer arrived in summer * Has children		-0.039	(0.080)	-0.01	(0.020)
Recently ended employment[2]	+	-0.095*	(0.057)	-0.024*	(0.014)
Recently began employment[2]	-	-0.501***	(0.077)	-0.125***	(0.019)
Employed[2]		1.136***	(0.059)	0.284***	(0.015)
Annual earnings (thousands of 2013 $)[2]		-0.027***	(0.002)	-0.007***	(0.000)
N (Households)		16,179			

Notes: The sample is Chicago households living in private market housing at baseline (July 1997) and offered a housing voucher between 1997 and 2003. Each point estimate and standard error are from a logit regression of an indicator equal to one if the household leased up on the covariates. The regression also includes offer year fixed effects and indicators for missing values. Standard errors are heteroskedasticity-robust. 1 = At voucher offer. 2 = During year prior to voucher offer. 3 = During two years prior to voucher offer. 4 = At baseline (July 1997). 5 = Marginal effects calculated at the means of all covariates included in the model. *** Significant at the 1% level. ** 5% level. * 10% level.

Table I.2: Logit Estimation of Lease Up for Households in Public Housing

	Predicted Sign	Coefficient	Std. Error	Marginal Effect[5]	Std. Error
	(1)	(2)	(3)	(4)	(5)
Demographics (household head)					
Male		-0.541***	(0.171)	-0.133***	(0.042)
Black		0.37	(0.274)	0.091	(0.068)
Hispanic		-0.539	(0.658)	-0.133	(0.000)
Has spouse		-0.12	(0.209)	-0.029	(0.052)
Probability of finding and leasing suitable unit[1]					
Number of adults (including household head)	-	0.102	(0.063)	0.025	(0.015)
Number of children	-	0.02	(0.050)	0.005	(0.012)
MSA vacancy rate	+	10.874	(6.723)	2.683	(1.659)
Offer arrived in winter		0.205	(0.145)	0.051	(0.036)
Offer arrived in fall		0.433***	(0.156)	0.107***	(0.039)
Offer arrived in summer		0.25	(0.188)	0.062	(0.046)
Expected net benefit of a voucher					
Recently moved[2]	-	0.199	(0.147)	0.049	(0.036)
Average composite test scores of children[3]	-	0.044	(0.132)	0.011	(0.032)
Household children arrested[3]	+	0.035	(0.216)	0.009	(0.053)
Poverty rate[4]	+	0.507	(0.382)	0.125	(0.094)
Fraction black[4]		0.142	(0.277)	0.035	(0.068)
Property crime rate (per 1,000)[4]	+	0.001	(0.001)	0.000	(0.000)
Violent crime rate (per 1,000)[4]	+	0.001	(0.002)	0.000	(0.000)
Distance to nearest school (miles)[1]	+	-0.425	(0.429)	-0.105	(0.106)
Distance to nearest hospital (miles)[1]	+	-0.131	(0.085)	-0.032	(0.021)
Cost of finding a unit					
Age[1]	-	-0.028***	(0.005)	-0.007***	(0.001)
Disabled	-	-0.03	(0.125)	-0.007	(0.031)
Received public assistance[2]	+	0.12	(0.514)	0.03	(0.127)
Difficult to categorize					
Has children		0.12	(0.290)	0.029	(0.072)
Average age of children	-	-0.019	(0.020)	-0.005	(0.005)
Distance to nearest rail or bus stop (miles)[1]		0.266	(0.528)	0.066	(0.130)
Household head arrested[3]		-0.721***	(0.163)	-0.178***	(0.040)
Offer arrived in summer * Has children		0.145	(0.237)	0.036	(0.059)
Recently ended employment[2]	+	-0.069	(0.182)	-0.017	(0.045)
Recently began employment[2]	-	-0.448**	(0.218)	-0.111**	(0.054)
Employed[2]		0.810***	(0.176)	0.200***	(0.043)
Annual earnings (thousands of 2013 $)[2]		-0.017**	(0.007)	-0.004**	(0.002)
N (Households)		1,930			

Notes: The sample is Chicago households living in public housing at baseline (July 1997) and offered a housing voucher between 1997 and 2003. Each point estimate and standard error are from a logit regression of an indicator equal to one if the household leased up on the covariates. The regression also includes offer year fixed effects and indicators for missing values. Standard errors are heteroskedasticity-robust. 1 = At voucher offer. 2 = During year prior to voucher offer. 3 = During two years prior to voucher offer. 4 = At baseline (July 1997). 5 = Marginal effects calculated at the means of all covariates included in the model. *** Significant at the 1% level. ** 5% level. * 10% level.