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THE EFFECTS OF HOUSING VOUCHERS ON CHILDREN'S OUTCOMES

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THE EFFECTS OF HOUSING VOUCHERS ON CHILDREN'S OUTCOMES

ABSTRACT

This paper examines the causal effects of housing vouchers on children's outcomes using data from a randomized housing-voucher wait-list lottery conducted in Chicago in 1997. Unlike with MTO, where the offer of a voucher to families in public housing lead to large changes in neighborhood environments, our families are all in private-market housing at baseline, and for them voucher receipt represents a fundamentally different treatment: vouchers lead to almost no change in neighborhood attributes, but generate massive increases in housing consumption and cash income (from reductions in out-of-pocket spending on housing). We estimate fairly precise zero impacts of voucher receipt on achievement test scores. However housing vouchers reduce problem or criminal behavior among youth, particularly for males, and the monetized value of these benefits are relatively large compared to the government cost of the voucher subsidies. Ignoring these types of behavioral impacts may lead analyst to understate the benefits associated with means-tested transfer programs more generally. Our findings are also consistent with the idea that non-cognitive skills may be more malleable and susceptible to policy intervention over the life course than are cognitive skills.

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I. INTRODUCTION

In this paper we present what we believe are the first credible estimates for the causal effects on children's outcomes from expanding the nation's housing voucher program,[1] using data from a randomized housing voucher lottery. Currently only around 28 percent of renters with incomes below 50 percent of the local median (the usual eligibility limit for housing programs) receive any government housing assistance (Olsen, 2003). Evidence about the effects of expanding housing programs on poor children is directly relevant for public policy, since an important goal of federal housing policy since at least the Housing Act of 1949 has been to improve family well being broadly defined.

Our analytic sample consists of families who at the time they applied for a housing voucher were living in private-market housing without any government housing assistance. For these families, voucher receipt generates massive changes in the consumption of housing and other goods: the average subsidy value is on the order of $7,600 per year, compared to a mean (reported) baseline household income of around $10,700. Families can take a large share of this subsidy as cash by reducing their out-of-pocket spending on housing, and would have spent a large share of a cash transfer on housing anyway. Winning the housing voucher lottery is close to receiving a huge "helicopter drop" of money.

In this sense we are evaluating a fundamentally different policy treatment than the one experienced by families in the well-known Moving to Opportunity (MTO) study,

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[1] Throughout the paper we use the term "housing voucher" as shorthand for tenant-based rental subsidies. 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 that is above the usual program limit 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 the Housing Choice Voucher program.

who were already receiving government subsidies (in the form of public housing) when they were offered housing vouchers.[2] MTO families are not able to reduce out-of-pocket spending on housing, since rules about participant rent contributions are the same in the public housing and housing voucher programs, and mainly experience large, persistent changes in neighborhood environments. MTO identifies the effects of a policy of changing the mix of tenant- and project-based housing subsidies, which is an important but different question than the one we address here.

Our research question should be of interest to economists for at least two reasons. One motivation follows from the substantial resources that are devoted to means-tested housing programs in the U.S. In 2002, around $24 billion was spent on housing programs for poor families with children, almost as much as on EITC benefits for such families (around $28 billion), and more than what was spent on TANF (about $22 billion), Food Stamps ($13 billion), SSI ($5 billion), and child care (around $9 billion) (Currie, 2006, p. 157-8). Yet there is remarkably little persuasive evidence about the effects of housing programs on children's life chances.

A second reason our study should be of interest is that we have an unusually strong research design that may help shed light on broader debates about the effects of income transfers on children's outcomes. The standard human capital model raises the possibility that credit constraints may cause many low-income families to make sub-

[2] Because program requirements for what families contribute towards rent is the same in public housing and with a housing voucher, MTO families who receive vouchers are not able to substantially expand their spending on other goods by reducing spending on housing. They may experience some gain in housing consumption, but this change will be difficult to quantify since there is nothing like a market rent for public housing. Most of the policy treatment dose in MTO comes from changing neighborhoods, which stands in market contrast to our own analytic sample here, which as we demonstrate below experiences almost no change in neighborhood conditions from receiving a housing voucher. In the MTO study voucher receipt has no detectable effect on achievement test scores for children but leads to improvements in other behaviors for female youth, and on balance detrimental impacts for male youth (Kling, Ludwig and Katz, 2005, Sanbonmatsu et al., 2007, Kling, Liebman and Katz, 2007).

optimal investments in their children. On the other hand, Mayer (1997) notes that the parental investments most highly correlated with children's outcomes – such as books in the home, or trips to museums – depend more on parent time and interest than on money.[3]

Identifying the effects of income transfers on poor children is complicated by the fact that benefits from most social programs are available to all eligible families. As a result, most studies rely on variation across states or over time in the generosity of program benefits or other program rules (Mayer, 1997, Dahl and Lochner, 2005), or in economic shocks like layoffs (Oreopolous, Page and Stevens, 2005). The results of this literature are mixed, and given the non-experimental research designs employed, necessarily always subject to some concerns about selection.[4]

We are able to overcome these identification problems by taking advantage of the excess demand that exists for housing assistance. We examine the housing voucher program in the city of Chicago, which in the mid-1990s was transferred from the Chicago Housing Authority (CHA) to a local private firm, CHAC, Inc. In 1997, CHAC re-opened the voucher program's wait list for the first time in a dozen years. A total of 82,607 eligible families applied, far in excess of the number of available vouchers, and so applicants were assigned by random lottery to a wait list. We show below that a family's position in the wait-list lottery substantially affects their chances of receiving a voucher,

[3] In addition sociologists and anthropologists dating back at least to Oscar Lewis (1959, 1966) have argued that a "culture of poverty" makes it difficult for low-income parents to carry out the sort of productive, pro-social activities that will help their families escape from poverty.

[4] Only two previous studies use experimental variation, Morris, Duncan and Rodrigues (2004), who study welfare-to-work experiments, and Mills et al. (2006), who evaluate the effects of HUD's Welfare to Work Voucher program. We discuss the limitations of these studies for our question in detail below.

but is unrelated to a variety of basic demographic characteristics or pre-lottery outcomes – that is, the random voucher lottery was indeed random.[5]

The analytic sample for the present paper consists of CHAC voucher applicants who were already living in private-market housing when they applied, which is about 90% of the 82,606 CHAC applicant families. We measure cognitive outcomes for children by matching data on CHAC applicants to student-level school records from the Chicago Public Schools (CPS). We measure non-cognitive outcomes for youth in our sample through CPS data on school dropout, as well as through official arrest histories obtained from the Illinois State Police (ISP). Studying non-cognitive skills is important because previous research suggests such skills are quite important for children's long-run life outcomes, and may be more malleable than cognitive skills throughout the life course (Shonkoff and Phillips, 2000, Carniero and Heckman, 2003, Knudsen et al., 2006).

We find that housing vouchers have no detectable impacts on children's cognitive outcomes, in the form of reading and math scores on the Iowa test of basic skills (ITBS), but do have beneficial impacts on non-cognitive outcomes like school persistence or avoidance of criminal activity. Given our large sample, the zero impacts on achievement test scores are fairly precisely estimated: our results suggest that the effect of a $7,600 voucher subsidy on ITBS reading scores is no larger than 4% of a standard deviation, while the impacts on math scores can be no larger than 7% of a standard deviation. Interestingly, the beneficial impacts of housing vouchers on non-cognitive outcomes are largely concentrated among male youth, which differs from previous studies that provide

[5] Shea (2000, p. 182) hopes that "perhaps future researchers will focus on more convincingly exogenous sources of parental income variation, such as lottery winnings or large changes in public transfers." We essentially combine both of these sources of variation (a random lottery for receipt of public transfers).

in-kind benefits like preschool or residential mobility,[6] but are quite consistent with other resource-transfer programs like New Hope (Duncan, Huston and Weisner, 2007). While our findings can only be directly generalized to low-income urban minority families who would volunteer for a housing voucher program, these results at least raise the possibility that expansions of means-tested transfer programs could potentially help narrow the gender gap in outcomes between African-American males and females (see Kling, Ludwig and Katz, 2005).

The findings from our study are also important in part because there are not many social programs that have been demonstrated to be effective in reducing problem behaviors among high-risk youth. The costs of crime to American society are enormous, perhaps as much as $2 trillion per year (Ludwig, 2006). Our estimates suggest that ignoring the behavioral impacts of means-tested transfer programs on children could lead analysts to substantially understate the social benefits of such programs.

The remainder of the paper is organized as follows. The next section discusses the housing voucher lottery that is the key to our research design, while Section III discusses our data and the basic characteristics of our analytic sample. Section IV reviews potential behavioral mechanisms through which housing vouchers might affect children's outcomes as well as previous evidence on these mechanisms. Section V presents our empirical strategy, Section VI discusses our main findings, and Section VII discusses some implications of our results.

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[6] Preschool programs have found more beneficial impacts for girls than for boys; for example Schweinhart et al. (2005) for Perry Preschool, Campbell et al. (2002) for Abecedarian, and Anderson (2007) who re-examines data from both programs. Similar findings of bigger gains are found in MTO; see Kling, Ludwig, and Katz (2005), Kling, Liebman and Katz (2007), and Sanbonmatsu et al. (2007).

II. THE CHICAGO HOUSING VOUCHER LOTTERY

Housing vouchers subsidize low-income families to live in private-market housing.[7] Eligibility limits for housing programs are a function of family size and income, and 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). The maximum subsidy available to families is governed by the Fair Market Rent (FMR), which equaled the 45th percentile of the local private-market rent distribution through 1995, was lowered to the 40th percentile in 1995, and then in 2001 selected metropolitan areas, including Chicago, have been allowed to set FMR equal to the 50th percentile. By way of background, the FMR for a two-bedroom apartment in the Chicago area was equal to $699 in 1994, $732 in 1997, and $762 in 2000.

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 are not counted. The voucher covers the difference between the family's rent contribution and the lesser of the FMR or the unit rent. 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.

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

As noted above, housing assistance is not an entitlement. In Chicago, as in other big cities, there are generally extremely long waiting lists to receive housing assistance, especially for housing vouchers. Once a family receives a housing voucher they can keep the subsidy for as long as they meet the program's income and other eligibility requirements. Despite the excess demand for housing vouchers, not all families offered vouchers wind up using them. Many apartments have rents above the FMR limit, some landlords may avoid renting to voucher families,[8] and families offered vouchers have a limited time (usually 3 to 6 months) to use the voucher to lease up a unit.

The Chicago housing voucher lottery that we evaluate in this study was conducted during a decade of considerable turmoil in the city's low-income housing programs. In 1995, the U.S. Department of Housing and Urban Development (HUD) took over the CHA's operations in response to the latter's poor management of the city's low-income housing programs. In addition to demolishing thousands of the city's project-based housing units and turning others over to private companies to operate,[9] the new CHA management also made the decision to turn over operation of the city's voucher program to a new private organization, the Chicago Housing Authority Corporation (CHAC).

In July 1997, CHAC conducted an open registration for housing vouchers, the first time in twelve years that the city's voucher wait list had been opened. More than 105,000 households applied to CHAC for vouchers, of whom 82,607 were found to be income-eligible for tenant-based housing subsidies. While CHAC's initial plan had been

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[8] Some landlords may avoid renting to voucher families because of the paperwork requirements, the program's minimum housing quality standards (which must be verified by an inspection, although failed units can be modified and re-inspected), and a previous rule that has since been abolished that limited the ability of landlords to turn away future voucher applicants ("take one, take all").
[9] "CHA Turnaround is No Overnight Project," Chicago Sun-Times, by Gilbert Jimenez, 12/3/95, p. 61, and "Room for Improvement at CHA," Gilbert Jimenez, Chicago Sun-Times, 5/26/96, p. 33.

to randomly assign 25,000 families to the voucher waiting list, given the strong demand for these subsidies the agency randomly ordered all eligible applicants and assigned the first 35,000 to the active wait list.[10] The other eligible households (that is, with lottery numbers from 35,001 to 82,607) were not placed on any waiting list; because these families had no realistic prospects of receiving a voucher in the foreseeable future we use this group as our preferred control group in our analyses.

By August 1997, CHAC notified families by mail of their position on the voucher wait list, and began the process of offering housing vouchers to a limited number of households with the lowest (that is, the best) lottery numbers. Roughly 4,625 families were offered vouchers in the first year of the program. Service of the 1997 wait list was interrupted in August 1998 as CHAC was required to provide vouchers to a special waiting list of Latino families in response to a discrimination lawsuit filed against the city of Chicago.[11] CHAC began to serve their original wait list again at the beginning of 2000, when 2,500 families were offered vouchers. Another 5,800 families were offered vouchers in 2001, while 4,700 were offered vouchers in 2002 so that by the end of that year nearly half the original 1997 active wait-list (17,663 out of 35,000) had been offered a voucher. In May 2003 CHAC had reached a point where the agency was over-leased, at which point the agency sent out a letter to all families still on the wait list asking them to verify their current address and continued interest in receiving a voucher,[12] and then notified the respondents that they would likely have to wait at least a year and in most

[10] "CHA to expand Sec. 8 waiting list by 10,000," Leon Pitt, Chicago Sun-Times, August 19, 1997.
[11] "CHA, HUD Settle Suit Over Bias Against Hispanics," G. Jimenez, Chicago Sun-Times, 4/23/96, p. 12. At that point, CHAC notified families still on the waiting list that they could expect to be offered a voucher "*at least* one year later than originally planned" (emphasis in original).
[12] That is, CHAC had issued as many or more vouchers than it had funding to pay for, and the turnover rate was low enough that it only provided enough vouchers for a series of special programs such as public housing relocation, victim assistance, witness protection, etc. that receive the highest priority for vouchers.

cases longer for a voucher.[13] In the present study we focus on families who were offered vouchers by CHAC through 2003.

III. DATA

In this section we discuss how we identify children living in CHAC applicant households, since they are not listed by name on the voucher application forms submitted to CHAC, and then discuss the administrative data sources that we use to measure voucher impacts on children's cognitive and non-cognitive outcomes.

A. Identifying Children in Voucher-applicant Households

One challenge for our study is that the CHAC voucher application forms ask for the name, DOB and SSN for the household head and (if relevant) spouse, as well as the *number* of children in the home, but not for the names of children in the home. We take advantage of the fact that most of our families are low-income and received social services at some point prior to the CHAC lottery, and use these pre-lottery social program records to identify children. Our specific three-step process for identifying children is:

(1) First, we identify the most recent social program spells for CHAC applicant adults that occurred before the CHAC housing-voucher lottery (July, 1997);

(2) Identify the other people listed as household members in these program spells;

(3) Eliminate people who were obviously not living with the CHAC applicant as of July 1997 as best we can tell from social program data.[14]

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[13] Roughly 9,300 families responded by the September 5, 2003 deadline. Around 4,000 letters were returned by the post office and the remainder did not respond. In a follow-up letter, CHAC indicated that families with numbers between 18,110 and 20,853 should expect to wait at least one year; numbers between 20,854 and 27,455 at least two years; 27,457 to 33,902 at least three years and the remainder at least four years. Personal communication with Ken Coles, CHAC, on 4/8/2004.

[14] Specifically, we examine whether the other candidate household members identified by the first two steps above show up on another welfare case that is subsequent to the start of the CHAC applicant's most recent pre-lottery welfare case. If so, we compare the address listed for the candidate household member's

Our process for identifying household members is, while carefully done, necessarily imperfect. For example the average household size identified by our imputation procedure is somewhat smaller than what is recorded on the CHAC application forms (2.4 versus 3.0). Our procedure seems to do a fairly good job identifying school-aged children in CHAC applicant households, obviously key for present purposes, and does a bit less well capturing children who are ages 0 to 5 at the time of the CHAC lottery.[15] Importantly, these errors in identifying household size are not related to a family's position on the CHAC housing-voucher wait list.

B. Administrative Data

We measure the outcomes of housing-voucher receipt using administrative data from a variety of sources. These include student-level school records from the Chicago Public Schools (CPS), which are available through the 2004-5 academic year and include information about each child for each semester they were enrolled in the CPS. This information includes the school attended, grade, home address, race, gender, legal guardian, and special education status. For younger children (through middle school) we have standardized reading and math achievement tests on the Iowa Test of Basic Skills (ITBS). For older children we look at high school absences, grades, course credits, and school dropout. For additional details on these data see Jacob (2004).

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more recent welfare case with the address listed for the CHAC applicant's most recent welfare case, and then assume that people with a different address are not living with the CHAC applicant at baseline.

[15] When families leased up with a CHAC voucher they are required to complete a HUD 50058 form that asks them to report the name and DOB of everyone in the home, including children. This provides identifying information for all household members for 10,422 families. Compared to the household composition we identify using the social program matching procedure described above, the 50058 data show about 1,500 more children who are ages 0-5 at baseline, and similar numbers of children who are 5-12 at the time of the CHAC lottery. One potential problem with the 50058 data is that not everyone living with the CHAC applicant when they apply may actually move with the applicant when they are given a voucher, while other relatives not living with the applicant at the time of the lottery may decide to join the household after lease-up with a voucher. This is difficult to rule out, but the average household size from the 50058 forms is quite close to the figure reported on each household's CHAC application form.

For older children in the CHAC housing-voucher lottery we examine involvement with criminal behavior by examining official arrest histories maintained by the Illinois State Police (ISP), which capture all arrests made by law enforcement at any level in the state of Illinois over the period 1990 to 2005.[16] Arrests are linked to individuals using biometric data (fingerprints), and so we will capture a given person's entire arrest history even if they report a false name at the time of one of their arrests. These arrest histories include information on the date of each arrest, all criminal charges filed as a result of the arrest, and the disposition of each arrest.[17] In cases where the arrestee is charged with multiple criminal charges (16% of all arrests), we assign the arrest the most serious criminal charge based on the class of the offense under Illinois state law. Because additional family resources may have different effects on different types of criminal behavior, we separately examine arrests for different types of crime (violent, property, drugs, and other). For additional details see Kling, Ludwig and Katz (2005).

Finally, we track post-assignment addresses for a 10% randomly selected sub-sample of everyone on the CHAC waiting list (regardless of voucher receipt status) through a check of credit bureau records, change of address forms and other passive-tracking sources conducted for us by the National Opinion Research Center. (We track only a random sub-sample of families for budgetary reasons). These address histories also enable us to examine whether lottery numbers are systematically related to the probability that families move out of the state of Illinois, which in turn contributes to

[16] In practice the degree to which these data capture juvenile arrests seems to improve over the 1990s, which is good news for our study since the post-assignment period begins about the middle of 1997.

[17] The entire CHAC applicant pool experienced a total of 340,156 arrests in the ISP data system from 1990 to 2005, of which we have valid information on the date of the criminal offense itself for just 148,513 cases, and so we rely on the date of arrest instead. In 0.16% of cases the listed date of offense comes before the date of arrest, while 95.6% of these cases the date of the arrest and offense are the same. Fully 97.9% of all arrests are within 1 month of the offense data, 98.66% of arrest dates are within 3 months of the offense date, and 99.5% of these cases have an arrest date that is within one year of listed offense date.

missing data and sample attrition in our study since we are relying on state-level administrative records to measure outcomes.

Table 1 provides descriptive statistics for our main analytic sample – children who we have identified as members of CHAC applicant households and ages 8-18 at the time of application (7/1/97), who show up in the CPS data some time in any of the three academic years prior to the CHAC lottery.[18] We have 26,956 children who were living in private-market housing at the time their parents applied to CHAC who were then assigned a lottery number of 35,001 or higher; these families were told by CHAC that they would not receive a voucher at any point in the foreseeable future, and serve as our control group in the analysis below. Our main treatment group has 7,550 children in private-market housing at baseline whose families were offered vouchers through 2003.

Our program population is quite disadvantaged at baseline, both economically and academically. Almost all of the families in our analytic sample are headed by an unmarried African-American woman (columns 1 and 2 of Table 1). Around 60 percent of these households were receiving AFDC benefits at the time they applied for a housing voucher. The average child in our sample scored at the 32nd percentile on the ITBS tests in the year before the voucher lottery (AY 1996-7); around 13% received special education services, and about one-quarter were older than we would expect given the grade in which they were enrolled, suggesting they had been retained at least once before. Relatively few had been arrested prior to the voucher lottery but that is most likely because much of our sample is still quite young at the time of the lottery.

[18] We impose the requirement that children show up in the CPS data before the lottery because of our concern that lottery outcomes might affect the probability that children enroll in the Chicago public schools (rather than private schools) during the post-lottery period. If for example families use part of their voucher resources to send their children to private schools, the composition of children observed in our CPS data for the treatment versus control groups will be systematically different.

While our primary interest in the present paper is in families who were already in private-market housing when they applied for vouchers, for completeness we also show the baseline characteristics for families living in public housing at the time they applied to CHAC (columns 5 and 6). The families living in public housing at baseline are even more disadvantaged than those voucher applicants living in private housing: around 75 percent were receiving AFDC when they applied for a voucher.

Table 1 also provides some evidence to confirm that the randomized housing voucher lottery implemented by Abt Associates on behalf of CHAC was in fact random. We compare the baseline average characteristics of our "treatment" group (families with lottery numbers from 1 to 18,102, who had been offered vouchers by CHAC through 2003) with the remaining "control" families. Table 1 shows that the average baseline characteristics of the treatment and control families living in private-market housing at baseline were quite similar, which is confirmed by a more formal F test.[19]

IV. BEHAVIORAL MECHANISMS AND PREVIOUS RESEARCH

The most obvious mechanisms through which vouchers might improve children's cognitive and non-cognitive outcomes is by improving the quality of the housing (or neighborhoods) that families consume, and by allowing families to consume more of all other goods as well by reducing their out-of-pocket spending on housing. Previous research on the effects on children's outcomes from housing vouchers, or income transfers more generally, is limited and yields mixed results.

A. Voucher "first stage"

[19] That is, we run a stacked regression of all of the baseline characteristics shown in Table 1 against an indicator for treatment rather than control lottery assignment. We then conduct an F-test for the joint significance of the treatment indicator, adjusting for the non-independence of baseline characteristics within households.

In the present paper we focus our attention on the 90% of CHAC applicants who were living in private-market housing when they applied for a voucher in July 1997. For these families, we expect voucher receipt to have very little impact on neighborhood characteristics based on previous evaluations of a randomized rental-subsidy experiment from the 1970s (Struyk and Bendick, 1981). In the results section below we confirm this is the case in our CHAC sample, and so do not devote any discussion here to a discussion of the mechanisms through which neighborhoods might influence child outcomes.

On the other hand our calculations suggest that receipt of a housing voucher will generate very large changes in consumption of both housing and all other goods for families living in private housing when they applied to CHAC. While the CHAC voucher applications do not ask people to report their baseline incomes, using our administrative data described above we estimate combined earnings and TANF income of nearly $9,900 per household at baseline.[20] To this figure we add EITC benefits. The average EITC benefit amount for all recipients in the U.S. in 1997 was on the order of $1,600 per year (Hotz and Scholz, 2003, p. 155). Since around half of CHAC applicants were working at baseline and EITC utilization rates are generally quite high, we assume an average of $800 per year across all CHAC applicants in EITC income. In sum we estimate total reported income the year before the lottery to be on average about $10,700.

The value of the housing voucher subsidies to families is a very large share of this baseline income. The government cost of these voucher subsidies is determined by the amount of the family's contribution, which we can estimate since this is 30% of the

[20] We use UI data to calculate each household's earnings during the four quarters before the lottery (96:3, 96:4, 97:1 and 97:2). We have social program participation data that allows us to determine which households are receiving TANF benefits in each of these quarters, but not the amount of the benefits, so we use the data on TANF receipt together with published figures for maximum TANF benefit amounts conditional on earnings and the UI earnings data to estimate each household's TANF benefits.

family's adjusted income, as well as the FMR, which we can calculate exactly since we know household size and the gender mix of children from the CHAC application forms and which equals about $860 on average per month for our households. Our calculations suggest that the average voucher subsidy in our study is around $7,600 per year.[21]

We do not know baseline rents for CHAC applicants, but from data from the 2000 Census on rents paid by other households with similar family structures living in the same baseline census tracts, we estimate average baseline rents of perhaps around $500 per month. Our rent estimate suggests that families are paying more than half their baseline reported income on rent. But our sample of low-income single-parent households probably have some unreported income as well (Edin and Lein, 1997), and we might be overstating baseline rent expenditures somewhat if voucher applicants were spending a bit less on housing then neighboring households that did not apply for vouchers. These calculations suggest that a voucher will enable families to reduce out-of-pocket spending on housing by up to $3,200, or somewhat less if our estimates overstate actual baseline rent expenditures by these families.

Since families are able to substantially scale back their out-of-pocket on spending, and the marginal propensity to consume housing is in general fairly high, the cash equivalent to receiving a housing voucher is not so far below the government cost of the

[21] The average FMR for families in our sample in 1997 is $860 / month ($10,320 / year). We estimate the average family contribution towards rent to be about $2760. We estimate about a $1500 difference between gross income and adjusted income in our sample given that the average household has about 1.5 children and 26% contain at least one disabled member (Table 1), plus EITC benefits are excluded from the adjusted income calculation under voucher program rules. Adjusted income could be somewhat lower than we estimate since we do not have information about medical expenses (those over 3% of gross income are excluded from adjusted income) or child care expenses, which can also be deducted from gross income.

voucher subsidy.[22] Previous estimates suggest the equivalent variation of a voucher is typically on the order of 70% of the subsidy cost, which in our case implies the equivalent variation for the average family in our sample is about $5,300.

While these estimates for baseline incomes, rents, subsidy values and equivalent variation are subject to some uncertainty, it is clear in any case that vouchers generate large proportional changes in total consumption (and even disposable cash) for families in our study. Note also that any errors in our baseline rent and income calculations will be orthogonal to CHAC voucher lottery numbers by construction.

In addition to changing housing consumption and disposable cash available to families, voucher receipt generates a small increase in the amount of time parents have to spend with their children (or do anything else) from a slight reduction in parental labor supply. Jacob and Ludwig (2007) estimate that for non-elderly, non-disabled adults – i.e., those who would most likely be the parents of the children who serve as the present paper's sample – around 60 percent of the control group is working post-lottery, and the effect of voucher utilization is to reduce work rates by around 4.6 percentage points.

B. Effects of household resources on children's outcomes

Previous research yields ambiguous predictions about whether we should expect the large resource changes described above from voucher receipt to improve children's cognitive and non-cognitive outcomes. The standard human capital model suggests increased income will enable families to invest more in their children, which recent studies suggest could be particularly valuable when made early in the life course (Shonkoff and Phillips, 2000, Carniero and Heckman, 2003, Knudsen et al., 2006).

[22] Previous research from the housing experiments of the 1970s suggest the income elasticity of housing consumption for low-income families is equal to around .3 to .4, with price elasticities on the order of -.1 to -.2 (Greenberg and Shroder, 2007).

Yet society already tries to ensure a minimum level of investment in all children by providing some basic social program supports to poor families. In principle there could be diminishing marginal returns to extra investments in children, although in dynamic models that allow for "learning to beget learning" (see for example Carniero and Heckman, 2003) the potential effects of marginal dollars invested in children at different parts of the life course become harder to predict. In addition, on average families appear to devote much of their increased income on housing, eating out more often, and perhaps transportation, which are not very highly correlated with children's outcomes (Mayer, 1997). In contrast those parental "inputs" that are strongly correlated with child outcomes, like visiting a museum, do not cost very much money.[23]

It is well known from previous research that family income is positively correlated with many important child outcomes (for example Haveman and Wolfe, 1995, Duncan and Brooks-Gunn, 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. For instance 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-

[23] It is also 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. 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.

parent versus two-parent households does not appear to be much different in states with generous versus less-generous AFDC benefits.

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, 1999, Blau, 1999).

More recently Dahl and Lochner (2005) use variation in family income over time due to changes in EITC benefits and in returns to particular worker characteristics, and find relatively large effects of income on children's achievement test scores. Specifically they 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 labor market and in the EITC schedule. They find that each $1,000 increase in family income is associated with increased children's test scores of .021 standard deviations in math and .036 standard deviations in reading.

Identification in the Dahl-Lochner study appears to assume there were no other changes in the effects of these parental characteristics on children's test scores during the 1990s. However families who would have disproportionately benefited from increases in the EITC over the 1990s may also have benefited more from the tripling over this period in federal Head Start spending (Haskins, 2003) or from the fact that over this decade the violent crime rate declined by nearly 30% and the homicide rate declined by nearly 40% (US Statistical Abstracts, 2001), the fraction of American children covered by Medicaid increased by perhaps as much as two thirds (Mann et al., 2003), and the welfare caseload declined by around one half (Sawhill et al., 2002).

Oreopolous, Page and Stevens (2005) 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.

Aside from the NIT studies of the 1970s, which yield mixed findings (Mayer, 1997), the one study that has used experimental variation in family income to estimate effects on children's outcomes is by Morris, Duncan, and Rodrigues (2004). Their analysis finds randomized welfare-to-work experiments that offer income supplements together with work requirements yield bigger gains in children's achievement scores than do work-only programs. Specifically they use interactions of indicators for the different welfare-to-work experiments with indicators for treatment group assignment as instruments for family income, and find that an increase of $1,000 in family income increases achievement test scores for children 2-5 years old by .06 standard deviations. However family income changes have little effect on children 6-9 years old, and may, if anything, have *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 increased utilization of center-based care among families that experience higher income. Using data from the same set of welfare-to-work experiments examined by Morris and colleagues, Gennetian et al. (2006) 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. These results suggest that increased income within the context of increased maternal labor supply may have beneficial effects that are limited to preschool-age children.[24]

C. Voucher effects on children's outcomes

The best previous study of the effects of expanding the housing voucher program on children's outcomes is the evaluation by Mills et al. (2006) of HUD's Welfare to Work (WtW) voucher experiment, which by random lottery offered housing vouchers to a sample of welfare recipients. The evaluation found no statistically significant effects on children's behavior problems, delinquency, risky behavior, and 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.

[24] Whether increased income improves outcomes for preschool children in 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, 2004). Unfortunately our hypothesis about the interactive effects of increased family income and increased maternal labor supply cannot be directly tested by the data utilized by Morris et al., since all of the welfare-to-work experiments increase maternal labor supply.

One limitation of the WtW evaluation is that eligibility was limited to families for whom local agencies determined that "the housing assistance provided through the WtW voucher program was critical to the families' ability to obtain or retain employment" (Mills et al., 2006, p. 7). How these findings generalize to the population of low-income families eligible for housing programs is somewhat unclear. More importantly, children's outcomes are measured entirely by parent survey reports on their children, which at least in the MTO evaluation were found to be not nearly as informative as children's own self reports regarding their developmental outcomes. In addition, parent-on-youth reports are available from a relatively small sample (2,481), which limits the statistical power of the WtW study to detect program impacts on key children's outcomes. Our study improves upon both of these limitations by measuring cognitive and non-cognitive outcomes for nearly 50,000 school-aged children using official longitudinal administrative records that will not be susceptible to misreporting problems.

V. EMPIRICAL STRATEGY

The 1997 housing voucher lottery in Chicago provides a unique opportunity to examine the causal effect of a large change in family resources on children's outcomes using a large, representative sample of low-income families. Following the program evaluation literature, we begin by estimating the effect of being offered a housing voucher, referred to as the Intent-to-Treat (ITT) effect, which fully exploits the strength of our research design that is afforded by random assignment of low-income families to the CHAC voucher wait list. We then estimate the effect of utilizing a voucher (i.e., receiving the subsidy), known as the effect of Treatment-on-the-Treated (TOT).

A. Effect of a Voucher Offer

Given the evidence presented in Tables 1 and 2 that the voucher wait-list lottery was in fact random, a simple comparison of means between those individuals who were offered vouchers and those who were not will provide an unbiased estimate of the effect of the voucher offer. Our application is complicated slightly by the fact that people were offered housing vouchers at different points in calendar time, and by the fact that we measure outcomes over a period of several years.

To describe our empirical approach let y_{it} be an outcome for individual *i* in academic year *t*. Let $Offer_{it}$ be an indicator variable that takes on a value of one if individual *i* has been offered a housing voucher in any period *prior* to *t*, and zero otherwise, so that for CHAC applicants never offered vouchers at all, all observations of $Offer_{it}$ will take on a value of zero in every person-year in our panel. A simple OLS estimate of β_1 in equation (1) captures the average effect of offering an individual a housing voucher on outcomes in all post-offer periods, that is, the ITT effect. To improve the precision of our estimates we condition on a set of individual and family baseline characteristics $\mathbf{X}$ and control for period effects, λ_t. Our standard errors are clustered by household to account for within-household correlations as well as serial correlation (Bertrand et al., 2004).

(1) $$y_{it} = \alpha + \beta_1(Offer_{it}) + \mathbf{X}\Gamma + \lambda_t + \varepsilon_{it}$$

In principle there could be some "anticipation effects" from the expectation of getting a voucher sometime in the future, which could complicate our analysis. We address this problem in part by excluding from our analytic sample those families who expect to receive a voucher at some point but had not been offered one yet by the end of 2003 (specifically, those with lottery numbers 18,103 to 35,000). We also include in our empirical specification a separate indicator (Pre_Offer_{it}) equal to 1 for the person-quarters for those people who were on the active wait list but had not been offered vouchers yet by quarter *t*, equal to 0 else. Identification of β_1 in equation (1) thus comes from a within period comparison of the average outcomes of our "treatment group" (lottery numbers 1 to 18,103) who had been offered vouchers by that quarter with those families in our "control group" (lottery numbers 35,001 to 82,607) who were not assigned to the active wait list and so never expected to be offered vouchers by CHAC.[25]

One limitation with the setup in equation (1) is the assumption that the effects of a voucher offer are independent of how long ago the offer was made. But behavioral responses to vouchers may become more pronounced over time if, for example, developmental benefits to children accumulate with exposure to increased resources. To explore the possibility of time-varying impacts we also estimate equation (2):

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

where voucher effects are captured by the coefficients on a series of dummy variables representing the time since voucher offer. More specifically, let the binary variable D_{it}^k = 1 if, in period *t*, individual *i* had received a voucher offer *k* periods earlier. The coefficients δ_k therefore capture the effect of a voucher offer over time.

B. Estimating the Effect of Receiving a Housing Subsidy

Under the assumption that the voucher offer does not have an impact on those who choose not to take it,[26] one can use the exogenous variation in voucher offers to

[25] In practice including families who might experience "anticipation effects" in the data has almost no impact on our estimates, perhaps because of credit constraints or uncertainty by families waiting for vouchers about when or whether would actually ever get a voucher.
[26] While we do not think this assumption is strictly true, we believe that it is a reasonable approximation.

estimate the effect of utilizing a housing voucher, often referred to as the effect of treatment-on-the-treated (TOT). In practice, we implement this strategy in a two-stage least squares framework where the equations of interest are (3) and (4), where $Leased_{it}$ equals one if individual *i* had leased up with a voucher offered as part of the 1997 CHAC wait-list lottery any time up to academic year *t*,[27] and the instruments in the first stage equation (3) include the series of dummy variables D_{it}^{k} indicating the whether the individual had received a voucher offer *k* quarters earlier.

(3) $$Leased_{it} = \alpha + \sum_{k} D_{it}^{k}\theta_k + \mathbf{X}\Gamma + \lambda_t + \varepsilon_{it}$$

(4) $$y_{it} = \alpha + \pi_1(Leased_{it}) + \mathbf{X}\Gamma + \lambda_t + \varepsilon_{it}$$

Note that there were some other, smaller voucher allocations made during the course of our study period. Our main estimates do not count families who receive other vouchers as "treated." In this case the parameter π_1 essentially captures the effects of expanding Chicago's main housing voucher program on those who lease up with these vouchers, recognizing that some other smaller voucher programs will continue to operate in the background and provide services to some families as well. In any case we demonstrate below that our TOT estimates are similar when we define "treatment" more broadly as the use of any voucher since the number of other vouchers issued is small.[28]

As a benchmark for judging the size of our TOT estimates, we also present what Katz, Kling and Liebman (2001) call the control complier mean (CCM), which could be

[27] In addition to the set of Hispanic families provided vouchers under a consent decree between CHAC and Latinos United, a small number of vouchers were offered under other programs such as a Welfare-to-Work demonstration and a program designed to help unify families.

[28] Note that the monotonicity condition for our instrument would still hold even if we defined treatment more broadly as use of a voucher offered through any allocation (1997 wait-list lottery or any other voucher program), if we assume families would always use the first voucher offered to them (there is no reason to think otherwise since the vouchers follow the same underlying program rules for the most part). In this case being assigned a good wait-list lottery number would never push the time of first voucher offer back.

different from the overall control mean (CM) if the families who would lease-up with a voucher if given the chance are systematically different from other families. We would ideally like to know how large the TOT gain is for the treatment group compliers as a share of the average outcome they would have experienced had they not used a voucher. We cannot observe this directly, but we can estimate the mean outcome for the people in the control group who would have complied had they been assigned to the treatment group, even though we cannot directly observe who would have been a complier within the control group. This estimate for the CCM comes from subtracting the TOT estimate from the observed mean outcome for the treatment group compliers.

We can also estimate how the effects of voucher utilization change with time since the voucher offer. The estimate for being leased up with a voucher in the k[th] period since the time the voucher was offered is given by the parameter γ_k in equation (6).

(5) $$Leased_{it}^{k} = \alpha + \sum_{k} D_{it}^{k}\theta_k + \mathbf{X}\Gamma + \lambda_t + \varepsilon_{it}$$

(6) $$y_{it} = \alpha + \gamma_k(Leased_{it}^{k}) + \sum_{j \neq k} D_{it}^{j}\theta_k + \mathbf{X}\Gamma + \lambda_t + \varepsilon_{it}$$

Note that our estimate is different from the effect of being leased up for k periods. In the k[th] period following a CHAC wait-list voucher offer, treatment families will be leased up for different amounts of time – some families will have leased-up with their voucher right away, others will have taken longer to search and find a rental unit and so would have been leased up for a relatively shorter amount of time, while many treatment group families will not have leased up at all. Our estimate for γ_k is a weighted average of the causal responses to being leased up with a CHAC 1997 wait-list voucher among families who are induced by a good wait-list lottery number to lease up for different periods of time – the average causal response (ACR) of Angrist and Imbens (1995).[29]

The ACR may change with time since the voucher offer because the causal effects from leasing up with a voucher change with time since lease up, or because more families lease up with vouchers with additional time since the vouchers are offered and the average effect on the new leasers differ from those who have already leased up. We cannot identify how the effects of voucher use change with time since lease-up without imposing additional assumptions on the data.[30] From this perspective, the TOT estimate derived from equation (4) represents in turn a weighted average of the ACRs for all of the periods since voucher offer that are represented in our dataset.

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[29] Let S_1 represent the number of periods a family will be leased up with a voucher in period k after the voucher offer if they get a good draw in the wait-list lottery and are offered a voucher by CHAC, and let S_0 be the number of periods they would be leased up if they were randomized into the control group. (S_0=0 for all families since the treatment is use of a voucher from the CHAC lottery.) Let Y_j represent the outcome for a child if their family had been leased up for (j) periods in the k^{th} period since the CHAC voucher offer, with $j \leq k$. For each child we imagine a full set of potential outcomes for different possible voucher lease-up durations Y_j; the causal effect of an increase of one period in time leased up is $Y_j – Y_{j-1}$. The estimated coefficient from applying two stage least squares to (5) and (6) is a weighted average of the effects of various one-unit increments in the amount of time spent leased up in the period (k) after voucher offer. As Angrist and Imbens (1995, p. 435) note, "the weight attached to the average of $Y_j – Y_{j-1}$ is proportional to the number of people who, because of the instrument [in our case, CHAC wait-list lottery position] change their treatment from less than *j* units to *j* or more units. This proportion is $\Pr(S_1 \geq j > S_0)$." For people for whom assignment of a good CHAC wait-list lottery number causes them to be leased up for more than 1 period in the kth period after voucher offer, we are estimating an average of the effects on them from a series of one-period increases in voucher lease-up duration.

[30] In each period (k) since voucher offer, we can directly estimate the ITT impact as well as the shares of the treatment and control groups that have been leased up for 1 period, 2 periods, up to (k) periods. But the data are not directly informative about how to apportion the observed ITT impact across those families who have been leased up for different periods of time. Mills et al. (2006) try to identify the effect of being leased up for a given amount of time by starting with the ITT impact the first period after voucher offer, estimating the effect for being leased up for 1 period, then moving on to the second period after voucher offer and backing out the effect of being leased up for two periods by using the effect of being leased up for 1 period derived from the previous period together with information on how many more families are leased up for 1 period in this second period since voucher offer. This strategy is then used to recover the effects for being leased up for (k) periods. But the approach adopted by Mills and colleagues assumes that the effects of leasing up with a voucher is not affected by when in calendar time a family leases up, which need not be the case if (as seems likely) there are changes over time in housing market conditions, and also assumes that families who lease up with some lag after voucher offer experience the same impacts as those who lease up right away, which also need not be the case. We thank Jeffrey Kling for this observation.

VI. RESULTS

The CHAC voucher lottery has very large "first stage" effects on the probability of leasing up with a voucher. The voucher offer, and voucher utilization itself, have no detectable impacts on cognitive outcomes for children but do have important impacts on non-cognitive outcomes for older children. Interestingly, these impacts are concentrated among males in our sample, consistent with the findings from the New Hope experiment that, like our study, also involved large resource transfers to low-income families.

A. First Stage Effects of CHAC Voucher Offers on Voucher Use

Table 2 shows the magnitude of the "first stage" relationship between the outcomes of the 1997 CHAC voucher wait-list lottery and use of a voucher offered by this lottery. The table presents the results of estimating equation (1) above using the $Leased_{it}$ indicator for whether the family had leased up at any point up through that academic year as the dependent variable of interest.

The control means are very close to zero in Table 2, as we would expect given that "treatment" is defined as use of a voucher that was offered through the 1997 wait-list lottery. Having been offered a voucher by CHAC as a result of being assigned a good lottery number increases the chances of having leased-up with a voucher by nearly 43 percentage points for children who were 4-18 years old at the time of the voucher lottery. This impact is less than 100 percentage points because not all families are willing or able to find and successfully lease-up an apartment with rent below the FMR and meets program quality standards within the housing-voucher program's limit on search time. This figure is slightly higher among children 4-11 at baseline, who serve as the main analytic sample for our analysis of ITBS test score impacts below, and slightly lower among youth 8-18, suggesting that lease-up rates are somewhat lower for families with relatively older children.

The results shown in Table 2 suggest that the TOT estimates will typically be about (1 / .493) ≈ 2 times as large as the ITT estimates for outcomes like test scores where we focus on children 4-11 at baseline who are exposed to CPS testing requirements, while the TOT will be around (1 / .395) ≈ 2.5 times the ITT estimates for outcomes like arrests or dropout that are only developmentally appropriate for slightly older youth, which we define here as those 8-18 at baseline.

B. Effects on Cognitive Outcomes

While voucher receipt generates a very large change in the resources available to families already living in private-market housing, this large change in resources does not generate any detectable impacts on children's achievement test scores, as shown in Table 3. Because the CPS only administers standardized reading and math achievement tests to children in grades 3 through 8 (the Iowa Test of Basic Skills, or ITBS), our analytic sample for analyses of test score outcomes initially focuses on those children who were between the ages of 4 and 11 at the time of the CHAC lottery.

Table 3 shows that we see no statistically significant impact on either reading or math ITBS scores for children ages 4-11 at baseline. Statistically insignificant estimates are only interesting to the extent to which they are able to rule out economically meaningful estimates, which raises the question: How precisely estimated are these null impacts? The ITBS results are presented in national percentile terms, and so are uniformly distributed over the interval from 1 to 100 and will have a standard deviation of 28 percentile points. The TOT effect for ITBS reading scores equal around -.2

percentile points, with a standard error of about 0.6 percentile points. As a result the 95% confidence interval here enables us to rule out an impact on reading scores any larger than around 4% of a standard deviation. For ITBS math scores we can rule out an impact that is larger than about 7% of a standard deviation.

One way to think about the size of these estimates is by comparison to the widely cited recent results by Dahl and Lochner (2005), who suggest each extra $1,000 of cash income for minority families increases children's test scores by .03sd in math and .05sd in reading. If our estimate is correct that the cash equivalent of a housing voucher is about $5,300, and if there is nothing particular about the other in-kind features of housing vouchers that would have *negative* effects on children (and so partially offset any gains from increased resources), then the Dahl and Lochner estimates would predict that housing vouchers should increase reading and math scores among our low-income minority Chicago sample of children by about .265 standard deviations in reading and .159 standard deviations in math. So our TOT estimates let us rule out reading impacts that are any larger than about one-seventh of Dahl and Lochner's estimates, while our results let us rule out any math impact any larger than about two-fifths of their estimate.[31]

Our analytic sample so far has been restricted to children who are already school age (4 to 11) at baseline, and so if children are more developmentally malleable during the very first few years of life then in principle test score impacts could be more pronounced among the youngest children at baseline in our sample. However in other results (not shown here), we have replicated our analyses with all children who are ages 0

[31] Morris et al. (2004) Table 6, estimate that each extra $1,000 in family income boosts test scores by .061 standard deviations for children ages 2-5 at the time of random assignment. That effect would imply that housing vouchers (which we estimate to have a cash equivalent value of $7,000) should increase test scores by about .43 standard deviations.

to 6 at the time of the voucher lottery and find results that are qualitatively similar to those shown in Table 3 but less precisely estimated.

A second potential objection to our test score findings is that the impacts might vary by gender, consistent with previous research discussed above suggesting that null impacts of social interventions may mask considerable heterogeneity in how boys and girls respond to the policy treatment. But Table 3 shows that we do not see statistically significant impacts for either boys or girls when we analyze their data separately.

A third potential critique of these results is that the impacts of increased family income on children may accumulate over time with increased exposure to more developmentally productive environments. However when we calculate either ITT or TOT impacts by time since voucher offer, we see no evidence of any clear trends in voucher impacts on children's achievement test scores.

A fourth potential objection comes from the possibility that the voucher treatment could affect the probability that children are actually administered ITBS achievement tests. For example if families who receive vouchers now send their children to schools that are more likely to test children with learning disabilities, this would increase the representation of children in the left tail of the test score distribution among the treatment group and mask any beneficial voucher impact on mean ITBS results. However Table 3 shows fairly precisely estimated zero impacts on the probability that children are tested.

C. Impacts on Non-Cognitive Outcomes

In contrast to the insignificant estimates for voucher effects on reading and math scores, we find some evidence for impacts on non-cognitive outcomes for older children (8 to 18 at baseline). Interestingly, these impacts on non-cognitive outcomes are concentrated among males, as in the New Hope program which, like ours, also involved providing very low-income families with substantial additional resources.

Table 4 presents results for school persistence and related outcomes using data from the Chicago Public Schools. The results for all youth 8-18 at baseline, pooling boys and girls together, shows no strong evidence for a voucher impact. For example the TOT estimate for high school graduation rates (.024) is almost exactly the same magnitude but the opposite sign of the TOT estimate for voucher effects on the probability of leaving the CPS for another school district (-.023).

On the other hand, the estimated effect of voucher utilization on high school graduation probabilities for males is .043, which is about one-fifth of the CCM of 18 percent. Even if we very conservatively assume that every male youth who left the CPS as a result of voucher receipt would have graduated, the graduation impact would be on the order of (.043 - .014) = .029.

The findings for school persistence are also quite consistent with what we see for criminal behavior, as seen in Table 5. Using data from the Illinois State Police arrest records we estimate ITT and TOT effects of housing vouchers on arrests for all crimes, and for separate crime types, for children who are ages 8-18 at the time of the CHAC lottery. Voucher utilization reduces arrests for all crimes (excluding motor vehicle violations) by around .034 arrests per child per academic year, compared to a control complier mean of about .19.

As with school persistence, we see voucher impacts on arrests only among male youth, for whom voucher receipt reduces all arrests by .066 arrests per academic year, about one-fifth of the CCM of .336. This impact is driven in large part by fewer arrests for violent offenses (TOT of .015, compared to a CCM of .066) and drug offenses (TOT of .028, compared to a CCM of .132).

Table 6 provides additional details about the specific crimes averted, which is important for policy given the substantial variation across crime types in social costs (Cohen et al., 2004, Cohen, 2005). The voucher impact on drug arrests is driven mostly by reductions in drug possession. On the other hand the voucher effect on violent crimes occurs mostly among the most serious types of violent crimes – murder, rape, and robbery. Because there are just not many of these offenses in our sample, despite the relatively large sample size, it is reasonable to wonder whether our voucher estimates here are simply an artifact of applying OLS to a dependent variable dominated by zero values. But when we replicate our ITT results using a negative binomial estimator we obtain findings that are similar in magnitude to OLS as a proportion of the control mean.

The implication of having voucher effects on the most serious – and socially costly – types of violent crimes can be seen in Table 7, which shows ITT and TOT impacts of housing vouchers on the social costs of crime using the cost estimates for each different type of crime from Miller, Cohen, and Wiersema (1996).[32] For males voucher receipt reduces the social costs of crime by around \$6,200 per year per male youth, a huge share of the CCM of around \$10,700 per male annually. The TOT estimate for all youth (boys and girls together) is still quite large (nearly \$3,300, compared to a CCM of \$5,600). Because these types of cost-of-crime estimates are typically quite sensitive to how one assigns a value to the social costs of homicide, we show what happens when we

[32] Motor vehicle thefts and larcenies are recorded separately, but we combine these two offenses into the same category and use the more conservative social cost estimate for larcenies since larcenies by far outnumber motor vehicle thefts in the Chicago crime data. Furthermore, Illinois does not distinguish between MV thefts and other thefts, so all car thefts in Illinois will be counted as larcenies in our data.

trim the costs of murder to equal just twice the Miller et al. estimate for the social costs of rape. In this case the TOT is a smaller share of the CCM ($740 compared to a control complier mean of around $2700), but still highly statistically significant and still economically important. Because different people disagree about the degree to which drug offenses impose costs on society, in Table 7 we replicate our estimates again setting the social costs of drug offenses to $0, and then again both trimming the estimated costs of murder and setting drug offenses to $0 simultaneously. The TOT is always statistically significant and equals from one-third to two-thirds of the CCM.

The pattern of results so far suggests that the large resource transfers generated by housing vouchers have larger impacts on non-cognitive outcomes (school persistence, arrests) than on cognitive outcomes (reading and math scores on the ITBS). However so far we have been using slightly different analytic samples to estimate impacts on the two types of outcome domains: children ages 4-11 at baseline for test scores, and those ages 8-18 at baseline for non-cognitive outcomes. We could be confounding differences in treatment effects by age with different treatment effects on different outcome domains. Yet when we re-run our estimates for the much smaller subset of children for whom we have data on both cognitive and non-cognitive outcomes (those 8-11 at baseline), we see a qualitatively similar pattern to what is reported above – no detectable impacts on achievement test scores, but statistically significant impacts on arrests.

D. Extensions

One potential concern with our estimates is that we are relying on administrative data from the city of Chicago to measure schooling outcomes, and administrative data from the Illinois State Police to measure arrest outcomes. If voucher receipt affects the probability that families move out of the city of Chicago, or out of the state of Illinois, our estimates could be affected by selective attrition. However Jacob and Ludwig (2007) present analyses showing that there is no effect of housing voucher receipt on the probability of living outside of Illinois.

More generally Jacob and Ludwig (2007) find that there is almost no impact of voucher receipt on residential mobility (cumulative numbers of different addresses according to our administrative address tracking data) or neighborhood characteristics such as census tract poverty rate, fraction tract population that is black, or the chance that the tract poverty rate is below 20 percent. We also find that there is no trend in voucher impacts on tract poverty rates; the 95 percent confidence interval rules out any impact that is larger than about 1 percentage point, so these are quite precise estimates. Finally, voucher receipt also does not seem to generate much change in school characteristics.

VII. DISCUSSION

Our findings suggest that increased family income among low-income families generates detectable impacts on non-cognitive outcomes but not cognitive test scores in reading or math. Our null findings for achievement test scores hold even when we restrict our sample to children who were very young (0 to 6) at the time of random assignment.

These results presented in this paper are not necessarily inconsistent with those presented by Morris, Duncan and Rodrigues (2004), who find very large effects on test scores for children 2-5, because the two studies evaluate different interventions. We find no effect on test scores from increased income holding maternal labor supply constant (or even with very slight reductions in maternal work). Morris et al. instead find that increased income paired with more income improves test scores for preschool children, apparently because parents devote the extra cash to center-based care. The two studies taken together suggest that if the goal is to improve children's test scores through cash transfer programs, the design of the program's work incentives may be quite important in determining whether the desired impact on test scores is achieved. These findings also raise the question of whether programs focused more narrowly on providing early childhood educational services might be a more efficient way to increase test scores of young children compared to cash transfer programs.

On the other hand we do find that housing vouchers improve the non-cognitive outcomes of older children (8-18 at baseline). In general cash transfer programs induce some deadweight loss by reducing work effort and generating administrative costs, and housing voucher programs add to these costs by constraining families to consume more of the subsidy value in the form of extra housing compared to what families would otherwise choose with an equivalently valued cash transfer. But our estimates suggest that the behavioral impacts of housing vouchers on children's non-cognitive outcomes that generate very large benefits relative to the size of these deadweight losses that need to be considered in benefit-cost analyses of this and perhaps related social programs.[33]

Interestingly, we find that the effects of increased family income on non-cognitive outcomes are concentrated among males in our sample. This finding seems to be

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[33] Table 2 implies that each voucher applicant family contains around 3.2 children on average, of whom presumably half are males. Recall that our program population is almost entirely African-American. We find that voucher utilization increases graduation probabilities for black males by around 4 percentage points (TOT estimate, Table 4). Henry Levin, Cecilia Rouse and Clive Belfield estimate that the social benefits from preventing dropout for black males equals \$186,500 per dropout averted. In the steady state if there are 1.6 black male children per voucher family then the expected value per voucher household of reduced dropout rates equals (1.6 ×.04 × \$186,500) = \$11,936. Our estimates also suggest social costs savings per male youth that range from \$800 to \$6200 per year, which would be even larger if we account for the fact that not all violent crimes result in arrest. By comparison the government cost of a housing voucher is \$7600, and we estimate the cash equivalent value to families is around \$5300.

consistent with evidence from the New Hope experiment, which increased family resources within the context of a welfare-to-work demonstration and found behavioral impacts concentrated among males (see for example Duncan, Huston and Weisner, 2007). But other social program evaluations for early childhood programs and residential mobility have found bigger benefits for girls than boys. Why boys respond more positively to some interventions – specifically, those that provide additional household resources – and girls respond more to other types of programs (which provide educational or mobility services) remains unclear.

This last point raises the larger question of why the increased resources associated with receiving a housing voucher improved youth non-cognitive outcomes. We find no evidence that housing vouchers lead to improved residential stability, neighborhood environments or school quality – the types of "inputs" that we might most expect to matter if vouchers change behavior through a housing-specific mechanism. It could be the case that housing vouchers provide poor families with sufficient extra resources to enable male youth to stay in school rather than try to supplement family income by joining the labor market or turning to crime. We will try to address this point in the next version of the paper by examining voucher impacts on youth employment using data from the Illinois unemployment insurance (UI) system, but for now the effects of housing vouchers on youth non-cognitive outcomes seem encouraging but unexplained.

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TABLE 1 – SUMMARY STATISTICS FOR YOUTH SAMPLE 8-18 & THEIR HHHs

	Families in Private Housing at Baseline				Families in Public Housing	
	Control Group	Treatment Group	Treatment		Control Group	Treatment Group
			CP	NCP		
Youth Characteristics (1996-7 AY)						
Male	0.487	0.494	0.471	0.511	0.497	0.462
Age	12.5	12.5	11.7	13.0	12.4	12.5
Hispanic	0.039	0.037	0.025	0.046	0.011	0.008
Special education	0.131	0.132	0.120	0.141	0.138	0.124
Tested	0.741	0.737	0.817	0.677	0.773	0.754
Score excluded	0.097	0.092	0.082	0.102	0.104	0.098
Iowa test score	31.97	32.77	32.62	32.91	28.33	28.40
Grade	5.9	5.9	5.3	6.4	5.7	5.9
Old for grade	0.260	0.249	0.231	0.262	0.278	0.265
Foster Care	0.024	0.026	0.018	0.033	0.029	0.025
Number of pre-lottery youth arrests						
Violent crime	0.014	0.012	0.005	0.018	0.015	0.015
Property crime	0.007	0.006	0.005	0.007	0.007	0.010
Drug crime	0.019	0.022	0.004	0.035	0.019	0.021
Other crime	0.010	0.010	0.002	0.015	0.011	0.017
HHH Demographics						
Male	0.039	0.045	0.015	0.066	0.030	0.019
Age	34.3	34.2	34.2	34.2	34.4	34.3
Number in HH	4.6	4.5	4.4	4.6	5.1	4.9
Number Adults in HH	1.5	1.5	1.3	1.7	1.5	1.5
Black	0.927	0.931	0.945	0.921	0.962	0.962
Hispanic	0.040	0.037	0.026	0.045	0.013	0.007
White	0.016	0.017	0.015	0.019	0.008	0.016
Other Race	0.007	0.005	0.005	0.005	0.005	0.003
Spouse	0.088	0.090	0.064	0.108	0.060	0.066
Disabled	0.230	0.225	0.253	0.206	0.221	0.197
Any wages	0.414	0.429	0.386	0.459	0.296	0.297
Receive SSI	0.207	0.213	0.240	0.195	0.208	0.205
Receive AFDC	0.623	0.595	0.644	0.561	0.748	0.759
# of days following lottery announcement that the applicant submitted her application	9.0	9.0	8.7	9.2	8.4	8.2
HHH Pre-Lottery						

Labor Market Attachment						
Received TANF, FS, or Med in 1997q2	0.781	0.765	0.829	0.721	0.901	0.901
Received TANF in 1997q2	0.594	0.576	0.648	0.526	0.739	0.728
Employed in 1997q2	0.235	0.223	0.233	0.217	0.218	0.239
Earnings in 1997q2	794	764	803	737	753	763
HHH number of pre-lottery arrests						
Violent crime	0.104	0.101	0.108	0.096	0.137	0.151
Property crime	0.130	0.113	0.126	0.103	0.100	0.129
Drug crime	0.090	0.082	0.071	0.090	0.086	0.055
Other crime	0.118	0.118	0.107	0.125	0.121	0.129
F-stat (p-value) on joint test of all characteristics	1.01 (0.46)				1.17 (0.22)	

Notes: Sample is all MTO youth who were 8-18 at time of random assignment and showed up in CPS dataset prior to lottery. The sample is limited to households with lottery numbers below 18,103 and above 35,000. The treatment group includes families with a lottery number from 1 to 18,103; that is, the set of families who were offered a voucher by May 1, 2003. Since each youth is associated with a particular HHH, the HHH statistics shown above are implicitly weighted by the number of youth in the household. The control group includes families with lottery numbers from 35,000 to 82,602; that is, the families who were initially told that they would not be offered a voucher at any point. CP refers to compliers, which are families in the treatment group who utilized a housing voucher. NCP refer to non-compliers – families in the treatment group who did not utilize a housing voucher.

TABLE 2 – FIRST STAGE EFFECTS OF CHAC HOUSING VOUCHER OFFER ON VOUCHER LEASE-UP RATES,
YOUTH IN PRIVATE-MARKET HOUSING

	Dependent variable = ever utilized a housing voucher		
	All Youth 4-18	Youth 4-11	Youth 8-18
HH received a voucher offer in a prior year	0.427** (0.007)	0.493** (0.008)	0.395** (0.008)
Control mean	0.006	0.006	0.005
Number of observations	301,607	166,831	216,784

Notes: The unit of observation is person-academic-year. Robust standard errors clustered at household level. ** = significant at 5% level, * = significant at 10% level.

TABLE 3 – EFFECTS OF HOUSING VOUCHERS ON SCHOOLING OUTCOMES,
YOUTH AGE 4-11 AT BASELINE, PRIVATE-MARKET HOUSING SAMPLE

	CM	ITT	TOT	CCM
All Youth				
Tested	0.803	0.001 (0.003)	0.003 (0.006)	0.742
Reading Score	35.36	-0.101 (0.352)	-0.191 (0.630)	34.77
Math Score	36.64	0.320 (0.415)	0.513 (0.743)	36.25
Males				
Tested	0.805	-0.002 (0.005)	-0.002 (0.009)	0.751
Reading Score	32.68	-0.356 (0.474)	-0.642 (0.848)	33.26
Math Score	34.91	0.149 (0.573)	0.276 (1.023)	34.92
Females				
Tested	0.801	0.005 (0.004)	0.008 (0.008)	0.733
Reading Score	38.06	0.119 (0.495)	0.200 (0.890)	36.39
Math Score	38.38	0.454 (0.562)	0.695 (1.007)	37.67

Notes: The unit of observation is person-academic year. CM = Control Mean. ITT = Intent-to-Treat. TOT = Treatment-on-the-Treated. CCM = Control Complier Mean. See text for discussion of these estimates. Robust standard errors clustered at household level. ** = significant at 5% level, * = significant at 10% level.

TABLE 4 – EFFECTS OF HOUSING VOUCHERS ON SCHOOLING OUTCOMES, YOUTH AGE 8-18 AT BASELINE, PRIVATE-MARKET HOUSING SAMPLE

	CM	ITT	TOT	CCM
All Youth				
Graduated High School	0.267	0.010* (0.006)	0.024 (0.015)	0.263
Dropped out of the CPS	0.374	-0.005 (0.007)	-0.012 (0.017)	0.357
Left the CPS to attend private school	0.036	-0.001 (0.003)	-0.001 (0.007)	0.037
Left CPS to move out of the district	0.158	-0.010* (0.006)	-0.023 (0.014)	0.134
Males				
Graduated High School	0.203	0.018** (0.008)	0.043** (0.020)	0.180
Dropped out of the CPS	0.435	-0.009 (0.010)	-0.020 (0.024)	0.423
Left the CPS to attend private school	0.036	-0.001 (0.004)	-0.000 (0.010)	0.041
Left CPS to move out of the district	0.169	-0.006 (0.008)	-0.014 (0.020)	0.137
Females				
Females				
Graduated High School	0.330	0.001 (0.009)	0.002 (0.021)	0.346
Dropped out of the CPS	0.315	0.001 (0.009)	0.001 (0.021)	0.290
Left the CPS to attend private school	0.037	-0.002 (0.004)	-0.004 (0.009)	0.035
Left CPS to move out of the district	0.149	-0.014* (0.007)	-0.031* (0.017)	0.131

Notes: The unit of observation is person-academic year. CM = Control Mean. ITT = Intent-to-Treat. TOT = Treatment-on-the-Treated. CCM = Control Complier Mean. See text for discussion of these estimates. Robust standard errors clustered at household level. ** = significant at 5% level, * = significant at 10% level.

TABLE 5 – EFFECTS OF HOUSING VOUCHERS ON THE NUMBER OF ARRESTS PER YEAR, YOUTH AGE 8-18 AT BASELINE, PRIVATE-MARKET HOUSING SAMPLE

	CM	ITT	TOT	CCM
All Youth				
# of arrests for any crime	0.137	-0.014** (0.006)	-0.034** (0.016)	0.189
# of violent crime arrests	0.028	-0.003* (0.002)	-0.007* (0.004)	0.041
# of property crime arrests	0.012	-0.001 (0.001)	-0.003 (0.003)	0.017
# of drug crime arrests	0.046	-0.005* (0.003)	-0.011 (0.007)	0.061
# of other crime arrests	0.051	-0.005 (0.003)	-0.012 (0.008)	0.070
Males				
# of arrests for any crime	0.242	-0.026** (0.012)	-0.066** (0.031)	0.336
# of violent crime arrests	0.044	-0.006** (0.003)	-0.015** (0.007)	0.066
# of property crime arrests	0.017	-0.002 (0.002)	-0.005 (0.004)	0.024
# of drug crime arrests	0.087	-0.008 (0.006)	-0.019 (0.014)	0.114
# of other crime arrests	0.094	-0.011* (0.006)	-0.028* (0.015)	0.132
Females				
# of arrests for any crime	0.035	0.001 (0.003)	0.001 (0.008)	0.047
# of violent crime arrests	0.013	0.000 (0.002)	0.000 (0.004)	0.018
# of property crime arrests	0.008	-0.001 (0.001)	-0.002 (0.002)	0.011
# of drug crime arrests	0.005	-0.001 (0.001)	-0.002 (0.002)	0.008
# of other crime arrests	0.009	0.002 (0.001)	0.005 (0.004)	0.010

Notes: The unit of observation is person-academic year. CM = Control Mean. ITT = Intent-to-Treat. TOT = Treatment-on-the-Treated. CCM = Control Complier Mean. See text for discussion of these estimates. Robust standard errors clustered at household level. ** = significant at 5% level, * = significant at 10% level.

TABLE 6 – EFFECTS OF HOUSING VOUCHERS ON THE NUMBER OF ARRESTS PER YEAR, YOUTH AGE 8-18 AT BASELINE, PRIVATE-MARKET HOUSING SAMPLE, BY CRIME SUBCATEGORIES

	All Youth 8-18		*Males*		*Females*	
	TOT	CCM	TOT	CCM	TOT	CCM
Violent Crimes						
Assault	-0.003 (0.004)	0.033	-0.006 (0.006)	0.048	-0.000 (0.004)	0.017
Aggravated Assault	0.001 (0.002)	0.013	0.001 (0.003)	0.019	0.001 (0.002)	0.008
Murder	-0.001** (0.0003)	0.001	-0.001** (0.001)	0.002	-0.000 (0.000)	0.000
Rape	-0.001** (0.0003)	0.002	-0.002** (0.001)	0.003	0.000 (0.000)	0.000
Robbery	-0.002* (0.001)	0.006	-0.004** (0.002)	0.011	0.000 (0.000)	0.000
Drug Crimes						
Possession	-0.013** (0.006)	0.052	-0.023** (0.012)	0.099	-0.002 (0.002)	0.007
Dealing	0.002 (0.002)	0.008	0.005 (0.004)	0.015	-0.001 (0.001)	0.002
Property Crimes						
Larceny	-0.002 (0.002)	0.014	-0.002 (0.004)	0.017	-0.002 (0.002)	0.011
Burglary	-0.001 (0.001)	0.004	-0.002 (0.002)	0.007	0.000 (0.000)	0.000
Other Crimes						
Trespassing	-0.002 (0.003)	0.021	-0.004 (0.006)	0.038	0.001 (0.001)	0.005
Weapons Charges	-0.005 (0.003)	0.021	-0.010 (0.006)	0.041	0.001 (0.001)	0.002
Disobey	-0.003 (0.002)	0.012	-0.006 (0.004)	0.022	0.001 (0.001)	0.001
Resisting Arrest	0.000 (0.000)	0.000	0.000 (0.000)	0.000	-0.000 (0.000)	0.000
Disorderly Conduct	-0.002 (0.001)	0.008	-0.004 (0.003)	0.013	0.000 (0.001)	0.002
Prostitution	0.000 (0.001)	0.000	-0.001 (0.000)	0.001	0.001 (0.001)	-0.001
Parole Violation	-0.000 (0.000)	0.000	-0.000 (0.000)	0.000	0.000 (0.000)	0.000

Notes: The unit of observation is person-academic year. CM = Control Mean. ITT = Intent-to-Treat. TOT = Treatment-on-the-Treated. CCM = Control Complier Mean. See text for discussion of these estimates. Robust standard errors clustered at household level. ** = significant at 5% level, * = significant at 10% level.

TABLE 7 – EFFECTS OF HOUSING VOUCHERS ON SOCIAL COSTS OF CRIME, YOUTH 8-18, PRIVATE-MARKET HOUSING SAMPLE

	CM	ITT	TOT	CCM
All Youth				
Estimates based on methodology in Miller, Cohen and Weirsema (1996)	2951	-1253** (482)	-3282** (1213)	5605
Using Miller, Cohen and Weirsema (1996), but trimming cost of murder to twice that for rape	1008	-159** (55)	-394** (137)	1490
Using Miller, Cohen and Weirsema (1996), but sets social costs of drug crimes to zero	2635	-1249** (480)	-3281** (1208)	5233
Using Miller, Cohen and Weirsema (1996), but trimming cost of murder to twice that for rape and setting social costs of drug crimes to zero	692	-155** (43)	-392** (108)	1118
Males				
Estimates based on methodology in Miller, Cohen and Weirsema (1996)	5503	-2325** (928)	-6201** (2411)	10679
Using Miller, Cohen and Weirsema (1996), but trimming cost of murder to twice that for rape	1810	-293** (105)	-738** (272)	2689
Using Miller, Cohen and Weirsema (1996), but sets social costs of drug crimes to zero	4899	-2333** (924)	-6241** (2400)	9995
Using Miller, Cohen and Weirsema (1996), but trimming cost of murder to twice that for rape and setting social costs of drug crimes to zero	1205	-301** (82)	-777** (212)	2004
Females				
Estimates based on methodology in Miller, Cohen and Weirsema (1996)	456	-157 (248)	-481 (617)	774
Using Miller, Cohen and Weirsema (1996), but trimming cost of murder to twice that for rape	225	-12 (27)	-36 (66)	329
Using Miller, Cohen and Weirsema (1996), but sets social costs of drug crimes to zero	422	-148 (248)	-457 (616)	713
Using Miller, Cohen and Weirsema (1996), but trimming cost of murder to twice that for rape and setting social costs of drug crimes to zero	190	-2 (25)	-12 (61)	268

Notes: The unit of observation is person-academic year. The sample is limited to households with lottery numbers above 35,000 or below 18,103. CM = Control Mean. ITT = Intent-to-Treat. TOT = Treatment-on-the-Treated. CCM = Control Complier Mean. See text for discussion of these estimates. Robust standard errors clustered at household level. ** = significant at 5% level, * = significant at 10% level.