Evicting Children

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This study identifies children as a risk factor for eviction. An analysis of aggregate data shows that neighborhoods with a high percentage of children experience increased evictions. An analysis of individual data based on an original survey shows that among tenants who appear in eviction court, those with children are significantly more likely to receive an eviction judgment. These findings indicate that policymakers interested in monitoring and reducing discrimination should focus not only on the front end of the housing process—the freedom to *obtain* housing anywhere—but also on the back end: the freedom to *maintain* housing anywhere.

Every year in the United States a vast number of families are evicted from their homes. Analysts have estimated the number to be in the several millions (Hartman and Robinson 2003). In Milwaukee, the setting of this research, roughly 16,000 adults and children are evicted through the court system in an average year. In Milwaukee's predominantly black inner city, one renter-occupied household in 14 is evicted annually (Desmond 2012). For the inner-city poor, it seems, eviction has become commonplace.

Because landlords often turn away applicants with recent evictions on their records, evicted families regularly experience prolonged homelessness and increased residential mobility (Burt 2001; Crane and Warnes 2000). When they do locate subsequent housing, they frequently must accept substandard conditions in disadvantaged neighborhoods. Eviction often prevents families from qualifying for housing programs, past evictions and unpaid rental debt being counted as strikes against those who have applied for assistance. Studies have linked eviction to psychological trauma (Fullilove 2005) and have identified it as a risk factor for suicide (Serby et al. 2006). Recent research has found that mothers who were evicted in the previous year experienced more

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material hardship and were more likely to suffer from depression, compared with those who avoided eviction (Desmond and Kimbro 2013).

Although much work has been dedicated to documenting systematic disparities when it comes to buying or renting housing, analysts virtually have ignored differences in eviction. Our efforts to monitor and reduce housing discrimination have been almost entirely concentrated on *getting in*; we have overlooked, meanwhile, the process of *getting (put) out*. Landlords can evict a tenant for a number of reasons, and they can show great discretion in initiating the eviction process. One early study, for example, demonstrated that eviction rates fluctuated considerably in a public housing complex when eviction power was transferred from a board of city officials to one comprising citizen volunteers, implying that the causes of eviction are located not only in the behavior of the evicted but also in the decisions of the evictor (Lempert and Ikeda 1970). If this is the case, then how do landlords decide whom to evict and whom to spare? Are they more likely to target certain groups for eviction?

Drawing on two unique data sources, this study advances a pair of complementary findings. First, we analyze court-ordered eviction records to demonstrate that neighborhoods with larger percentages of children experience higher evictions. All else equal, a 1 percent increase in the percentage of children is predicted to increase a neighborhood's evictions by 6.5 percent. We then turn to a survey of tenants in eviction court, finding that the presence of children in a household significantly increases the odds of receiving an eviction judgment. On average, the probability of a household with children to receive an eviction judgment is about .17 higher than that of a household without children. The effect of children on the likelihood of receiving an eviction judgment remains after controlling for the amount of rent tenants owed, race and single-mother households as well as after accounting for selection bias with respect to the likelihood of having children.

Housing Discrimination against Families with Children

"At present," she wrote, "I am living in an unheated attic room with a one-year-old baby. Both of us have severe colds, and everywhere I go the landlords don't want children. I also have a ten-year-old boy who is at present staying with my mother. She really has no room, but I can't keep him with me because the landlady objects to children. Is there any way that you can help me to get an unfurnished room, apartment, or even an old barn—any place where I can get my family together again and be fairly comfortable? I can't go on living like this because I am on the verge of doing something desperate."

This statement was made by one of over 5,000 people who applied for public housing in Detroit between 1946 and 1948. It was recorded in a report prepared by the Detroit Housing Commission (1948) displaying "written testimony of 45 typical housing victims." Another applicant living in a basement wrote, "My children are now sick and losing weight. . . . I have tried, begged, and pleaded for a place but [it's] always 'too late' or 'sorry, no children.'" Yet another wrote: "I am a widow woman, have three children. . . . The lady where I am rooming put two of my children out about three weeks ago and don't want me to let them come back. . . . If I could get a garage I would take it." As Sugrue has observed in *The Origins of the Urban Crisis* (2005, 53), "In the tight postwar housing market, landlords took advantage of their power to screen out any tenants who might be risky. Blacks, especially those with large families, suffered the greatest hardships. Landlords regularly turned away prospective tenants with children, and the birth of a child was often cause for an eviction."

Practices restricting families' access to rental housing continued well into the postwar years. When Congress passed the Fair Housing Act in 1968, it did not consider families with children to be a protected class. Although a handful of states passed laws prohibiting discrimination against children, these laws largely were toothless and weakly enforced. For the most part, landlords could openly turn away or evict with impunity families with children. Often, they placed costly restrictions on large families, including by charging "children damage deposits" in addition to standard rental fees. One report from 1985 told of a Washington development that required tenants with no children to put down a $150 security deposit but charged families with children a $450 deposit plus a monthly surcharge of $50 per child (Buchanan 1985).

When in 1980 HUD commissioned a nationwide study to assess the magnitude of the problem, researchers found that "only one quarter of rental housing units [were] available to families with children with no restrictions" (Colten and Marans 1982, 49). Landlords were more likely to bar families from smaller, more affordable units. After prolonged and intense debate—landlords had much to lose by relinquishing their right to deny children housing, including high premiums commanded for adult-only units (*Harvard Law Review* 1981)—Congress passed the Fair Housing Amendments Act (FHAA) in 1988, prohibiting housing discrimination against children and families. In the 2 years that followed, fully "half of all HUD complaints alleged family discrimination" (Allen 1995, 303).

If HUD complaints are any indication, then housing discrimination against children and families remains widespread today. In 2009, 20 percent of all HUD complaints alleged discrimination based on family status (HUD 2010). Local audit studies also have documented evidence of housing discrimination against families with children. An audit study based in the Mississippi Gulf Coast found that "families with children will face discrimination in 7 out of 10 housing searches" (Gulf Coast Fair Housing Center 2004, 10). Another conducted in Sonoma County, California found that families with children who spoke to landlords over the phone encountered differential treatment in 42 percent of cases. The study also found that, by and large, notifying landlords about unfair treatment did not cause them to stop discriminating against families (Fair Housing of Marin 2002).

Unlike discrimination based on race or gender, discrimination against families and children often is not even recognized *as* discrimination. A report based on a nationwide sample of Americans found that the majority of respondents recognized discrimination based on race, religion and ability to be illegal, but only 38 percent were "aware that it is illegal to treat households with children differently from households without children" (Abravanel and Cunningham

2002, 10). Unlike other forms of discrimination, then, differential treatment of families with children often occurs out in the open. "To the extent that there are any modern cases [of blatant housing discrimination]," one legal scholar has observed, "they tend to involve discriminatory provisions targeting children or families with children" (Oliveri 2008, 5).

Children can cause landlords problems. Throughout the inner city, children are crowded into rooms, frequently sleeping two or three to a mattress. Fearing street violence, many parents or guardians in disadvantaged neighborhoods require children to stay indoors most hours (Kimbro and Schachter 2011). Overcrowded and cooped up children are hard on apartments. They not only are a recurrent source of noise, but they also deface property. Tenants' children may also result in landlords coming under increased state scrutiny. Young children can test positive for lead poisoning, which in turn can lead to an abatement order from the Environmental Protection Agency (with a price tag in the thousands). Child Protective Services, too, can take an interest in a child's health, which in turn can lead a caseworker to inspect a unit for unsanitary or dangerous code violations (Roberts 2001). And teenagers, especially young black and Hispanic boys, can attract the attention of the police (Rios 2011), who in recent years increasingly have enforced local ordinances that sanction property owners for their tenants' disorderly or criminal behaviors (Desmond and Valdez 2013). For all these reasons, far from acting as a mitigating factor in the eviction decision, children may act, as they did throughout the 20th century, as an aggravating one.

Data and Methods

This article relies on two unique data sources. The first combines court records of all evictions that took place in Milwaukee County in 2010 with data from the 2010 U.S. Census and the American Community Survey (2006-2010). The second is the result of an in-person survey of 251 tenants in Milwaukee's eviction court.

Court Records

We extracted the complete records of court-ordered evictions that took place in Milwaukee County between January 1, 2010, and December 31, 2010. In Milwaukee, a landlord may evict tenants for falling behind in rent, for committing a number of other violations (e.g., property damage, drug distribution), or (should their lease be a month-to-month contract) for no reason whatsoever. These records include all such evictions and therefore constitute the complete universe of eviction cases that took place in the city.

Addresses of evicted households were geocoded, aggregated to the block group level, and merged with data from the U.S. Census (2010) as well as the American Community Survey (2006-2010). Over 99 percent of eviction cases were placed into block groups within Milwaukee County and merged with demographic data; the remaining 1 percent were dropped. This resulted in a sample size of 6,034 eviction cases with complete geographic information and data on neighborhood social and economic characteristics. For the purpose of these analyses, a "neighborhood" was defined as a block group. In 2010, Milwaukee County was divided into 857 populated block groups, each with an average population of 1,106 people and 218 renting households.

The primary strength of analyzing court records lies in the accuracy of the information. Ecological-level data on sensitive topics, such as eviction, often are much more precise than estimates gleaned from individual-level surveys (Schwartz 1994). And because tenants tend to have strict (and often misguided) conceptions of eviction, many who in fact were evicted do not believe as much (Desmond 2012). By analyzing court records, we were able to generate an exact measure of the incidence of court-ordered eviction by neighborhood. Similarly, Census 2010 data provide a reasonably accurate count (rather than a sample) of potential confounding variables. Of course, these data are limited by their lack of individual-level information. They allow for an aerial view of the geography of evictions across Milwaukee, but they do not allow us to zoom in and inspect the characteristics of evicted households. Our claims pertaining to these data, then, are relegated to comparisons between neighborhoods and demonstrate how neighborhood-level demographic characteristics relate to neighborhood eviction counts.

Court Survey

We supplemented our analysis of administrative records with one drawing on the *Milwaukee Eviction Court Study*, an in-person survey of tenants appearing in eviction court every weekday between January 17 and February 26, 2011 (with the exception of January 31). During this six-week period, 1,328 eviction cases took place. In 378 cases, tenants appeared in court; of those, 251 were interviewed, resulting in a response rate of 66.4 percent. Of the 127 households appearing in eviction court that were not interviewed, only 21 refused to participate. Most of the remaining 106 were taken to auxiliary rooms and did not return to the main courtroom, the location of the study. Tenants were interviewed after their case was heard.

Tenants were asked about their current residence (e.g., rent, number of bedrooms), the outcome of their hearing and demographic information. Additionally, the survey collected a roster of all adults and children in each household. Whenever possible, interviewers scanned with a portable scanner each tenant's Eviction Summons and Complaint—a court-issued document listing the charges against them—or copied its contents directly onto the questionnaire ($N = 105$). Doing so provided high-quality data about the reasons for eviction and the amount of back rent owed, this information being listed clearly on the document. If tenants did not have their Summons and Complaint or if they preferred interviewers not look at it, then they were asked to provide the reasons they were called to eviction court ($N = 146$).

Eviction hearings typically result in four outcomes. Tenants may receive an eviction judgment and be ordered to vacate the premises by an allotted date; their case may be dismissed; they may be asked to return to court another day;

or they may settle their case with a stipulation agreement in which tenants agree to vacate the premises or pay their debt by a certain date. If tenants satisfy the stipulation agreement, their eviction is dismissed; if they default, landlords may obtain a judgment of eviction and a writ of restitution without having to take tenants to court again.

The *Milwaukee Eviction Court Study* provides data on what separates the evicted from the near evicted. One limitation of in-court studies, however, is that tenants who do not appear in court are not interviewed. To investigate if tenants who appeared in court were different from those who did not, we conducted a supplemental analysis based on case- and neighborhood-level information from the 1,328 eviction cases that took place over the course of the study. We constructed a mixed-effects logistic regression model explaining appearance in court. At the household level, we accounted for arrearage, women-only leaseholders and distance to the courthouse; at the neighborhood level, for crime rate and percentage of black residents, of rental housing units, of poor families, of female-headed households and of children in the neighborhood. No variable was a statistically significant (results available upon request). This finding complemented results of previous studies (Bezdek 1992; Larson 2006) and reaffirmed observations that the decision to appear in eviction court is not easily explained by the strength of one's case, the composition of one's household, or the characteristics of one's neighborhood (Desmond 2012).

A Two-Pronged Analysis

Our analysis proceeds in two steps. We begin by applying a Poisson regression model to data aggregated to the block group level. To correct probability distributions, we exposed the model by the number of renting households in a neighborhood. (In the average Milwaukee neighborhood, 48% of housing units are renter-occupied.) Vuong tests suggested zero-inflation, as 188 block groups (22%) hosted no evictions in 2010. The zero-inflated Poisson regression (ZIP) combines a pair of models for two distinct populations. While the first model predicts the likelihood of an eviction not occurring in a neighborhood, the second predicts the count of expected evictions, the count being a non-negative integer. In other words, the first model is a logit model for "certain zero" cases: block groups with no evictions. The second is a Poisson model for "uncertain zero" cases (Martin et al. 2005). Our ZIP model, then, can be represented as follows:

$$
\begin{aligned}
\Pr(Y_i = 0 \mid x, z) &= 1 - p(x_i)\exp(-\lambda(z_i)), \\
\Pr(Y_i = r \mid x, z) &= \frac{\exp(-\lambda(z_i))\lambda(z_i)^r}{r!}, \qquad r = 1, 2, \ldots,
\end{aligned} \tag{1}
$$

where

$$
\text{logit}(p(x_i)) = \alpha_0 + \beta_0(x_i)
$$

$$\log(\lambda(z_i)) = \alpha_1 + \beta_1(z_i).$$

Here, $p(x_i)$ is the probability of observing at least one eviction in neighborhood i, and $\lambda(z_i)$ is the number of renting households in neighborhood i. In the first equation, $\lambda(z_i)$ is a function of the explanatory variables, x (covariates used to predict eviction counts); in the second, $\lambda(z_i)$ is a function of the explanatory variables, z (covariates used to inflate the model). The constants are represented by α_0 and α_1, while the vectors representing the estimated coefficients for each explanatory variable are β_0 and β_1.

In the ZIP model, our dependent variable is the number of evictions that occurred in a neighborhood in 2010, constrained by possible exposure to the risk of eviction by the number of renting households and predicted by several neighborhood-level covariates (x).[1] To determine if there is a relationship between the percentage of children in a neighborhood and its eviction count, we account for the percentage of neighborhood residents who are younger than 18 years of age.[2] To assess the socioeconomic status of neighborhoods, we introduce the percent of the population in the neighborhood living below the poverty line (as measured by the American Community Survey 5-year estimates, 2006-10). Because there is significant evidence of racial discrimination against African Americans (Pager and Shepherd 2008), we account for the percentage of residents in a neighborhood who are black. Because female-headed households with children may struggle to make rent (Edin and Lein 1997), we examine the percent of households headed by single females. Additionally, we control for the percentage of neighborhood residents who are women.

Last, we consider the percentage of vacant housing units in a block group as an indicator of neighborhood distress. Covariates used to inflate the model (z) included the percentage of rental housing and the percentage of people residing in group quarters (e.g., college dormitories, prisons, nursing homes), as those who own and who live in group quarters are not evicted through the court system. We also inflated the model by the percentage of residents who are black, owing to the concentration of eviction in predominately black areas, as well as the percentage of the population with a 4-year college degree, owing to the lack of evictions in more exclusive neighborhoods. Each of these predictors of excess zeros proved to be statistically significant. Table 1 displays the descriptive statistics of the aggregate data.

The second step of our analysis draws on data from the *Milwaukee Eviction Court Study*. Because we were interested in understanding why some tenants who appeared in court were evicted while others were not, the probability of receiving an eviction judgment was predicted by a logistic regression model:

$$\log[p_i / (1 - p_i)] = \alpha_0 + \beta_1 x_i + \varepsilon_i, \quad (2)$$

where β_1 represents the estimated coefficients for explanatory variable x, with constant α_0 and the random error ε_i.

We account for the presence of children in the household while controlling for a number of important factors. Because we expected tenants who owed more to

Table 1. Summary Statistics for Aggregate Data

Variable	Mean	SD	Min	Max
Eviction cases	7	10.3	0	86
Renting households	217.9	171.4	3	1,138
% Children	25.4	9	0	50.3
% Black	29.8	35.3	0	98.5
% Female	51.8	3.9	8.8	69.5
% Female-headed households	20	14	.4	74.4
% In poverty	20.1	18.5	0	91.1
% Vacant housing units	8.6	5.7	0	34.7
% Living in group quarters	1.8	7.5	0	98.8
% College educated	.3	.2	0	.9

Note: N = 857 block groups. SD = standard deviation.

have a higher likelihood of eviction, we accounted for months of arrearage: the total amount owed divided by the monthly rent payment. The average tenant in eviction court was 2 months behind. The median amount of back rent owed was $900. We also controlled for the total household income (per month). Most respondents were poor—the average monthly household income was $1,410—and 94 percent received no housing assistance. Meanwhile, the average tenant paid $590 a month in rent. A full third of respondents devoted at least 80 percent of their household income to rent. Given this, it is unsurprising that 232 (92%) respondents were summoned to eviction court for falling behind. The remaining 19 were accused of violating the lease in some other way (e.g., property damage).

Additionally, we accounted for the number of leaseholders per household, adults in the household of whom the landlord is aware. (Controlling for all adults in the household, those on an off the lease, did not affect our main findings.) We controlled for lessees' gender and racial identity. Gender was represented with a categorical variable with three values: only women lessee(s), only male lessee(s) and both women and men lessees (reference category). The majority of children in our sample (58%) lived in households with one adult, usually their mother. Single mothers not only rank among the poorest of all demographic groups in the United States, but their incomes often are quite fixed, making them especially prone to eviction after incurring unanticipated expenses (e.g., medical costs, funeral bills). It was important, then, to control for household composition.

We also included a binary variable if at least one lessee was African American. Because some evidence suggests that younger and older tenants may be at heightened risk of eviction (Hartman and Robinson 2003), we account for the lessees' age. In the case of multiple lessees, we take the average age. The age of respondents varied widely—the youngest was 19; the oldest was 69—indicating that eviction affects poor people at multiple points along the life course. Table 2 displays the descriptive statistics from the in-court survey.

Table 2. Summary Statistics for Individual Data

Variable	Mean	SD	Min	Max
Months of arrearage	2	1.8	0	14.6
Children in household	.6	.5	0	1
Number of leaseholders	1.2	.5	1	4
African American leaseholder(s)	.8	.4	0	1
Women-only leaseholder(s)	.6	.5	0	1
Men-only leaseholder(s)	.2	.4	0	1
Age of leaseholder(s)	34	10	19	69
Monthly household income ($1,000s)	.1	.2	0	2.5

Note: N = 251. SD = standard deviation.

To further address possible bias introduced by treatment selection and to produce a more accurate estimate of the effects of children on the likelihood of a household being evicted, we conducted a propensity score regression analysis.[3] If selection is contingent on observed covariates, then after conditioning on the propensity scores households with and without children should have the same distribution of covariates and overt bias in the estimates of treatment effects should be eliminated (Rosenbaum and Rubin 1983). Paired households with similar propensity scores in cross treatment conditions serve as counterfactuals for each other; as such, the difference between their observed outcomes can be interpreted as an estimate of the treatment effects conditional on the propensity scores. By integrating over propensity scores, then, we can obtain a consistent estimate of the average treatment effects.

In addition to propensity score regression, we employ a complementary technique that directly matches households across treatment groups based on their covariates. This strategy does not rely on propensity scores and grants us more control over the matching process. The propensity scores are the predicted probabilities derived from a logit model for which the dependent variable is whether a household has children or not (the treatment). To specify this model, we included all the covariates used to explain eviction judgment, along with two additional covariates—the squared term of the average age of leaseholder(s) and amount of rent owed. More information about our propensity score analysis can be found in the appendix.

Last, we conduct a sensitivity analysis, one that does not assume that selection is based on observable covariates. Following Rosenbaum (2010), we evaluate possible bias in our estimates because of selection on unobserved factors, estimating how much hidden bias would be necessary to render insignificant our estimates of the treatment effect.

Evictions in Children Zones

The 6,034 eviction cases that took place in Milwaukee in 2010 involved 7,372 adult leaseholders. In other words, approximately 20 people a day were evicted in the city. This number does not include adults not listed within the eviction records or those evicted through informal agreements, lockouts or other techniques of persuasion that occurred beyond the purview of the court. Nor does it include most likely thousands of children who belong to evicted households.

As Figure 1 shows, most evictions occurred in Milwaukee's predominately black inner city. Of the 6,034 households evicted in 2010, 4,364 (72%) were located in neighborhoods in which at least a third of the population was black. Given the extreme disadvantage and poverty of many inner-city neighborhoods (Sampson 2012; Wilson 2009), the concentration of evictions in Milwaukee's central city is not surprising. What is, however, is the evident variation in evictions *within* the inner city. The inset map of Figure 1 focuses on the "core" of Milwaukee's ghetto: inner-city neighborhoods with more than 80 percent black residents. The shading of the inset represents not the percentage of black residents in a neighborhood, as in the larger map, but the percentage of children in a neighborhood. Increasing the magnification to inspect only the core of the inner city reveals that the number of evictions varied considerably from one block group to the next and that the variation appears to correspond to the percentage of children in a neighborhood: those with a higher percentage of children hosted more evictions. Citywide, in neighborhoods where children accounted for at least 25 percent of the population, 1 in every 18 renting households was evicted; in those where children made up at least 35 percent of the population, 1 in every 14 households was evicted; and in those where children made up at least 40 percent of the population, 1 in every 12 households was evicted. Only 1 in 123 renting households was evicted in neighborhoods where children made up less than 10 percent of the population.

Table 3 displays the findings of our ZIP model through incident rate ratios. The basic model reports a positive and significant association between the percentage of children in a neighborhood and the number of evictions that took place in the neighborhood. The association between a neighborhood's percentage of children and its eviction rate remains strong and significant even after controlling for a number of important factors. It is not explained away after introducing measures of socioeconomic status or neighborhood distress, for example, poverty, vacant housing units. After accounting for the proportion of children and African Americans in a neighborhood, the percentage of residents living in poverty has little influence on evictions. Female-headed households, meanwhile, are associated with a slight reduction in eviction cases. In other words, the relationship between the proportion of children in a neighborhood and its number of evictions is not a "race effect" or a "single mother effect." The final model demonstrates that the number of households evicted in a neighborhood is primarily a function of its percentage of African Americans and children. In fact, the coefficient for the percentage of children in a neighborhood is larger than that for any other variable. All else equal, a 1 percent increase in the percentage of children is associated with a 6.5 percent increase in a neighborhood's eviction cases.

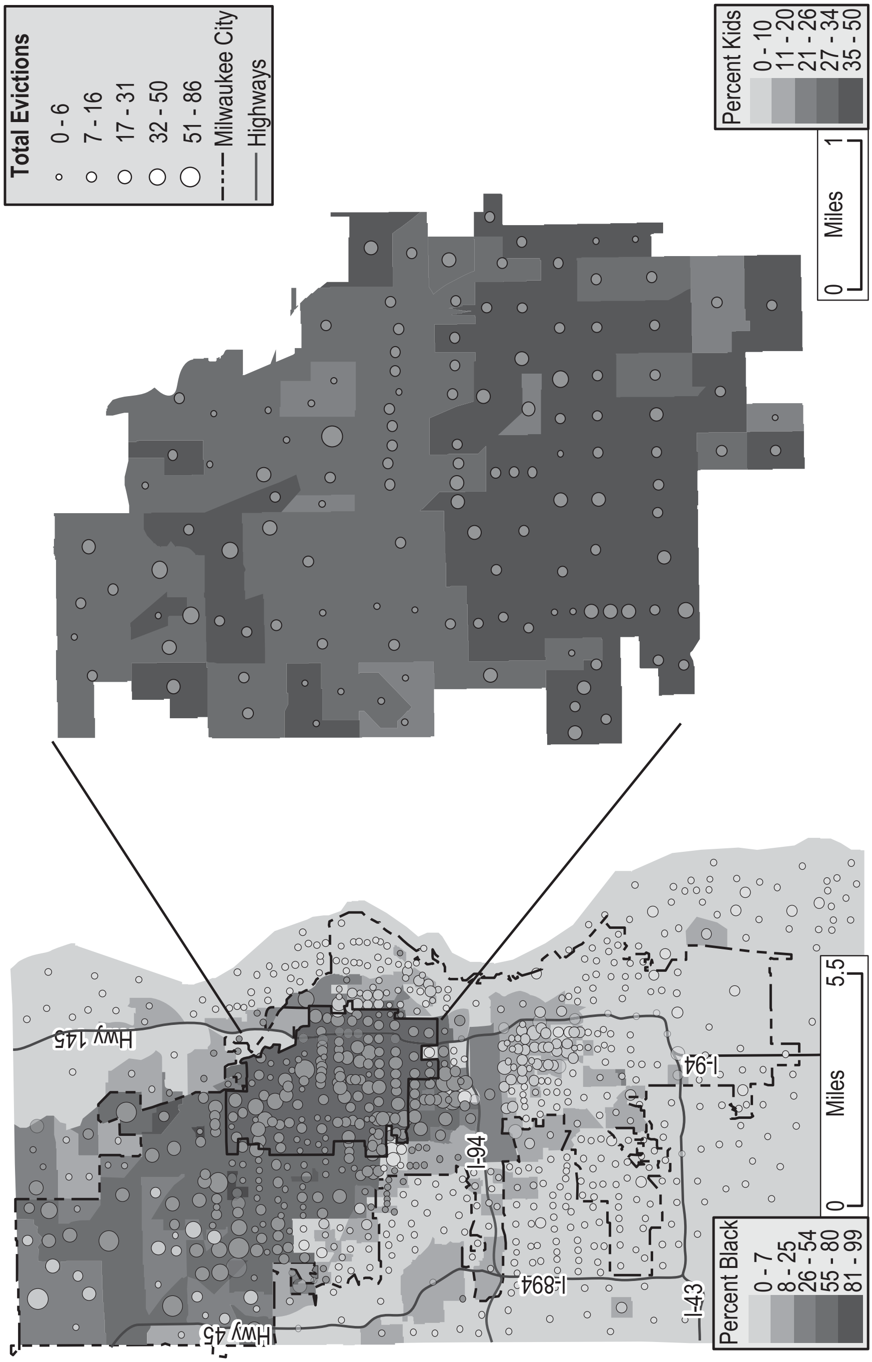


Figure 1. Milwaukee Neighborhood Evictions by Percent Black and Percent Children

Table 3. Zero-Inflated Poisson Regression Predicting Neighborhood Evictions

	Basic Model		Full Model	
	IRR	SE	IRR	SE
Poisson				
% Children	1.069	.002***	1.065	.003***
% Black			1.021	.001***
% Female			.944	.004***
% Female-headed households			.976	.003***
% Vacant housing units			1.001	.003
% Poverty			1.003	.001***
Inflation				
% Living in group quarters	.028	.02	.046	.019**
% Rental units	-.031	.01***	-.041	.009***
% Black	-.12	.055**	-.073	.032**
% College educated	.038	.007***	.049	.009***

* $p \leq .1$ ** $p \leq .05$ *** $p \leq .01$
Note: N = 857 block groups. IRR = incident rate ratios; SE = standard error.

The Influence of Children on the Eviction Judgment

We now turn to the results of the *Milwaukee Eviction Court Study*. Of the 251 tenants interviewed, 29 (11.6%) had their cases dismissed; 59 (23.5%) had to return to court on another day; 90 (35.9%) settled their case with a stipulation agreement; and one moved out prior to the hearing. The remaining 72 (28.7%) were evicted and ordered to vacate the premises in short order. (Additionally, a default eviction judgment was entered for most of the 940 cases in which tenants did not appear in court, providing that the landlord or a representative was present.) Of the 72 evicted tenants, 35 were black women, 13 were black men, 13 were white men, 4 were white women, 4 were Hispanic men, and 3 were Hispanic women.

It is striking to recognize the number of children affected by eviction. Sixty-two percent of tenants who appeared in court lived with children. Over a third of them were women who lived with children but with no other adults. Of the 353 children that lived in respondents' households, 115 belonged to those that received judgments for eviction. The average age of evicted children was 7; the youngest was 4 months old; the oldest was 17. As Table 4 shows, over half the children forced to move by eviction orders were school-aged. Over 77 percent of those children lived in African American households.

Households with children did not appear to have weaker cases than those without children. The average household with children owed slightly less than the average childless household ($1,325 vs. $1,254) and brought in slightly less income ($1,307 vs. $1,427 per month). Only 6.04 percent of households with children were accused of multiple lease violations (e.g., nonpayment plus

Table 4. Children in Evicted Households

Age Range	N
2 years of age or younger	22
3-5 years of age	24
6-8 years of age	22
9-11 years of age	16
12-14 years of age	13
15 years of age or older	13
Total	110

Note: N = 72 households (5 ages unknown).

unauthorized boarder or property damage); the same was true for 6.67 percent of childless households. None of these differences were statistically or substantively significant.

Table 5 reports the findings of our logistic regression models. Model 1 includes all the variables used to predict receiving an eviction judgment. The more concise Model 2 includes only statistically significant covariates. Both models document a large, positive and statistically significant relationship between the presence of children in the household and the likelihood of receiving an eviction judgment. Notably, the effect of children on the likelihood of receiving an eviction judgment remains after accounting for household income and arrearage as well as after accounting for the number and gender of leaseholders, indicating that the effect of children is due to neither the degree of poverty and debt of the children's caretakers nor the influence of single-mother households. Finally, because African American leaseholders were not more likely to receive eviction judgments, the relationship between children in a household and eviction was not reducible to race effects.

To address treatment selection, we turn to our propensity score analysis. Model 3 includes in the full model the predicted propensity of each household to have children. We recognize that it is somewhat redundant to include both the propensity score and individual covariates in the outcome regression because conditioning on the former is theoretically equivalent to conditioning on the latter. But we have done so to provide better adjustment for covariate imbalance. Because the propensity score is a nonlinear combination of the covariate effects, it is only *in expectation* that conditioning on the propensity score will achieve covariate balance; covariate imbalance may still be present after conditioning on the propensity score. Conditioning on covariates may improve covariate balancing and increase efficiency.

In Model 3, the propensity to have children itself has a very large but statistically insignificant effect on the probability of receiving an eviction judgment. After accounting for the propensity to have children, the effect of children on the likelihood of receiving an eviction judgment remains substantively large and statistically significant (its size and significance being virtually unchanged between Models 2 and 3). Model 3 estimates that, all else equal, having

Table 5. Logistic Regression Predicting Eviction Judgment

	Model 1		Model 2		Model 3		Model 4	
	OR	SE	OR	SE	OR	SE	OR	SE
Children in household	2.51	1.02**	2.77	1.04***	2.77	1.36**	2.79	1.42**
Months of arrearage	1.21	.11**	1.25	.1***	1.29	.13**	1.28	.16*
Number of leaseholders	.3	.28	.46	.19*	.42	.41	.28	.25
African American leaseholder(s)	.81	.29	–	–	.77	.3	.64	.26
Women-only leaseholder(s)	.57	.62	–	–	3.83	6.97	.9	1.02
Men-only leaseholder(s)	.12	.13**	.19	.07***	.23	.27	.12	.13**
Age of leaseholder(s)	.99	.02	–	–	1	.03	.97	.02
Household income ($1,000s)	.98	.08	–	–	.97	.09	.96	.09
Propensity to have children					7.14	13.96	–	–
Intercept	4.29	9.03	.87	.53	.18	.65	6.77	15.54
N	232		246		213		213	
Pseudo R^2	.1		.1		.13		.15	

* $p \leq .1$ ** $p \leq .05$ *** $p \leq .01$

Note: Model 3 includes propensity scores as a covariate. Model 4 is a doubly robust regression weighted by propensity scores. OR = odds ratio; SE = standard error.

children is associated with an increase in the odds of a household receiving an eviction judgment by a factor of 2.77.[4] Holding other variables at their means, the probability of receiving an eviction judgment for a tenant with no unpaid rent would be .08 if they did not live with children and .20 if they did, an effect that is equivalent to falling 4 months behind in rent.

While the propensity score regression of Model 3 assumes that the propensity score model is correctly specified, the propensity score weighting estimator of Model 4 provides double robustness in the sense that if either the propensity score model or the outcome model is correctly specified, then the estimated treatment effects are unbiased. The results of the doubly robust regression weighted by propensity scores reported in Model 4 are similar to those reported in Model 3.

Table 6 displays the predicted probability of receiving an eviction judgment derived from multiple estimation procedures. The models reported in Table 6 estimate that on average the probability of households with children receiving an eviction judgment is .16 to .17 higher than households without children.

Additionally, we conducted three distinct matching analyses, employing both single and double matching. First, we compared households with similar propensity scores across treatment (e.g., children in the household). This technique solves the curse of dimensionality by measuring similarity between units according to a single indicator. The propensity score matching estimator is robust to a certain degree of misspecification in the propensity score model—because the ranking rather than the specific values of the propensity scores are more important in the matching procedure—but it can result in an unequal reduction of imbalance for covariates. Second, we matched households directly on their covariates by using the Euclidian difference in covariates weighted by the covariates' sample variances (Abadie et al. 2004). Matching on covariates is more efficient than propensity score matching estimators because no estimated quantities (e.g., propensity scores) are used for matching; however, when there are multiple continuous variables this approach can be difficult and introduce more bias.

Table 6. Predicted Probabilities of the Effects of Children on Eviction

	Coef.	SE
Logistic regression (Model 1)	.16	.07**
Propensity score regression (Model 3)	.17	.08**
Doubly robust regression (Model 4)	.17	.08*
Propensity score matching (1)	.21	.11*
Propensity score matching (2)	.19	.1*
Matching on covariates (1)	.18	.08***
Matching on covariates (2)	.16	.07***
Genetic matching on covariates (1)	.17	.08**
Genetic matching on covariates (2)	.16	.07**

* $p \leq .1$ ** $p \leq .05$ *** $p \leq .01$
Note: SE = standard error.

Third, we matched households on their covariates by employing weights generated by genetic matching (Sekhon 2011). Genetic matching creates weights to achieve optimal or full balancing of the covariates. This technique relies on a stochastic optimization process that can be computationally slow and sometimes unstable. In large samples, these multiple estimators converge to the same limit. But in small samples, it is difficult to judge which is better. Accordingly, we use all three and find that the results are consistent across different estimators and robust to the different modeling assumptions. Each analysis reports a consistent finding, that the probability of households with children receiving an eviction judgment is .16 to .21 higher than households without children (see Table 6). Across multiple estimation techniques, then, our main finding remains robust and statistically significant.[5]

Up to this point, we have assumed that any bias resulting from treatment selection was conditional only upon observed covariates. But what of possible bias introduced by unobserved factors? To address this question, we conducted a sensitivity analysis to evaluate the extent to which any hidden bias would nullify our primary finding. Given the hidden bias produced by an unobserved binary variable, we can calculate the upper and lower bounds of the estimates of treatment effects along with their associated p values (Rosenbaum 2010). Table 7 reports the results of this sensitivity analysis, displaying varying degrees of possible hidden bias (gamma) along with corresponding upper and lower bounds of p values. When there is no hidden bias, gamma is equal to one and the p values are indistinguishable from zero, lending strong support for the finding that the presence of children in a household significantly increases the odds of receiving an eviction judgment, all else equal. As we increase the size of possible hidden bias, the upper bound of the p value approaches, and when gamma is set to 1.9 eventually crosses, .1. This means that when hidden bias is so great that the odds of living with children for some households is 1.9 times that of households with the same covariates, the estimated treatment

Table 7. Sensitivity Analysis

Gamma	P–	P+
1.0	0	0
1.1	0	0
1.2	0	0
1.3	0	0
1.4	0	.01
1.5	0	.02
1.6	0	.03
1.7	0	.05
1.8	0	.09
1.9	0	.13
2.0	0	.18

effects would be reduced to insignificance. Because the evicted population (and our sample) is relatively homogenous along several important dimensions (Desmond 2012), we believe such a situation to be unrealistic and find our results robust to hidden bias.[6]

Discussion

Our study found that neighborhoods with higher percentages of children have more evictions, even after controlling for neighborhood racial composition, poverty, female-headed households, vacancy rates and a number of other key factors. It also found that among tenants who appear in eviction court, the likelihood of receiving an eviction judgment increases significantly if tenants live with children, a finding that remains after accounting for household income, family status, the amount of back rent owed, and the propensity to have children. Surprisingly, then, the presence of children in a household was more important to explaining the distribution of evictions across neighborhoods and the distribution of eviction judgments across tenants who appeared in court than were factors associated with race, gender or class.

The data used in this study, then, allowed us to make inferences about two target populations. First, focusing on the population of all Milwaukee neighborhoods, we were able to explain the geography of forced dislocation across the city. Second, focusing on the target population of Milwaukee renters facing eviction who appear in court, we were able to explain why some tenants elude eviction while others are ordered from their homes. In both cases, children were a deciding factor.

Most evictions can be understood as proceeding through three events. First, a tenant violates the rental agreement (typically by nonpayment); second, the landlord decides to begin eviction proceedings against the tenant; and, third, the tenant receives a judgment for eviction in court. The results of our analysis of aggregate data could reflect the influence of children in the neighborhood during one step or multiple steps of the process: neighborhoods with more children may be more likely to have households who violate the rental agreement *and/or* to be served notice *and/or* to be evicted. These data do not allow us to inspect each stage separately or to specify household-level processes. However, our in-court survey data have enabled us to analyze in detail the final moment at the household level. By accounting for household income and months of arrearage, we were able to assess if children exerted an independent effect on the eviction decision beyond the financial constraints of their caretakers. And by accounting for the sex of all leaseholders in the household, we were able to distinguish the effect of children from that of single mothers. Although household poverty and family status, along with sex and racial discrimination, may certainly determine why a tenant is served an eviction notice, we collected no evidence that these factors—other than arrearage—positively influence the in-court decision to evict. What does matter is the presence of children in the household.[7]

Ethnographic observation of Milwaukee's eviction court conducted by the first author revealed that landlords hold considerable sway over the outcome of eviction proceedings. Provided that all the paperwork is in order and that no egregious violations have been committed, court officials usually defer to landlords' decisions whether to work with tenants or to evict them. Our findings, then, likely reflect the outcome of landlords' decisions more so than those of the court.

If children increase the likelihood of eviction, then what are the consequences of eviction for children? We suspect they are many. For one, eviction often leads to homelessness and high rates of residential mobility (Burt 2001; Crane and Warnes 2000), which in turn can damage a child's school performance. Compared with their peers, homeless students and those with high rates of residential instability perform worse on standardized tests, have lower school achievement and delayed literacy skills, are more likely to be truant, and are more likely to drop out (Beatty 2010; Pribesh and Downey 1999). Increased residential mobility also has been linked to higher rates of adolescent violence (Haynie and South 2005) and children's health risks (Dong et al. 2005). If evicted families, desperate to find new housing, relocate to dwellings with severe housing problems, then this, too, can be harmful to children (Shaw 2004). Eviction can push families deeper into the slum, relocating them to more disadvantaged neighborhoods (Desmond and Shollenberger 2013), neighborhoods that can have detrimental effects on children's health, development and wellbeing (Brooks-Gunn, Duncan, and Aber 2000; Sampson, Morenoff, and Gannonn-Rowley 2002). And eviction itself can contribute to neighborhood disadvantage. High eviction rates can unravel the fabric of a community; the rapid turnover of households thwarting efforts to establish and maintain social capital, local cohesion and community investment (Sampson, Morenoff, and Earls 1999). Eviction, then, can result in negative consequences, not only for children of evicted households, but also for *all* children who live in high-eviction neighborhoods.

That the results of both analyses, applied to two different data sources, complemented one another to a significant degree bolsters our confidence in the findings. But like all studies of this kind, ours is limited by the possibility of not accounting for all relevant factors (Pager and Shepherd 2008). However, approaches designed to move beyond this limitation (e.g., field experiments) could not have been applied in this context. Designing an audit study focused on eviction outcomes in the mold of those focused on measuring discrimination against housing applicants would raise a number of analytical, practical, and ethical problems. As with studies examining differences in job dismissals (e.g., Zwerling and Silver 1992), then, the best way to measure disparities in the eviction decision may be through the application of statistical analysis to observational data.

This study has made two important contributions to the sociology of housing and inequality. First, it has underscored the importance of further research on family discrimination by showing that among families facing eviction who appear in court, those with children disproportionately receive eviction judgments. Although sociologists have amassed a large body of work on racial discrimination in housing (Ross and Turner 2005), comparatively little work deals with the frequency (or consequences) of discrimination based on family type. Discrimination based on family status appears quite common in the housing sector and is a mechanism that exacerbates childhood and family poverty and hardship. It is a topic deserving of far more attention than it has received.

Second, and more broadly, in examining the outcome of eviction, this study has highlighted a previously overlooked event in the study of discrimination. Although the degree to which discrimination influences the processes of securing an apartment, job, or loan have been thoroughly studied, analysts largely have ignored how prejudice against minorities, women, or children may influence the consequential decision of whether to evict. Owing to the frequency of eviction in the lives of poor families as well as to the host of negative outcomes brought about by eviction, understanding who landlords put out is just as important as understanding who they let in.

The results of this study also have implications for how we think about children in poverty. Much research today focuses on the effects of structural conditions or life events on poor children, for example, the effects of parental incarceration or neighborhood disadvantage on children's behavior and development (Sampson, Sharkey, and Raudenbush 2008; Wildeman 2010). To fully understand the lives of poor families, however, it is necessary to examine not only the effects of poverty on children, but also the effects of children on poverty. By studying the degree to which children in and of themselves affect the eviction decision, this study has promoted the latter perspective. But more work investigating how children alter fundamentally the lived experiences of poverty is sorely needed.

Low-income single mothers confront certain obstacles and hardships with respect to employment, housing, policy, and intimate life, not only because they belong to one of the poorest demographics in the United States but also because they are *mothers*. This is why research that investigates the existence of "motherhood penalties"—with respect to wages, hiring, housing choice, and residential stability—faced by low-income single mothers could do much to deepen our understanding of the feminization of poverty (cf. Budig and England 2001; Correll, Benard, and Paik 2007).

If rejecting a tenant's housing application on the basis of race, sex, family status or other characteristics contributes to racial segregation, material hardship and systematic denial of opportunities, then so, too, does evicting tenants on the basis of such characteristics. Property owners who disproportionately target families with children for eviction stand in violation of the *Fair Housing Act* (Oliveri 2008; Schwemm 2001). This implies the need to expand equal opportunity programs to prevent vulnerable groups from being targeted for eviction. Policymakers interested in identifying and sanctioning discrimination, then, should focus not only on the front end of the housing process—the freedom to *obtain* housing anywhere—but also on the back end: the freedom to *maintain* housing anywhere. More generally, housing policy should be particularly attuned to hardships faced by poor families with children.

Notes

1. As further robustness checks, we repeated our analyses on eviction records from 2003 through 2007, these merged with GeoLytics population estimates from

corresponding years, arriving at similar results each year. Also, in addition to the ZIP model, we employed ordinary least squares, Poisson, negative binomial and zero-inflated negative binomial models, arriving at similar conclusions each time. Results are available upon request.

2. When analyzing both the aggregate- and individual-level data, we explored if children of certain ages (e.g., preschool children, teenagers) were more consequential to eviction outcomes than others. In both cases, we found no meaningful patterns that would have warranted separating by age groups our variables related to children.
3. The propensity score methods we use here address treatment, not sample, selection. Propensity score methods were developed in statistics to deal with treatment selection (Rosenbaum and Rubin 1983), while similar methodological procedures concomitantly and independently were developed in economics to adjust for sample selection (Heckman 1979). In a *treatment selection model*, one is interested in the effects of a treatment—the difference between the conditional expectations of outcomes for two different groups—and so there is always an indicator in the main outcome model standing for treatment assignment, namely, $Y = AD + XB + CP + e$, where D stands for treatment assignment and P for the propensity scores. In a *sample selection model*, one is interested in addressing bias that arises when some of the dependent variables are not observed or selected either owing to censoring or to missing data. A selection model is first used to estimate the probability of each observation to be observed or selected; then, predicted probabilities (or, more precisely, inverse mills ratios) are plugged into the main outcome model to account for selection bias. The main goal here is to provide an unbiased and consistent estimate of the conditional expectation of the dependent variable, should it be observed. In other words, the main outcome model is in the form of $Y = XB + CM(P) + e$, where the covariates X do not include any indicator for sample selection (or treatment assignment) and M(P) stands for the mills ratio based on the predicted selection probabilities. A sample selection model is inappropriate for our analyses.
4. A model that included only the treatment (the presence of children in the household) and the propensity score reported similar results (treatment odds ratio = 2.2; $p < .1$).
5. In our propensity score regression, doubly robust regression and propensity score matching, our estimates of statistical significance are conservative. This is because of an inflation of standard errors caused by propensity scores being estimated quantities (Rubin and Thomas 1992; Abadie and Imbens 2009).
6. The significance levels in Table 6 are slightly different from those in Table 5 because the predicted probabilities are averages of the effects of children over each case. The *p* values in Table 7 are slightly different from those displayed by our propensity score analyses because the test we used in our sensitivity analysis is nonparametric.
7. Which is to say, different modes of discrimination may be more acute at different stages of the eviction process. While women living in poor black neighborhoods may be more likely to be served an eviction notice and less likely to stay the eviction after being served notice (Desmond 2012), among those who appear in court, women who live with children are more likely to receive an eviction judgment.

Appendix: Propensity Score Analysis

Table A1 displays the results of the propensity score model. With a Pseudo R^2 of .28, the model is a relatively good fit.

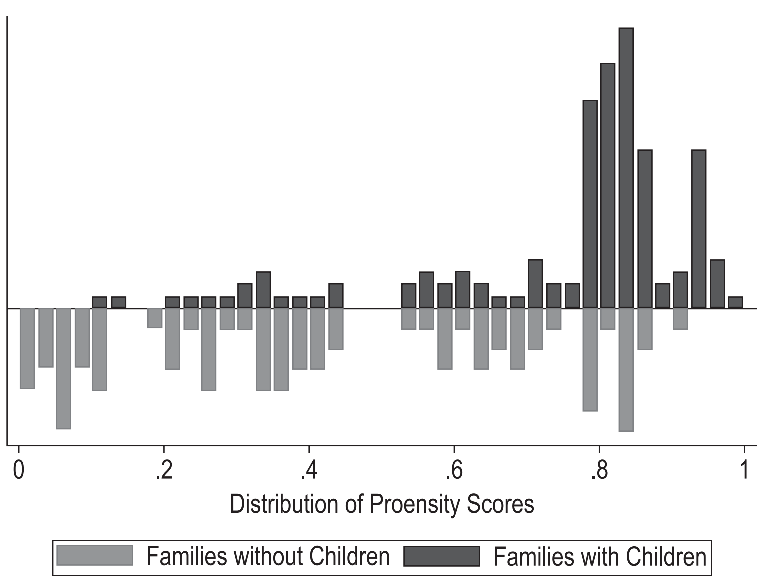
Figure A1 graphs distributions of the estimated propensity scores by treatment status. There is a sufficient amount of overlap of the propensity scores between

Table A1. Logistic Regression Predicting Propensity Scores to Have Children

	OR	SE
Months of arrearage	.90	.17
Number of leaseholders	.36	.21*
African American leaseholder(s)	1.31	.55
Men-only leaseholder(s)	.01	.01***
Women-only leaseholder(s)	.09	.08***
Age of leaseholder(s)	1.47	.23**
Age square of leaseholder(s)	.99	.00***
Household income ($1,000)	1.06	.12
Amount of rent owed	1.08	.39
Intercept	.49	1.47
N	213	
Pseudo R^2	.28	

* $p \leq .1$ ** $p \leq .05$ *** $p \leq .01$
Note: OR = odds ratio; SE = standard error.

Figure A1. Distribution of Propensity Scores by Treatment Status



households with and without children (a rare condition in many observational studies), which facilitates comparable subjects from across the two groups being matched. The balance of the covariates improved significantly after matching. For the majority of the covariates, the standardized bias (the mean difference between the two groups divided by the sample variance) shrank by over 50 percent, and the mean standardized bias shrank from 37.9 to 16.8 percent. Overall,

Table A2. Covariate Balancing in Unmatched and Matched Samples

Variable	Sample	Treated	Control	% Bias	% Reduced	t	P
Months of arrearage	Unmatched	2.17	2.20	-1.70		-.12	.90
	Matched	2.17	2.13	2.20	-25.00	-.31	.76
Number of leaseholders	Unmatched	1.25	1.14	26.20		1.79	.08
	Matched	1.25	1.17	18.30	30.10	1.07	.29
African American leaseholder(s)	Unmatched	.76	.73	6.20		.44	.66
	Matched	.76	.72	8.80	-41.40	-.27	.79
Men-only leaseholder(s)	Unmatched	.07	.43	-92.00		-7.06	.00
	Matched	.07	.20	-34.10	62.90	.00	1.00
Women-only leaseholder(s)	Unmatched	.74	.51	49.10		3.53	.00
	Matched	.74	.69	11.30	77.00	-.67	.50
Age of leaseholder(s)	Unmatched	31.93	39.24	-75.20		-5.54	.00
	Matched	31.93	35.38	-35.50	52.80	-1.21	.23
Age square of leaseholder(s)	Unmatched	1084.00	1661.60	-80.00		-5.96	.00
	Matched	1084.00	1346.80	-36.40	54.50	-1.36	.17
Household income ($1,000)	Unmatched	1.38	1.49	-4.40		-.32	.75
	Matched	1.38	1.33	2.10	50.90	-.91	.36
Amount of rent owed	Unmatched	1.28	1.22	6.00		.43	.67
	Matched	1.28	1.25	2.60	57.40	-.01	.99
Overall Covariate Balancing							
Sample	Pseudo R2	LR chi2	P	Mean Bias	Median Bias		
Raw	.28	78.38	.00	37.90	26.20		
Matched	.01	5.89	.75	16.80	11.30		

there were no statistically significant differences between households with and without children (see Table A2). The balancing in the covariates confirms that our propensity score model was well specified. All matching estimates were conducted with replacement, were corrected for bias and their standard errors were adjusted according to Abadie and Imbens (2006).

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