THE IMPACT OF HOUSING ASSISTANCE ON CHILD OUTCOMES: EVIDENCE FROM A RANDOMIZED HOUSING LOTTERY*

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One long-standing 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 focusing 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 nonhousing 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. *JEL Codes:* D10, H23, I38.

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I. 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 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 rent contributions, MTO represented a change in the form rather than the amount of a family's housing assistance. This article 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

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

2. We use the phrase "housing voucher" as shorthand for tenant-based rental subsidies. See Online Appendix A for details.

3. Results of the five-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).

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 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% 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, and were more likely to repeat a grade. However, the analysis relied on parent reports of child outcomes and had only a modest sample size, so many of the null findings are imprecisely estimated (see Online Appendix B for additional discussion).

In this article, 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

4. See online summary of a Census report at https://www.census.gov/library/publications/2013/demo/p70-136.html.

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%. See "Chicago in Focus," Brookings Institute (November 2003), http://www.brookings.edu/research/reports/2003/11/livingcities-chicago.

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% 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 for boys 0–6 at baseline (pair-wise error rate $p \sim .05$), but only 0.003 standard deviations for girls age 0–6 at baseline (standard error of 0.03), and just 0.01 and 0.03 standard deviations for boys and girls who were of school age at baseline (standard errors of about 0.03). Our IV estimate for the effects of vouchers on inpatient or emergency room visits is never higher than about 1 percentage point (versus a control complier mean [CCM] of 25%), and for high school graduation is about 2 or 3 percentage points (compared to CCMs of 41% and 58% 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 prespecified 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 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 2011). 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-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 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, and the limitations and implications of our results are in Section VII.

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

II. 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 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.

II.A. 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) calls "very low-income households," with incomes for a family of four below 50% 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 varies over time and across areas.[8]

Families receiving vouchers are required to contribute toward rent 30% 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, 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 Online Appendix A.)

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.

II.B. 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: (i) by improving the quality of the housing conditions in which children reside; (ii) by allowing parents to invest more in nonhousing goods that may be developmentally productive for children; (iii) by changing parent behavior due to the conditions of the housing program; (iv) by reducing parental labor supply; and (v) increasing the number of residential moves families make. The first three mechanisms should improve children's outcomes, whereas the effects of the fourth (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 U.S. households tend to be very mobile anyway.[9]

Figure I 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. The family must decide how to allocate income I between the consumption of housing (H) and other (nonhousing) 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 I), whereas the FMR for these families is on average \$16,220, so the maximum change in housing consumption from using a voucher is on average

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 eight-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% of the CCM.

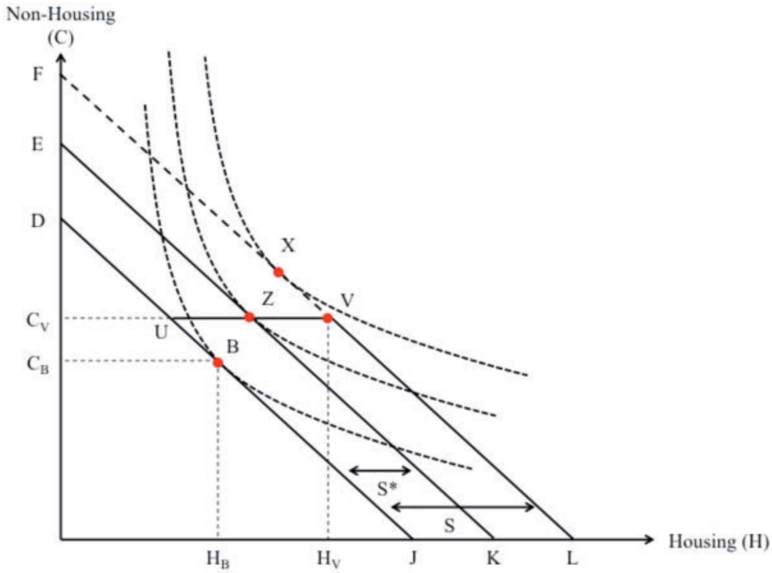

FIGURE I

Budget constraint and consumption with and without housing voucher

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 (FMR). 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 nonhousing goods (C_V–C_B) as a family experiences when it receives a housing voucher that costs S to the government. One of our model specifications in the tables 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 nonhousing consumption (S^*) to estimate the change in children's outcomes for a \$1,000 gain in family income.

$H_V - H_B =$ \$6,849. This represents a 73% increase in housing consumption, or about 36% 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% of voucher users in the treatment group remain in the same housing unit.

TABLE I

BASELINE CHARACTERISTICS OF TREATMENT AND CONTROL GROUP HOUSEHOLDS AND CHILDREN

	(1)	(2)	(3)	(4)	(5)
		Treatment group			
	Control group	All	p-value (T-C comparison)	Compliers	Noncompliers
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[a]	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, Medicaid, or FS 1997:II	0.782	0.769	0.025**	0.831	0.686

TABLE I
(CONTINUED)

	(1)	(2)	(3)	(4)	(5)
		Treatment group			
	Control group	All	p-value (T-C comparison)	Compliers	Noncompliers
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

TABLE I
(CONTINUED)

	(1)	(2)	(3)	(4)	(5)
		Treatment group			
	Control group	All	p-value (T-C comparison)	Compliers	Noncompliers
# of prior other crime arrests	0.011	0.012	0.762	0.009	0.015
Enrolled in the Chicago Public Schools prelottery	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 black in child's school	0.848	0.853	0.220	0.871	0.829
Fraction Hispanic 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 of null hypothesis that all T-C differences equal zero					
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. Household members' earnings average approximately 55% of total household income. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

[a]Household income includes earnings of all household members (adults and children); estimated tax gain/loss; and TANF and food stamps benefits.

[b]Includes missing indicators.

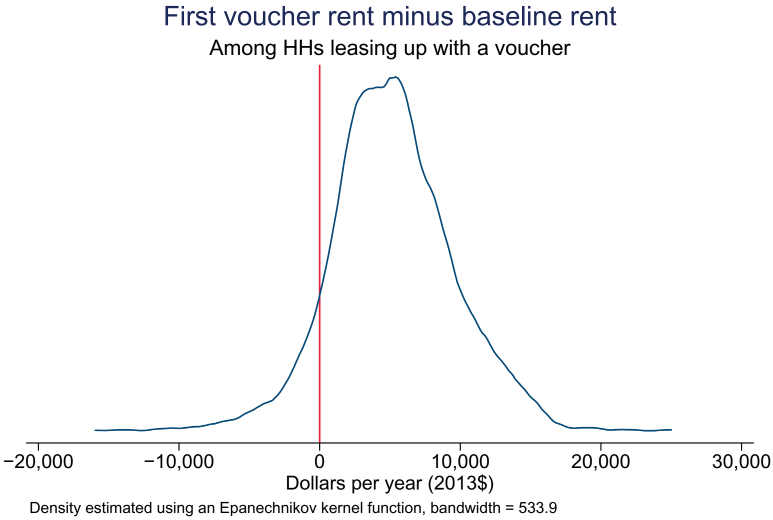

FIGURE II

Distribution of Change in Housing Consumption among Leased-up Sample

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 Online Appendix D). 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 II 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 (prelottery) 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

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 Online Appendix D for details.

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 nonhousing goods (from C_B to C_V in Figure I) 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 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 (nonhousing) goods by $C_V - C_B = \$5,653$, which equals 45% of the total voucher subsidy cost to the government and 29% of average baseline income for families. This represents a 59% gain in nonhousing consumption. Whether this extra consumption improves children's outcomes obviously depends on how this large infusion of additional income is spent.

Although 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 Online Appendix B), 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 standard deviations overall, by 0.06 for black or Hispanic youth, and by

11. Some observers have noted that 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 here 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.

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 standard deviations 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 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 age 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

12. The estimates we report in the text are slightly different from those reported in the original papers we cite because we have rescaled 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.

13. In Ludwig and Phillips (2008), table 1, the median effect of participation in Head Start is about 0.016 standard deviations 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 standard deviations (Schanzenbach 2007). Borman and Hewes (2002) estimate the effects per $1,000 in 2013 dollars on math scores equal to 0.027 standard deviations.

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

30% of adjusted income toward rent). In our previous work examining data through eight years after the voucher lottery, we find voucher receipt reduced parents' work rates by 3.6 percentage points compared to a CCM of 61% (Jacob and Ludwig 2012). Over the 14-year follow-up period that we examine here, 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.

III. 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]

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 use rate) was 1,540 (50.3%) in 1997; 3,085 (50.1%) in 1998; 2,631 (43.6%) in 2000; 5,733 (44.5%) in 2001; 4,674 (49.7%) in 2002; 446 (42.7%) in 2003.

17. Although these families may have expected to receive a voucher, our results are not sensitive to including them.

IV. 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 Online Appendix D. The starting point for constructing our sample are the application forms for the 1997 wait list, which provide baseline 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.

IV.A. 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 prerandomization sources to preserve the strength of the experimental design. From the Chicago Public Schools we obtained student-level school records for the academic years 1994–95 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 here 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% of our sample is ever enrolled at some point during 2000–2008, and 45% are enrolled at a point in time, with at most a 1.4 percentage point treatment-control difference in enrollment rates as we will show. 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); although still a coarse measure of health outcomes, it may at least capture dramatic changes in health status among sample members.

IV.B. Sample

Table I presented 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 because 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 I) 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]

18. We use the suest command in Stata to conduct an F-test for the joint significance of the treatment indicator, adjusting for the nonindependence of baseline characteristics within households. This test essentially consists of regressing

V. 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.

V.A. 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:

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

$PostOffer_{it}$ 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 0 otherwise. We also control for year effects, γ_t, and to increase precision we control for a set of baseline characteristics (see Online Appendix D). Standard errors are clustered by household (Bertrand, Duflo, and Mullainathan 2004). Identification of the ITT effect $\beta_1$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 0 otherwise. The coefficient $\beta_2$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).

lottery numbers against all of the baseline characteristics shown in Table I 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 .44.

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.

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:

$$y_{it} = \alpha + \sum_k D_{it}^k \delta_k + X_i \Gamma + \gamma_t + \varepsilon_{it}, \tag{2}$$

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 in the Online Appendix (discussed below).

V.B. 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%, as shown shortly).[20] Under the assumption that the voucher offer does not affect those who choose not to take it, we can use 2SLS 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 used a voucher provided by any source (the CHAC lottery or some other allocation) by or in period t.[21]

$$Leased_{it} = \alpha + \theta_1 PostOffer_{it} + \theta_2 PreOffer_{it} + X_i \Gamma + \gamma_t + \varepsilon_{it} \tag{3}$$

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 (Online Appendix Figure I). The results do not change much if we instead define the treatment as using a housing voucher in period t.

$$y_{it} = \eta + \pi_1 Leased_{it} + X_i \Pi + \mu_t + v_{it}. \tag{4}$$

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 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.

Table II 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 two to four 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% 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)).

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 II
HOUSING VOUCHER EFFECT ON LEASE-UP

	(1)	(2)	(3)	(4)
	Leased using			
	Any voucher		1997 CHAC voucher	
	Current period	Ever	Current period	Ever
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% level. ** Significant at the 5% level. * Significant at the 10% level.

V.C. 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, Burghardt, and McConnell 2008) although studies in social science often fail to make such adjustments (Anderson 2008).

First, we prespecify 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, Duncan, and Rodrigues (2004) and Duncan, Morris, and Rodrigues (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 rescaled 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

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 Online Appendix D and E, respectively.

resampling 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 effects even with our focus on the FDR strengthens our conclusions about the limited effect of even large resource transfers on children's outcomes.

VI. Results

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

VI.A. *Effects of Housing Vouchers on Children's Outcomes*

Table III presents the impact estimates for our primary outcome measures.[24] Even if we initially ignore multiple testing issues and focus on pair-wise error rates, just 1 out of 12 ITT estimates is significant at the usual 5% threshold (social costs of crime committed by females), and another is significant at the 10% 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 Online Appendix E), 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 standard deviations for boys and girls respectively, with standard errors of about 0.027 standard deviations. 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%, with standard errors of about 1 percentage point.

For other outcomes, it appears that we have less statistical power. For example, the 95% 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%. The IV estimate for

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.

TABLE III

HOUSING VOUCHER EFFECTS ON EDUCATION, CRIMINAL BEHAVIOR, AND HEALTH

		(1)	(2)	(3)	(4)	(5)	(6)	(7)
							ITT p-value	
Baseline Age	Outcome	Children/obs.	CM	ITT	IV	CCM	Pair-wise	FDR
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	Social costs of crime	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)			

TABLE III
(CONTINUED)

		(1)	(2)	(3)	(4)	(5)	(6) ITT p-value	(7)
Baseline Age	Outcome	Children/obs.	CM	ITT	IV	CCM	Pair-wise	FDR
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	Social costs of crime	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% level. * Significant at the 10% level.

TABLE IV

HOUSING VOUCHER EFFECT ON GEOGRAPHIC OUTCOMES (10% SAMPLE)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1997–2005 addresses				2012 Address[a]			
	CM	ITT	IV	CCM	CM	ITT	IV	CCM
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%[b,c]	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[b,c]	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[b,c]	0.794	0.0012	0.0023	0.837	0.760	−0.0011	−0.0020	0.789
		(0.0084)	(0.0172)			(0.0155)	(0.0290)	

TABLE IV
(CONTINUED)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1997–2005 addresses				2012 Address[a]			
	CM	ITT	IV	CCM	CM	ITT	IV	CCM
Social capital[b,d]	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[b,d]	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)[e]	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)[e]	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. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

[a]Outcome measures based on the American Community Surveys for 2005–9.

[b]Measured at the census tract level.

[c]Data from the decennial 1990 and 2000 censuses and the American Community Surveys for 2005–9 (interpolating values for intercensus years).

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

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

social costs of crime committed by boys is –\$344, or about 10% of the CCM of \$3,482, with a confidence interval that ranges from about −21% to +2% of the CCM. However, as we discuss next, 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 IV shows that there is no treatment-control difference in the fraction of quarters living outside of Illinois 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 Online Appendix E).

The results do not differ qualitatively for those children whose families received vouchers when they were most developmentally plastic. When we recalculate 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 Online Appendix E). 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 article.

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 (Online Appendix Figure II). 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.

VI.B. 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 IV 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% random subsample 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 V 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 that families must be taking most of their increased housing consumption in the form of better housing units rather than better neighborhoods.

25. The results are similar when using address data from public assistance program records; see Online Appendix E.

26. Frequent changes of a child's school attended are a major problem in urban districts; see NRC/IOM (2010).

TABLE V
HOUSING VOUCHER EFFECTS ON CHILD'S SCHOOL CHARACTERISTICS AND MOVING

Outcome	(1) Children/obs.	(2) CM	(3) ITT	(4) IV	(5) CCM
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% level. ** Significant at the 5% level. * Significant at the 10% level.

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 Online Appendix E).

VII. RECONCILING OUR RESULTS WITH THOSE OF OTHER TRANSFER PROGRAMS

The results described herein 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.

VII.A. 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 nonhousing 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 subsection). 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 III by S, that is, $\pi_{income} \approx \frac{\pi_1}{S} \times 1{,}000$. In practice we estimate this by applying 2SLS to a variant of equations (3) and (4) shown earlier:

$$Leased_{it} * S_i = \alpha + \theta_1 PostOffer_{it} + \theta_2 PreOffer_{it} + X_i\Gamma + \gamma_t + \varepsilon_{it} \tag{5}$$

$$y_{it} = \eta + \pi_{income} Leased_{it} * S_i + X_i\Pi + \mu_t + v_{it}. \tag{6}$$

The estimate of π_{income} based on the voucher value S is shown in column (3) of Table VI. 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.

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 nonhousing 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.

TABLE VI

ESTIMATED EFFECTS OF CASH TRANSFERS ON EDUCATION, CRIMINAL BEHAVIOR, AND HEALTH

		(1)	(2)	(3)	(4)	(5)
				IV rescaled by implied voucher value		
Baseline Age	Outcome	Children/obs.	CM	S	S^*	ΔC
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	Social costs of crime	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)

TABLE VI
(CONTINUED)

		(1)	(2)	(3)	(4)	(5)
				IV rescaled by implied voucher value		
Baseline Age	Outcome	Children/obs.	CM	S	S^*	ΔC
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	Social costs of crime	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 rescaled 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 nonhousing 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 nonhousing 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% level. * Significant at the 10% level.

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 nonhousing consumption. Because the income elasticity of housing is nonzero, a family receiving cash would spend some of it on housing. To calculate the size of the cash subsidy S^* (see Figure I) needed to generate the same increase in nonhousing consumption ΔC as the housing voucher given baseline income I, rent H_B, and elasticity of housing consumption $e_{H,I}$, we solve:

$$\Delta C = C_V - C_B = S^* - \left[\frac{S^*}{I} \times e_{H,I} \times H_B\right]. \tag{7}$$

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), Temporary Assistance to Needy Families, and the monetary value of food stamps benefits received (see Online Appendix D). We assume an income elasticity of housing consumption of 0.35 (Polinsky and Ellwood 1979; Mayo 1981). We then substitute our estimate of S^* for S in estimating equations (5) and (6). These estimates are shown in column (4) of Table VI.

Finally, we create an even more conservative estimate by assuming the income elasticity of housing consumption is 0. 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, that is, 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 VI.

The results shown in Table VI 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 standard deviations with a standard error of 0.006. Our confidence intervals enable us to rule out an effect of cash on test

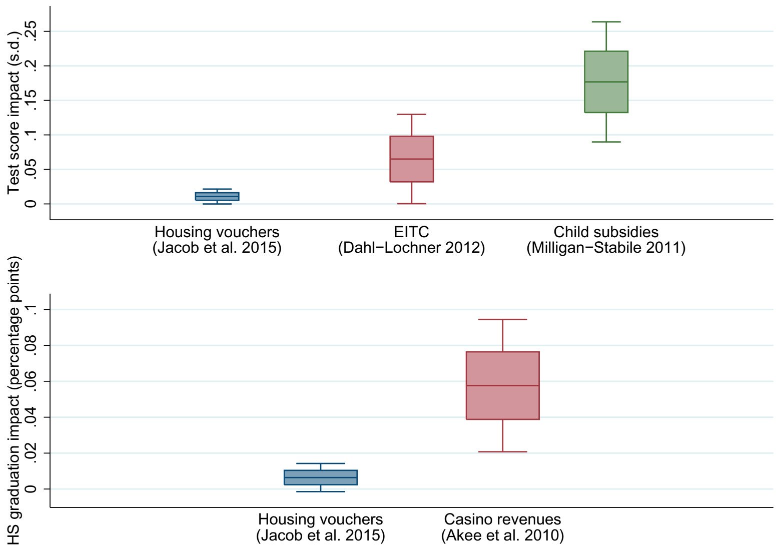

FIGURE III

Effects of Cash Transfers on Educational Outcomes of Males across Studies

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 VI, column (5), using as the dependent variables (i) an average of reading and math achievement test scores for males 0–6 at baseline and (ii) 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 std. dev. 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 std. dev. 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 and 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.

scores that is any larger than 0.022 standard deviations. As Figure III 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 already noted, Akee et al. (2010) use data from a casino opening 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 VI, suggest an effect per \$1,000 of extra income on high school graduation rates for boys equal to 0.6 percentage point, with a standard error of 0.4 percentage point; for girls, the point estimate and standard error both equal about 0.4 percentage point. 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 nonhousing consumption (Online Appendix E).

VII.B. 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 data sets, so there is inevitably some chance those estimates suffer from selection bias. But that cannot be the whole explanation since studies such as Duncan, Morris, and Rodrigues (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 already discussed. 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 because 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. 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 Online Appendix E for details).

One 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 here. In contrast for example in Duncan, Morris, and Rodrigues (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 is the way 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 how 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 Online Appendix C). 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, Gennetian, and Duncan (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 preschool-age children show gains in outcomes from extra income in those welfare-to-work experiments (Morris, Duncan, and Rodrigues 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 and less parental time, which may change the way resources are spent, particularly on key inputs like early childhood education or care.

VIII. Conclusions

In this article, 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 child care.

Our findings do not imply that cash transfers or other antipoverty 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 United States would not harm children’s outcomes, since the effect of income on children’s outcomes is almost certainly nonlinear, 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 trade-off 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 standard deviations, 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.

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Supplementary Material

An Online Appendix for this article can be found at QJE online (qje.oxfordjournals.org).

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