Government Assistance Protects Low-Income Families from Eviction

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Abstract

A lack of affordable housing is a pressing issue for many low-income American families and can lead to eviction from their homes. Housing assistance programs to address this problem include public housing and other assistance, including vouchers, through which a government agency offsets the cost of private market housing. This paper assesses whether the receipt of either category of assistance reduces the probability that a family will be evicted from their home in the subsequent six years. Because no randomized trial has assessed these effects, we use observational data and formalize the conditions under which a causal interpretation is warranted. Families living in public housing experience less eviction conditional on pre-treatment variables. We argue that this evidence points toward a causal conclusion that assistance, particularly public housing, protects families from eviction.

INTRODUCTION

Housing eviction has recently been recognized as a substantial problem in the U.S. In 2016 alone, 2.3 percent of renter-occupied U.S. households were evicted (Desmond et al., 2018). The problem became especially severe during the housing bubble that led to the Great Recession, with the prevalence of eviction peaking at 3.1 percent in 2006 (Desmond et al., 2018). Families with children face an especially high risk of eviction (Desmond et al., 2013), particularly those with low incomes. More than one in four children born into deep poverty in large U.S. cities in 1998 to 2000 were evicted by age 15 (Lundberg & Donnelly, 2019). The affordable housing crisis and the high prevalence of eviction demand a policy solution, especially for families with children. A first step toward this goal is to know whether current policies for rental assistance protect families from eviction.

While prior scholarship has investigated the benefits of housing assistance for children (e.g., academic achievement) and families (e.g., satisfaction with neighborhood quality), it has generally ignored a more proximate—and disruptive—outcome: whether housing assistance protects children from eviction. This omission is important because evidence on more distal outcomes is mixed. Some studies find that children raised with certain types of assistance have much more positive outcomes than those raised with other types of assistance (e.g., Chetty, Hendren, & Katz, 2016) while others find minimal benefits (e.g., Jacob, Kapustin, & Ludwig, 2014). It is therefore important to know whether assistance is beneficial for a short-run outcome for which it may be particularly effective: eviction.

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For a population of children born in large U.S. cities, we estimate that residence in public housing at age 9 reduces the probability of eviction between ages 9 and 15 by 8 percentage points (95 % CI: −0.14, −0.01). Eviction would be roughly three times as common for this group if they had received no assistance. We do not find evidence that other types of assistance (e.g., vouchers) protect families from eviction. Other types of assistance do, however, reduce the probability that a family misses a rent payment. Because our research goal is causal while the data are observational, we discuss the plausibility of the assumption required for this statistical evidence to point toward a causal effect. We conclude that an expansion of public housing would likely reduce the prevalence of eviction for this population of families with children.

Housing Assistance Programs Have Mixed Effects on Children

Housing assistance programs provided directly to renters take a variety of forms but primarily fall into two categories: public housing and government vouchers to subsidize rent in the private market. Public housing refers to units owned and operated by public housing authorities. Families that receive public housing use their assistance in specific buildings owned by a housing authority, and their rent is limited to 30 percent of their household income. While this program provides important assistance to about a million households in the U.S. (U.S. Department of Housing and Urban Development, 2018b), public housing stock has deteriorated in quality over time and has also contributed to racial segregation (Massey & Denton, 1993). Beginning in the 1970s, the United States shifted its investments in housing assistance to vouchers (which allow recipients to use their assistance for rental units in the private market) and tax credits, particularly the Low Income Housing Tax Credits (LIHTC, used to develop affordable housing in the private market). Often, voucher recipients use their vouchers in LIHTC-developed housing. The reach of the voucher and LIHTC programs have eclipsed that of public housing (Kingsley, 2017).

A substantial literature explores how child and family outcomes differ across different housing situations. Because types of assistance vary widely, the reference group in these studies is not constant (public housing vs. vouchers, no assistance vs. vouchers, comparisons across vouchers with different restrictions, etc.). To our knowledge, no prior study has explored the relationship between housing assistance and eviction. For the outcomes that this literature does study, evidence is mixed as to whether housing assistance is beneficial. We briefly review this mixed evidence to motivate the need to study a more proximate outcome (eviction) for which effects may be especially large.

Studies Showing Benefits of Housing Assistance

Existing studies provide some evidence that assistance programs are beneficial. Some of these studies directly compare those receiving assistance with those receiving no help. Compared with those children whose families receive no assistance, residence in public housing appears to improve mental health (Fenelon et al., 2018), reduce the probability of being held back a grade (Currie & Yelowitz, 2000), and ultimately promote employment and earnings in adulthood (Newman and Harkness 2002). Given prior evidence that children's outcomes are best when the housing cost burden is near 30 percent of household income (Newman & Holupka, 2016), these results may arise because public housing keeps housing costs near this level.

A larger literature theorizes that housing assistance is beneficial by improving families' neighborhood conditions. Among those residing in public housing, residence in high-income neighborhoods seems to improve health and neighborhood satisfaction (Fauth, Leventhal, & Brooks-Gunn, 2004), and residence outside of the inner city reduces fear of crime (Burby & Rohe, 1989). Voucher-based assistance to reside in the suburbs during childhood increases college attendance, employment, and earnings in adulthood compared to urban public housing (Chetty, Hendren, & Katz, 2016; Kaufman & Rosenbaum, 1992). One mechanism for this finding might be that moves to voucher-based assistance can improve families' social capital by putting them in contact with advantaged neighbors (de Souza Briggs, 1998).

Studies Showing a Lack of Evidence for Benefits of Housing Assistance

Meanwhile, other studies cast doubt on the benefits of housing assistance programs. The Moving to Opportunity (MTO) study began with a sample of families in urban public housing projects and randomly assigned some to a treatment condition that gave them a voucher for use in the private market in a low-poverty neighborhood. Effects of the voucher on children were mixed, with reductions in violent behavior for all but increased risky behaviors for boys and improved mental health for girls, and no evidence of effects on academic achievement (Sanbonmatsu et al., 2011, but see Chetty, Hendren, & Katz, 2016, for evidence of long-run benefits). More recently, a lottery study in Chicago compared families randomly assigned to vouchers with unsuccessful lottery applicants but found almost no effect on education, crime, or health outcomes (Jacob, Kapustin, & Ludwig, 2014). In a similar study, the Department of Housing and Urban Development commissioned an evaluation of the effect of vouchers among those eligible for but not receiving assistance before the intervention; giving tenant-based assistance to these families yielded no clear pattern of beneficial effects (Mills et al., 2006). Null results are not unique to randomized studies in particular subpopulations; observational panel data from a national sample likewise show no mean benefit of housing assistance for children's cognitive, behavioral, or health outcomes (Newman & Holupka, 2017). Using the Fragile Families and Child Wellbeing Study (the data source of this paper), Fertig and Reingold (2007) find no evidence that public housing promotes health and some evidence that it may be harmful. Overall, the evidence suggests that housing assistance may be beneficial for some child and family outcomes but harmful for others, with weak effects for many outcomes.

Assistance May Protect Families from Nonpayment and Eviction

Given mixed evidence that housing assistance is beneficial for more distal outcomes, an important question is whether assistance is beneficial for more proximate outcomes for which one would expect effects to be large. Although eviction has not been the focus of prior studies, there are conceptual reasons to hypothesize that housing assistance would protect families from eviction through at least three mechanisms: improved financial well-being, legal protections, and connections to additional social services.

Assistance may reduce eviction by easing the financial stress that families face. This mechanism may be of particular importance to low-income renters, about three-quarters of whom spend more than 30 percent of their income on rent (Joint Center for Housing Studies, 2018). By freeing up more resources for other expenses, housing assistance may reduce the risk that an unexpected large expense would prevent families from making a rent payment. Families with children who reside in public housing are less likely to experience housing cost burden than those who do not receive housing assistance (Gold, 2020). While the incidence of housing cost burden among voucher holders has decreased over time (McClure, 2005), just under a third of voucher holders still experience housing cost burden (Mast, 2012), and evidence from the MTO experiment finds that vouchers are not associated with reduced housing cost burden (Comey, Popkin, & Franks, 2012).

Housing assistance may also provide legal protections against eviction, though these protections are stronger among families in public housing than those with vouchers due to differences in program design. Families living in public housing can only be evicted if there is "good cause" which includes not paying rent, substantial violations of the rental agreement, or repeated minor violations of the rental agreement (Code of Federal Regulations 24 Part 247). Additionally, public housing authorities are required to go through a judicial process to evict, which may make informal evictions less common in this context (Code of Federal Regulations 24 Part 247). Voucher recipients are also protected by "good cause" reasoning, but landlords can choose to terminate their acceptance of a voucher at the end of a lease or after the initial lease period for personal or business reasons (Code of Federal Regulations 24 Part 982). Some families may interpret this termination as an eviction. In terms of legal protections, public housing may reduce self-reported eviction to a greater degree than other assistance.

A third mechanism through which housing assistance may protect families from eviction is by connecting them to other social services. Low-income families receiving housing assistance may also receive other supports such as childcare subsidies, food assistance, and job training (e.g., Park, Fertig, & Metraux, 2014). By reducing financial hardship, these other programs may indirectly protect against missed rent payments and eviction.

Prior Evidence Touches Only Indirectly on Eviction

We would like to know whether receipt of assistance protects families from eviction in the years immediately after assistance is received. Prior studies provide only indirect evidence on this question. The MTO study followed up with respondents 10 to 15 years after treatment assignment and asked whether in the past 12 months participants had ever been threatened with eviction due to nonpayment. Those who had been assigned to either voucher condition were non-significantly less likely to report this threat (Sanbonmatsu et al., 2011), but the decade-long gap between receipt of assistance and the period for which eviction is reported limits the informativeness of this estimate. Mills et al. (2006) find that vouchers reduce the risk of homelessness, which might be seen as an indirect indicator of eviction.

We found only one study that directly examined housing hardship in the period immediately following rental assistance. Using data on 417 families in the Detroit area in 2009 to 2011, Kim, Burgard, and Seefeldt (2017) construct a housing insecurity scale in which eviction is one of many items. Receipt of assistance is associated with reduced scores on this scale, but the aggregate nature of the scale makes it difficult to draw conclusions about any individual outcome (i.e., eviction). Overall, prior literature provides surprisingly little evidence about the effect of housing assistance on eviction.

METHODS

We use data from the Fragile Families and Child Wellbeing Study (hereafter Fragile Families Study), which sought to draw a probability sample of children born from 1998 to 2000 in hospitals in U.S. cities with populations over 200,000. The study interviewed families when children were approximately 1, 3, 5, 9, and 15 years old. We focus on the subsample born in 16 probabilistically-selected cities. Within these cities, the study over-sampled births to unmarried parents, producing a large sample of urban families at especially high risk of both housing assistance and eviction. This sampling design promotes efficiency of our estimates, while still allowing estimation of population parameters by weighting. We restrict our sample to children born in probabilistically-selected cities (3,442) whose mother responded to the age 9 survey (2,661) and who resided with their mother at least half of the time at that interview (2,410). We further restrict the sample to those who theoretically have some chance of receiving housing assistance: those who do not report owning their home (1,858) and do not report incomes above 200 percent of the federal poverty threshold at the age 9 interview (1,356). Finally, we restrict our sample to those with valid responses to the questions used to construct the indicator of housing assistance (1,302). All remaining missing values in predictors and the outcome are imputed with 30 imputations using the *Amelia* package in R (Honaker, King, and Blackwell, 2011; see the Appendix).[1]

Key Variables

The treatment variable is receipt of housing assistance. In the survey questionnaire when the child was approximately 9 years old, the focal child's mother answered a series of questions about her housing situation. Those who did not own their home (homeowners were excluded from our analyses) and did not report living in a house or condo owned by another family member (coded as no assistance in our analyses) were asked whether their current residence was in a public housing project. Those who said "YES" are coded as receiving public housing ($N = 224$). An additional question, which was skipped for those living with family or friends and paying no rent (coded as no assistance in our analyses), asks whether the federal, state, or local government was helping the respondent to pay for their rent. Mothers who answered "YES" to this question are coded as receiving other assistance ($N = 202$). Those who answered "NO" to both questions are coded as receiving no assistance ($N = 876$). Those who refused or did not know the answer to either question are coded as missing and excluded from our analyses ($N = 54$).

The outcome variable is whether the mother reported at an interview at approximately child age 15 whether she had ever been evicted from her home or apartment for nonpayment of rent or mortgage (1) in the past 12 months and (2) at any point since the age 9 interview. We code mothers as being evicted if they said "YES" to either question ($N = 86$), not evicted if they said "NO" to both questions ($N = 1,013$), and missing otherwise ($N = 203$). Eviction therefore covers a six-year period immediately following the point at which housing assistance is reported. Some mothers are not interviewed at child age 15, either due to nonresponse or because they were no longer the caregiver of the child at age 15. We code these cases as missing, but do not exclude them from the sample to avoid selecting on the dependent variable. Instead, we multiply impute missing outcomes as a function of observed variables. If missingness is marginally related to eviction but independent of eviction given covariates, then this approach is preferable (see the Appendix for imputation details).[2]

The use of self-reported eviction has drawbacks and benefits. Unlike administrative records (Desmond et al., 2018), self-reports may exclude some experiences that count legally as evictions but which respondents do not recall as such. For example, landlords often file (threaten) eviction without executing the order (Garboden & Rosen, 2019); in these instances, it is unclear how respondents would answer the survey question. On the other hand, landlords may often use informal procedures

[1] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.
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to evict tenants, thereby producing higher experience of eviction in private rentals than administrative records would suggest. Self-reported eviction is more likely to include these informal experiences that are absent from administrative records. Finally, the survey question in this study explicitly references eviction for nonpayment of rent or mortgage; evictions that tenants perceive as occurring for other reasons, such as other lease violations, are missed. This restriction is likely to shape estimates only minimally because nonpayment is a leading cause of eviction. In one case study of Milwaukee court records, the court summons was for missed rent payments in 92 percent of cases (Desmond, 2012). It is also possible that we capture evictions that are truly foreclosures (essentially evictions for nonpayment of mortgage), which could occur if respondents moved to an owned home during the age 9 to 15 period and experienced a foreclosure.

The prevalence of eviction by our measure is comparable to other approaches. Our weighted analytic sample suggests that 7 percent (95% CI: 0.05, 0.10) of children in our target population experience eviction for nonpayment of rent or mortgage between ages 9 and 15. We provide a few alternative estimates for comparison. In a survey of Detroit-area households during the Great Recession, Gould-Werth and Seefeldt (2012) estimate that 2.4 percent experienced eviction in a 12-month period. Administrative records show that roughly 2 to 3 percent of U.S. households are evicted through court orders each year (Desmond et al., 2018). The prevalence of eviction in our study is consistent with these numbers if one considers the longer time window of our study: six years compared with one year. Estimates are higher in studies that encompass a wider variety of forced moves, such as building condemnations and landlord foreclosures (Desmond & Shollenberger, 2015). Overall, our results speak only to one definition of eviction, but the prevalence of eviction by this definition is comparable to alternative definitions one might consider.

Estimand

We define our estimand formally in the potential outcomes framework of causal inference (Imbens & Rubin, 2015; Neyman, 1923; Rubin, 1974). We index the target population by $i = 1, \ldots, N$ and denote the type of assistance received by D_i. For each child i, whether an eviction occurs between ages 9 and 15 is denoted Y_i. The observed eviction is only one of three potential outcomes: $\{Y_i(\text{Public housing}), Y_i(\text{Other assistance}), Y_i(\text{No assistance})\}$. These outcomes denote whether child i would have experienced eviction if exposed to each of the three treatment conditions. For instance, for a child residing in public housing at age 9, we observe $Y_i = Y_i(\text{Public housing})$, but we do not observe the potential outcome under other assistance or no assistance because these treatment conditions did not occur for this child. We can learn about these other potential outcomes only by making assumptions that allow information to be shared across children in different treatment conditions (see the next subsection on causal identification). The causal estimands $\tau_{\text{Public housing}}$ and $\tau_{\text{Other assistance}}$ are the population average treatment effect on eviction of public housing and other assistance, respectively, among those who participate in these programs.

$$\tau_{\text{Public housing}} = \frac{1}{N_{\text{Public}}} \sum_{i:\, D_i=\text{Public housing}} \left(Y_i\,(\text{Public housing}) - Y_i\,(\text{No assistance})\right) \quad (1)$$

$$\tau_{\text{Other assistance}} = \frac{1}{N_{\text{Other}}} \sum_{i:\, D_i=\text{Other assistance}} \left(Y_i\,(\text{Other assistance}) - Y_i\,(\text{No assistance})\right) \quad (2)$$

The potential outcomes and causal effects are defined with respect to assistance received at age 9. Notably, the treatment category at age 9 is not always experienced throughout the entire period over which the outcome is measured, from ages 9 to 15 (Appendix Table A3).[3] For instance, a family residing in public housing at age 9 may be evicted at age 12 and reside in a private rental by age 15. This family is still coded in the public housing group because that was the assistance category at age 9. If we restricted the sample to those for whom housing assistance did not change between ages 9 and 15, this would potentially condition on consequences of the earlier treatment (assistance at age 9) and the outcome (eviction between ages 9 and 15). Instead, by defining the treatment at age 9, we avoid selecting on the dependent variable or conditioning on post-treatment events. In short, this decision maintains proper temporal ordering between the treatment (at age 9) and the outcome (between ages 9 and 15). We therefore focus on causal estimands (equations 1 and 2) that are the difference in outcomes at ages 9 to 15 between treatment categories experienced at age 9.

Our research goal is to estimate a causal effect in a population (equations 1 and 2), but data are available only for a sample in which the treatment is not randomized. Conclusions therefore require assumptions about treatment assignment for identification of causal effects and sample inclusion for identification of a population average. We discuss the causal identification assumption below and assumptions for population inference in the Appendix.[4]

Causal Identification

For families in the sample who received assistance, the potential eviction outcome under assistance is known. Because the Fragile Families Study is a probability sample, we can estimate the potential outcome under assistance for those receiving assistance by a simple weighted average. The key challenge to causal identification is how to estimate the potential eviction outcome that these families would have experienced in the absence of assistance $Y(0)$. To do so, we assume this potential outcome in the absence of assistance is independent of housing assistance D within subgroups of pre-treatment covariates $\vec{X}$ (equation 3).

$$Y(0) \perp D \mid \vec{X} \tag{3}$$

To build an argument for this assumption, we first summarize the process by which housing assistance D is assigned. Whether a family qualifies for housing assistance is primarily determined by three eligibility criteria: annual gross income relative to family size, family composition, and immigration status. Families are eligible if their incomes fall below income limits that vary geographically based on the area median income, with priority given to families below 50 percent (very low income) and 80 percent (low income) of the median income (U.S. Department of Housing and Urban Development, 2018a). Those whose family compositions include someone who is elderly, disabled, or a child typically receive higher priority. Public housing authorities may also choose to prioritize other populations, including homeless

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families or victims of domestic violence. The Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA, welfare reform) limits federal assistance to citizens and those with legal immigration status; housing authorities may require applicants to provide evidence of this for all household members, though the specific requirements vary across housing authorities (McCarty & Siskin, 2012). Many housing authorities ban those with heavy alcohol or drug use or criminal convictions (Curtis, Garlington, & Schottenfeld, 2013). Finally, those granted vouchers for use in the private market must find a landlord willing to accept the voucher, a factor that limits lease-up rates among those with vouchers (Chyn, Hyman, & Kapustin, 2019).

Some of the determinants of housing assistance are likely to have large direct effects on eviction even in its absence, thereby producing confounded treatment assignment. To minimize confounding, we include a set of pre-treatment covariates $\vec{X}$. Our aim is to include in $\vec{X}$ the most important covariates through which housing assistance and eviction would be non-causally associated. We select these covariates on the basis of our theoretical understanding of the treatment assignment process and the process generating the outcome: We include variables known to affect the probability of housing assistance (above) which are likely to also affect eviction directly. The aim is to produce a conditioning set such that housing assistance D is independent of potential eviction in its absence $Y(0)$, within subgroups of $\vec{X}$ (equation 3). All variables are reported by the mother at the age 9 interview. We include indicators of whether the mother reported not paying the full amount of rent or mortgage and whether she reported eviction, in (a) the 12 months immediately preceding the age 9 survey when treatment is defined and (b) at any survey wave prior to this.[5] We measure family income relative to the poverty threshold in the year preceding interviews at child ages 1, 3, 5, and 9, including the age 9 measure and the average of the reports from prior waves. A disability variable measured at child age 9 indicates whether the mother reported a health condition that limited the type or amount of work she could do. Because housing authorities may screen for criminal history and drug use, we include measures of these variables. We include whether at child age 9 the mother reports that she drinks four or more drinks in a day at least once per month, and whether she reports drug use in the past 12 months.[6] We measure criminal conviction by whether the mother reported being convicted of any charges beyond minor traffic violations between the child's first birthday and the age 9 interview. We include whether the parents were married at the child's birth as well as the mother's race (black, white, Hispanic, or other) and education (less than high school, high school, some college, or a college degree). We include two scaled scores from assessments of the mother when the child was approximately 3 years old: a cognitive score from a modified version of the Wechsler Adult Intelligence Scale (Wechsler, 1981) and a score on a modified version of Dickman's (1990) impulsivity scale. We refer the reader to the survey documentation (Fragile Families and Child Wellbeing Study, 2006) for further details on these scales as implemented in the study. Finally, we include a categorical variable to adjust for geography, based on the 16 cities of birth in which the sample is nested. Youth who reside in the Metropolitan Statistical Area (MSA) of their birth city are categorized by birth city.

[5] We keep the age 9 eviction indicators separate because they may be especially important sources of confounding. Because eviction is rare and prior indicators may be less relevant to confounding, aggregating prior measures may improve precision with minimal harm to identification.
[6] We determine drug use based on a series of questions about types of drugs: sedatives, tranquilizers, amphetamines, analgesics, inhalants, marijuana, cocaine, LSD, or heroin. Mothers who reported using any of these types without a doctor's prescription, in larger amounts than prescribed, or for a longer period than prescribed are coded as using drugs.

Table 1. Means of pre-treatment covariates by treatment group.

	No assistance	Public housing	Other assistance
Income / poverty line	0.99	0.74	0.74*
At age 9			
Average at ages 1, 3, and 5	1.29	0.87*	0.92*
Education (mother)			
Less than high school	0.42	0.48	0.49
High school	0.37	0.4	0.34
Some college	0.18	0.11	0.17
College	0.04	0.01	0.00
Race (mother)			
Black	0.31	0.52*	0.52*
Hispanic	0.44	0.26*	0.26
White/other	0.25	0.23	0.22
Parents married at birth	0.47	0.26	0.13
Impulsivity (mother, Dickman, 1990, range 1–4)	2.88	2.87	3.02*
WAIS-R cognitive score (mother, range 0–15)	6.36	5.85*	6.77*
Ever convicted of a crime (mother)	0.07	0.10	0.03
Drug use (mother)	0.08	0.05	0.20
Heavy alcohol use (mother)	0.06	0.07	0.08
Disability (mother)	0.15	0.21	0.17
Nonpayment of rent or mortgage			
In 12 months preceding age 9 interview	0.27	0.08*	0.21
Ever in 12 months preceding ages 1, 3, or 5	0.29	0.29	0.26
Evicted			
In 12 months preceding age 9 interview	0.05	0.01	0.03
Ever in 12 months preceding ages 1, 3, or 5	0.05	0.02	0.08

Notes: Missing values are imputed and means are calculated with sampling weights. Asterisk indicates a significant difference from the mean among those receiving no assistance.
$^{*}p < .05$; $^{**} p < .01$; $^{***}p < .001$.

All who do not reside in the MSA at age 9 are coded in one extra category (14 percent of the weighted sample).

Selection into housing assistance by these covariates is evident in our sample (Table 1). Several aspects of selection would lead us to expect that, if assistance programs were not protective, those receiving assistance would have higher risk of eviction. Compared with those receiving no assistance, children residing in public housing or receiving other assistance are disadvantaged in numerous ways: they have lower incomes relative to the poverty line, their mothers have less education and are more likely to be black, and their parents are less likely to have been married when they were born. All these sources of disadvantage suggest that those receiving assistance come from a population that may face a particularly high risk of eviction. On the other hand, those residing in public housing or receiving other assistance also have some characteristics that would suggest a lower probability of eviction. Their mothers are less likely to report eviction or missed rent or mortgage payments in the 12 months preceding the age 9 survey, though this could be because they were already receiving assistance during some of that period. Selection into government assistance therefore involves a complex set of countervailing processes, so that it is difficult to make an *a priori* prediction about the direction of selection. Adjustment for measured covariates is essential to an interpretable causal estimate.

There are also reasons to expect that the individual-level assignment of housing assistance involves substantial exogenous variation that is independent of potential eviction. Housing authorities typically receive far more applications for assistance than their resources can support, leading to long waiting lists that sometimes close when availability is severely limited (U.S. Department of Housing and Urban Development, 2018a). In 2012, the last time national data on housing authority waiting lists was collected, only 4 percent of public housing agencies (PHAs) reported that assistance was available without a waiting list, 90 percent of agencies had open waiting lists, and 6 percent of waiting lists were closed (Public and Affordable Housing Research Corporation, 2016). There is even greater unmet demand for vouchers: only 1 percent of housing authorities reported voucher availability with no wait and almost half of waiting lists were closed (Public and Affordable Housing Research Corporation, 2016). In 2015, only 25 percent of households whose incomes met the general cutoff for eligibility (below 50 percent of area median income) received some form of government housing assistance (Joint Center for Housing Studies, 2018, p. 5). Many housing authorities open the wait list in particular periods, accept applications, and then use a lottery to determine which applicants are permitted to join the wait list (Moore, 2016). The limited availability of assistance is central to the plausibility of our required assumption that housing assistance is assigned independently of potential eviction in its absence, within subgroups of measured covariates.

Despite our detailed set of control variables, the assumptions required for a causal interpretation may be violated to some degree. Housing assistance may correlate negatively with potential eviction in the absence of assistance. Families with the skill set to navigate the housing authority and secure assistance (D) may also be better equipped to negotiate with a landlord and avoid an eviction even without assistance $Y(0)$. Nonetheless, we believe violations of this assumption are likely to be small enough that our results will be informative. We hope that future research will reassess this question with randomized designs.

To draw inferences about the target population, we make additional assumptions about ignorable sample inclusion, including that attrition from the study can be ignored. These assumptions are credible for three reasons: The Fragile Families Study sought to achieve a probability sample in the first wave of data collection, the study maintained a high response rate even in subsequent waves, and analyses use weights designed by the study to address this problem. The Appendix discusses the specific assumptions required for population inference.[7]

Statistical Estimation

Our identification assumption focuses on the independence of housing assistance and potential eviction within subgroups of pre-treatment covariates. Because many subgroups contain only a few observations, inference requires additional estimation assumptions to share information across subgroups. We follow a model-based imputation approach sometimes called the parametric g-formula (Hernán & Robins, 2020, Ch. 13) or the imputation estimator (Abadie & Imbens, 2006, p. 241; Abadie & Imbens, 2011, p. 3; Hahn, 1998, p. 321). The advantage of this approach over focusing on the coefficient of a regression model is that it explicitly estimates the average treatment effect on the treated. The advantage over propensity score weighting is that the estimator does not suffer from the high variance that occurs when some propensity scores are near zero or one.

Because potential eviction under assistance is observed for those receiving assistance (our target population), we do not require a statistical model of eviction for

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these groups. To impute potential eviction in the absence of assistance, we assume a linear relationship between eviction and pre-treatment covariates among those not receiving assistance (equation 4). We estimate this model by ordinary least squares (OLS).[8] Because the model already conditions on the variables that most strongly determine weights (city of birth and parents' marital status at birth) and assumes that associations are constant across the population, we estimate this model without survey weights (Winship & Radbill, 1994). Survey weights are incorporated in a later step. Because the coefficients $\vec{\beta}$ have no causal interpretation, they are omitted from the main text and presented in the Appendix.[9]

$$P\left(Y|D = \text{No assistance}, \vec{X}\right) = \vec{X}'\,\vec{\beta} \tag{4}$$

For each family i receiving assistance, this model and the preceding identification assumptions allow us to impute the probability of eviction in the absence of assistance by $\vec{X}_i'\widehat{\vec{\beta}}$. Our estimator for the population average treatment effect on the treated is the weighted average across treated units using survey weights w_i (equations 5 and 6). This weighted average involves 224 families who resided in public housing and 202 families who received other assistance, who were chosen as part of the probability sample from the target population.

$$\hat{\tau}_{\text{Public}} = \underbrace{\frac{1}{\sum_{i:D_i=\text{Public}} w_i}}_{\substack{\text{Weighted average over}\\ \text{those in public housing}}} \sum_{i:\ D_i=\text{Public}} w_i \left(\underbrace{Y_i}_{\substack{\text{Observed outcome}\\ \text{in public housing}}} - \underbrace{\vec{X}_i'\hat{\vec{\beta}}}_{\substack{\text{Estimated P(Eviction)}\\ \text{if received no assistance}}} \right) \tag{5}$$

$$\hat{\tau}_{\text{Other}} = \underbrace{\frac{1}{\sum_{i:D_i=\text{Other}} w_i}}_{\substack{\text{Weighted average over}\\ \text{those in other assistance}}} \sum_{i:\ D_i=\text{Other}} w_i \left(\underbrace{Y_i}_{\substack{\text{Observed outcome}\\ \text{in other assistance}}} - \underbrace{\vec{X}_i'\hat{\vec{\beta}}}_{\substack{\text{Estimated P(Eviction)}\\ \text{if received no assistance}}} \right) \tag{6}$$

Readers familiar with matching estimators for causal effects may see some resemblances in equation (5) and (6). Like matching estimators, these equations explicitly average over treated units indexed by i and compare the observed outcome Y_i with an estimated outcome that would be realized in the absence of treatment. Matching estimators impute the unobserved counterfactual for each unit by the observed values of one or more units with similar covariates $\vec{X}_i$. A regression imputation estimator imputes the unobserved counterfactual by the predicted value $\vec{X}_i'\widehat{\vec{\beta}}$. Both strategies share an explicit focus on directly estimating the unknown potential outcome for each unit and then averaging across an explicit set of units (all treated individuals in the sample).

[8] We use OLS instead of a logit or probit model because it is an unbiased estimator, and because in this application it is not consequential if some predicted probabilities fall outside the [0,1] interval.
[9] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Construction of Confidence Intervals

Because the Fragile Families Study is clustered in 16 U.S. cities (Reichman et al., 2001), standard procedures for statistical inference that assume simple random sampling may incorrectly capture sampling variability. Instead, we employ a jackknife variance procedure that is standard for complex survey samples. We first produce point estimates of all quantities reported in this paper, using the full sample. Then, we reestimate those quantities on 16 subsamples that exclude each city (Primary Sampling Unit) in turn. We calculate the sampling variance of each estimate based on the sum of squared differences between the subsample-specific estimates and the overall point estimate, using the *survey* package in R (Lumley, 2011, 2019). This is the same package that survey administrators used to generate the study weights (Si & Gelman, 2014). Finally, we construct confidence intervals by a normal approximation applied to the jackknife variance estimate. The Appendix provides more details and shows that the resulting intervals are slightly wider than the intervals one would arrive at by ignoring the clustered sample design.[10]

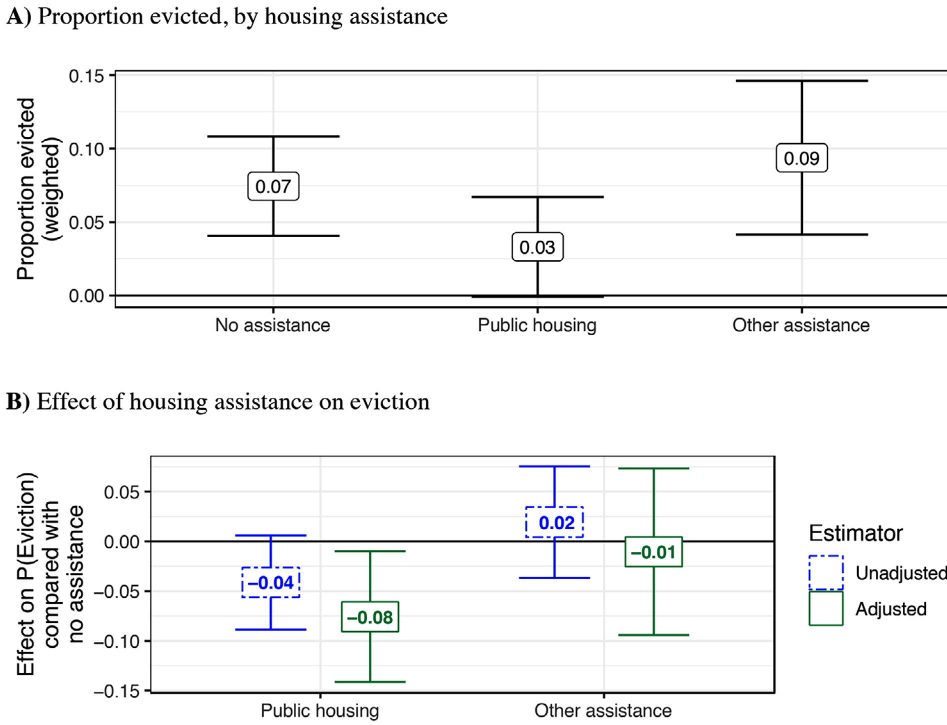
RESULTS

Figure 1 summarizes the relationship between housing assistance and eviction. Descriptively, panel A shows that the proportion of families evicted between the interviews at child ages 9 and 15 is much lower among those who reside in public housing (3 percent) than among those receiving no assistance (7 percent) or other assistance (9 percent). These unadjusted differences suggest the possibility that public housing may protect families from eviction. Panel B shows results that adjust for pre-treatment variables by the OLS imputation estimator. Adjustment increases the uncertainty around the estimates, but, if anything, makes public housing appear more protective than one would conclude from the unadjusted estimate. Public housing reduces the probability of eviction by 8 percentage points, with a 95 percent confidence interval (−0.14, −0.01). This effect is substantively large. The probability of eviction in public housing is 3 percent compared to a predicted 11 percent in the absence of public housing; in other words, public housing reduces eviction to be roughly one-third as common as it would otherwise be. Our models suggest that other assistance reduces the probability of eviction by only one percentage point, although the wide confidence interval (−0.09, 0.07) centered near zero suggests a need for future research to yield a more precise estimate of this quantity.

As a secondary analysis, we repeat all estimators as described above but focus on a different outcome: nonpayment of rent or mortgage between the interviews at child ages 9 and 15. Figure 2 summarizes findings for this related outcome. Nonpayment is much more common in all three groups than eviction, occurring for more than 20 percent of families in all treatment groups (panel A). In contrast to the results for eviction, the results for nonpayment in panel B suggest that public housing has little effect on nonpayment whereas other assistance substantially reduces the probability of nonpayment by 13 percentage points, with a 95 percent confidence interval (–0.22, −0.05). We observe missed payments for only 22 percent of these families but would have expected 35 percent to miss the rent in the absence of assistance, given their pre-treatment characteristics.

Taken together, the results suggest that public housing protects families from eviction despite doing little to help them make rent payments. Meanwhile, other assistance reduces the risk of nonpayment, but this does not translate into reduced risk

[10] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.



Notes: Panel (A) reports the estimated proportion of children in the target population to be evicted between ages 9 and 15, by treatment group. Panel (B) estimates the difference in the probability of eviction across treatment groups, both with and without adjustment for pre-treatment covariates. Error bars present 95 percent confidence intervals.

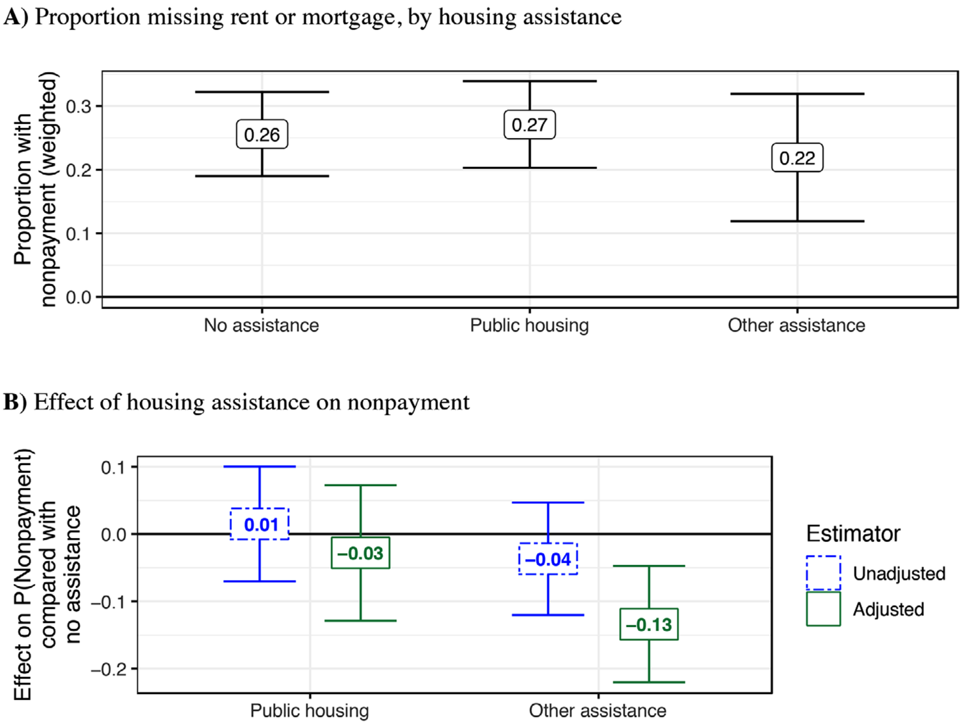
Figure 1. Effect of Housing Assistance on Eviction.
[Color figure can be viewed at wileyonlinelibrary.com]

of eviction. These results reaffirm that eviction is not a deterministic consequence of nonpayment. Public housing may protect families from eviction by weakening the link between nonpayment and eviction, perhaps through legal protections not available under other forms of assistance. The discussion returns to this possibility.

LIMITATIONS

Our claims are limited by the possibility of unobserved confounding, by limited statistical power, and by potential measurement problems. In addition, our claims apply to a period of particular interest—the aftermath of the Great Recession—but may not generalize to other periods.

The most well-known limitation of any study seeking to establish a causal effect with observational data is the possibility of unobserved confounding. However, the degree of unobserved confounding required to nullify our claims would have to be large in this case. We estimate that public housing reduces the probability of eviction from 11 to 3 percent; unobserved factors that affect selection into public housing would have to collectively play a very large protective role in order to fully account for this finding. While our estimates may be somewhat biased by unobserved confounding, we believe that a large causal effect is more plausible in this setting than a large degree of unobserved confounding. Nonetheless, we call for future research



Notes: Panel (A) reports the estimated proportion of children in the target population whose mother misses a rent or mortgage payment between ages 9 and 15, by treatment group. Panel (B) estimates the difference in the probability of nonpayment across treatment groups, both with and without adjustment for pre-treatment covariates. Error bars present 95 percent confidence intervals.

Figure 2. Effect of Housing Assistance on Nonpayment of Rent or Mortgage. [Color figure can be viewed at wileyonlinelibrary.com]

with randomized treatment assignments to provide more definitive evidence. Although observational data are limited, they offer important insights that represent a critical first step toward a more complete understanding of the effect of housing assistance programs.

Second, our estimates suffer from limited statistical power. Although we can reject the null hypothesis that public housing has no effect on eviction, we remain uncertain about the value of the population parameter capturing the degree to which public housing is protective. Because questions about eviction are not commonly included in household surveys or studies of housing assistance, future research to provide more precise estimates will require new large-scale data collection efforts.

Third, our measurement (a survey question) may understate the prevalence of both nonpayment and eviction. Previous research has demonstrated that tenants who are evicted may not report their experience as such in a survey, possibly due to disagreement about what constitutes an eviction, social desirability bias, or a desire to portray maximum control over their lives (Desmond & Gershenson, 2017; Desmond, Gershenson, & Kiviat, 2015; Desmond & Shollenberger, 2015). Future research using new data sources that ask multiple questions about a variety of forced moves (e.g., Desmond & Shollenberger, 2015) will be needed to overcome this limitation. The question wording in our sample also limits us to a focus on how housing assistance affects whether families are ever evicted; future research on the number of such events could provide important insights into effects on serial eviction. Finally, the survey question includes all evictions for nonpayment of rent or mortgage; future research is needed to assess effects that are specific to eviction from rental units.

Finally, the results from these analyses may be particular to the aftermath of the Great Recession. In the period after the Great Recession, the housing affordability crisis in the United States worsened and the gap between the supply and demand for affordable housing widened (Joint Center for Housing Studies, 2011). Even among families with housing assistance, rent burden was common during this period, with about 59 percent paying at least a third of their income towards housing and a third paying more than 50 percent (Joint Center for Housing Studies, 2011). In this environment, housing assistance may not have been enough to prevent eviction due to nonpayment of rent. It is possible that housing assistance may more effectively prevent housing hardship in other periods in which it more successfully reduces the proportion of income spent on rent.

DISCUSSION AND SUGGESTIVE THEORIES

Evidence-based policymaking demands an answer to the question of how housing assistance affects eviction. Numerous studies have assessed effects of housing assistance on more distant outcomes, such as child behavior and adult earnings, with mixed results. By focusing on a more proximate outcome (eviction), we shift attention to a domain of family well-being that is important in its own right, and for which housing assistance programs may be especially effective. We find that families residing in public housing have lower risk of eviction than similar families not receiving assistance. Parallel analyses of effects on nonpayment of rent or mortgage reveal a different pattern: Other forms of assistance reduce the probability of nonpayment of rent but the effect of public housing on this outcome is very small. This section speculates about some of the operative forces that may lie beneath these results.

Tenants May Prioritize Different Bills

First, the effect of other assistance on nonpayment is consistent with a theory in which this assistance changes low-income tenants' decisions about which bills to pay under a constrained budget. Prior research demonstrates a common strategy of paying the bill that is most consequential (Edin & Lein, 1997). Receipt of a voucher may make the rent the most important bill to pay: Failure to pay can lead to a loss of the voucher. Tenants receiving other assistance may therefore prioritize rental payments over other bills, such as utilities. This factor would be less applicable in public housing, where utilities are often included as part of the rent.[11] This theory could produce our finding that public housing does not reduce nonpayment while other forms of assistance do.

[11] This theory aligns with a finding from the MTO study: individuals in the treatment group (who moved from public housing into the private market *with a voucher*) were less likely than the control group (which, at baseline, lived in public housing) to report late rent or mortgage payments (Sanbonmatsu et al., 2011). These same voucher recipients reported *more* trouble paying utilities (Sanbonmatsu et al., 2011), although this may be because utilities are typically included in the rent in public housing but paid separately by voucher recipients. While the control group in the MTO study differs from that in the current study (public housing at baseline compared to no assistance at baseline), the results are consistent with our speculative theory.

Tenants May Benefit from Different Grounds for and Process of Eviction

Second, the effect of public housing on eviction is consistent with a theory in which public housing reduces the probability of eviction through mechanisms other than nonpayment, such as extra legal protections for tenants, which are not present in other forms of assistance. These legal protections affect the grounds for eviction and the process of eviction. To investigate this possibility, we briefly review the federal regulations on eviction from public housing and from private rentals.

Allowable grounds for eviction differ in public housing and the private market (including vouchers). In public housing, federal regulations delimit the grounds for eviction: failure to make payments, failure to fulfill household obligations (rules governing residence in the unit), being over the income limit, or other good cause such as criminal activity or alcohol abuse (Code of Federal Regulations 24, Part 247). Meanwhile, eviction from private rentals can occur for a more expansive set of reasons. For instance, a landlord can evict a tenant because they plan to take the unit off the rental market or plan to move into the unit themselves, with specific regulations varying across states. In short, the conditions that suffice to evict a tenant are considerably lower in the private market than in public housing. Because voucher recipients rent in the private market, only public housing residents benefit from the public housing regulations. The differences in justifiable grounds for eviction may be one reason public housing is protective against eviction.

The process of eviction also differs in public housing as compared with the private market. Most notably, residents of public housing have a right to an internal grievance hearing before eviction moves to formal proceedings in a court (Code of Federal Regulations 24, Part 966). The tenant also has a right to examine the trial documents before a trial in court and a right to legal counsel. While all tenants have rights in eviction court, it is possible that the formal structure of the eviction process in public housing creates stronger opportunities for mediation before eviction procedures are brought to their culmination.

Public Housing May More Effectively Reduce Cost Burden

Reduced housing cost burden is a plausible but incomplete theory for our results. One reason for eviction is the high cost of rental housing: Roughly half of all renter households spend more than 30 percent of their income on rent (Joint Center for Housing Studies, 2019, p. 4). Public housing protects against, though does not completely eliminate, housing cost burden compared to renting without housing assistance (Gold, 2020). Although vouchers are also intended to reduce housing cost burden, the combination of rent and utilities can become large relative to income even among those receiving vouchers. In 2002, 38 percent of voucher recipients paid more than 31 percent of their income for rent and utilities, for instance (McClure, 2005). Voucher holders are permitted to pay up to 40 percent of their income toward rent and utilities, which can be enough to leave tenants in a financial squeeze. Because voucher holders may have higher housing costs than families living in public housing, they may be more susceptible to nonpayment of rent and eviction when they face an unexpected financial shock in some other domain of life (e.g., healthcare costs).

An Uncompelling Theory: PHA Budgets and Rental Payments

Beyond differing regulations in public housing and the private market, one might argue that public housing authorities are more willing to keep tenants who do not pay the full amount of rent because their budgets are not as reliant on tenant payments. However, we believe this argument is not compelling because rent payments are essential to the budgets of PHAs; government funding is largely insufficient. As evidence of this insufficiency, the U.S. Department of Housing and Urban Development (2017) notes a $26 billion maintenance backlog in U.S. public housing (estimated as of 2010). Insufficient public funding means that tenant rent payments play a critical role in the balanced budget of the PHA. The preference of a housing authority is thus similar to that of a private landlord: If it is possible to remove a nonpaying tenant and replace them with a paying tenant, both public and private landlords will prefer to follow this path. For this reason, we think that direct government funding alone is not sufficient to explain our results.

IMPLICATIONS FOR POLICY

Although we focus on proximate outcomes, our result paired with prior findings suggests that housing assistance may have collateral benefits for other downstream outcomes. The consequences of eviction are far-reaching and include further material hardship (Desmond & Kimbro, 2015), residential instability (Desmond, 2012; Desmond, Gershenson, & Kiviat, 2015), and worse maternal and child health (Desmond & Kimbro, 2015). Future landlords may look unfavorably on a history of eviction, making it hard for those with an eviction to find stable housing in the future (Desmond, 2012). Protection against eviction may therefore protect families indirectly from numerous more distal harms.

The implications of these results for policymakers depend on the goal that policymakers seek to achieve. For those whose sole aim is to reduce eviction, our results point toward expansion of public housing. The need for public housing already far outstrips availability: 96 percent of all public housing authorities in 2012 had waiting lists for public housing (Public and Affordable Housing Research Corporation, 2016). If public housing were expanded to serve more families, then eviction may become less common. For policymakers who view eviction as only one of many outcomes of interest, the implications of our results should be taken in the context of research on the effects of public housing on other outcomes. Expansion of public housing may produce unwanted side effects, such as increases in income segregation or reductions in quality as public housing falls into disrepair. Those more concerned about these side effects may prefer to invest in other types of housing assistance (e.g., vouchers and LIHTC), which are currently growing. For these policymakers, our results suggest a need for future research exploring whether certain aspects of public housing (e.g., legal protections from eviction) could be incorporated into other programs such as voucher-based assistance. For example, policymakers could create a grievance procedure for voucher recipients or tenants in the private market, with guaranteed legal representation for tenants, so that landlords would need to lay out the justification for the eviction in a setting focused on mediation before filing for eviction in the court system. Such a policy might create opportunities to resolve disputes while simultaneously reducing the volume of caseloads in housing court.

This study provides one piece of evidence in a larger body of work that may help policymakers provide affordable housing. We therefore recommend that researchers continue to build the evidence base on this question and that policymakers begin to act on the evidence available today, expanding public housing or modifying other programs to incorporate some features of public housing, to better serve the needs of American families.

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APPENDIX

Details of Multiple Imputation

We multiply impute all missing values using the *Amelia* package in R (Honaker, King, & Blackwell, 2011), which assumes a multivariate normal model modified by nonlinear transformations (e.g., logit) for categorical variables. This section discusses the specification of the imputation model and the assumptions required for inference.

The imputation model includes all variables from the main regression models. Variables that are included in aggregate form in the analysis (e.g., any history of nonpayment, eviction, and conviction) are each imputed in disaggregate form (e.g., separate columns for the value at ages 1, 3, and 5) and then aggregated post-imputation. This maintains the maximal amount of information in the imputation model while reducing complexity of the primary analytic model. Some variables enter the analytic model only through the age 9 indicator: income, housing assistance, disability, drug use, and alcohol use. We nonetheless include all prior-wave reports of these variables because these lagged values may help to predict missing values at age 9 and help to satisfy the missing at random assumption. The only age 15 variables included in the imputation model are eviction and nonpayment of rent or mortgage (included as a potentially strong predictor of eviction).

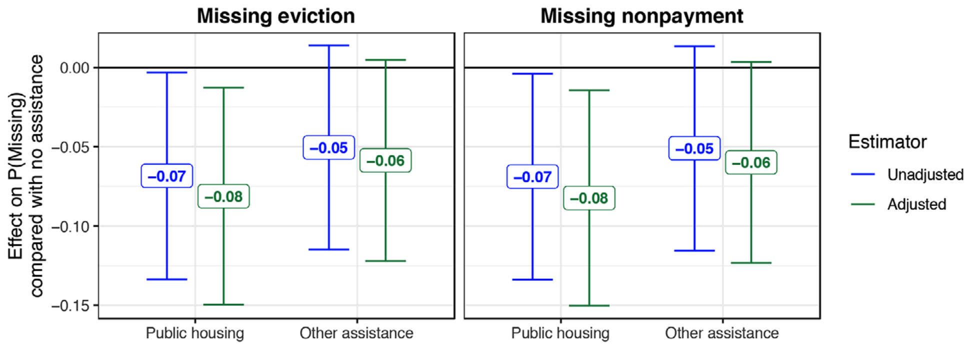
Multiple imputation addresses the issue that those who are missing on the outcome may tend to have covariates for which the outcome is likely to be high or low. The validity of this procedure does not require that imputed and observed values be similar; they may differ because covariates of imputed and observed cases differ. Nonetheless, it may be reassuring to know how imputed and observed values differ. Table A1 presents this information. The mean value of each outcome (eviction and nonpayment) is very similar for the imputed and observed cases within each treatment group. The largest difference in the table—imputed values of nonpayment among those receiving other assistance are 12 percentage points higher than observed values—would push in the opposite direction of our substantive result (that other assistance protects families from nonpayment). In short, our finding is not driven by a quirk of the imputed values.

Inference in the presence of missing data requires that whether an observation is missing is independent of the true value of that missing case, given the values of all variables observed for that case. This assumption is unlikely to hold perfectly. For instance, experiencing eviction may reduce the probability that a respondent will answer a question about eviction. Nonetheless, the assumption is likely to be more plausible conditional on the values of all other variables. For this reason, estimates

Table A1. Means of imputed and observed values of each outcome between ages 9 and 15, by treatment group at age 9.

Outcome	Treatment	Outcome mean if imputed	Outcome mean if observed
Eviction	Public housing	0.04	0.03
	Other assistance	0.09	0.09
	No assistance	0.11	0.07
Nonpayment	Public housing	0.19	0.28
	Other assistance	0.33	0.21
	No assistance	0.31	0.25

Note: All estimates are weighted.



Notes: Public housing and other assistance reduce the probability of subsequent missing reports of eviction and of nonpayment of rent or mortgage. If missingness is positively related to eviction in ways not captured by covariates, then it is likely that the probability of eviction is most understated for the group receiving no assistance at age 9. Thus, our estimates may understate the protective effect of government assistance on eviction.

Figure A1. Effect of Housing Assistance on Missing Survey Responses.

based on multiple imputation are preferable to those we could make by simply dropping all cases with missing values. Further, we note that missingness is rare, affecting less than 16 percent of observations for any given variable in the analytic sample.

Missingness in the outcome is perhaps of greatest consequence to the validity of the study. If eviction (the outcome) causes attrition from the study, then our estimates may understate eviction prevalence in a way that cannot be addressed by multiple imputation. We would especially worry about this problem if public housing appeared to *reduce* the probability of eviction and *increase* the probability of missingness. In this case, one could argue that we have simply missed many of the evictions because the families are hard to find. However, the opposite is true: We provide evidence here that public housing reduces *both* the probability of eviction *and* the probability of missingness. The simplest evidence for this point is marginal: Missing eviction reports are most common among those receiving no assistance at age 9 (15 percent missing, weighted) compared with those residing in public housing (8 percent missing, weighted) or receiving other assistance (10 percent missing, weighted). Second, we repeat the procedure from the main text but replace the outcome with an indicator for missing eviction and nonpayment reports. Both public housing and other assistance reduce the probability that each outcome variable is missing (Figure A1). For this reason, it seems unlikely that the protective effect of public housing on eviction is driven by missing data. Rather, we are likely to understate the probability of eviction the most for those receiving no assistance, who are missing at the highest rate. This bias would lead us to understate the degree to which government programs are protective.

Population Inference

To draw inference about a population-average causal effect, we assume that inclusion in the sample S is ignorable. This allows us to use our sample to draw inference about a population: the average effect for all children born in large U.S. cities who meet our inclusion criteria and receive a given type of assistance at age 9. To estimate the probability of eviction among those residing in public housing or receiving other assistance, the ignorability assumption involves no covariates and requires that eviction is independent of sample inclusion given survey weights W (equations A.1 and

A.2). To estimate the probability of eviction within subgroups of $\vec{X}$ among those receiving no assistance requires that eviction is independent of sample inclusion within these subgroups (equation A.3).

$$Y \perp S \mid W, D = \text{Public housing} \tag{A.1}$$

$$Y \perp S \mid W, D = \text{Other assistance} \tag{A.2}$$

$$Y \perp S \mid \vec{X}, D = \text{No assistance} \tag{A.3}$$

The credibility of this assumption stems from the fact that the Fragile Families Study sought to draw a probability sample, conducted repeated attempts to reach families to minimize nonresponse, and constructed weights to adjust for nonresponse bias to the extent possible. Each of these assumptions is imperfect. Although the Fragile Families Study sought to draw a probability sample, it (like all sample surveys) has some non-coverage problems due to certain hospitals refusing to participate or adding particular rules about which parents could be interviewed (Reichman et al., 2001). Although the study conducted repeated contacts to minimize attrition, some families are lost to follow-up. The present paper relies on the study weights to address these problems, which combine the benefits of a probability sample with poststratification adjustments that seek to correct unavoidable problems of data collection in the best manner possible (Si & Gelman, 2014).

CONSTRUCTION OF CONFIDENCE INTERVALS

Standard procedures to construct confidence intervals often assume a simple random sample. The Fragile Families Study is a cluster sample: Cities were sampled and then individuals within those cities were sampled. A cluster sample reduces data collection costs, in this case because one team of interviewers could interview many respondents in a single city. However, cluster sampling also increases the variance of the resulting estimates by creating dependence between the sample inclusion indicators for a pair of individuals within a cluster. A variance estimator for this sample should optimally account for the clustered nature of data collection within 16 cities, each of which is a Primary Sampling Unit (PSU).

An added complication is that the Fragile Families Study selected cities by a stratified sample of all cities with populations over 200,000. The gold standard approach would estimate the variance of the estimate across PSUs within each stratum and then aggregate over strata. This gold standard is not possible in the Fragile Families Study. The Fragile Families Study stratified U.S. cities by a series of labor market characteristics and then sampled one PSU in each of eight strata and eight PSUs from the remaining stratum of all other cities (Reichman et al., 2001). This design creates problems for variance estimation: For strata that contain only one sampled PSU, it is impossible to estimate the within-stratum between-PSU variance. In this project, we pool across strata and treat the study as though cities were selected by an unstratified sample from all U.S. cities with populations over 200,000.[12] We expect that treating the design as though it were unstratified will yield confidence intervals

[12] The Fragile Families Study replicate weights make a different decision to deal with this problem: They split cities that are the lone PSU within their stratum into several units denoted by the NATPSU variable. If we used this variable, we would estimate the within-stratum between-city variance in these strata by the within-city between-group variance for some undisclosed sub-city unit selected by survey

that overcover (are too wide), since stratified designs (the actual design of the study) generally have lower sampling variability than unstratified designs (the design our variance estimator assumes).

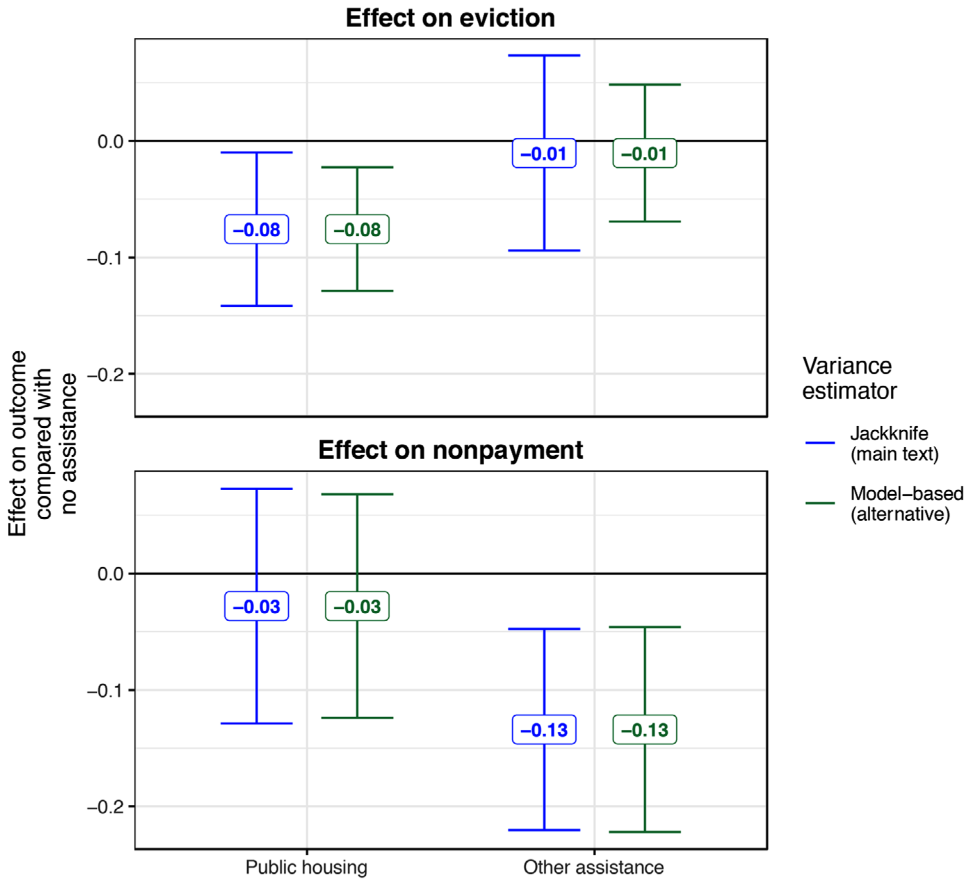
Using the *survey* package in R (Lumley, 2011), we generate a set of 16 jackknife replicate weights that each omit one sample city. We then conduct the entire estimation procedure on each of the 16 subsamples: multiply impute missing values, fit the OLS model, and aggregate to a point estimate of the quantity of interest (e.g., the causal effect of public housing on eviction, or the mean of some pre-treatment variable). For every target quantity θ, this results in one point estimate $\hat{\theta}$ that uses all the data and 16 jackknife estimates $\hat{\theta}_{(-c)}$, each of which comes from a subsample with one city omitted. The intuition of this estimator is to capture how the estimate might change if a different set of cities were randomly sampled for inclusion in the study. Indexing the subsample estimates by the omitted city c, the jackknife variance estimator is given below.

$$\hat{\mathrm{V}}\left(\hat{\theta}\right) = \frac{16-1}{16} \underbrace{\sum\nolimits_{c=1}^{16} \left(\underbrace{\hat{\theta}_{(-c)}}_{\substack{\text{Estimate}\\ \text{with city c}\\ \text{omitted}}} - \underbrace{\hat{\theta}}_{\substack{\text{Overall}\\ \text{point}\\ \text{estimate}}} \right)^2}_{\text{Sum over cities}} \quad \text{(A.4)}$$

We implement this variance estimator using the *survey* package in R (Lumley, 2011, Ch. 2; Lumley, 2019). The advantage of the jackknife approach, as compared with an analytical formula for a linearized variance, is that it is entirely computational: Our procedure to produce the estimate $\hat{\theta}$ can involve regression, imputation, and aggregation without requiring new variance formulas. We then construct confidence intervals by a normal approximation for the sampling distribution of $\hat{\theta}$ with the estimated variance $\hat{\mathrm{V}}(\hat{\theta})$.

Readers less familiar with the analysis of multi-stage survey samples may wonder how our variance estimator compares to a more standard approach that proceeds under the imperfect assumption of a simple random sample without clustering. In the setting of a simple random sample, one might construct a confidence interval based on the variance-covariance matrix of the regression coefficients $\hat{\beta}$, translate this to an estimate of the variance for the predicted outcome under no treatment, add the variance of the mean outcome under control, and then pool the resulting variances across 30 multiply-imputed datasets by Rubin's rules. Figure A2 plots the resulting model-based confidence intervals as compared with the jackknife confidence intervals from the main text. The model-based intervals are slightly narrower than the jackknife intervals. This is as one might expect, since a clustered sample

administrators. This might be the correct strategy for outcomes, such as birth weight, which might vary within cities across some sub-units (e.g., hospitals) in ways similar to their variation across cities. In our setting, however, we expect eviction to vary across cities in ways not captured by any measure of within-city variation. As a result, we choose a variance estimator that is designed to capture the between-city variance correctly. As noted in the text, we believe ignoring the stratified design is likely to make our confidence intervals wider than necessary, which is the direction in which we prefer to err.



Notes: Confidence intervals are slightly wider when constructed by the jackknife procedure of the main text compared with a model-based procedure that would be the standard choice if the data were a simple random sample.

Figure A2. Comparison of Variance Estimators.

is less efficient than a simple random sample and the jackknife accounts for the clustered nature of the sample.

COEFFICIENTS OF OLS REGRESSION

Because our aim is to estimate the effect of housing assistance on eviction, the OLS regression coefficients on pre-treatment variables are ancillary parameters with no clear interpretation. They capture the association between pre-treatment variables and the outcome, among those receiving no assistance at age 9. We therefore omit these estimates from the main text. Table A2 presents these estimates.

Table A2. OLS coefficients for models of eviction and of nonpayment given pre-treatment variables, among those receiving no assistance ($N = 876$).

	Eviction	Nonpayment
Evicted		
In past 12 months (age 9)	0.23*	0.12*
	−0.07	−0.06
Ever at age 1, 3, or 5	0.07	0.21*
	−0.06	−0.07
Nonpayment of rent or mortgage		
In past 12 months	0.06*	0.21*
	−0.02	−0.05
Ever at age 1, 3, or 5	0.03	0.11*
	−0.02	−0.04
Income / poverty threshold		
In past 12 months	0.02	0.04
	−0.02	−0.02
Average at ages 1, 3, and 5	−0.04*	−0.07*
	−0.01	−0.03
Disability	0.01	−0.05
	−0.03	−0.04
Conviction	0.08	−0.01
	−0.04	−0.07
Drug use	0.05	0.03
	−0.03	−0.08
Heavy alcohol use	−0.06	0.01
	−0.03	−0.06
Education (mother, less than high school omitted)		
High school	−0.01	0.02
	−0.02	−0.03
Some college	−0.05*	0.02
	−0.02	−0.04
College	0.06	0.13
	−0.09	−0.16
Parents married at birth	0.01	−0.05
	−0.02	−0.04
Race (mother, black omitted)		
Hispanic	−0.06	−0.13*
	−0.03	−0.06
White/other	0.00	0.04
	−0.02	−0.07
WAIS-R cognitive score	0.01	0.01*
	−0.01	−0.01
Impulsivity (Dickman 1990)	0.00	−0.04*
	−0.02	−0.02
Intercept	0.07	0.35*
	−0.06	−0.11

Notes: Coefficients on MSA indicators are omitted from the table for space.
*$p < .05$; **$p < .01$; ***$p < .001$.

Table A3. Crosstab of housing assistance at age 9 versus age 15.

		Housing assistance at age 9		
		Public housing	Other assistance	No assistance
Housing assistance at age 15	Public housing	0.31	0.09	0.08
	Other assistance	0.19	0.37	0.05
	No assistance	0.37	0.43	0.69
	Missing	0.13	0.11	0.18

Notes: Table shows unweighted column proportions in the analytic sample. Table illustrates that housing assistance at age 15 is often different from housing assistance at age 9.