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THE EFFECTS OF HOUSING AND NEIGHBORHOOD CONDITIONS ON CHILD MORTALITY

Brian A. Jacob
Jens Ludwig
Douglas L. Miller

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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 to information on every child in public housing that applied for a housing voucher in Chicago in 1997 (N=11,848). 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 1.11 for all children (95% CI 0.54 to 2.10), 1.50 for boys (95% CI 0.72 to 2.89) and 0.00 for girls – that is, the voucher offer is perfectly protective for mortality for girls (95% CI 0 to 0.79). Our paper also addresses a methodological issue that may arise in studies of low-probability outcomes – perfect prediction by key explanatory variables.

Brian A. Jacob
Gerald R. Ford School of Public Policy
University of Michigan
735 South State Street
Ann Arbor, MI 48109
and NBER
bajacob@umich.edu

Douglas L. Miller
University of California, Davis
Department of Economics
One Shields Avenue
Davis, CA 95616-8578
and NBER
dlmiller@ucdavis.edu

Jens Ludwig
University of Chicago
1155 East 60th Street
Chicago, IL 60637
and NBER
jludwig@uchicago.edu

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 Moving to Opportunity), 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 Jody Heymann and Aron 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 (MTO), found that MTO-assisted moves to less distressed neighborhoods reduced obesity and mental health problems among adults, had mixed effects on risky behaviors by youth, with girls doing better and boys on balance doing worse as a result of the moves, and had no detectable effects on survey-reported child health outcomes (Kling, Ludwig and Katz, 2005, Kling, Liebman and Katz, 2007, Fortson and Sanbonmatsu, 2010). 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 1997-2005 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=11,848). 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. 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 1.11 for all children (95% CI 0.54 to 2.10), 1.50 for boys (95% CI 0.72 to 2.89) and 0.00 for girls – that is, the voucher offer is perfectly protective for mortality for girls (95% CI 0 to 0.79). 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 no deaths after voucher assignment. 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 perfectly protective, and Logit and Probit models do not directly compute 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 with strong research designs. The third section discusses our data and empirical approach. Section four 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 2007 were unintentional injuries (325 deaths) and homicides (168), which are disproportionately likely to occur at the hands of parents or caregivers. Congenital anomalies accounted for 119 deaths in 2007 to this age group, while much less frequent were deaths from important diseases such as cancer (55), heart disease (43), chronic lower respiratory diseases (30), or influenza and pneumonia (29).[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 and homicides, which for

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

teenagers and adults is strongly related to anti-social behavior and lifestyle decisions that themselves are thought by social scientists to be influenced by neighborhood environments. For example, in Chicago in 2008 fully 92 percent of homicide offenders and, more surprisingly, 72 percent of victims had a prior arrest record (CPD, 2008). Nearly two-thirds of homicides were thought by police to result from altercations, while nearly nine of every ten victims 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 and Mirowsky (2001) find that living in a disadvantaged neighborhood is associated with lower levels of self-reported health and physical functioning; and Browning and Cagney (2002) find that individuals residing in neighborhoods with greater collective efficacy report better overall health. Diez Roux et al (2001) find 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, Dooley, & Phipps, 2004; Lumeng et al., 2006).

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, 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 for most other adult physical health outcomes, including self-rated health, hypertension, physical limitations, or asthma (Kling, Liebman & Katz, 2007).

Among MTO youth ages 15-20 at the time of the data collection (4-7 years after baseline), analyses of an overall index of the absence of different health problems revealed worse health for males in the experimental group relative to controls, but positive effects on female youth (Kling, Liebman and Katz, 2007). This gender difference in health impacts is also found in MTO impacts on a range of risky behaviors with the one notable exception of violent-crime arrests, which seemed to decline among both male and female youth who moved through MTO.

No statistically significant MTO effects were found for specific measures of individual health problems when examined separately, or for younger teens and children in the interim MTO study (Orr et al., 2003; Fortson & Sanbonmatsu, 2010).

One potential concern with the previous MTO research on child health stems from the measurement of child health outcomes, which were all from parent reports of their children's health or 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 nothing is known about the effects of MTO on objectively-measured health outcomes for children, including 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, of whom 8,738 were in public housing at the time. All applicants were randomly assigned to a wait-list. A total of 1,930 of the families in public housing at baseline were offered vouchers by May 2003, at which point CHAC stopped offering new vouchers. Our analytic sample consists of the 11,848 children ≤18 living in public housing when their families applied for a voucher.

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, forthcoming), which include information about baseline addresses and socio-demographic characteristics. Probabilistic matching was used to match our sample to nationwide mortality records from 1997:Q3-2005:Q4 from the National Death Index (NDI) using identifiers such names and dates of birth.

Our main analyses focus on 69 cases with probabilistic match scores high enough to be deemed "true" deaths by the NDI (National Death Index User's Guide, 2009). 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, 54 occurred among the 9,342 control group children over our 8.5 year study period,for an annual mortality rate of 68 per 100,000 (vs. 43 / 100,000 for blacks 1-19 nationwide in 2004) (WONDER, 2009). As a sensitivity analysis we also present results using a lower match-quality threshold yielding 117 deaths to our sample, which provides one (admittedly imperfect) check on the possibility that our results are driven by classification error in the matching of mortality data to our sample.

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 1,000 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, Raudenbush and Earls, 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 1-18,110, and so were offered a voucher by May 2003; the control group is everyone assigned a higher lottery number. We first conduct an omnibus F-test for differences between the treatment and control group using methods described in detail elsewhere (Jacob and Ludwig, forthcoming).

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 equation (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 whether the family is offered a voucher some time *after* quarter *t*, gender, spline functions in baseline age (kinks at 1, 2, 5, 8 and 15) and calendar time (kinks every 6 calendar quarters). We cluster standard errors at the household level to account for serial correlation (Bertrand, Duflo, and Mullainathan, 2004).

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

Since not all families who are offered vouchers use them to lease up, 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 equations (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, Imbens and Rubin, 1996). 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 (Katz, Kling and Liebman, 2001).

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

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

For our main ITT estimates of voucher offer effects on mortality itself, we estimate equation (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. We also present the excess risk difference implied by our odds ratios, presented in terms of deaths per 100,000 per year, and defined as the difference between the predicted risk for the treatment group and the baseline risk (control mean, or CM):

(4) Excess risk difference = [(OR*CM) / (1 – CM + OR*CM)] - CM

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 equation (1) separately by gender.

One data complication we encounter is that in our sample there are no deaths to treatment-group females after the offer of a voucher, so a value of 1 for *PostOffer* is perfectly predictive of mortality outcomes. As such the Logit coefficient on treatment is negative infinity, and Logit standard errors are undefined. This makes typical construction of standard errors infeasible. Without consistent standard errors (or finite point estimates) we are unable to construct Wald test statistics 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 the unrestricted model, the "treated-by-post" observations are perfectly predicted, and so contribute zero to the log likelihood. In the restricted model, we impose the null hypothesis on the treatment parameter, maximize the likelihood function over the remaining parameters, and use the resulting log likelihood for the test. In our tables, we report p-values from Likelihood Ratio (LR) tests of the null hypothesis of no impact (Lehmann and Romano, 2005).

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."

We view this problem as essentially a "small sample" problem, even though we have large samples (N>200,000) of at-risk children-quarters. Presumably the true impact of the voucher on mortality is not perfectly protective, but deaths are uncommon enough that a strongly protective treatment, plus a bit of luck, can result in zero observed deaths. 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 equation (1) to estimate ITT effects and to equations (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 2,506 treatment group youth and the 9,342 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, 66% leased up with a voucher at any point during our study period (50% with a voucher from the CHAC lottery). Among the control group, 36% 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 7 percentage points (95% CI -12 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 1).

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 equation (1) with Logit maximum likelihood. For the full sample, the odds ratio for the ITT effect on all-cause mortality is equal to 1.11 (95% CI 0.54 to 2.10), and for males equals 1.50 (95% CI 0.72 to 2.89).

Our main finding is that being offered a voucher is perfectly predictive of mortality for females, so that the estimated odds ratio is 0 and the 95% likelihood ratio confidence interval ranges from 0 to 0.79. The likelihood ratio test enables us to reject the null hypothesis of no effect with a p-value of .03. The second panel of Table 3 replicates our estimates controlling for all the baseline measures from Table 1, while the third panel uses our alternative definition of death using a lower NDI match-quality threshold.

The qualitative results for boys and girls are different (for girls the coefficient is negative and 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 = 0.09).

Tables 4 and 5 present results separately for different causes of death using Logit and linear probability models, respectively. The estimated voucher effect on deaths from disease are of the same sign for male and female youth, but for homicides and fatal accidents the estimated effects are of the opposite signs for males versus females. The linear probability model results suggest the effect of using a voucher (TOT) for females is on the order of -130.2 per 100,000 (95% CI -207.6 to -50.4), driven by declines in deaths from disease (-73.6, 95% CI -132.92 to -14.28) and homicide (-41.36, 95% CI -83.32 to 0.56).

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 are no deaths in our sample to females after their families are offered housing vouchers, but presumably the true underlying probability of death to these girls is not zero. The 95% CI for the odds ratio of the estimated effect of a voucher offer on all-cause mortality for girls in our sample is (0, 0.79), as shown in Table 3, while the average annual mortality rate during our study period is 38.2 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 at least -8 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 (Kling, Liebman and Katz, 2007, Kling, Ludwig and Katz, 2005). The fact that the point estimate for voucher effects on disease mortality is of the same sign for male and female youth, but of opposite signs for homicides and accidental deaths, suggests gender differences in mobility effects on risky behavior as a possible behavioral mechanism underlying these findings. 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 suggests a very large effect for girls - at least a 20% reduction.

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 is around 13 percent (Census, 2010), the baseline census tracts for our sample were 60 percent poor. Their average baseline police beats had violent crime rates of 39 per 1,000, compared to a citywide average of 23 (CPD 2009), and nationwide average of 6 (FBI, 1997).

In terms of the population excess risk, the Chicago Housing Authority's 2000 annual report suggests there were 9,269 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 mortality rate for all youth black females in public housing would have declined by 5 per 100,000 (17 percent).

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 versus housing vouchers (Olsen, 2003, Friedman, 1962, Quillian, 2005). Forecasting the effects of community-development interventions from our results is complicated by the fact that the data available to us severely 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, Fisher, and Skinner, 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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Table One: Baseline Statistics

	Overall	Control Group	Treatment Group
African-American	0.98 (0.13)	0.98 (0.13)	0.98 (0.14)
Age	8.39 (4.70)	8.35 (4.69)	8.51 (4.75)
Female	0.50 (0.50)	0.50 (0.50)	0.51 (0.50)
Head of Household Received TANF Second Quarter 1997	0.78 (0.42)	0.78 (0.41)	0.76 (0.43)
Head of Household Second Quarter Earnings 1997	1085.63 (1999.23)	1051.57 (1980.02)	1212.58 (2064.73)
Census Tract Percent Minority	0.95 (0.14)	0.95 (0.14)	0.96 (0.13)
Census Tract Percent Black	0.89 (0.24)	0.89 (0.24)	0.89 (0.24)
Census Tract Poverty Rate	0.60 (0.20)	0.61 (0.20)	0.60 (0.20)
Census Tract Has Poverty Rate <20%	0.03 (0.16)	0.03 (0.17)	0.02 (0.15)
Census Tract Collective Efficacy Score	3.56 (0.37)	3.55 (0.38)	3.58 (0.32)
Census Tract Social Capital Score	3.41 (0.33)	3.41 (0.35)	3.42 (0.28)
Neighborhood Poverty Crime Rate	120.19 (65.75)	120.33 (65.98)	119.69 (64.89)
Neighborhood Violent Crime Rate	39.14 (25.94)	39.32 (25.89)	38.48 (26.10)
Observations (number of children)	11,848	9,342	2,506

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 1,000 residents measured at the "beat" level.All income measured in 2007 dollars. See text for discussion of all estimates.

Table Two: 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	0.21 (-0.16, 0.59)	0.83 (-0.54, 0.19)	2.19
Census Tract Percent Black	0.84	0.01 (-.06, 0.07)	0.02 (-0.21, 0.25)	0.87
Census Tract Poverty Rate	0.48	-0.07 (-.12, -.03)	-0.26 (-.46, -0.06)	0.64
Tract Has Poverty Rate <20%	0.10	0.09 (0.01, 0.17)	0.31 (-0.03, 0.64)	-0.11
Tract Collective Efficacy Score	3.65	0.04 (-.02, 0.09)	0.12 (-0.06, 0.31)	3.56
Tract Social Capital Score	3.45	0.04 (0.01, 0.07)	0.12 (0.01, 0.24)	3.37
Property Crime Rate	84.47	-1.31 (-11.0, 8.36)	-4.26 (-35.47, 26.94)	74.52
Violent Crime Rate	21.94	-0.61 (-2.93, 1.72)	-1.96 (-9.52, 5.60)	18.86
Observations				
# Children		658		
# Children-quarters		31,647		

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. Table comes from estimating equation (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 Three: 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	1.11	1.50	0.00
Likelihood Ratio 95% CI	(0.54, 2.10)	(0.72, 2.89)	(0, 0.79)
Likelihood Ratio test of null hypothesis of no effect (p-value)	0.75	0.26	0.03
Excess Risk Implied by Odds Ratio Estimate	7.63	50.09	-38.16
Excess Risk 95% CI	(-31.93, 76.27)	(-28.07, 189.06)	(-38.16, -8.011)
N deaths (total / control / treatment pre / treatment post)[a]	(69/55/3/11)	(53/40/2/11)	(16/15/1/0)
Expanded Covariates (add controls for baseline characteristics in Table 1)			
Odds Ratio	1.14	1.54	0.00
Likelihood Ratio 95% CI	(0.55, 2.14)	(0.73, 2.97)	(0,0.82)
Likelihood ratio test of null hypothesis of no effect (p-value)	0.71	0.24	0.03
Excess Risk Implied by Odds Ratio Estimate	9.71	54.09	-38.16
Excess Risk 95% CI	(-31.24, 79.04)	(-27.07, 197.04)	(-38.16, -6.87)
N deaths (total / control / treatment pre / treatment post)[a]	(69/55/3/11)	(53/40/2/11)	(16/15/1/0)
Default specification with lower threshold for match quality to National Death Index			
Log Odds Estimate	0.93	1.38	0.18
Likelihood Ratio 95% CI	(0.49, 1.63)	(0.68, 2.53)	(0, 0.88)
Likelihood Ratio test of null hypothesis of no effect (p-value)	0.81	0.35	0.03
Excess Risk Implied by Odds Ratio Estimate	-8.30	54.22	-77.17
Excess Risk 95% CI	(-60.49, 74.62)	(-45.70, 217.93)	(-94.13, -11.29)
N deaths (total / control / treatment pre / treatment post)[a]	(119/94/12/13)	(76/57/7/12)	(43/37/5/1)
Observations			
Children	11,848	5,928	5,920
Children-quarters (main results)	401,822	200,811	201,011
Children-quarters (alternative death measure)	400,695	200,263	200,432

Notes: a = Total deaths reported over the entire study period (the 8.5 years between 1997:Q3 through 2005:Q4). 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 equation (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.

Table Four: Logit Estimates for Housing Voucher Intent to Treat Effects On Mortality: Different Causes of Death

	Boys and Girls	Boys	Girls
Disease			
Odds Ratio	0.37	0.82	0.00
Likelihood ratio 95% CI	(0.02, 1.88)	(0.04, 4.59)	(0, 1.46)
Likelihood ratio test of null hypothesis of no effect (p-value)	0.27	0.84	0.09
Excess Risk Implied by Odds Ratio Estimate	-14.31	-4.06	-22.88
Excess Risk 95% CI	(-22.27, 19.985)	(-21.66, 80.91)	(-22.88, 10.52)
N deaths (total / control / treatment pre / treatment post)[a]	(19/18/0/1)	(10/9/0/1)	(9/9/0/0)
Homicide			
Odds Ratio	1.32	1.58	0
Likelihood ratio 95% CI	(0.52, 2.96)	(0.61, 3.63)	(0, 3.06)
Likelihood ratio test of null hypothesis of no effect (p-value)	0.53	0.32	0.22
Excess Risk Implied by Odds Ratio Estimate	10.50	30.53	-12.72
Excess Risk 95% CI	(-15.76, 64.30)	(-20.54, 138.28)	(-12.72, 26.19)
N deaths (total / control / treatment pre / treatment post)[a]	(35/26/2/7)	(31/21/2/7)	(5/5/0/0)
Accident			
Odds Ratio	2.11	2.56	0
Likelihood Ratio 95% CI	(0.44, 7.79)	(0.52, 9.87)	(0, 21.98)
Likelihood ratio test of null hypothesis of no effect	0.31	0.22	0.48
Excess Risk Implied by Odds Ratio Estimate	11.23	27.38	-2.54
Excess Risk 95% CI	(-5.67, 68.66)	(-8.43, 155.49)	(-2.54, 53.26)
N deaths (total / control / treatment pre / treatment post)[a]	(12/8/1/3)	(10/7/0/3)	(2/1/1/0)
Observations			
Children	11,848	5,928	5,920
Children-quarters	401,822	200,811	201,011

Notes: a = Total deaths reported over the entire study period (the 8.5 years between 1997:Q3 through 2005:Q4). 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 equation (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.

Table 5: ITT and TOT Effects of Housing Vouchers on Mortality from Linear Probability Models

Boys and Girls	**CM**	**ITT**	**TOT**	**CCM**
Death All Causes	69.44	8.32	27.04	63.60
		(-47.48, 64.12)	(-154.72, 208.84)	
Death From Disease	22.72	-13.48	-43.92	56.86
		(-32.52, 5.56)	(-106.2, 18.36)	
Death From Homicide	32.84	13.36	43.56	8.24
		(-31.4, 58.12)	(-102.4, 189.56)	
Death From Accident	10.12	12.20	39.72	-13.83
		(-15.2, 39.6)	(-49.52, 128.96)	
Observations				
Children		11,848		
Children-quarters		401,822		
Boys Only				
Death All Causes	100.32	60.44	203.88	-15.804
		(-51.52, 172.4)	(-175.12, 582.92)	
Death From Disease	22.56	-3.69	-12.44	39.31
		(-37.68, 30.28)	(-127.12, 102.24)	
Death From Homicide	52.68	42.00	141.64	-34.16
		(-48.76, 132.72)	(-165.6, 448.88)	
Death From Accident	17.56	29.88	100.84	-47.12
		(-25.6, 85.4)	(-86.72, 288.44)	
Observations				
Children		5,928		
Children-quarters		200,811		
Girls Only				
Death All Causes	38.16	-40.88	-130.20	129.00
		(-65.28, -16.52)	(-207.6, -50.4)	
Death From Disease	22.88	-23.32	-73.60	73.61
		(-41.92, -4.76)	(-132.92, -14.28)	
Death From Homicide	12.72	-13.12	-41.36	41.37
		(-26.32, 0.09)	(-83.32, 0.56)	
Death From Accident	2.54	-4.44	-14.00	14.02
		4.48	14.20	
		(-13.28, 4.36)	(-41.88, 13.84)	
Observations				
Children		5,920		
Children-quarters		201,011		

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 equation (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.

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Appendix Table 1: 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	2.46	0.45 (0.04, 0.85)	2.04 (-0.36, 4.44)	1.15	2.45	0.01 (-0.45, 0.47)	0.04 (-1.53, 1.60)	2.85
Census Tract Percent Black	0.85	-0.04 (-0.12, 0.05)	-0.14 (-0.50, 0.21)	0.93	0.83	0.05 (-0.01, 0.12)	0.16 (-0.04, 0.35)	.76
Census Tract Poverty Rate	0.47	-0.10 (-0.16, -0.05)	-0.40 (-0.69, -0.10)	0.72	0.49	-0.05 (-0.10, 0.01)	-0.15 (-0.33, 0.04)	.58
Tract Has Poverty Rate <20%	0.11	0.15 (0.05, 0.26)	0.59 (0.06, 1.13)	-0.29	0.10	0.03 (-0.05, 0.11)	0.09 (-0.18, 0.35)	.03
Tract Collective Efficacy Score	3.65	0.04 (-0.01, 0.10)	0.15 (-0.06, 0.35)	3.56	3.65	0.03 (-0.03, 0.10)	0.1 (-0.11, 0.32)	3.57
Tract Social Capital Score	3.45	0.03 (0.00, 0.07)	0.12 (-0.01, 0.25)	3.38	3.45	0.04 (0.00, 0.08)	0.13 (-0.01, 0.26)	3.37
Property Crime Rate	83.79	0.18 (-11.31, 11.68)	0.63 (-39.03, 40.29)	71.69	85.1	-2.16 (-11.69, 7.36)	-6.61 (-35.73, 22.51)	42.23
Violent Crime Rate	21.89	-0.82 (-3.78, 2.14)	-2.83 (-12.97, 7.31)	19.58	21.99	-0.25 (-2.53, 2.04)	-0.76 (-7.77, 6.25)	17.8
Observations								
# Children		311				347		
# Children-quarters		15,015				16,632		

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. Table comes from estimating equation (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.