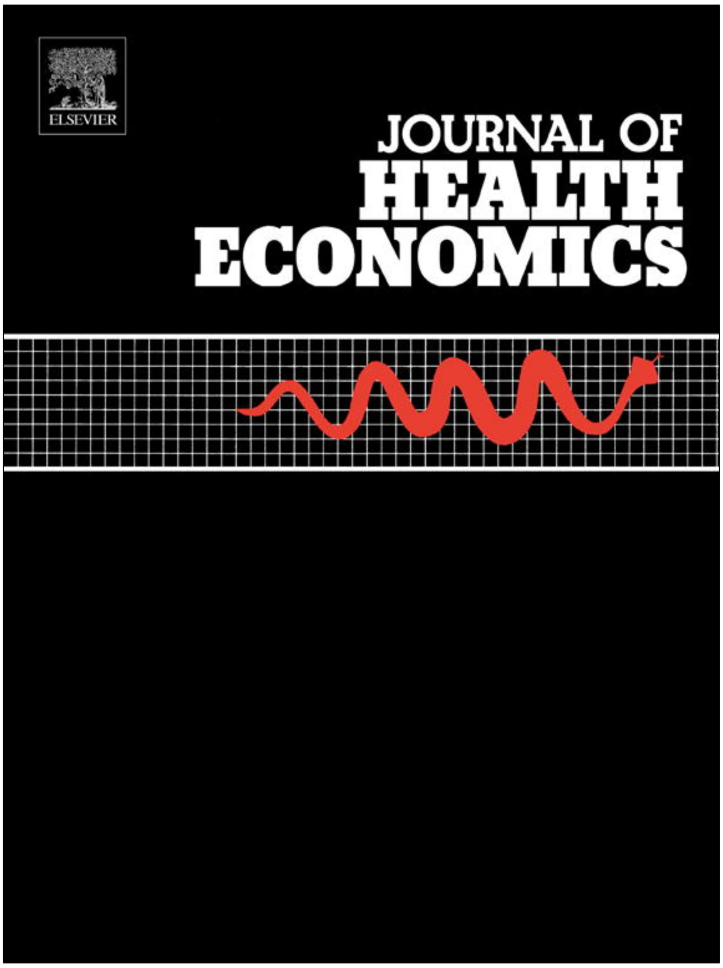
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The effects of housing and neighborhood conditions on child mortality

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ABSTRACT

In this paper we estimate the causal effects on child mortality from moving into less distressed neighborhood environments. We match mortality data covering the period from 1997 to 2009 with information on every child in public housing that applied for a housing voucher in Chicago in 1997 ($N = 11{,}680$). Families were randomly assigned to the voucher wait list, and only some families were offered vouchers. The odds ratio for the effects of being offered a housing voucher on overall mortality rates is equal to 1.13 for all children (95% CI 0.73–1.70), 1.34 for boys (95% CI 0.85–2.05) and 0.21 for girls (95% CI 0.01–1.04).

1. Introduction

In this paper we seek to estimate the causal effects on mortality among disadvantaged children from moving into less dangerous, economically distressed housing and neighborhood environment. Our study takes advantage of a natural experiment created by the random assignment of housing vouchers to public housing families in the 3rd largest city in the U.S. (Chicago). Our study sample consists of every public housing family that applied for a voucher in Chicago in 1997, when the city opened its housing-voucher wait-list for the first time in a dozen years. Ours is thus one of the largest randomized experiments involving voucher-induced changes in social environments (together with the U.S. Department of Housing and Urban Development's Moving to Opportunity randomized mobility experiment), and the first that we know of to examine one particularly important and well measured health outcome – mortality.

Health outcomes for children and adults vary dramatically across neighborhoods within the United States, even after statistically controlling for various individual- or family-level risk and protective factors. These patterns have generated concern among both policymakers and scientists that health outcomes may be causally affected by neighborhood attributes such as the physical environment (e.g., housing stock, environmental toxins, crime), local institutions (e.g., health care providers, grocery stores, parks), or aspects of the social environment that may shape people's information, preferences and norms about health-related behaviors (Kawachi and Berkman, 2003; Sampson, 2003). Yet variation across neighborhoods in health could instead reflect differences in neighborhood compositions. Observational studies may confound the causal effects of neighborhood and housing conditions with those of difficult-to-measure individual or family attributes associated with both health and residential sorting.

As Heymann and Fischer (2003) have argued in their review of this literature: "The best solution-oriented research to date has been conducted on moving people out of hard-hit neighborhoods" through government housing programs. For example, the one randomized mobility experiment that has been conducted to date, Moving to Opportunity, found that over the long term (10–15 years after random assignment) MTO-assisted moves to

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less distressed neighborhoods generated improvements in adult mental and physical health (Ludwig et al., 2011, 2012), and had mixed impacts on youth outcomes, with girls doing better and boys on balance doing worse as a result of the moves (Kessler et al., 2012; Sanbonmatsu et al., 2011). The results for children and youth in MTO are particularly surprising in light of the large social–epidemiological literature. What remains unclear is whether the gender difference in effects in MTO are idiosyncratic to that sample, and, perhaps even more importantly, whether neighborhoods really do not matter much for child health outcomes or if instead survey measures of health are too limited to capture any impacts.

Ours is the first study we know of to use a plausibly exogenous source of identifying variation to estimate the effects of changes in housing and neighborhood conditions on a particularly important, and well measured, child health outcome – mortality. We match Vital Statistics mortality data from 1997 to 2009 to information on every child age ≤ 18 in every public housing household that applied for a housing voucher in Chicago in 1997, when the city opened its housing voucher wait-list for the first time in a dozen years ($N = 11{,}680$). Our research design exploits the fact that families were randomly assigned to the voucher program's wait list, and only some families were offered vouchers. We estimate a discrete-time hazard model on overall mortality rates from randomization in 1997 through the end of calendar year 2009. Given previous findings from MTO for important differences in the effects of neighborhood mobility on youth outcomes, we examine mortality impacts for males and females separately, as well as for the pooled study sample.

The odds ratio for the effects of being offered a housing voucher on overall mortality rates is equal to 1.13 for all children (95% CI 0.73–1.70), 1.34 for boys (95% CI 0.85–2.05) and 0.21 for girls (95% CI 0.01–1.04). These findings suggest that social environments may play an important role in affecting the health outcomes of some of our nation's most disadvantaged children. The gender difference we find in the effects on health from changing social environments echoes those from MTO, although as with MTO, the reasons why responses are so different for males and females remain poorly understood. It is interesting that in our data the suggestive (but not statistically significant) indications of increased mortality to male youth from residential mobility are concentrated among homicides, while declines in mortality to female youth are concentrated among deaths due to disease and accidents.

In addition to our substantive findings, our paper addresses a methodological issue which we believe may increasingly arise in quasi-experimental research on low-probability outcomes such as mortality. Among girls receiving the housing voucher, there were very few deaths after voucher assignment (just one). This may result from having a large but not massive data sample, a strongly protective treatment, and a low probability outcome. For girls, the treatment is predicted to be nearly perfectly protective, and Logit and Probit standard errors do not lead to reliable confidence intervals. We solve this issue by using "profile likelihood ratio confidence intervals." These are constructed by finding the set of parameter values that would not be rejected by a LR test at a 5% significance level.

The next section of the paper discusses the potential mechanisms through which changes in housing and neighborhood conditions may affect child health, and provides a selective review of the previous empirical literature with an emphasis on studies that employ strong research designs that address the problem of endogenous sorting into neighborhoods. Section 3 discusses our data and empirical approach. Section 4 presents our findings and the final section provides some interpretation of these results.

2. Conceptual framework and previous literature

2.1. Mechanisms

Housing interventions that change people's housing and neighborhood environments could plausibly impact mortality through multiple channels, related to both the physical and social environments of the neighborhood.

Mobility could affect health for purely mechanical reasons, because housing and neighborhoods are bundled with environmental health risk exposures. The physical or institutional environment could also matter for health by affecting distance to, and hence the price of accessing, health-related inputs. For example, a great deal of public attention has been devoted to the possibility that disadvantaged urban neighborhoods may have limited access to health care services, particularly preventive care, and to grocery stores that sell fresh fruits and vegetables – or "food deserts." Public concern has also focused on the possibility that liquor stores, bars, and fast food outlets (or advertising for these products) are disproportionately located in high-poverty urban communities.

Mobility to less dangerous and distressed housing and neighborhood conditions could also affect health through social interactions, a possibility that has been of growing interest among economists (see for example Manski, 2000; Becker and Murphy, 2001). Local social environments could influence health-related behaviors through what Manski calls "preference interactions," if for example the preferences of one's peers influences one's own drinking, through "constraint interactions," as when elevated rates of criminal behavior by other neighborhood residents dilute the amount police resources available to stop and apprehend each offender, or "expectations interactions," if people's views about, say, the health consequences of some behavior are shaped by the distribution of that behavior and health outcomes in the area.

For health outcomes to young children, we expect exposure to risk and protective factors in the physical or institutional environment to be most relevant, as well as any "neighborhood effects" on the behavior of parents that wind up influencing the health inputs (and risks) that children experience. Data from the nationwide Vital Statistics system shows that the two leading cause of death to blacks ages 1–4 in 2009 were unintentional injuries (288 deaths, or 28 percent of all deaths to this group) and homicides (143, or 14 percent), which are disproportionately likely to occur at the hands of parents or caregivers. Congenital anomalies accounted for 87 deaths in 2009 to this age group (8 percent), while much less frequent were deaths from important diseases such as cancer (56, or 5 percent), heart disease (50, or 5 percent), chronic lower respiratory diseases (33, or 3 percent), or influenza and pneumonia (29, or 3 percent).[1]

For older children and adolescents, their own behavior may be increasingly important in determining their health outcomes. Particularly relevant may be behaviors that put young people at risk for unintentional injuries, which accounted for 913 deaths (23 percent) to blacks ages 10–20 in 2009, and particularly homicide, which accounted for 1496 deaths (37 percent) to this group and for teenagers and adults is strongly related to anti-social behavior and lifestyle decisions that may be influenced by neighborhood environments. For example, in Chicago in 2011 fully 87 percent of homicide offenders and, more surprisingly, 77 percent of victims had a prior arrest record (CPD, 2011). For homicides where the police can determine the motivation, fully 70 percent of homicides

[1] www.cdc.gov/injury/wisqars/index.html.

were attributed to an altercation; just 9 percent were attributed to gang or other organizational disputes over narcotics. Homicide risk is also highly skewed by gender: nearly nine of every ten homicide victims and offenders in Chicago are male.

Our study design and data provide us with limited power to disentangle the importance of these different behavioral mechanisms. Our reduced-form estimates instead capture the combined net influence of these different mechanisms that might be affected by housing and neighborhood conditions on the behavior of parents and children or youth. We try to gain some information about pathways by generating estimates separately by gender, and by cause of death.

2.2. Relevant studies

Previous epidemiological studies find strong correlations between neighborhood socio-economic composition or social processes and a range of health outcomes, even after regression-adjusting for people's own individual health risk- and protective-factors. For example, Waitzman and Smith (1998) find that people living in federally designated poverty areas have higher rates of mortality even after controlling for individual characteristics; Ross et al. (2001) find that living in a disadvantaged neighborhood is associated with lower levels of self-reported health and physical functioning; and Browning and Cagney (2003) find that individuals residing in neighborhoods with greater collective efficacy report better overall health. Diez Roux (2001) finds that adults living in disadvantaged neighborhoods are at significantly greater risk of developing coronary heart disease, even after controlling for income, occupation, and education. Pickett and Pearl., 2001, Kawachi and Berkman (2003), and Macintyre and Ellaway (2003) provide excellent reviews of this literature. More recently, Bird et al. (2010) have found that "good cholesterol" (high density lipoprotein or HDL), and lower systolic and diastolic blood pressure were associated with lower neighborhood socioeconomic status controlling for other factors. The pediatric epidemiology literature suggests that for children as well as adults, living in a high-poverty urban setting or unsafe neighborhoods is associated with adverse health outcomes (Curtis et al., 2004; Lumeng et al., 2006).

These previous observational studies are not strictly comparable to either MTO or the present study, which focus on families living in public housing. At first glance another seeming difference between observational studies and the present paper is that our study (like MTO) identifies neighborhood effects through families that move to lower-poverty areas, and the act of moving itself could affect people's outcomes or at least moderate the behavioral impacts of living in a less-distressed neighborhood. But mobility rates are high in general in the U.S. Around 14 percent of the population moved from 2002 to 2003; the annual mobility rate is even higher among lower-income Americans, and equaled fully 31 percent among renter households (Schachter, 2004).

One concern with these studies arises from the possibility of endogenous sorting of people into neighborhoods. Observational datasets cannot perfectly measure every determinant of health outcomes, or of residential choices. As a result, with epidemiological studies there is always some question about the possible confounding of the causal effects of neighborhood environments on health with the influences of unmeasured or hard-to-measure background factors that influence health directly and are also associated with neighborhood selection.

Votruba and Kling (2009) try to overcome this selection problem by examining the effects on health from the Gautreaux Assisted Housing Program, which starting in 1976 helped African-American public housing residents in Chicago to move to other parts of the city or to very affluent, mostly-white suburban areas. Accounts of how Gautreaux was implemented suggest that families had limited ability to choose where they relocated (Rubinowitz and Rosenbaum., 2000). They find that mortality rates among black males are relatively lower among those whose families relocated to neighborhoods where a relatively larger share of residents have a college degree. While these findings are suggestive, Gautreaux did not randomly assign participants to locations and there is some evidence of neighborhood self-selection.

The one true randomized experiment that has helped move poor families out of distressed public housing into less disadvantaged areas is the U.S. Department of Housing and Urban Development's (HUD) Moving to Opportunity (MTO) demonstration. Starting in 1994, MTO enrolled a total of 4600 low-income public housing families with children located in high-poverty census tracts in five cities – Baltimore, Boston, Chicago, Los Angeles, and New York.

Among adults, MTO-assisted moves to less distressed neighborhoods had no detectable impacts 10–15 years after randomization on economic outcomes but generated improvements in subjective well-being, adult mental and physical health (Ludwig et al., 2012). Particularly large were the MTO impacts on extreme obesity and diabetes, with a treatment-on-the-treated (TOT) effect on body mass index[2] $\geq$40 equal to a decline of 7 percentage points, or 41 percent of the control mean, and a TOT effect on diabetes (measured from blood samples, defined as glycosylated hemoglobin, or HbA1c, equal to $\geq$6.5 percent) of 7 points, or 45 percent (Ludwig et al., 2011, Supplemental Table 3).[3]

Among MTO youth who were ages 13–17 at the end of 2007 at the start of the long-term (10–15 year) follow-up data collection, MTO moves had mixed impacts on youth outcomes, with girls doing better with respect to mental health outcomes and some measures of risky behavior, and boys on balance doing worse as a result of the moves (Kessler et al., 2012; see also Sanbonmatsu et al., 2011). These findings are qualitatively similar to those found in a mostly non-overlapping sample of youth who were examined in the interim (4–7 year follow up) who were ages 15–20 at the end of 2001 (Kling et al., 2005, 2007). The interim and long-term MTO surveys also showed few detectable effects on survey-reported child health outcomes (Fortson and Sanbonmatsu, 2010; Sanbonmatsu et al., 2011).

One potential concern with the previous MTO research on child health stems from the measurement of child health outcomes, which (aside from measured height and weight in the long-term follow-up) were all from either parent reports of their children's health or from child and teen self-reports. For example, MTO moves into less economically distressed areas could improve access to medical care, which could in turn increase awareness of health problems relative to the control group that lives in more disadvantaged neighborhoods. It could also be that the standards that people use to decide what counts as good or bad health, or even what rises to the level of trouble with some specific health or functional problem, might be a function of the health status of others in the community. To date little is known about the effects of MTO on objectively-measured health outcomes for children, including

[2] Body mass index is defined as weight in kilograms, divided by height in meters, squared. For a woman who is five feet four inches tall, a BMI of 40 would correspond to a body weight of about 235 pounds.

[3] Among adults, medium-run findings from the interim evaluation (4-7 years after baseline, pooling data from all five sites) showed a lower prevalence of obesity (BMI $\geq$ 30) for adults in the experimental group than the control group (42.0% vs. 46.8%), together with some signs of increased rates of exercise, improved diet, and improved mental health. No statistically significant effects were detected in either the interim or long-term data for most other adult physical health outcomes, including self-rated health or hypertension (Kling et al., 2007; Sanbonmatsu et al., 2011).

one particularly important measure (and the focus of our study) – mortality.

3. Background, data, and methods

3.1. Chicago's housing voucher program

In July 1997 the firm running Chicago's housing voucher program, CHAC, Inc., opened the program wait list for the first time in 12 years. 82,607 income-eligible families applied in June 1997, of whom 8738 were in public housing at the time. We focus on families living in public housing at baseline because when these families are offered vouchers, the main effect of the voucher is to enable them to move into a less-distressed neighborhood. (There are no other income effects or changes in disposable cash available from reductions in out-of-pocket spending on housing because rent rules are the same for public housing and vouchers.) In contrast, when families living in private-market housing receive a voucher, they tend to not change neighborhood environments much at all, and instead experience the voucher mostly as a massive income shock and increased marginal tax rates on earnings (Jacob and Ludwig, 2012).

CHAC did a preliminary eligibility screen and then subcontracted with Abt Associates to randomly assign all eligible applicants to the voucher program wait-list in July 1997. The use of a random lottery to allocate a valuable public service for which there is excess demand has precedent in other areas in Chicago, such as for charter-school slots. Families were told their wait-list positions in summer 1997.

Families were then offered vouchers in descending order off the wait list, as vouchers became available to CHAC. No new voucher applications were accepted after July 1997.[4] A total of 1930 of the families in public housing at baseline were offered vouchers by May 2003, at which point CHAC stopped offering new vouchers.[5] Our analytic sample consists of the 11,680 children $\leq$18 living in public housing when their families applied for a voucher in July 1997.

Housing vouchers subsidize families to live in the private housing market with a subsidy equal to the difference between something called the "fair market rent," or FMR (HUD's definition has changed over time but is usually set to something between the 40th and 50th percentile of the metropolitan area's rent distribution) and 30 percent of the family's income, after netting out various deductions (see Jacob and Ludwig, 2012). The FMR value also depends on family size. The average FMR for our study sample was on the order of about $12,000 per year, while the average subsidy value was on the order of something like $8000 per year (Jacob and Ludwig, 2012). Families have a limited amount of time to find an apartment from when they are offered a voucher, usually three to six months. No special restrictions were imposed on where families could use the vouchers allocated through the July 1997 wait-list.

Over the course of our study sample, a few other smaller and more specialized voucher allocations occurred in the background, for example vouchers for families whose public housing projects were demolished as a result of the HUD HOPE VI program. This means that a small share of control group families obtained vouchers through other means besides the July 1997 wait-list lottery. Below, we discuss how we handle this in our analysis.

3.2. Data

The study sample is constructed using CHAC voucher application forms and administrative records from the Illinois Department of Human Services (Jacob and Ludwig, 2012), which include information about baseline addresses and socio-demographic characteristics. Probabilistic matching was used to match our sample to nationwide mortality records from 1997:Q3 to 2009:Q4 from the National Death Index (NDI) using identifiers such as first and last name, and month, day and year of birth.

Our main analyses focus on 143 cases with probabilistic match scores high enough to be deemed "true" deaths by the NDI (National Death Index User's Guide, 2009). Around 92 percent of these deaths occurred in Illinois. Previous validation studies find the NDI captures 93% of all deaths and 84% of deaths to blacks (Calle and Terrell, 1993). Of the deaths in our sample, 111 occurred among the 9189 control group children over our 11.5 year study period, for an annual mortality rate of 103 per 100,000 (vs. 40 per 100,000 for blacks ages 1–19 nationwide in 2009, and 84 per 100,000 for blacks 1–30) (WONDER, 2012). As a sensitivity analysis we also present results using alternative match-quality thresholds that yield between 109 and 136 deaths.

We use ICD-9 and ICD-10 codes in the NDI data to create measures of death from specific causes: homicide, suicide, accidents, and all other causes, which for convenience we call "disease" (the most common of which are deaths during the perinatal period, leukemia/neoplasms, cardiovascular disease, and respiratory problems). There are too few suicides to analyze separately, so we focus on all-cause mortality and our three specific causes.

For cost reasons we carried out post-lottery passive address tracking for a random subsample of families. We link addresses to tract-level data from the 2000 census, annual beat-level data on violent and property crimes per 1000 residents from the Chicago Police Department, and data from the 1995 community surveys of the Project on Human Development in Chicago Neighborhoods, which includes measures of social disorder and "collective efficacy," defined by sociologists as social cohesion and local social control measured at the level of a "neighborhood cluster" that contains 2.5 census tracts on average (Sampson et al., 1997). Unfortunately, no data on housing unit quality are available for our sample.

3.3. Empirical strategy

We define our "treatment group" as children whose families were assigned a wait-list number from 1 to 18,110, and so were offered a voucher by May 2003; the control group is everyone assigned a higher lottery number. We conduct an omnibus *F*-test of null hypothesis that the difference in the full set of baseline characteristics of children randomized to the treatment and control groups are jointly zero by estimating a seemingly unrelated regression, where all of the baseline characteristics shown in Table 1 are

[4] CHAC did interrupt service to the July 1997 voucher wait-list to provide vouchers to Hispanic families, as the result of litigation between a local Hispanic advocacy organization and the local housing authorities. This lawsuit was filed in part because of the traditionally low rates of housing-program enrollment by Hispanic families in Chicago; for example the vast majority of families in the July 1997 voucher wait-list were African-American. Because none of the Hispanic families served as part of that process are part of either our treatment or control groups, this has no impact on our analysis. In 2008, Chicago again opened its voucher wait-list and received over 220,000 voucher applications, compared to just a few thousand vouchers to give out. We do not have any data from the 2008 voucher application system, so it is possible that a very small share of our control group might have been offered a voucher during the last two years of our study sample period and are not counted as voucher recipients in our analysis.

[5] Of the total set of families offered vouchers from July 1997 through 2003, 9 percent were offered vouchers in calendar year 1997; 17 percent were offered vouchers in 1998; none were offered in 1999, as CHAC served Hispanic families only as a result of a discrimination lawsuit; in 2000, 15 percent of the total set of families offered vouchers off the wait-list were offered vouchers; 32 percent were offered in 2001; 26 percent were offered in 2002; and around 2 percent were offered in 2003.

Table 1
Baseline statistics.

	Overall	Control group	Treatment group
African-American	0.98	0.98	0.98
	(0.13)	(0.13)	(0.14)
Age	8.41	8.38	8.51
	(4.70)	(4.69)	(4.75)
Female	0.50	0.49	0.51
	(0.50)	(0.50)	(0.50)
Head of household received TANF second quarter 1997	0.78	0.78	0.76
	(0.41)	(0.41)	(0.43)
Head of household second quarter earnings 1997	1090.84	1056.10	1219.20
	(2000.06)	(1979.42)	(2069.89)
Census tract percent minority	0.95	0.95	0.96
	(0.14)	(0.14)	(0.13)
Census tract percent black	0.89	0.89	0.89
	(0.24)	(0.25)	(0.24)
Census tract poverty rate	0.60	0.60	0.60
	(0.20)	(0.20)	(0.20)
Census tract has poverty rate < 20%	0.03	0.03	0.02
	(0.16)	(0.17)	(0.15)
Census tract collective efficacy score	3.56	3.55	3.58
	(0.37)	(0.38)	(0.32)
Census tract social capital score	3.41	3.41	3.42
	(0.33)	(0.35)	(0.28)
Neighborhood poverty crime rate	120.30	120.44	119.77
	(66.49)	(66.88)	(65.00)
Neighborhood violent crime rate	39.11	39.30	38.42
	(26.20)	(26.21)	(26.16)
Observations (number of children)	11,680	9189	2487

Notes: The unit of analysis is individual child at baseline. Sample consists of all children 18 and younger whose families were living in public housing at the time they applied for a housing voucher in Chicago in July, 1997. Standard deviations in parentheses. Crime rates are per 1000 residents measured at the "beat" level. All income measured in 2007 dollars. See text for discussion of all estimates.

stacked as dependent variables and the only explanatory variables are outcome-specific indicators for treatment group assignment and intercepts (see also Jacob and Ludwig, 2012).

We then measure how the *offer* of a housing voucher affects the average post-lottery neighborhood environments in which families live by essentially comparing the neighborhoods of families offered vouchers with those of families randomly assigned to the control group, known as the "intent to treat" (ITT) effect. Specifically we use ordinary least squares to estimate Eq. (1) with a person-quarter panel dataset for 1997:Q3 through 2005:Q4, where y_{it} measures child *i*'s neighborhood in quarter *t*, $PostOffer_{it} = 1$ if child *i*'s family was *offered* a voucher prior to *t*, else 0, and *X* is a set of controls including an indicator for whether the family is offered a voucher some time *after* quarter *t* (so that the effect of PostOffer is identified just by the contrast between those who have been offered a voucher already vs. controls, excluding treatment group families who will be offered a voucher in the future, and so in principle could experience an "anticipation effect"), gender, spline functions in baseline age (kinks at 3, 7, 11 and 15) and calendar time (kinks every 10 calendar quarters). Controlling for baseline covariates helps improve the precision of our estimates by accounting for residual variation in the outcomes.[6] We cluster standard errors at the household level to account for serial correlation (Bertrand et al., 2004).

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

[6] Our point estimates are not sensitive to excluding controls except for controlling for calendar time, which is as we would expect – because treatment group families are served in descending order off the wait list, and so offered vouchers at different points in time, the distribution of person-quarters across calendar time are not the same for the treatment and control groups. Given that there are secular trends in mortality rates over time, particular due to homicide, controlling for calendar quarter identifies the effect of voucher offers by averaging across a series of point-in-time-specific treatment-control comparisons of mortality rates.

Since not all families who are offered vouchers use them to lease up and rent a private-market housing unit, we also estimate the effect on neighborhood environments of using a voucher or not (the "effect of treatment on the treated," or TOT) by applying two-stage least squares to Eqs. (2) and (3). Intuitively, the TOT effect is essentially the ITT effect divided by the difference in voucher utilization rates between treatment and control groups. We calculate the TOT effect using $PostOffer_{it}$ as an instrumental variable (IV) for an indicator variable $Leased_{it} = 1$ if the family leases up with a housing voucher obtained from any source – either the CHAC lottery or one of the smaller, specialized voucher allocations that occurred during our study period, such as for families whose public housing projects were demolished (Bloom, 1984; Angrist et al., 1996). The results are identical in sign and significance if we instead define "treatment" as just use of a voucher offered through the July 1997 randomized wait-list. TOT point estimates in these models are about 1/3 smaller, reflecting an identical reduced form and a larger first stage. Our preferred treatment variable of "use of any voucher" reflects the possibility of substitution between voucher opportunities.

As a benchmark for judging the size of the TOT effect, π_1, we present our estimate for the control complier mean (CCM): the average outcome of children in the control group whose families would have used a voucher if assigned to the treatment group, which can take on negative values because of sampling variability. The CCM is calculated as the average value of the outcome for the treatment-group compliers minus the TOT estimate (Katz et al., 2001).

$$Leased_{it} = \alpha + \theta_1 \; PostOffer_{it} + \mathbf{X}\Gamma + \gamma_t + \varepsilon_{it} \qquad (2)$$

$$y_{it} = \alpha + \pi_1 \; Leased_{it} + \mathbf{X}\Gamma + \gamma_t + \varepsilon_{it}, \qquad (3)$$

For our main ITT estimates of voucher offer effects on mortality itself, we estimate Eq. (1) within the framework of a discrete-time hazard model using Logit models with Maximum Likelihood estimation (Allison, 1984). We use an unbalanced person-quarter panel dataset that runs through either 2005:Q4 or the last quarter in which the child is alive, whichever comes first. We report the coefficient β_1 in odds ratio (OR) terms for the probability of death in each quarter. The control variables are as above. As a sensitivity analysis, we also present results that control for a broader set of covariates (all variables in Table 1 except race, which has insufficient variation to include as a covariate). We also present the excess risk difference implied by our odds ratios defined as the increase in number of deaths per 100,000 child-quarters due to voucher receipt.[7]

We re-estimate (1) separately for different causes of death (homicide, accident, and disease) where deaths from other causes besides than the one being examined are treated as censoring events. Motivated by previous findings from the MTO experiment for a gender difference in how youth respond to residential mobility, we also estimate Eq. (1) separately by gender.

One data complication we encounter is that in our sample there is only one death to treatment-group females after the offer of a voucher, so a value of 1 for *PostOffer* is nearly perfectly predictive of mortality outcomes. As such the Logit coefficient on treatment is may be influenced by small-sample variability, despite the large number (290,637) of at-risk child-quarters. For related reasons, the usual Normal approximation to the distribution of the estimated treatment parameter may be inaccurate, invalidating standard Wald test statistics ("*T*-tests") for hypothesis testing or confidence intervals constructed in the usual way (based on inverting the Wald test statistic). Additionally, this data circumstance makes us doubtful of whether the linear probability model (which does give a point estimate and estimated standard error) can accurately approximate the binary outcome response to treatment for girls.

We are still able to test null hypotheses about the parameter of interest using likelihood ratio (LR) tests. In our tables, we report *p*-values from likelihood ratio (LR) tests of the null hypothesis of no impact (Lehmann and Romano, 2005). Additionally, for our main models we perform a permutation test for the null hypothesis of no impact. To implement this permutation test, we randomly permute (across households) the "lottery offer date" (including "never" as an option), then re-construct our key variable $PostOffer_{it}$ (and control variable $PostOffer_{it}$). We implement 999 permutations, and use the placement of the main estimate in the resulting distribution to construct *p*-values. Although we can only test the hypothesis of no impact using this method, it provides an alternate approach which may be helpful given the issue of few deaths. Additionally, because the permutation occurs across households, it allows for inference that accounts for within-household correlation of outcomes.

We can also use LR tests to construct 95% confidence intervals. To do so we find the set of parameter values that would not be rejected by a LR test at a 5% significance level (Lehmann and Romano, 2005). Due to the process of trying out a range of values and testing each, the resulting confidence intervals are called "profile likelihood ratio confidence intervals."[8]

We view this problem as essentially a "small sample" problem, even though we have large samples ($N > 200{,}000$) of at-risk children-quarters. We believe that this type of problem may be common in settings where researchers combine high-quality quasi-experiments with large but not immense data samples and low-probability outcomes (such as mortality or uncommon diseases or conditions). We believe that profile likelihood ratio confidence intervals can be a useful approach in such settings.

We also replicate our mortality results by applying linear probability models to Eq. (1) to estimate ITT effects and to Eqs. (2) and (3) to obtain TOT estimates. We report results in terms of deaths per 100,000 children per year. We use linear probability models for this sensitivity analysis, despite their well-known limitations, because non-linear IV estimates can be sensitive to functional form assumptions (Angrist, 2001; Angrist and Pischke, 2009). We view the LPM results as least reliable for the girls-only analysis, for reasons discussed just above.

4. Results

Table 1 presents summary statistics of the baseline characteristics for the 2487 treatment group youth and the 9189 control youth. The *p*-value on the *F*-test of the null hypothesis that the full set of treatment and control group means are jointly identical is .46. Among treatment families, 10% had leased up by second quarter 1998, 25% had leased up by 4th quarter 2000, and 50% had leased up by 3rd quarter 2002. In total, 64% of treatment families leased up with a voucher at any point during our study period (78% of these families with a voucher from the CHAC lottery). Among the control group, 10% had leased up by first quarter 2000, and 25% by 3rd quarter 2003. In total, 34% ever leased up with a voucher (none from the CHAC lottery).

The third row of Table 2 shows that being offered a housing voucher (the ITT effect) reduces the poverty rate in the average census tract in which families live over the 8.5 year study period (1997:Q3–2005:Q4) by 8 percentage points (95% CI −13 to −3 percentage points), compared to a control mean of 48 percent. The TOT effect is 26 percentage points (95% CI −46 to −7), compared to a control complier mean of 64 percent. The difference between the CM and CCM implies families who would live in the most distressed neighborhoods are the ones most likely to lease up with a voucher if offered one (results by gender are in Appendix Table A1).

The top panel of Table 3 presents our main results for the effects of being offered a voucher (the intent-to-treat effect) on overall mortality rates for all children 18 and under at baseline, and for males and females separately, from estimating Eq. (1) with Logit maximum likelihood. For the full sample, the odds ratio for the ITT effect on all-cause mortality is equal to 1.13 (95% CI 0.73–1.70), and for males equals 1.34 (95% CI 0.85–2.05).

Our main finding is that being offered a voucher is nearly perfectly predictive of mortality for females, with only one death among girls after being offered the voucher. The estimated impact on the odds ratio of mortality is 0.21 and the 95% likelihood ratio confidence interval ranges from 0.01 to 1.04. The likelihood ratio test enables us to reject the null hypothesis of no effect with a *p*-value of .054. A permutation test of the hypothesis of no impact gives similar results for all children (LR $p = 0.57$, Permutation $p = 0.58$), boys (LR $p = 0.20$, Permutation $p = 0.18$), and girls (LR $p = 0.054$, Permutation $p = 0.030$). The second panel of Table 3 replicates our estimates controlling for all the baseline measures from

[7] We calculate this using the formula: Excess risk difference $(\text{ERD}) = 100{,}000 \times \{[(\text{OR} \times \text{CM})/(1 - \text{CM} + \text{OR} \times \text{CM})] - \text{CM}\}$, with CM equal to the per-quarter mortality probability for the control group, and OR the estimated odds ratio impact of treatment. This is derived in the following way: first translate the control mean to "control odds" using $\text{CO} = \text{CM}/(1 - \text{CM})$; second derive treated odds as $\text{TO} = \text{OR} \times \text{CO}$; third derive treated mean as $\text{TM} = \text{TO}/(1 + \text{TO})$; and fourth take the difference between treated and control means and normalize to rates per 100,000 child-quarters $\text{ERD} = 100{,}000(\text{TM} - \text{CM})$.

[8] In an earlier version of this paper, there were **no** deaths to treated girls. This extreme version of the problem meant that it was infeasible to compute standard errors, and led us to pursue the likelihood based profile confidence interval approach. Also, at the urging of a referee, we pursued additional years of mortality tracking data. This resulted in the current data set with one death.

Table 2
Effects of voucher offer (intent to treat) and voucher utilization (treatment on the treated) on neighborhood poverty and residential mobility.

	Control mean	Intent to treat	Treatment on treated	Control complier mean
Number of moves	2.46	0.23 (−0.14, 0.61)	0.90 (−0.66, 2.46)	2.16
Census tract percent black	0.84	0.00 (−0.06, 0.07)	0.01 (−0.21, 0.23)	0.88
Census tract poverty rate	0.48	−0.08 (−0.13, −0.03)	−0.26 (−0.45, −0.07)	0.64
Tract has poverty rate < 20%	0.10	0.09 (0.01, 0.18)	0.32 (−0.006, 0.64)	−0.11
Tract collective efficacy score	3.66	0.04 (−.02, 0.09)	0.12 (−0.06, 0.29)	3.58
Tract social capital score	3.46	0.04 (0.00, 0.07)	0.11 (0.003, 0.22)	3.39
Property crime rate	84.74	−0.52 (−9.9, 8.8)	−1.64 (−31.146, 27.864)	71.92
Violent crime rate	21.9	−0.56 (−2.8, 1.7)	−1.76 (−8.89, 5.37)	18.73
Observations				
# Children		932		
# Children-quarters		25,975		

Notes: The unit of analysis is the child-calendar quarter. For cost reasons, address tracking was carried out for just a random subset of 10% of our sample. For the variable "number of moves", there are 30,756 child-quarter observations. For the other variables, there are between 25,266 and 25,975 observations. Table comes from estimating Eq. (1) with ordinary least squares, controlling for gender, an indicator for whether the family is offered a voucher at some point after quarter t, and splines in baseline age and calendar time (see text). For ITT and TOT results, table presents point estimate and 95% CI in parentheses. For TOT estimates, "treatment" is defined as use of any voucher from any allocation during our study period (see text).

Table 3
Logit results for intent to treat effects of housing voucher offer on all-cause mortality.

	Boys and girls	Boys	Girls
Default specification (control for spline in baseline age and calendar time)			
Odds Ratio Estimate	1.13	1.34	0.21
Likelihood ratio 95% CI	(0.73, 1.70)	(0.85, 2.05)	(0.01, 1.04)
Likelihood ratio test of null hypothesis of no effect (p-value)	0.57	0.20	0.05
Excess risk implied by odds ratio estimate	3.20	13.23	−7.65
Excess risk 95% CI	(−6.61, 17.04)	(−5.80, 40.67)	(−9.58, 0.34)
N deaths (total/control/treatment pre/treatment post)[a]	(143/111/4/28)	(119/89/3/27)	(24/22/1/1)
Expanded Covariates (add controls for baseline characteristics in Table 1)			
Odds ratio	1.13	1.35	0.20
Likelihood ratio 95% CI	(0.73, 1.71)	(0.85, 2.07)	(0.01, 0.98)
Likelihood ratio test of null hypothesis of no effect (p-value)	0.56	0.19	0.04
Excess risk implied by odds ratio estimate	3.24	13.63	−7.77
Excess risk 95% CI	(−5.60, 41.42)	(−5.60, 41.42)	(−9.59, −0.21)
N deaths (total/control/treatment pre/treatment post)[a]	(143/89/3/27)	(119/89/3/27)	(24/22/1/1)
Observations			
Children	11,680	5856	5824
Children-quarters (main results)	581,438	290,801	290,637
Children-quarters (control children)	457,659	230,674	226,985
Children-quarters (treatment pre)	34,850	16,921	17,929
Children-quarters (treatment post)	88,929	43,206	45,723

Notes: Figures for treatment pre and treatment post are for the person-quarters before and after treatment group family was offered a voucher through the CHAC 1997 voucher lottery (see text). The unit of analysis is the child-calendar quarter. Table comes from estimating Eq. (1) with Logit maximum likelihood, controlling for gender, an indicator for whether the family is offered a voucher at some point after quarter t, and splines in baseline age and calendar time. Excess risk figures are reported as deaths per 100,000 children per year.

[a] Total deaths reported over the entire study period (the 12.5 years between 1997:Q3 through 2009:Q4).

Table 1. In Appendix Table A2 we present results based on alternate NDI match-quality thresholds.

The qualitative results for boys and girls are different (for girls the coefficient is negative and nearly significantly different from zero, for boys it is positive and insignificant). To test for the equality of treatment effect across boys and girls, we estimate two models. In the first all the coefficients are allowed to vary by sex, including that of treatment. In the second, we constrain the treatment effect to be equal for boys and girls. However, we continue to allow the other coefficients to vary by sex. These two models are then compared via a likelihood ratio test. This test results in moderate evidence against the hypothesis of equal treatment effects ($p = 0.106$).[9]

Tables 4 and 5 present results separately for different causes of death using Logit and linear probability models, respectively. The

[9] If we force the other control variables to be the same across sexes (allowing only for an intercept shift by sex), the likelihood ratio test gives $p = 0.07$. A test of equality of coefficients for control variables across sexes is not rejected ($p = 0.68$).

Table 4
Logit estimates for housing voucher intent to treat effects on mortality: different causes of death.

	Boys and girls	Boys	Girls
Disease			
Odds ratio	0.91	1.92	0.00
Likelihood ratio 95% CI	(0.30, 2.22)	(0.60, 5.27)	(0, 0.67)
Likelihood ratio test of null hypothesis of no effect (*p*-value)	0.84	0.25	0.02
Excess risk implied by odds ratio estimate	−0.55	5.18	−6.17
Excess risk 95% CI	(−4.13, 7.20)	(−2.26, 24.05)	(−6.17, −2.02)
N deaths (total/control/treatment pre/treatment post)[a]	(32/27/0/5)	(18/13/0/5)	(14/14/0/0)
Homicide			
Odds ratio	1.07	1.11	0.69
Likelihood ratio 95% CI	(0.60, 1.79)	(0.61, 1.90)	(0.03, 4.15)
Likelihood ratio test of null hypothesis of no effect (*p*-value)	0.81	0.71	0.72
Excess risk implied by odds ratio estimate	1.01	2.87	−0.97
Excess risk 95% CI	(−5.82, 11.61)	(−10.06, 23.29)	(−2.98, 9.71)
N deaths (total/control/treatment pre/treatment post)[a]	(87/67/3/17)	(79/60/3/16)	(8/7/0/1)
Accident			
Odds ratio	2.13	2.35	0
Likelihood ratio 95% CI	(0.66, 5.99)	(0.72, 6.77)	(0, 27.61)
Likelihood ratio test of null hypothesis of no effect	0.19	0.14	0.52
Excess risk implied by odds ratio estimate	2.97	6.36	−0.44
Excess risk 95% CI	(−0.90, 13.09)	(−1.35, 27.52)	(−0.44, 11.72)
N deaths (total/control/treatment pre/treatment post)[a]	(18/12/1/5)	(16/11/0/5)	(2/1/1/0)
Observations			
Children	11,680	5856	5824
Children-quarters	581,438	290,801	290,637

Notes: Figures for treatment pre and treatment post are for the person-quarters before and after treatment group family was offered a voucher through the CHAC 1997 voucher lottery (see text). The unit of analysis is the child-calendar quarter. Table comes from estimating Eq. (1) with Logit maximum likelihood, controlling for gender, an indicator for whether the family is offered a voucher at some point after quarter *t*, and splines in baseline age and calendar time (see text). Excess risk reported as deaths per 100,000 children per year.

[a] Total deaths reported over the entire study period (the 12.5 years between 1997:Q3 through 2009:Q4).

estimated voucher effect on deaths for disease, homicides, and fatal accidents are of the opposite signs for males vs. females. The linear probability model results suggest the effect of using a voucher (TOT) for females is on the order of −25.5 per 100,000 quarters-at-risk (95% CI −45 to −6), driven by declines in deaths from disease (−20.7, 95% CI −33 to −9).

Appendix Table A2 shows that our results are qualitatively similar when we use alternative probabilistic match thresholds to define what counts as a match to the National Death Index data. The odds ratios for the mortality effects to girls in particular shifts toward 1 as we add in relatively more ‘false negative’ matches but continue to imply large protective effects, although the precision of our estimates also declines as we change the match threshold (see appendix for additional discussion).

5. Discussion

Our study examines the effects of moving into less distressed housing and neighborhood conditions with the assistance of a housing voucher, taking advantage of a natural experiment in Chicago resulting from the random assignment of voucher applicants to the program wait-list. We show voucher receipt causes large declines in neighborhood disadvantage, including for example a decline in census tract poverty rates of 26 percentage points (40 percent of the control complier mean). While we do not have measures of housing quality for our sample, data from the American Housing Survey suggest that 10% of public housing units vs. 7% of those in the private market have moderate physical housing problems, with no difference in severe housing problems (HUD, 2009).

We find that moving out of high-poverty public housing projects in Chicago leads to large declines in mortality rates for female children and youth. Determining the exact magnitude of this impact is somewhat difficult in our study by the fact that there is only one death in our sample to females after their families are offered housing vouchers. The odds ratio for the estimated effect of a voucher offer on all-cause mortality for girls in our sample is 0.21 (95% CI 0.01–1.04), as shown in Table 3, while the average annual mortality rate during our study period is 38.8 per 100,000 for girls in the control group (Table 5). Together these results imply that the intent to treat effect on all-cause mortality from being offered a housing voucher is −30.6 deaths per 100,000 per year.

We also find that moving out of disadvantaged public housing does not have the same protective effects on mortality outcomes for male youth. This pronounced gender difference in mobility impacts on mortality echoes findings from the MTO mobility experiment for other youth outcomes (Kessler et al., 2012; Kling et al., 2007, 2005; Sanbonmatsu et al., 2011). Although our findings for homicides and accidents for boys are limited in their statistical power, the point estimates are consistent with the MTO studies, finding that boys were more likely to be injured or engage in other problem behaviors.

The main challenge this study faces is limited statistical power due to few deaths. This illustrates the difficult tradeoff for research in this field. Using population-level observational data will give improved power, but at the cost of relying on less-credible research designs. In contrast our study has the strength of a strong research design, but at the cost of observing few deaths. It is unlikely that any study with randomized housing treatment would be able to assemble a larger study sample, since our sample is the full census of public housing families that applied for housing vouchers in the 3rd largest city in the US. Finally, we can use our LR confidence intervals to provide bounds on the magnitude of the impact, and these bound rule out harmful effects of the voucher for girls.

In thinking about the populations to which our findings may generalize, it is important to recognize that our sample is extremely disadvantaged with respect to both their living conditions and health outcomes. While the U.S. poverty rate has fluctuated

Table 5
ITT and TOT effects of Housing Vouchers on Mortality from linear probability models.

	CM	ITT	TOT	CCM
Boys and girls				
Death all causes	24.3	3.4 (−9.2, 16.1)	11.2 (−30.1, 52.6)	21.4
Death from disease	5.9	−0.5 (−6.0, 4.9)	−1.8 (−19.5, 15.9)	10.4
Death from homicide	14.6	1.1 (−8.9, 11.1)	3.6 (−28.9, 36.2)	13.6
Death from accident	2.6	2.9 (−2.2, 8.1)	9.6 (−7.3, 26.4)	−4.4
Observations				
Children		11,680		
Children-quarters		581,438		
Boys only				
Death all causes	38.6	15.9 (−9.4, 41.1)	53.9 (−32.1, 139.8)	14.2
Death from disease	5.6	5.6 (−4.8, 16.0)	19.0 (−16.5, 54.4)	−0.9
Death from homicide	25.9	3.7 (−16.2, 23.5)	12.5 (−54.9, 79.9)	23.6
Death from accident	4.7	6.7 (−3.9, 17.2)	22.6 (−13.3, 58.4)	−11.5
Observations				
Children		5856		
Children-quarters		290,801		
Girls only				
Death all causes	9.7	−8.1 (−14.3, −1.9)	−25.5 (−45.4, −5.7)	25.5
Death from disease	6.2	−6.6 (−10.3, −2.8)	−20.8 (−32.8, −8.7)	20.7
Death from homicide	3.1	−1.0 (−5.9, 3.9)	−3.2 (−18.6, 12.2)	3.2
Death from accident	0.4	−0.5 (−1.6, 0.6)	−1.6 (−5.0, 1.9)	1.6
Observations				
Children		5824		
Children-quarters		290,637		

Notes: For counts of total number of deaths, please see Tables 3 and 4. The unit of analysis is the child-calendar quarter. Table comes from estimating Eq. (1) with Logit maximum likelihood, controlling for gender, an indicator for whether the family is offered a voucher at some point after quarter *t*, and splines in baseline age and calendar time. For ITT and TOT results, table presents point estimate and 95% CI in parentheses.

between 11 and 15 percent the last 20 years,[10] the baseline census tracts for our sample were fully 60 percent poor. Their average baseline police beats had violent crime rates of 39 per 1000, compared to a citywide average of 23 (CPD, 2009), and nationwide average of 6 (FBI, 1997).

On the other hand our study sample is not so different from the set of families living in high-poverty neighborhoods in the U.S., and so our results are more likely to generalize to efforts to change community environments for that population. Kneebone et al. (2011) examine the population living in “extreme-poverty” census tracts (with poverty rates of 40 percent or more) in the 2005–2009 American Community Survey in 100 metropolitan areas, and find that 79 percent of residents in such areas are black or Latino, 38 percent of adults are high school dropouts, and 23 percent of households are headed by women with children. Moreover despite policies intended to help reduce the concentration of poverty, such as HOPE VI public-housing demolitions, the number of people in extreme-poverty tracts rose by fully one-third from 2000 to 2005–09 (Kneebone et al., 2011). More generally, residential segregation by income in the U.S. has been increasing steadily since 1970 (Reardon and Bischoff, 2011).

In terms of the population excess risk, the Chicago Housing Authority's 2000 annual report suggests there were 9269 black females 18 and under living in public housing. If the mortality rate in public housing overall was similar to our study's control mean, then if every public housing family that applied for a voucher in 1997 had been offered one, the quarterly mortality rate for all youth black females in public housing would have declined by 5 per 100,000 (50 percent).[11]

Our findings have implications for a wide range of housing policies that affect the geographic concentration of poverty in America, including zoning rules that affect the availability of low-cost housing (Roberts, 2009), siting decisions for new housing projects (Hunt, 2009), and decisions about whether to fund housing projects vs. housing vouchers (Olsen, 2003; Friedman, 1962; Quillian, 2005).

Our findings may also have implications for community-development interventions that seek to modify neighborhood environments without relocating people. In principle one might worry that mobility interventions like MTO and our study could have more modest impacts than community-level interventions,

[10] http://upload.wikimedia.org/wikipedia/commons/b/b7/US_poverty_rate_timeline.gif.

[11] We calculate this based on the following numbers: 5828 girls in households who applied for vouchers and 9269 girls in total, Table 3's excess risk reduction of 7.65 per 100,000 per quarter, and Table 5's control mean of 9.7 deaths per 100,000 per quarter. Specifically, 5824 × (7.65)/9269 = 4.8, which is 50% of 9.7.

given the potentially disruptive effects of moving itself.[12] But as noted above, mobility rates are high in general in the U.S.; around 31 percent of renter households move *each year* (Schachter, 2004). If a community-level intervention improved some neighborhood, within a few years a large share of the residents of that neighborhood would have moved in from somewhere else after the intervention was launched – and so experienced a "treatment" not so different from that of MTO or our own study here. The more important challenge with forecasting the effects of community-development interventions from our results is that the data available to us limit our ability to identify key mechanisms of action.

Our findings also potentially have implications for health policy debates about whether to try to equalize health spending across areas. Previous studies have shown, for example, that a family's own income helps explain some – but only some – of the variation across areas in health expenditures (Sutherland et al., 2009). Our results suggest that the geographic concentration of poverty within an area may also matter for health beyond each family's own individual poverty status.

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[12] This possibility also provides a candidate explanation for why we find more adverse impacts on mortality rates for male youth from moving to a less-distressed area than do Votruba and Kling's (2009) analysis of the Gautreaux mobility program, in which controls moved as well (to other parts of Chicago).

Appendix A. Alternative classifications of death match

In this appendix we explore sensitivity of our results to decisions regarding the death match. Our NDI matched deaths are based on two types of classifications: (1) the SSN matches, as well as other pertinent identifying information (these fall into NDI's "class 2" matches, as we did not have a middle initial to submit for comparison); (2) there was no ability to match the SSN, but many other identifying fields matched (these are NDI's "class 4" matches). Each potential match is given a score to represent the quality of the match; this score is increasing in the number of items (date of birth, name, race, sex, state of residence) which match. NDI offers guideline thresholds for classifying each potential match as a "true death" or not, and our baseline results follow their recommendation.

We are concerned about both "false negative" and "false positive" matches. The former will mean that we are missing out on important information, which may reduce the precision of our estimates. The latter may tend to bias our estimates toward a finding of "no effect", as we expect that the probability of false positives should be unrelated to treatment status.

First we examine lower thresholds for declaring a potential match "true deaths". We consider two lower thresholds, both of which are listed in [NID user's guide citation]. The first relaxes the threshold score by 5 "points", and the second by 10 "points". In our data these changes only impact the "class 4" matches without SSN comparisons. [NID user's guide citation] indicates that these class 4 matches may have many false positives. In the example in their Table A1, they show that decreasing the threshold score by 10 points from their recommended level results in nearly twice as many new "false positives" as newly correctly classified "true deaths". Some of the new "matches" brought in with the lower threshold indeed look somewhat marginal. To limit exposure to these false positives, we also try a specification where we do not count matches that appear problematic. We classify a match as "problematic" if it meets two conditions. These are (1) it is a "class 4" match, with no SSN comparison; (2a) the Year of birth is different by more than 5 years, OR (2b) three or more of the following: sex, race, month of birth, day of birth, or year of birth do not match (1 year off is treated as matching), or first name's match is only based on the first initial. We also explore two a more restrictive matches than the baseline, in which we only keep "class 2" matches.

In Appendix Table A2, we explore several alternative death classifications. First we re-present the baseline results from Table 3 based on the recommended matching criteria. Next we consider two lower thresholds, one by 5 "points" and one 10 "points". Next we consider the "10 points lower" threshold, but also throw out "problematic matches". Finally, we consider a more restrictive matching criterion, using only matches based on SSN records. Appendix Table A2 shows the results of these models. For boys, the results are similar across all match definitions. For girls, the results weaken when we use the "10 points" lower threshold – regardless of whether we drop problematic matches. With this match definition, the point estimate is still highly protective (OR = 0.66), but the confidence interval expands to include meaningfully harmful impacts as well. When we use a more restrictive match definition, the results are more similar to the baseline results (OR = 0.26), but with a somewhat wider confidence interval.

Due to the high likelihood of "false positives" when using the lower thresholds, and the attenuating impact of these, we tend to trust the results from the NDI recommended threshold. However, we also acknowledge the sensitivity of the results to lowering the threshold for match quality.

Table A1
Effects of voucher offer (intent to treat) and voucher utilization (treatment on the treated) on neighborhood poverty and residential mobility, by gender.

	Boys only				Girls only			
	Control mean	Intent to treat	Treatment on the treated	Control complier mean	Control mean	Intent to treat	Treatment on the treated	Control complier mean
Number of moves	2.47	0.43 (0.02, 0.83)	1.89 (−0.38, 4.15)	1.30	2.45	0.06 (−0.40, 0.51)	0.20 (−1.37, 1.77)	2.75
Census tract percent black	0.85	−0.04 (−0.12, 0.05)	−0.14 (−0.48, 0.20)	1.00	0.83	0.05 (−0.02, 0.11)	0.14 (−0.04, 0.32)	.77
Census tract poverty rate	0.47	−0.10 (−0.16, −0.05)	−0.38 (−0.66, −0.11)	0.70	0.50	−0.05 (−0.11, 0.01)	−0.16 (−0.34, 0.03)	.59
Tract has poverty rate < 20%	0.11	0.15 (0.05, 0.26)	0.57 (0.07, 1.08)	−0.27	0.10	0.03 (−0.05, 0.11)	0.10 (−0.16, 0.36)	.01
Tract collective efficacy score	3.66	0.04 (−0.02, 0.09)	0.12 (−0.07, 0.31)	3.59	3.66	0.03 (−0.03, 0.10)	0.1 (−0.10, 0.30)	3.57
Tract social capital score	3.45	0.03 (0.01, 0.07)	0.10 (−0.02, 0.22)	3.40	3.46	0.04 (0.00, 0.08)	0.12 (−0.01, 0.24)	3.38
Property crime rate	83.9	0.37 (−11.2, 11.9)	1.24 (−37.16, 39.64)	71.13	85.5	−0.94 (−9.6, 7.7)	−2.87 (−29.2, 23.4)	71.34
Violent crime rate	21.8	−0.83 (−3.75, 2.09)	−2.75 (−12.22, 6.84)	19.53	22.0	−0.14 (−2.3, 2.0)	−0.44 (−6.90, 6.03)	17.6
Observations								
# Children		443				489		
# Children-quarters		12,370				13,605		

Notes: The unit of analysis is the child-calendar quarter. For cost reasons, address tracking was carried out for just a random subset of 10% of our sample. For the variable "number of moves", there are 30,756 child-quarter observations across boys and girls. For the other variables, there are between 25,266 and 25,975 observations. Table comes from estimating Eq. (1) with ordinary least squares, controlling for gender, an indicator for whether the family is offered a voucher at some point after quarter t, and splines in baseline age and calendar time. For ITT and TOT results, table presents point estimate and 95% CI in parentheses.

Table A2
Logit estimates for housing voucher intent to treat effects on mortality: alternative classifications of death.

	Boys	Girls
Lower threshold (5 points)		
Odds ratio	1.31	0.18
Likelihood ratio 95% CI	(0.83, 2.00)	(0.01, 0.88)
Likelihood ratio test of null hypothesis of no effect (p-value)	0.23	0.03
N deaths (total/control/treatment pre/treatment post)[a]	(125/93/5/27)	(30/28/1/1)
Lower threshold (10 points)		
Odds ratio	1.27	0.66
Likelihood ratio 95% CI	(0.81, 1.93)	(0.22, 1.57)
Likelihood ratio test of null hypothesis of no effect (p-value)	0.27	0.37
N deaths (total/control/treatment pre/treatment post)[a]	(136/102/6/28)	(49/40/4/5)
Lower threshold (10 points), exclude problematic matches		
Odds ratio	1.33	0.59
Likelihood ratio 95% CI	(0.85, 2.02)	(0.14,.1.73)
Likelihood ratio test of null hypothesis of no effect	0.20	0.36
N deaths (total/control/treatment pre/treatment post)[a]	(129/96/5/28)	(33/26/4/3)
Only use SSN matches		
Odds ratio	1.47	0.26
Likelihood ratio 95% CI	(0.92, 2.26)	(0.01, 1.31)
Likelihood ratio test of null hypothesis of no effect	0.10	0.11
N deaths (total/control/treatment pre/treatment post)[a]	(109/79/3/27)	(18/17/0/1)
Observations		
Children	5856	5824
Children-quarters	290,801	290,637

Notes: Figures for treatment pre and treatment post are for the person-quarters before and after treatment group family was offered a voucher through the CHAC 1997 voucher lottery (see text). The unit of analysis is the child-calendar quarter. Table comes from estimating Eq. (1) with Logit maximum likelihood, controlling for gender, an indicator for whether the family is offered a voucher at some point after quarter t, and splines in baseline age and calendar time (see text). Excess risk reported as deaths per 100,000 children per year.

[a] Total deaths reported over the entire study period (the 12.5 years between 1997:Q3 through 2009:Q4).

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