HUMAN CAPITAL EFFECTS OF ANTI-POVERTY PROGRAMS:
EVIDENCE FROM A RANDOMIZED HOUSING VOUCHER LOTTERY

APPENDIX MATERIALS

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APPENDIX A:
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.

A1. 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, the *Housing Choice Voucher (HCV)* program. In what follows we focus on the core shared elements of these programs that are central to our study, and note important differences among the different 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 A1: 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:

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

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]

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 over 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).

A2. 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-study or those earned by dependent children. If families reduce their work without prior

[3] The child care 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.

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.

A3. 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 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]

[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).
[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.

APPENDIX B:
REVIEW OF PREVIOUS RESEARCH

This appendix briefly reviews the existing research literatures on the effects of housing programs, family income supports, and school-based educational interventions on child outcomes. 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. The implication of these studies, if correct, 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.

I. 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.[7]

[7] 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 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

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 themselves report. For example Theunissen et al. (1998) compare child and parent reports about the child's physical health, cognitive functioning, social functioning and emotions and find correlations between 0.44 and 0.61.

II. 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.

Mayer (1997) presents the results from a variety of empirical tests that improve upon correlational evidence, and finds 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

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results for adults are presented in Ludwig et al. (2011, 2012). Lessons from the MTO studies are discussed in Ludwig (2012).

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 Appendix Table B1. 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.[8]

The first row of Appendix Table B1 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

[8] 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% below those of the control group, and even 8 years later family income is around 15% lower than what it would have been otherwise. Children in these families that experience job displacement wind up with adult earnings levels that are about 9% below those of their comparison group counterparts. We do not include that in our review in Appendix Table B1 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. Loken (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.

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 families spend their money (see for example the model presented in Appendix C 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),[9] 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

[9] Base year for dollars comes from personal communication of Jens Ludwig with Randall Akee, July 8, 2013.

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).[10] 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 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, which yield mixed findings (Mayer 1997), 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 Dahl and Lochner'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% 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.

[10] 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.

Appendix C 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.

III. Promising Educational Interventions

Appendix Table B2 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 Appendix Table B1.[11]

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 (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 Dahl and Lochner, Duncan et al., and Milligan and Stabile tend to be at least as large (and in some

[11] 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.

cases substantially larger) than what we find from influential educational interventions like Head Start, class-size reduction or Success for All.[12]

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, Dahl-Lochner 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 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 Appendix Table B3. (Levin et al. only report point estimates, not standard errors, so that is what we include in our table.)[13] 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.

The over-arching point of this review is that if the recent wave of studies of cash transfer effects are correct, the effects on schooling outcomes from providing low-income families with extra cash are (with just one exception) at least as large as what we see from notable educational interventions that are explicitly designed to boost children's human capital outcomes.

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[12] 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.

[13] 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.

Appendix Table B1

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.

Appendix Table B2

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.

Appendix Table B3

Estimated Effect on High School Graduation Rates from Educational Interventions Deemed Promising or Proven by What Works Clearinghouse (from 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.

APPENDIX C:
Conceptual Model for Transfer Program Effects on Parental Labor Supply, Family 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.

C1. 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).[14] 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:

[14] 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.

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

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

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

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.

C2. Extending model to welfare-to-work application

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:

$$(13) \qquad L \geq L^*$$

Or equivalently:

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

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 child care. 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.

APPENDIX D:
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.

D1. 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.

<u>Gender</u> - 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.

<u>Race</u> - 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 (ie, 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.

Neighborhood Characteristics – 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.)[15] 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.

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

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

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.

D2. 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.

D3. 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.

a. <u>Household Member Imputation Procedure</u>

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.

b. 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 D1 presents a more thorough breakdown of whether our IDHS estimation procedure and the CHAC applications are identifying the same number of household members.

Table D1: To what extent does the IDHS estimation procedure over or underestimate household size? (N=75,145)

		Fraction of the cases in which:			
	CHAC and	CHAC greater than IDHS by:		IDHS greater than CHAC by:	
	IDHS equal	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 D2 presents comparisons for the average household size and compositions implied by the CHAC applications and our imputation procedure.

Table D2: 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.[16] 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.[17]

[16] 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.

[17] 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 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.

c. 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 up. Tables D3 and D4 indicate that the patterns in Tables D1 and D2 are also apparent in this subsample.[18]

Table D3: 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: One	CHAC greater than IDHS by: More than one	IDHS greater than CHAC by: One	IDHS greater than CHAC by: 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

[18] 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.

Total Household Size	0.49	0.21	0.17	0.07	0.05

Table D4: 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 D5 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 D5: The fraction of non-household heads who appear in IDHS records (n=3,417) that 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

d. 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.

D4. 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.

a. 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.[19]

b. 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

[19] 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.

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).[20]

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).[21] 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.

c. 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).

[20] 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.

[21] 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.

Legislated federal, state, and FICA tax refund or liability levels (including EITC): These were obtained using TAXSIM. [22] 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.[23] 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 household heads with listed "spouses" are married and file jointly.[24] 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.[25]

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 A, 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.[26] 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.[27]

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 A, benefit levels are a function of earned income, household size, childcare, and rent.[28] We do not have data on child care 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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[22] 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.
[23] 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.
[24] 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.
[25] Our baseline specification takes the number of dependents as given. We estimate an alternative specification that caps the number of dependents at six.
[26] 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.
[27] In roughly 3 percent of household-quarter observations during 1996Q3-1997Q2 in which our records indicate that the household head was receiving some TANF benefits, we estimate zero benefit levels.
[28] 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). [29]

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

d. 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 A above, 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. We assume the payment standard is the Fair Market Rent (FMR)[30] and the obligated rent payment as .3 times net income.[31]

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 A 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

[29] In roughly 3 percent of household-quarter observations during 1996Q3-1997Q2 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.
[30] 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.
[31] 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.

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.[32]

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.

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.

D5. 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.

D6. Medicaid claims data

[32] 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.

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,[33] 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.[34] The diagnosis-derived outcomes we focus on in the exploratory analysis (Appendix Tables V through VIII) 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:

[33] Emergency department use is recorded using outpatient claims with a "place of service" code indicating an urgent care facility, hospital emergency room, or ambulance.
[34] 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.

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

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 the main text are conditioned on being enrolled in fee-for-service care for six or more months during the academic year.

APPENDIX E:
Details on synthetic IV approach

We present IV estimates of the effect of voucher use scaled in terms of benefits per \$1,000 for comparison with the literature on the effects of cash transfers. Three different scaling factors—estimates of a housing voucher's implied value—are used depending on assumptions regarding the developmental productivity for children's human capital of housing and non-housing consumption, and the income elasticity of housing.

The first scaling factor assumes that increased housing (ΔH) and non-housing (ΔC) consumption from a voucher are equally developmentally productive for children. In this case, a voucher worth $S = \Delta H + \Delta C$ to the government should generate an impact on children's outcomes equivalent to a cash transfer of the same value. To be conservative, we assume that families receiving vouchers lease units with rents equal to the FMR, providing an upper bound on ΔH and thereby overstating the generosity of the voucher. The increase in non-housing consumption from using a voucher is the reduction in a family's out-of-pocket rent: imputed baseline rent minus the expected rent payment with a voucher (typically equal to 30% of adjusted income).

The second scaling factor assumes that housing consumption is not developmentally productive for children. In this case, any effect on children's outcomes attributable to increased consumption with a voucher is due only to non-housing consumption. Because the income elasticity of housing is likely not zero, a family receiving a cash transfer will consume some of it as housing. To calculate the size of the cash subsidy S^* necessary 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:

$$(1) \qquad \Delta C = C_V - C_B = S^* - \left[\frac{S^*}{I} \times e_{H,I} \times H_B\right]$$

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 (see Appendix D). We assume an income elasticity of housing consumption of 0.35.[35]

Finally, the third scaling factor goes even further by assuming the income elasticity of housing consumption is zero. In this case, as is evidenced by equation (1), the cash subsidy S^* needed to generate the same increase in non-housing consumption as the voucher is just ΔC.

We calculate all three scaling factors—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

[35] As suggested by the prior literature; see Mayo 1981, and Polinsky and Ellwood 1979.

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.

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Appendix Figure I: Durations of Leases Among Users of CHAC Vouchers

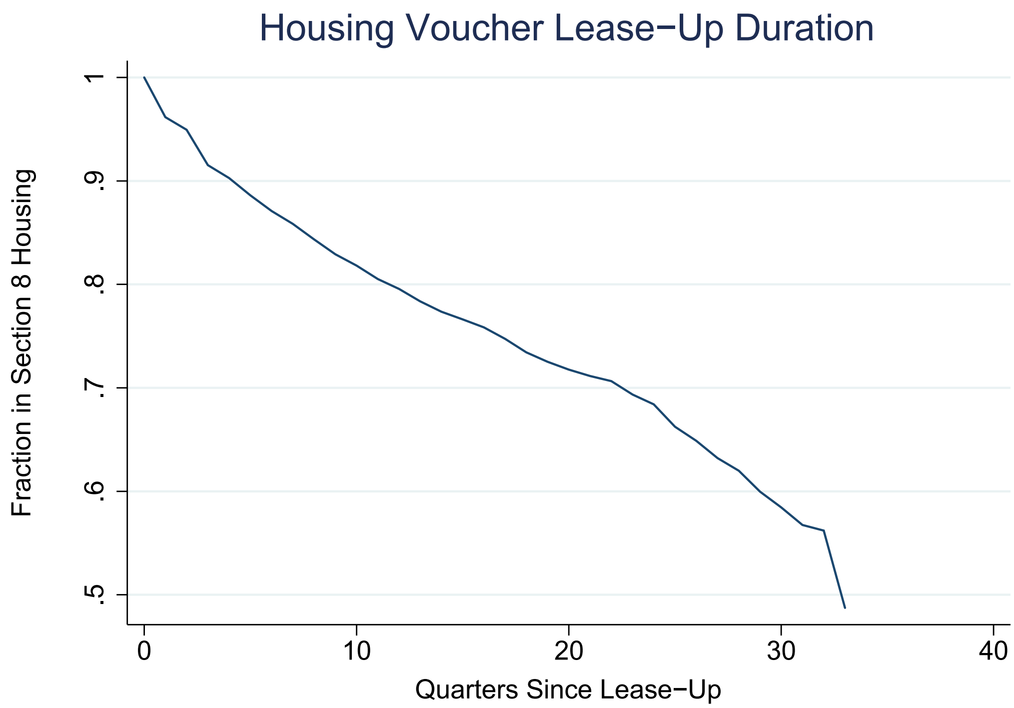

Figure presents the fraction of households using vouchers obtained through the 1997 lottery that remained leased-up for various durations.

Appendix Figure II: Effects of Cash Transfers on Test Scores of Young Females Across Studies

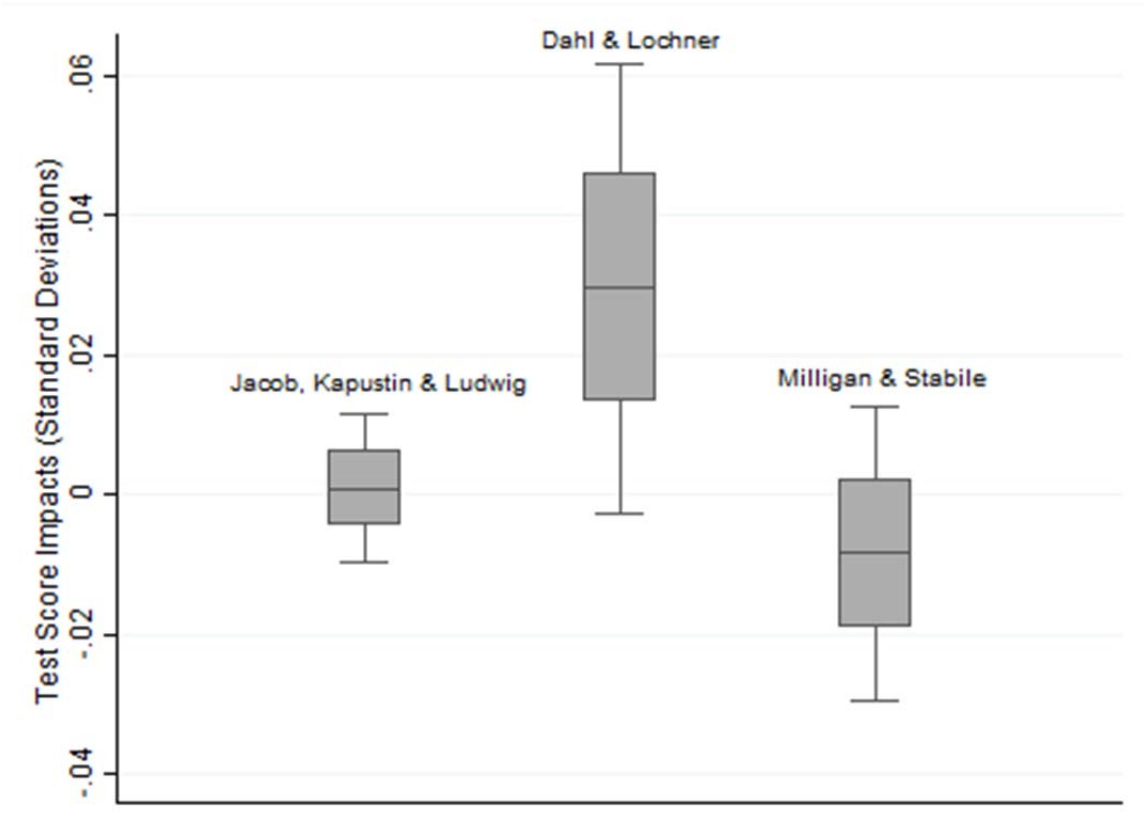

Figure reports the effects on children's achievement test scores per $1,000 change in family income (in 2013 dollars). The estimate from Jacob, Kapustin and Ludwig is for females 0-6 at baseline taken from Table VI, column 5, using as the dependent variable an average of reading and math achievement test scores 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 females (equal to 0.04 standard deviations in their paper reported in 2000 constant dollars, and equal to 0.03 when we update to 2013 dollars). Estimate from Milligan and Stabile (2011) is for math scores for females, taken from their Table 3, equal to -.011 standard deviations in their paper for a $1,000 change in Canadian 2004 dollars, and equal to -.008 when we update to 2013 US dollars.

Appendix Table I: Housing Voucher Effects on Education, Criminal Behavior, and Health (Full Lottery Sample)

Gender	Baseline Age	Outcome	Individuals (1)	CM (2)	ITT (3)	IV (4)	CCM (5)	Obs. (6)
Male	Age 0 to 6	Test score	10,833	-0.3247	0.0273	0.0484	-0.3627	64,396
					(0.0184)	(0.0326)		
Male	Age 6 to 18	Test score	18,114	-0.3240	0.0016	0.0033	-0.3539	86,753
					(0.0147)	(0.0273)		
Male	Age 6 to 18	High school graduation	16,608	0.3998	0.0095	0.0188	0.4228	16,608
					(0.0090)	(0.0178)		
Male	All	Soc. costs, most conservative	42,033	3,090.8431	-176.9409*	-390.7724*	3,487.0810	356,867
					(94.3204)	(204.3637)		
Male	Age 0 to 6	Inpatient or emergency claim	11,980	0.2445	-0.0010	-0.0011	0.2423	65,887
					(0.0061)	(0.0115)		
Male	Age 6 to 18	Inpatient or emergency claim	15,803	0.2447	-0.0032	-0.0058	0.2515	71,140
					(0.0057)	(0.0112)		
Female	Age 0 to 6	Test score	10,625	-0.1451	0.0011	0.0016	-0.1449	65,320
					(0.0176)	(0.0315)		
Female	Age 6 to 18	Test score	18,713	-0.1471	0.0149	0.0274	-0.2063	92,108
					(0.0139)	(0.0274)		
Female	Age 6 to 18	High school graduation	17,457	0.5785	0.0079	0.0154	0.5894	17,457
					(0.0090)	(0.0174)		
Female	All	Soc. costs, most conservative	41,762	584.1463	51.0656*	104.7383*	650.3712	358,450
					(29.2545)	(63.3220)		
Female	Age 0 to 6	Inpatient or emergency claim	11,669	0.2138	0.0004	0.0006	0.2230	63,249
					(0.0060)	(0.0112)		
Female	Age 6 to 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. Standard errors are reported in parentheses and are clustered at the household level.

* Significant at the 10 percent level.

Appendix Table II: 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						
HHH: Male	0.036	0.042	0.027	0.032	0.035	0.036
HHH: Black	0.947	0.948	0.949	0.954	0.941	0.953
HHH: Hispanic	0.033	0.029	0.035	0.031	0.037	0.030
HHH: White	0.018	0.020	0.013	0.013	0.020	0.015
HHH: Other race	0.003	0.002	0.003	0.002	0.002	0.002
HHH: Has spouse	0.081	0.085	0.076	0.078	0.076	0.070
# Adults in Household (Based on CHAC file)	1.434	1.432	1.361	1.360	1.402	1.355
# of kids 0-18 in HH (Based on CHAC file)	2.940	2.940	3.008	2.982	2.841	2.899
Age of HHH	32.017	31.875	30.661	30.429	31.705	31.544
Indicated interest in the 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	9.325	9.329	9.026	9.019	9.232	9.074
HH total income (2013 $) 1996:III to 1997:II	19,063.325	19,273.021	18,633.973	18,718.384	19,579.321	18,676.960
HHH income (2013 $) 1997:II	1,950.519	2,023.335	1,831.820	1,871.579	2,168.959	1,978.442
HHH employed 1997:II	0.462	0.471	0.449	0.456	0.487	0.476
HHH receiving TANF 1997:II	0.619	0.605	0.673	0.661	0.613	0.652
HHH receiving TANF, Med, or FS 1997:II	0.778	0.770	0.819	0.815	0.777	0.804
HHH: # of prior violent crime arrests	0.151	0.152	0.140	0.146	0.126	0.131
HHH: # of prior property crime arrests	0.277	0.236	0.246	0.212	0.250	0.258
HHH: # of prior drug crime arrests	0.131	0.130	0.125	0.130	0.096	0.143
HHH: # 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	74.576	75.041	74.784	75.146	75.452	73.367
Violent crime rate (beat-level, per 1,000) in 1997	38.970	39.219	39.165	39.066	38.777	38.104
Monthy rent (2013 $)	778.926	774.104	774.808	774.383	777.265	751.461
Monthly fair market rent (2013 $)	1,315.085	1,315.323	1,324.834	1,323.994	1,315.427	1,326.824
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	8.875	8.886	6.833	6.817	8.562	8.591
# 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	28.704	28.008	19.274	19.328	28.992	27.048
Fraction Black 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 HH 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.

Appendix Table III: 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		
Number of observations	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		
Number of observations	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.

* Significant at the 10 percent level.

Appendix Table IV: 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.

Appendix Table V: Voucher Effects for Males, Age 0-6 at Baseline

Education, Cross-Sectional Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled in CPS (1998-2011)	12,288	0.845	-0.0003	-0.0005	0.903	12,288
			(0.0017)	(0.0033)		
Cumulative GPA (Final Year)	4,332	1.656	-0.0208	-0.0353	1.647	4,332
			(0.0311)	(0.0526)		
Cumulative Credits (Final Year)	4,332	12.385	0.2587	0.4377	12.023	4,332
			(0.2576)	(0.4360)		
Final Non-missing Math Score	8,655	-0.411	0.0033	0.0057	-0.419	8,655
			(0.0203)	(0.0354)		
Final Non-missing Reading Score	8,660	-0.431	0.0429**	0.0750**	-0.473	8,660
			(0.0198)	(0.0347)		
Average Math Score (1998-2011)	8,731	-0.353	0.0213	0.0371	-0.367	8,731
			(0.0191)	(0.0333)		
Average Reading Score (1998-2011)	8,736	-0.348	0.0334*	0.0583*	-0.374	8,736
			(0.0189)	(0.0331)		
Non-missing Final Status	10,374	0.999	-0.0005	-0.0010	1.000	10,374
			(0.0009)	(0.0016)		
Attrited (Moved or Enrolled in Private)	10,360	0.329	-0.0464***	-0.0850***	0.324	10,360
			(0.0109)	(0.0198)		
Graduated	7,085	0.067	0.0085	0.0149	0.062	7,085
			(0.0060)	(0.0104)		
Enrolled 2-year school (public or private)	483	0.261	0.0006	0.0011	0.283	483
			(0.0496)	(0.0818)		
Enrolled 4-year public school	483	0.264	-0.0093	-0.0153	0.215	483
			(0.0473)	(0.0779)		
Enrolled 4-year private school	483	0.133	0.0376	0.0620	0.095	483
			(0.0398)	(0.0658)		

Education, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled in CPS in Academic Year	12,288	0.505	0.0328***	0.0658***	0.584	172,032
			(0.0067)	(0.0132)		
Grade 1 - 12	9,980	0.867	0.0019	0.0031	0.941	83,902
			(0.0017)	(0.0031)		
Old for Grade	9,533	0.228	-0.0079	-0.0139	0.254	72,858
			(0.0087)	(0.0150)		
Repeat	9,980	0.049	-0.0033*	-0.0059*	0.060	83,902
			(0.0017)	(0.0031)		
# Absences	4,699	36.123	-0.2258	-0.3796	37.383	10,795
			(0.8843)	(1.4873)		
# Credits	4,699	5.163	0.0381	0.0641	5.108	10,795
			(0.0619)	(0.1042)		
GPA in Current Year	4,699	1.677	0.0099	0.0166	1.638	10,795
			(0.0296)	(0.0497)		
Tested	8,784	0.942	0.0039	0.0067	0.939	54,461
			(0.0032)	(0.0054)		
Composite Test Score	8,659	-0.334	0.0369*	0.0634*	-0.377	51,339
			(0.0190)	(0.0325)		
Math Test Score	8,654	-0.330	0.0319	0.0549	-0.371	51,087
			(0.0200)	(0.0343)		
Reading Test Score	8,659	-0.334	0.0413**	0.0710**	-0.380	51,259
			(0.0198)	(0.0339)		

Crime, Cross-Sectional Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Arrested (1998-2011)	12,288	0.2809	0.0026	0.0051	0.3017	12,288
			(0.0092)	(0.0181)		
Sum of social costs (most conservative)	3,453	33,865.7253	-2,640.7577	-4,765.1412	37,146.2209	3,453
			(2,116.5021)	(3,826.5202)		
Sum of social costs (least conservative)	3,453	113,243.8726	-26,692.4030	-48,165.3673	152,319.8369	3,453

Appendix Table V: Voucher Effects for Males, Age 0-6 at Baseline

			(22,218.5900)	(40,142.6357)		
Total Violent Crime Arrests	3,453	1.0112	-0.0476	-0.0859	1.1014	3,453
			(0.0528)	(0.0954)		
Total Property Crime Arrests	3,453	0.5901	-0.0196	-0.0353	0.5725	3,453
			(0.0416)	(0.0751)		
Total Drug Crime Arrests	3,453	0.8849	0.0269	0.0486	0.8560	3,453
			(0.0681)	(0.1229)		
Total Other Crimes Arrests	3,453	1.8314	0.0026	0.0047	1.7058	3,453
			(0.1050)	(0.1895)		

Crime, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Arrested in Academic Year	12,288	0.1537	-0.0050	-0.0099	0.1740	46,390
			(0.0060)	(0.0118)		
Social costs (most conservative)	3,381	15,693.5701	-797.7825	-1,410.4873	17,082.9568	7,069
			(929.2289)	(1,644.8061)		
Social costs (least conservative)	3,381	53,925.5306	-10,618.3461	-18,773.3408	70,230.4966	7,069
			(10,769.8982)	(19,066.9866)		
# Violent Crime Arrests	3,381	0.4643	-0.0186	-0.0330	0.5139	7,069
			(0.0212)	(0.0376)		
# Property Crime Arrests	3,381	0.2745	-0.0044	-0.0078	0.2730	7,069
			(0.0188)	(0.0332)		
# Drug Crime Arrests	3,381	0.4219	0.0227	0.0402	0.4017	7,069
			(0.0286)	(0.0506)		
# Other Crime Arrests	3,381	0.8620	0.0133	0.0235	0.7965	7,069
			(0.0401)	(0.0710)		

Labor, Public Assistance, and Household Composition, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Total Earnings (2013 $)	7,013	517.110	-29.4496	-57.6955	564.831	14,121
			(62.5716)	(122.6556)		
Fraction of Year: Employed	7,013	0.068	0.0035	0.0069	0.068	14,121
			(0.0052)	(0.0101)		
Fraction of Year: Any Public Assistance	12,288	0.699	0.0184**	0.0368**	0.757	172,032
			(0.0075)	(0.0148)		
Fraction of Year: Foodstamps	12,288	0.516	0.0215***	0.0433***	0.563	172,032
			(0.0078)	(0.0154)		
Fraction of Year: AFDC/TANF	12,288	0.189	0.0048	0.0088	0.126	172,032
			(0.0042)	(0.0084)		
Fraction of Year: Medicaid	12,288	0.679	0.0174**	0.0348**	0.740	172,032
			(0.0076)	(0.0150)		
Fraction of Year: Address on File	12,288	0.696	0.0194***	0.0389***	0.751	172,032
			(0.0075)	(0.0148)		
# People in HH (annual average)	11,668	3.858	0.0087	0.0141	3.786	127,442
			(0.0294)	(0.0526)		
# Children in HH (annual average)	11,668	2.403	0.0294	0.0524	2.269	127,442
			(0.0185)	(0.0331)		
September: Any Public Assistance	12,288	0.704	0.0169**	0.0337**	0.761	172,032
			(0.0074)	(0.0146)		
September: Foodstamps	12,288	0.522	0.0217***	0.0437***	0.565	172,032
			(0.0078)	(0.0153)		
September: AFDC/TANF	12,288	0.205	0.0044	0.0080	0.137	172,032
			(0.0043)	(0.0086)		
September: Medicaid	12,288	0.685	0.0156**	0.0310**	0.746	172,032
			(0.0075)	(0.0148)		
September: Address on File	12,288	0.700	0.0178**	0.0356**	0.756	172,032
			(0.0074)	(0.0146)		
# People in HH (September)	11,614	3.916	0.0189	0.0344	3.821	121,025
			(0.0301)	(0.0535)		
# Children in HH (September)	11,614	2.470	0.0334*	0.0599*	2.334	121,025
			(0.0191)	(0.0339)		

Health, Cross-Sectional Estimates

Appendix Table V: Voucher Effects for Males, Age 0-6 at Baseline

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled in Medicaid (2000-2008)	12,288	0.776	0.0008	0.0015	0.826	12,288
			(0.0091)	(0.0179)		
Inpatient hospital claim	9,538	0.084	0.0044	0.0080	0.078	9,538
			(0.0065)	(0.0120)		
Emergency room claim	9,538	0.589	0.0157	0.0291	0.594	9,538
			(0.0119)	(0.0220)		
Inpatient or emergency claim	9,538	0.595	0.0132	0.0244	0.601	9,538
			(0.0119)	(0.0220)		
Outpatient claim	9,538	0.963	0.0034	0.0063	0.976	9,538
			(0.0044)	(0.0081)		
Injury, inpatient or emergency	9,538	0.395	0.0068	0.0125	0.409	9,538
			(0.0116)	(0.0215)		
Asthma	9,538	0.250	-0.0135	-0.0249	0.273	9,538
			(0.0101)	(0.0187)		
Routine medical exam	9,538	0.915	0.0027	0.0049	0.936	9,538
			(0.0066)	(0.0121)		

Health, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled in Medicaid in Academic Year	12,288	0.471	0.0108	0.0209	0.510	110,592
			(0.0086)	(0.0171)		
Inpatient hospital claim	9,538	0.022	0.0003	0.0009	0.020	52,378
			(0.0021)	(0.0039)		
Emergency room claim	9,538	0.240	0.0001	0.0010	0.237	52,378
			(0.0062)	(0.0113)		
Inpatient or emergency claim	9,538	0.245	-0.0012	-0.0014	0.242	52,378
			(0.0063)	(0.0114)		
Outpatient claim	9,538	0.880	0.0029	0.0046	0.888	52,378
			(0.0047)	(0.0085)		
Injury, inpatient or emergency	9,538	0.114	-0.0046	-0.0086	0.120	52,378
			(0.0039)	(0.0071)		
Asthma	9,538	0.132	0.0055	0.0106	0.147	52,378
			(0.0074)	(0.0135)		
Routine medical exam	9,538	0.679	0.0068	0.0119	0.707	52,378
			(0.0062)	(0.0114)		

Appendix Table VI: Voucher Effects for Males, Age 6-18 Baseline

Education, Cross-Sectional Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled in CPS (1998-2011)	21,112	0.812	-0.0003	-0.0006	0.859	21,112
			(0.0020)	(0.0042)		
Cumulative GPA (Final Year)	9,383	1.425	0.0184	0.0345	1.431	9,383
			(0.0188)	(0.0351)		
Cumulative Credits (Final Year)	9,383	16.379	0.4019*	0.7529*	16.624	9,383
			(0.2201)	(0.4120)		
Final Non-missing Math Score	14,278	-0.444	-0.0126	-0.0240	-0.428	14,278
			(0.0133)	(0.0252)		
Final Non-missing Reading Score	14,313	-0.461	-0.0249**	-0.0474**	-0.450	14,313
			(0.0124)	(0.0236)		
Average Math Score (1998-2011)	14,513	-0.363	0.0022	0.0042	-0.342	14,513
			(0.0120)	(0.0229)		
Average Reading Score (1998-2011)	14,573	-0.363	-0.0103	-0.0196	-0.343	14,573
			(0.0113)	(0.0216)		
Non-missing Final Status	17,168	0.999	-0.0001	-0.0002	0.998	17,168
			(0.0005)	(0.0011)		
Attrited (Moved or Enrolled in Private)	17,151	0.235	-0.0152**	-0.0301**	0.219	17,151
			(0.0075)	(0.0147)		
Graduated	13,183	0.394	0.0150	0.0286	0.412	13,183
			(0.0094)	(0.0178)		
Enrolled 2-year school (public or private)	5,308	0.392	0.0056	0.0103	0.395	5,308
			(0.0149)	(0.0275)		
Enrolled 4-year public school	5,308	0.193	-0.0060	-0.0110	0.193	5,308
			(0.0116)	(0.0214)		
Enrolled 4-year private school	5,308	0.150	-0.0032	-0.0059	0.146	5,308
			(0.0105)	(0.0193)		

Education, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled in CPS in Academic Year	21,112	0.324	0.0068**	0.0140**	0.290	295,568
			(0.0032)	(0.0067)		
Grade 1 - 12	15,972	0.996	-0.0009*	-0.0016	0.999	90,364
			(0.0005)	(0.0010)		
Old for Grade	15,911	0.319	0.0016	0.0029	0.373	89,860
			(0.0083)	(0.0151)		
Repeat	15,972	0.077	-0.0035	-0.0066	0.092	90,364
			(0.0025)	(0.0046)		
# Absences	11,991	26.571	-0.0709	-0.1122	26.044	32,380
			(0.4294)	(0.7648)		
# Credits	11,991	4.982	0.0444	0.0803	5.107	32,380
			(0.0429)	(0.0763)		
GPA in Current Year	11,991	1.478	0.0032	0.0044	1.516	32,380
			(0.0193)	(0.0344)		
Tested	15,320	0.881	0.0096**	0.0178***	0.865	77,974
			(0.0038)	(0.0069)		
Composite Test Score	14,348	-0.325	0.0068	0.0126	-0.364	68,787
			(0.0152)	(0.0273)		
Math Test Score	14,268	-0.318	0.0076	0.0136	-0.353	65,174
			(0.0177)	(0.0319)		
Reading Test Score	14,302	-0.326	0.0019	0.0043	-0.372	68,564
			(0.0154)	(0.0278)		

Crime, Cross-Sectional Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Arrested (1998-2011)	21,112	0.5977	0.0072	0.0149	0.6083	21,112
			(0.0076)	(0.0158)		
Sum of social costs (most conservative)	12,638	60,790.7393	-2,508.7597	-5,008.7081	61,595.1978	12,638
			(1,873.2777)	(3,742.6854)		
Sum of social costs (least conservative)	12,638	276,352.0885	-33,833.9441	-67,549.0569	255,388.7391	12,638

Appendix Table VI: Voucher Effects for Males, Age 6-18 Baseline

			(21,569.0028)	(43,067.6849)		
Total Violent Crime Arrests	12,638	1.3493	-0.0369	-0.0737	1.3942	12,638
			(0.0351)	(0.0700)		
Total Property Crime Arrests	12,638	0.5367	0.0205	0.0410	0.5114	12,638
			(0.0234)	(0.0467)		
Total Drug Crime Arrests	12,638	2.6052	-0.1072	-0.2141	2.7018	12,638
			(0.0686)	(0.1371)		
Total Other Crimes Arrests	12,638	3.4704	-0.0046	-0.0092	3.5112	12,638
			(0.0960)	(0.1916)		

Crime, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Arrested in Academic Year	21,112	0.2223	0.0004	0.0008	0.2336	236,701
			(0.0042)	(0.0088)		
Social costs (most conservative)	12,617	14,472.9985	-683.0589	-1,397.3501*	15,334.7704	52,437
			(416.5459)	(835.8209)		
Social costs (least conservative)	12,617	66,010.7205	-10,377.6025*	-21,539.5755**	66,496.5381	52,437
			(5,311.0186)	(10,669.7197)		
# Violent Crime Arrests	12,617	0.3199	-0.0077	-0.0154	0.3300	52,437
			(0.0074)	(0.0148)		
# Property Crime Arrests	12,617	0.1266	0.0030	0.0051	0.1242	52,437
			(0.0055)	(0.0110)		
# Drug Crime Arrests	12,617	0.6231	-0.0167	-0.0313	0.6301	52,437
			(0.0129)	(0.0257)		
# Other Crime Arrests	12,617	0.8303	0.0007	0.0014	0.8839	52,437
			(0.0183)	(0.0367)		

Labor, Public Assistance, and Household Composition, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Total Earnings (2013 $)	21,112	3,889.7830	38.1019	91.7312	3,596.7602	187,586
			(117.3116)	(250.6524)		
Fraction of Year: Employed	21,112	0.2579	0.0042	0.0095	0.2488	187,586
			(0.0046)	(0.0099)		
Fraction of Year: Any Public Assistance	21,112	0.5143	0.0158***	0.0337***	0.5306	295,568
			(0.0054)	(0.0112)		
Fraction of Year: Foodstamps	21,112	0.3624	0.0163***	0.0353***	0.3580	295,568
			(0.0048)	(0.0101)		
Fraction of Year: AFDC/TANF	21,112	0.1295	-0.0022	-0.0037	0.0809	295,568
			(0.0022)	(0.0047)		
Fraction of Year: Medicaid	21,112	0.4349	0.0099**	0.0213**	0.4394	295,568
			(0.0051)	(0.0107)		
Fraction of Year: Address on File	21,112	0.5085	0.0159***	0.0340***	0.5246	295,568
			(0.0054)	(0.0112)		
# People in HH (annual average)	19,146	4.0612	0.0388	0.0718	3.8398	167,736
			(0.0504)	(0.0938)		
# Children in HH (annual average)	19,146	2.0499	-0.0396**	-0.0724**	1.7924	167,736
			(0.0170)	(0.0315)		
September: Any Public Assistance	21,112	0.5235	0.0153***	0.0325***	0.5408	295,568
			(0.0054)	(0.0113)		
September: Foodstamps	21,112	0.3741	0.0147***	0.0318***	0.3691	295,568
			(0.0049)	(0.0103)		
September: AFDC/TANF	21,112	0.1441	-0.0023	-0.0041	0.0915	295,568
			(0.0023)	(0.0050)		
September: Medicaid	21,112	0.4474	0.0098*	0.0208*	0.4532	295,568
			(0.0051)	(0.0107)		
September: Address on File	21,112	0.5180	0.0156***	0.0331***	0.5350	295,568
			(0.0054)	(0.0113)		
# People in HH (September)	18,937	4.1602	0.0572	0.1044	3.9145	153,672
			(0.0523)	(0.0964)		
# Children in HH (September)	18,937	2.2005	-0.0329*	-0.0598*	1.9394	153,672
			(0.0182)	(0.0335)		

Health, Cross-Sectional Estimates

Appendix Table VI: Voucher Effects for Males, Age 6-18 Baseline

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled in Medicaid (2000-2008)	21,112	0.591	0.0077	0.0160	0.642	21,112
			(0.0076)	(0.0157)		
Inpatient hospital claim	12,526	0.095	0.0008	0.0015	0.090	12,526
			(0.0058)	(0.0110)		
Emergency room claim	12,526	0.529	0.0160	0.0300	0.534	12,526
			(0.0103)	(0.0193)		
Inpatient or emergency claim	12,526	0.536	0.0178*	0.0336*	0.540	12,526
			(0.0103)	(0.0193)		
Outpatient claim	12,526	0.887	-0.0001	-0.0001	0.899	12,526
			(0.0062)	(0.0117)		
Injury, inpatient or emergency	12,526	0.378	0.0033	0.0061	0.384	12,526
			(0.0100)	(0.0189)		
Asthma	12,526	0.183	0.0026	0.0049	0.180	12,526
			(0.0080)	(0.0150)		
Routine medical exam	12,526	0.706	0.0006	0.0011	0.730	12,526
			(0.0083)	(0.0155)		

Health, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled in Medicaid in Academic Year	21,112	0.295	0.0113**	0.0237**	0.304	190,008
			(0.0052)	(0.0109)		
Inpatient hospital claim	12,526	0.031	-0.0003	-0.0003	0.031	56,480
			(0.0024)	(0.0044)		
Emergency room claim	12,526	0.241	-0.0061	-0.0110	0.249	56,480
			(0.0059)	(0.0111)		
Inpatient or emergency claim	12,526	0.247	-0.0059	-0.0105	0.255	56,480
			(0.0060)	(0.0112)		
Outpatient claim	12,526	0.759	0.0040	0.0070	0.742	56,480
			(0.0059)	(0.0112)		
Injury, inpatient or emergency	12,526	0.133	-0.0033	-0.0060	0.137	56,480
			(0.0042)	(0.0080)		
Asthma	12,526	0.104	-0.0065	-0.0117	0.106	56,480
			(0.0058)	(0.0108)		
Routine medical exam	12,526	0.456	0.0006	0.0001	0.450	56,480
			(0.0059)	(0.0112)		

Appendix Table VII: Voucher Effects for Females, Age 0-6 Baseline

Education, Cross-Sectional Estimates

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

Education, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled in CPS in Academic Year	11,985	0.509	0.0203***	0.0398***	0.601	167,790
			(0.0068)	(0.0135)		
Grade 1 - 12	9,701	0.870	0.0031*	0.0056*	0.940	82,769
			(0.0017)	(0.0032)		
Old for Grade	9,218	0.156	0.0111	0.0197	0.156	72,076
			(0.0081)	(0.0141)		
Repeat	9,701	0.032	0.0022	0.0038	0.034	82,769
			(0.0015)	(0.0027)		
# Absences	4,844	33.399	-1.6719**	-2.9129**	33.606	12,090
			(0.7850)	(1.3721)		
# Credits	4,844	5.788	0.0559	0.0974	5.777	12,090
			(0.0494)	(0.0862)		
GPA in Current Year	4,844	2.122	0.0371	0.0647	2.103	12,090
			(0.0307)	(0.0536)		
Tested	8,571	0.958	0.0073***	0.0128***	0.955	54,293
			(0.0025)	(0.0044)		
Composite Test Score	8,488	-0.145	0.0019	0.0029	-0.151	52,107
			(0.0183)	(0.0316)		
Math Test Score	8,484	-0.206	-0.0094	-0.0165	-0.210	51,947
			(0.0193)	(0.0333)		
Reading Test Score	8,488	-0.081	0.0113	0.0190	-0.090	52,059
			(0.0192)	(0.0331)		

Crime, Cross-Sectional Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Arrested (1998-2011)	11,985	0.1466	0.0007	0.0013	0.1631	11,985
			(0.0075)	(0.0147)		
Sum of social costs (most conservative)	1,758	15,534.4335	862.5998	1,533.9852	17,003.7007	1,758
			(2,228.1402)	(3,959.2775)		
Sum of social costs (least conservative)	1,758	25,643.7108	18,757.8072	33,357.5311	30,451.6447	1,758

Appendix Table VII: Voucher Effects for Females, Age 0-6 Baseline

			(21,863.7796)	(38,848.9950)		
Total Violent Crime Arrests	1,758	0.7186	-0.0660	-0.1173	0.8055	1,758
			(0.0533)	(0.0950)		
Total Property Crime Arrests	1,758	0.4341	-0.0166	-0.0295	0.4649	1,758
			(0.0387)	(0.0689)		
Total Drug Crime Arrests	1,758	0.1044	-0.0097	-0.0172	0.0922	1,758
			(0.0283)	(0.0504)		
Total Other Crimes Arrests	1,758	0.6228	0.0126	0.0225	0.6402	1,758
			(0.0616)	(0.1096)		

Crime, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Arrested in Academic Year	11,985	0.0539	-0.0001	-0.0002	0.0612	45,095
			(0.0032)	(0.0063)		
Social costs (most conservative)	1,706	10,518.2574	721.8587	1,261.6285	11,428.4087	2,424
			(1,156.2647)	(2,018.7080)		
Social costs (least conservative)	1,706	17,829.9882	11,672.8706	20,401.2588	24,393.4829	2,424
			(13,109.7642)	(22,871.2301)		
# Violent Crime Arrests	1,706	0.4833	-0.0332	-0.0580	0.5211	2,424
			(0.0297)	(0.0519)		
# Property Crime Arrests	1,706	0.3060	0.0034	0.0060	0.2982	2,424
			(0.0252)	(0.0441)		
# Drug Crime Arrests	1,706	0.0734	-0.0063	-0.0111	0.0617	2,424
			(0.0157)	(0.0275)		
# Other Crime Arrests	1,706	0.4370	0.0257	0.0450	0.4133	2,424
			(0.0363)	(0.0634)		

Labor, Public Assistance, and Household Composition, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Total Earnings (2013 $)	6,788	712.718	-69.5465	-135.5828	842.742	13,593
			(62.0875)	(121.1878)		
Fraction of Year: Employed	6,788	0.107	-0.0055	-0.0107	0.117	13,593
			(0.0064)	(0.0125)		
Fraction of Year: Any Public Assistance	11,985	0.698	0.0248***	0.0498***	0.740	167,790
			(0.0076)	(0.0149)		
Fraction of Year: Foodstamps	11,985	0.520	0.0281***	0.0569***	0.549	167,790
			(0.0079)	(0.0155)		
Fraction of Year: AFDC/TANF	11,985	0.199	-0.0020	-0.0038	0.138	167,790
			(0.0042)	(0.0085)		
Fraction of Year: Medicaid	11,985	0.678	0.0251***	0.0502***	0.722	167,790
			(0.0077)	(0.0151)		
Fraction of Year: Address on File	11,985	0.696	0.0253***	0.0507***	0.739	167,790
			(0.0076)	(0.0149)		
# People in HH (annual average)	11,377	3.867	-0.0591**	-0.1069**	3.809	124,693
			(0.0297)	(0.0536)		
# Children in HH (annual average)	11,377	2.413	-0.0050	-0.0095	2.279	124,693
			(0.0186)	(0.0336)		
September: Any Public Assistance	11,985	0.703	0.0239***	0.0478***	0.745	167,790
			(0.0075)	(0.0148)		
September: Foodstamps	11,985	0.528	0.0270***	0.0544***	0.555	167,790
			(0.0079)	(0.0155)		
September: AFDC/TANF	11,985	0.215	-0.0020	-0.0040	0.150	167,790
			(0.0044)	(0.0087)		
September: Medicaid	11,985	0.683	0.0241***	0.0478***	0.727	167,790
			(0.0076)	(0.0150)		
September: Address on File	11,985	0.701	0.0244***	0.0488***	0.743	167,790
			(0.0075)	(0.0148)		
# People in HH (September)	11,331	3.926	-0.0552*	-0.0983*	3.858	118,311
			(0.0302)	(0.0542)		
# Children in HH (September)	11,331	2.479	0.0007	0.0013	2.346	118,311
			(0.0194)	(0.0349)		

Health, Cross-Sectional Estimates

Appendix Table VII: Voucher Effects for Females, Age 0-6 Baseline

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled in Medicaid (2000-2008)	11,985	0.783	0.0025	0.0048	0.825	11,985
			(0.0094)	(0.0185)		
Inpatient hospital claim	9,379	0.071	-0.0061	-0.0112	0.086	9,379
			(0.0060)	(0.0111)		
Emergency room claim	9,379	0.535	0.0234*	0.0434*	0.551	9,379
			(0.0121)	(0.0224)		
Inpatient or emergency claim	9,379	0.543	0.0226*	0.0419*	0.558	9,379
			(0.0121)	(0.0224)		
Outpatient claim	9,379	0.966	-0.0044	-0.0082	0.979	9,379
			(0.0048)	(0.0088)		
Injury, inpatient or emergency	9,379	0.296	0.0057	0.0106	0.315	9,379
			(0.0111)	(0.0205)		
Asthma	9,379	0.197	0.0065	0.0121	0.206	9,379
			(0.0097)	(0.0180)		
Routine medical exam	9,379	0.911	-0.0046	-0.0086	0.943	9,379
			(0.0070)	(0.0130)		

Health, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled in Medicaid in Academic Year	11,985	0.466	0.0145*	0.0303*	0.500	107,865
			(0.0088)	(0.0174)		
Inpatient hospital claim	9,379	0.018	-0.0013	-0.0022	0.022	50,549
			(0.0019)	(0.0035)		
Emergency room claim	9,379	0.208	0.0020	0.0034	0.215	50,549
			(0.0061)	(0.0111)		
Inpatient or emergency claim	9,379	0.212	0.0018	0.0032	0.220	50,549
			(0.0062)	(0.0113)		
Outpatient claim	9,379	0.876	-0.0049	-0.0099	0.899	50,549
			(0.0050)	(0.0090)		
Injury, inpatient or emergency	9,379	0.076	-0.0006	-0.0009	0.085	50,549
			(0.0033)	(0.0059)		
Asthma	9,379	0.098	0.0011	0.0017	0.112	50,549
			(0.0063)	(0.0113)		
Routine medical exam	9,379	0.674	-0.0075	-0.0151	0.710	50,549
			(0.0063)	(0.0114)		

Appendix Table VIII: Voucher Effects for Females, Age 6-18 Baseline

Education, Cross-Sectional Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled in CPS (1998-2011)	21,225	0.820	0.0014	0.0029	0.847	21,225
			(0.0019)	(0.0040)		
Cumulative GPA (Final Year)	11,074	1.880	0.0039	0.0073	1.857	11,074
			(0.0180)	(0.0337)		
Cumulative Credits (Final Year)	11,074	19.544	0.3041	0.5696	19.627	11,074
			(0.1867)	(0.3494)		
Final Non-missing Math Score	14,790	-0.349	0.0257**	0.0499**	-0.421	14,790
			(0.0124)	(0.0241)		
Final Non-missing Reading Score	14,837	-0.255	0.0215*	0.0418*	-0.319	14,837
			(0.0121)	(0.0236)		
Average Math Score (1998-2011)	14,948	-0.222	0.0338***	0.0659***	-0.291	14,948
			(0.0111)	(0.0218)		
Average Reading Score (1998-2011)	14,998	-0.148	0.0170	0.0331	-0.197	14,998
			(0.0109)	(0.0213)		
Non-missing Final Status	17,336	0.999	-0.0004	-0.0008	1.000	17,336
			(0.0005)	(0.0011)		
Attrited (Moved or Enrolled in Private)	17,323	0.208	-0.0154**	-0.0306**	0.174	17,323
			(0.0072)	(0.0142)		
Graduated	13,792	0.577	0.0101	0.0190	0.585	13,792
			(0.0094)	(0.0176)		
Enrolled 2-year school (public or private)	8,009	0.517	-0.0162	-0.0300	0.533	8,009
			(0.0127)	(0.0235)		
Enrolled 4-year public school	8,009	0.260	0.0005	0.0010	0.240	8,009
			(0.0107)	(0.0198)		
Enrolled 4-year private school	8,009	0.197	-0.0076	-0.0142	0.197	8,009
			(0.0097)	(0.0180)		

Education, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled in CPS in Academic Year	21,225	0.334	0.0151***	0.0322***	0.261	297,150
			(0.0031)	(0.0065)		
Grade 1 - 12	16,394	0.998	0.0001	0.0002	0.999	94,853
			(0.0003)	(0.0007)		
Old for Grade	16,357	0.225	-0.0115	-0.0213	0.266	94,519
			(0.0076)	(0.0146)		
Repeat	16,394	0.052	-0.0041**	-0.0079*	0.059	94,853
			(0.0021)	(0.0041)		
# Absences	13,160	24.352	0.0169	0.0114	24.721	38,141
			(0.3699)	(0.7018)		
# Credits	13,160	5.675	0.0197	0.0380	5.760	38,141
			(0.0330)	(0.0626)		
GPA in Current Year	13,160	1.920	0.0066	0.0121	1.912	38,141
			(0.0194)	(0.0368)		
Tested	15,599	0.909	0.0030	0.0052	0.902	80,607
			(0.0033)	(0.0065)		
Composite Test Score	14,855	-0.148	0.0168	0.0300	-0.208	73,389
			(0.0143)	(0.0273)		
Math Test Score	14,779	-0.183	0.0314*	0.0575*	-0.255	69,435
			(0.0163)	(0.0311)		
Reading Test Score	14,824	-0.112	0.0066	0.0116	-0.171	73,254
			(0.0151)	(0.0289)		

Crime, Cross-Sectional Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Arrested (1998-2011)	21,225	0.3479	0.0105	0.0218	0.3580	21,225
			(0.0077)	(0.0160)		
Sum of social costs (most conservative)	7,434	18,808.5482	2,064.0382**	4,037.2919**	18,380.1969	7,434
			(921.6434)	(1,799.7770)		
Sum of social costs (least conservative)	7,434	43,280.7437	-1,595.7060	-3,121.2269	45,963.1995	7,434

Appendix Table VIII: Voucher Effects for Females, Age 6-18 Baseline

			(9,385.6871)	(18,359.9645)		
Total Violent Crime Arrests	7,434	0.7600	0.0704**	0.1378**	0.7365	7,434
			(0.0322)	(0.0629)		
Total Property Crime Arrests	7,434	0.6079	-0.0211	-0.0412	0.5996	7,434
			(0.0267)	(0.0522)		
Total Drug Crime Arrests	7,434	0.4050	0.0457	0.0894	0.3515	7,434
			(0.0352)	(0.0690)		
Total Other Crimes Arrests	7,434	1.0064	0.0334	0.0653	1.0377	7,434
			(0.0501)	(0.0980)		

Crime, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Arrested in Academic Year	21,225	0.0640	0.0045**	0.0096**	0.0658	238,962
			(0.0021)	(0.0044)		
Social costs (most conservative)	7,410	8,977.1603	370.9570	589.9846	9,377.8922	15,526
			(341.0444)	(676.7448)		
Social costs (least conservative)	7,410	20,803.4026	-5,912.8921	-12,875.0477	28,338.8984	15,526
			(4,024.9645)	(8,099.6566)		
# Violent Crime Arrests	7,410	0.3616	0.0261**	0.0515**	0.3447	15,526
			(0.0124)	(0.0242)		
# Property Crime Arrests	7,410	0.2909	-0.0177	-0.0345	0.2815	15,526
			(0.0118)	(0.0231)		
# Drug Crime Arrests	7,410	0.1955	0.0114	0.0206	0.1773	15,526
			(0.0139)	(0.0273)		
# Other Crime Arrests	7,410	0.4848	-0.0068	-0.0133	0.5353	15,526
			(0.0176)	(0.0347)		

Labor, Public Assistance, and Household Composition, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Total Earnings (2013 $)	21,225	5,463.2029	-71.9918	-145.4267	5,800.1344	189,821
			(117.8536)	(249.3975)		
Fraction of Year: Employed	21,225	0.3791	-0.0033	-0.0068	0.3951	189,821
			(0.0049)	(0.0103)		
Fraction of Year: Any Public Assistance	21,225	0.6273	0.0215***	0.0458***	0.6610	297,150
			(0.0056)	(0.0119)		
Fraction of Year: Foodstamps	21,225	0.4777	0.0214***	0.0468***	0.4995	297,150
			(0.0054)	(0.0114)		
Fraction of Year: AFDC/TANF	21,225	0.1757	0.0026	0.0062	0.1216	297,150
			(0.0028)	(0.0061)		
Fraction of Year: Medicaid	21,225	0.5690	0.0148***	0.0316***	0.5950	297,150
			(0.0057)	(0.0120)		
Fraction of Year: Address on File	21,225	0.6250	0.0216***	0.0462***	0.6580	297,150
			(0.0057)	(0.0119)		
# People in HH (annual average)	20,019	3.4677	0.0052	0.0086	3.2240	203,385
			(0.0265)	(0.0511)		
# Children in HH (annual average)	20,019	1.7297	0.0037	0.0073	1.4282	203,385
			(0.0150)	(0.0290)		
September: Any Public Assistance	21,225	0.6323	0.0203***	0.0434***	0.6661	297,150
			(0.0056)	(0.0119)		
September: Foodstamps	21,225	0.4842	0.0210***	0.0458***	0.5032	297,150
			(0.0055)	(0.0115)		
September: AFDC/TANF	21,225	0.1884	0.0023	0.0055	0.1306	297,150
			(0.0029)	(0.0063)		
September: Medicaid	21,225	0.5756	0.0142**	0.0301**	0.6016	297,150
			(0.0056)	(0.0119)		
September: Address on File	21,225	0.6298	0.0204***	0.0436***	0.6632	297,150
			(0.0056)	(0.0119)		
# People in HH (September)	19,899	3.5856	0.0141	0.0254	3.3237	187,956
			(0.0274)	(0.0523)		
# Children in HH (September)	19,899	1.8503	0.0126	0.0243	1.5354	187,956
			(0.0155)	(0.0298)		

Health, Cross-Sectional Estimates

Appendix Table VIII: Voucher Effects for Females, Age 6-18 Baseline

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled in Medicaid (2000-2008)	21,225	0.754	0.0096	0.0200	0.792	21,225
			(0.0070)	(0.0146)		
Inpatient hospital claim	16,050	0.418	0.0153*	0.0299*	0.408	16,050
			(0.0082)	(0.0160)		
Emergency room claim	16,050	0.626	0.0138	0.0269	0.629	16,050
			(0.0088)	(0.0172)		
Inpatient or emergency claim	16,050	0.699	0.0239***	0.0467***	0.691	16,050
			(0.0081)	(0.0159)		
Outpatient claim	16,050	0.953	0.0003	0.0006	0.960	16,050
			(0.0039)	(0.0075)		
Injury, inpatient or emergency	16,050	0.310	0.0094	0.0184	0.306	16,050
			(0.0083)	(0.0163)		
Asthma	16,050	0.191	0.0022	0.0043	0.199	16,050
			(0.0072)	(0.0140)		
Routine medical exam	16,050	0.646	0.0020	0.0039	0.662	16,050
			(0.0080)	(0.0157)		

Health, Panel Estimates

Outcome	Individuals	CM	ITT	IV	CCM	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled in Medicaid in Academic Year	21,225	0.394	0.0112*	0.0239*	0.417	191,025
			(0.0058)	(0.0124)		
Inpatient hospital claim	16,050	0.137	0.0029	0.0053	0.140	75,526
			(0.0032)	(0.0063)		
Emergency room claim	16,050	0.311	0.0000	0.0001	0.324	75,526
			(0.0055)	(0.0108)		
Inpatient or emergency claim	16,050	0.370	0.0025	0.0047	0.382	75,526
			(0.0056)	(0.0108)		
Outpatient claim	16,050	0.861	0.0059	0.0117	0.859	75,526
			(0.0040)	(0.0079)		
Injury, inpatient or emergency	16,050	0.097	0.0001	-0.0002	0.099	75,526
			(0.0031)	(0.0060)		
Asthma	16,050	0.098	0.0018	0.0033	0.107	75,526
			(0.0049)	(0.0095)		
Routine medical exam	16,050	0.370	0.0055	0.0103	0.363	75,526
			(0.0046)	(0.0089)		

Appendix Table IX: Housing Voucher Effect on Test Scores, Cross-Sectional Models

								ITT Quantile Treatment Effect					IV Quantile Treatment Effect[1]				
Gender	**Baseline Age**	**Outcome**	**Individuals**	**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	Age 0 to 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)											
Male	Age 6 to 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	Age 0 to 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)											
Female	Age 6 to 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.

Appendix Table X: Housing Voucher Effect on Geographic Outcomes (IDHS Data)

	Individuals (1)	CM (2)	ITT (3)	IV (4)	CCM (5)	Obs. (6)
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.