The Effects of Evictions on Low-Income Households

Robert Collinson* and Davin Reed†‡

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Abstract

Each year in the U.S., more than two million renter households report being at risk of eviction, yet there is little causal evidence on how evictions affect low-income households. We assemble novel data linking individuals from housing court cases in New York City to administrative data and leverage the random assignment of cases to courtrooms to estimate the causal effect of evictions on homelessness, health, earnings, employment, and public assistance receipt. Evictions cause large and persistent increases in risk of homelessness, elevate long-term residential instability, and increase emergency room use. We find some evidence that evictions lower earnings modestly, but little evidence that they substantially worsen employment outcomes or increase receipt of public assistance. These results suggest that eviction prevention policies could provide important consumption smoothing benefits to low-income households but are unlikely to substantially reduce poverty on their own.

*Corresponding author: rcollinson@nyu.edu (New York University, Wagner School of Public Service, 295 Lafayette Street New York, NY 10012)

†davin.reed@phil.frb.org (Federal Reserve Bank of Philadelphia, Community Development and Regional Outreach Department, 10 N Independence Mall W, Philadelphia, PA 19106)

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I Introduction

Each year in the U.S., more than two million renter households report being threatened with an eviction notice. Many more renters report being at risk of losing their home through eviction than homeowners report being at risk of foreclosure.[1] While rent delinquency and evictions have been little studied in economics, recent work in sociology has shown that evictions play an important role in the lives of low-income renter households and may also contribute to poverty through cascading disruptive effects such as job loss, adverse health effects, and negative consequences for children (Desmond 2016).

However, it remains empirically unclear the extent to which evictions are a cause or consequence of poverty. Obtaining credible causal evidence on the effects of evictions is complicated by two challenges. First, evictions are rarely measured in survey data, and administrative records contain little identifying information with which to link evictions to longitudinal data on outcomes. Second, receiving a court-ordered eviction is likely to be correlated with unobservable characteristics that are correlated with individual outcomes. For both of these reasons, previous research has been unable to convincingly isolate a causal effect of eviction on individual outcomes.

In this paper we overcome both of these challenges to provide the first causal estimates of the effect of evictions on homelessness, health, employment and earnings, and receipt of public assistance.[2] We first link individuals appearing on housing court cases in New York City to more than a decade of administrative data covering the major sources of public assistance (food stamps, Medicaid, and cash assistance) to obtain detailed demographic information and identifiers.[3] We then link this matched sample to outcome data from various sources. Our primary research design exploits the random assignment of housing court cases to courtrooms to identify the causal effect of court-ordered evictions on low-income households. Housing court cases are randomly assigned to courtrooms, and judges typically rotate through courtrooms on a yearly basis.[4] Following the randomized screener literature that has studied the effects of incarceration (Kling 2006; Aizer and Doyle 2015; Mueller-

[1]According to the 2017 American Housing Survey, 800,000 renter households report being threatened with an eviction notice within the past three months, which could extrapolate to around 3.2 million within the past year. The Eviction Lab at Princeton University estimates that 2.6 million households received some type of eviction notice during 2016 (Desmond et al. (2018)).

[2]In a companion study we are exploring the consequences of eviction for children (Collinson and Reed 2018).

[3]The resulting matched sample comprises a majority of actual evictions in New York City and is demographically similar to the entire housing court population overall.

[4]There are some exceptions to random assignment, which we exclude, that are detailed in section 2.1 and appendix E. These include, but are not limited to, cases involving public housing tenants, military members, condo cases, drug cases, and zip-code based routing related to certain policy initiatives. Judge courtroom assignments can also change within the year, which we discuss in 2.1

Smith 2015; Bhuller et al. 2016), bankruptcy protections (Dobbie and Song 2015), and disability insurance (Maestas et al. 2013; French and Song 2014), we construct measures of courtroom leniency that are based on the eviction rate in all other cases handled in that courtroom and year. The leniency measure is then used as an instrumental variable (IV) for whether an individual assigned to that courtroom is actually evicted. The instrument is uncorrelated with pre-filing household characteristics and is a good predictor of eviction, with a 1 percentage point increase in the courtroom eviction rate corresponding to a 0.8 percentage point increase in the probability of eviction.

A drawback of our IV design is that it produces imprecise estimates for some of our outcomes. Presented with an empirically important bias-variance trade-off, we adopt a complementary panel difference-in-differences design, which is much more precise. This approach exploits the quarterly frequency of our outcomes data and ability to observe the exact timing of case filing, but it will biased if there are time-varying unobservables that are correlated with eviction status. Empirically, we find that evicted and non-evicted households experience similar pre-trends in the lead up to filing for most of outcomes, suggesting the validity of our assumptions for the difference-in-differences design. The similarity in our results across IV, OLS, and difference-in-differences gives us confidence in drawing our conclusions about the effects of eviction.

First, we examine the effects of eviction on homelessness and residential instability for low-income adults, two first-order concerns highlighted in previous research on evictions (Desmond and Kimbro 2015; Desmond 2016). To measure homelessness, we link our sample to administrative data covering all shelter use in New York City. The IV estimates suggest that evictions increase the probability of applying to homeless shelter by 14 percentage points, on a baseline of 3.4 percent among not-evicted households, and increase the share of days spent in shelter during the two years after filing by 5 percentage points, or about 36 days. Importantly, all of the estimated effects are large and persist beyond the first two years of case filing, through all post filing years. This suggests that avoiding eviction does not simply delay an inevitable bout of homelessness, but rather leads to a lasting difference in the odds of experiencing homelessness. Thus, anti-eviction policies may help individuals stay out of the shelter system altogether.

Next, we investigate the effects of eviction on adult physical and mental health, another area explored in previous work (Desmond and Kimbro 2015). We find that evictions worsen health, particularly mental health, and increase emergency room utilization. IV estimates suggest that evictions increase the probability of hospitalization for a mental health condition by about 9 percentage points in the two years after filing. The OLS results are smaller, 2.5 percentage points, though they are within the 95% confidence intervals of the

IV estimates. The more conservative OLS estimate is nevertheless large: it represents a 68 percent increase, and we estimate it to be about 2.5 times as large as the cross-sectional relationship between job loss and mental health hospitalizations in our sample. Being evicted increases the number of emergency room visits in the two years after filing by about 0.38 visits, an increase of over 70 percent. Evictions also worsen a composite health index by 0.45 standard deviations, though the OLS results are again more conservative (0.07 standard deviations). Taken together, these results imply sizable negative health effects from being evicted, particularly for marginal cases.

Finally, to assess whether evictions contribute substantially to measured poverty, we estimate the effect of evictions on formal sector earnings, employment, and receipt of public assistance. Our IV estimates are imprecise: while point estimates for employment are centered at zero, we are unable to rule out large positive or negative effects. The difference-in-differences and OLS results are much more precisely estimated and suggest that the labor market effects are small: in the first two years after filing, evictions reduce the probability of being employed by 1 percentage point (on a baseline of 46 percent) and lower quarterly earnings by around 200 dollars (on a baseline of 3,000). The small employment effect implies that the overall earnings effect is concentrated along the intensive margin. Importantly, adding individual controls to an OLS model without individual controls attenuates the estimates towards zero. This suggests that if we were able to control for any remaining omitted variables, the results would likely be attenuated further (Altonji et al. 2005). Thus, our OLS estimates with full controls may represent an upper bound on the magnitude of the negative impact of evictions on labor market outcomes. The similarity of our results across designs suggests a causal interpretation despite the imprecision of the IV estimates. We also find some evidence that evictions increase take-up of cash assistance, food stamps, and emergency assistance, but these effects are small. Overall, these results suggest that formal evictions may have a quantitatively small direct effect on poverty.

Even if formal evictions are not a major driver of poverty - as measured by earnings and benefits - avoiding eviction delivers important consumption smoothing benefits to low-income households through reductions in homelessness.[5] We estimate that households in housing court that manage to avoid eviction, by virtue of being assigned to a courtroom with a lower eviction rate, are considerably less likely to use homeless shelters, even several years after the initial non-payment filing. Quantitative and qualitative evidence suggests that a primary way in which courtroom experiences for tenants in housing court differ is

[5]Our labor market effects are in the formal sector and for adult heads of household, so they could miss larger effects in the informal labor market and do not incorporate effects on children's future earnings. We are also unable to capture all relevant sets of benefits, as we miss SSI, SSI-DI, and EITC payments.

by extending (or limiting) the time that tenants have to either repay rent or make other arrangements. We estimate that being assigned to a courtroom that is one percentage point more likely to evict is associated with a 14 day reduction in the length of the housing court process. Thus, housing court proceedings, and being assigned to a more lenient courtroom, may act as a de facto line of credit for many tenants. The fact that households who avoid eviction are able to avoid shelter even several years after the initial non-payment filing suggests that resolving short-term liquidity constraints may be important for the well-being of households in housing court.

Use of homeless shelters and emergency room, or so called "providers of last resort," is exceedingly costly and therefore frequently cited as justification for homelessness- and eviction-prevention policies. We perform a back of the envelope calculation using our estimates of the effects of eviction on homelessness, hospitalizations, and earnings and find a financial cost per eviction of roughly \$8,000 over the first two years after filing.[6] The largest component of this cost is increased use of homeless shelters, accounting for about 75 percent of the cost. The remaining cost is roughly evenly divided between lost earnings and increased emergency room utilization. Moreover, this calculation applies to just household heads, meaning costs will be larger when accounting for children and other household members. Thus, while eviction prevention policies may not substantially reduce poverty, they may still be welfare enhancing.

This work contributes to several literatures. First, we contribute to the small but growing literature on the effects of evictions on low-income households. Desmond and colleagues (Desmond 2012; Desmond and Kimbro 2015; Desmond 2016) have conducted novel qualitative and quantitative work on the role that evictions and housing instability play in the lives of low-income families. This work suggests large negative effects of evictions and housing instability on employment, homelessness, future housing stability, and health. However, compelling causal evidence from this work has remained elusive. We provide new rigorous evidence on the causal effects of eviction on several of the most important outcomes for adults. Our findings that eviction substantially raises the risk of homelessness, increases residential instability, and exacerbates mental health problems are largely consistent with findings from Desmond (2012; 2016), but our findings on employment, job loss, earnings, and receipt of public assistance suggest much more modest effects of formal eviction on traditional measures of poverty.

Second, our results connect to the literature establishing the importance of liquidity constraints in the consumption decisions of low-income households (Agrawal et al. 2007; Parker et al. 2013; Gross et al. 2014) and financially distressed homeowners (Herkenhoff and

[6] The calculation is described in detail in Appendix C.

Ohanian 2015; Ganong and Noel 2018). Ganong and Noel (2018) find that homeowners who received private sector mortgage modifications that reduced short-term payments through extended mortgage maturities were considerably less likely to default on their mortgage. Herkenhoff and Ohanian (2015) study the implications of lengthening the foreclosure process for employment, delinquency, and residential stability (homeownership rate) and find that extending the process increases the homeownership rate. Consistent with both studies, we find that households randomly assigned to more lenient courtrooms, who in turn experience lengthier housing court processes, are considerably more likely to stay in their unit several years after filing and much less likely to become homeless.

Third, we contribute to a larger literature on the relationship between housing stability and adult outcomes. Prior work has either focused on the effects of long-term housing subsidy programs (Jacob 2004; Jacob and Ludwig 2012; Ludwig et al. 2013; Chyn 2018), short-term rapid re-housing assistance (Evans et al. 2016), or shocks to housing such as foreclosures (Currie and Tekin 2015) or natural disasters (Gallagher and Hartley 2017). In general, this literature cannot separate housing instability from a mix of other, sometimes countervailing, forces such as income and substitution effects from housing transfers (Olsen 2003; Jacob and Ludwig 2012), neighborhood effects (Ludwig et al. 2013; Chetty et al. 2016; Chyn 2018), loss of home equity, or loss of assets. By contrast, our setting of evictions in New York City's housing court provides an opportunity to study the effects of acute housing instability on adult outcomes that is arguably more independent of other channels such as long-term changes to a household's budget constraint.[7]

Finally, we contribute to a growing literature examining the relationship between housing circumstances and health. Similar to Currie and Tekin (2015), we find that housing instability plays a causal role in worsening mental health. The Moving to Opportunity experiment found large improvements in the mental health of adults when they moved to lower poverty, safer neighborhoods (Ludwig et al. 2013). We find that households who are evicted relocate to neighborhoods with poverty and crime levels similar to those in their origin neighborhoods, suggesting our results are perhaps driven more by the stress of trying to secure stable housing rather than exposure to more destabilizing neighborhood environments. The effects on mental health that we document are consistent with homelessness being a contributor to mental health issues rather than just a symptom, which is consistent with work drawing from panel surveys (Scutella and Johnson 2018).

The remainder of the paper is organized as follows. Section 2 describes the institutional details of New York City's Housing Court and the assignment of housing court

[7]Everyone entering housing court has likely experienced some level of housing instability by virtue of having received a non-payment filing.

cases to courtrooms. Section 3 describes our sample and linked data. Section 4 details our instrumental variables and difference-in-differences research designs. Section 5 provides graphical event study plots and reduced form evidence. Section 6 presents OLS, IV, and difference-in-differences results, and Section 7 concludes.

II Housing Court

The Civil Court of New York City, part of the state Unified Court System, oversees the New York City Housing Court. Housing Court hears cases involving landlord-tenant disputes or proceedings involving housing code violations. Cases are handled by seven courthouses: one for each county (borough) in New York City (Bronx, Kings, New York, Queens, and Richmond) and two smaller, specialized courts in Harlem and Red Hook. The vast majority of cases heard in housing court are non-payment filings (82 percent) or holdover disputes (13 percent).[8] Non-payment filings are initiated by landlords claiming that a tenant has not made payments stipulated by a rental agreement. Holdover cases are when a landlord is seeking eviction of a tenant for reasons other than non-payment of rent. Examples of reasons for pursuing eviction in holdover cases include lack of a lease, termination of a tenancy at will, expiration of lease, lease violations, creating a public nuisance or criminal activity, or restricting access to the apartment for emergency repairs.

Nearly all cases are handled in Resolution Parts, which we refer to as courtrooms. Most non-payment cases in housing court initially result in a settlement, or "stip." Before appearing in front of a judge, the tenant and landlord will typically first negotiate a stipulation of settlement, which establishes terms of repayment for tenants. Landlords are usually represented by an attorney, while the vast majority of tenants are self-represented.[9] The landlord's attorney and the tenant negotiate the terms of a possible settlement, haggling over "time" - length of repayment - or "money" - the amount of past arrears to be repaid. They may also negotiate whether a judgment is entered against the landlord, such as for required repairs. Negotiations between the tenant and the attorney may occur in the hall outside the courtroom or in a private conference with a Court Attorney, a non-partial court representative serving at the behest of a presiding judge in a given courtroom. A Court Attorney does not represent either party but may exert varying degrees of influence over

[8] The next largest group of cases are "HP" cases, in which the tenant sues the landlord in order to make repairs (around 5 percent).

[9] This is now changing after the establishment of universal access to legal representation in court for low-income tenants facing eviction was enacted in August of 2017. Implementation is currently underway as part of a multi-year implementation.

agreement to a settlement. Tenants present defenses or counterclaims with a Court Attorney and when appearing before the judge.

Once a settlement has been reached, the landlord's attorney and the tenant appear before the judge presiding over that courtroom to present the settlement. In the case of a settlement, the judge will review the terms of the settlement with the tenant and landlord (or landlord's attorney). This may include discussing with the tenant their ability to meet the terms of the settlement or raising questions about the settlement, defenses, or counterclaims. If a settlement cannot be reached, the tenant can request a trial. If the judge approves a trial, the case will be reassigned to a trial part (courtroom) and tried that same day or scheduled to a new day.[10]

A settlement may include a money judgment, a possessory judgment, or both. A money judgment allows the landlord to collect the specified amount owed. A possessory judgment allows the landlord to evict the tenant if the terms of the settlement are not met. In addition to a possessory judgment, a landlord will also need a warrant in order to have a City Marshal carry out an eviction. If the tenant is able to pay the amount owed, then the judgment is satisfied and the possessory judgment goes away, thus avoiding eviction

The courtroom that a tenant is randomly assigned to can influence the outcome of their case through the behavior of the judge presiding over the case, or possibly through the behavior of a Court Attorney assisting the judge. Judges and Court Attorneys both review and discuss the terms of settlements (the stipulations) to ensure they are not unduly burdensome, which can affect a tenant's decision to agree to settlement. However, the judge is likely to be more influential as she can affect both the final outcome and timing of that outcome through their decision to adjourn cases (reschedule), accept motions, sign an Order to Show Cause (a decision to stop an eviction and re-open a case), or issue default judgments when tenants fail to appear at a court date.

We investigate the effects of an executed eviction.[11] That is, our treatment is the effect of a formal eviction, which differs significantly from evaluating the effect of issuing a judgment in favor of the landlord or the effect of issuing a warrant for an eviction (which is not necessarily executed). We focus on executed eviction warrants because, in our setting, this is a far more concrete measure of being evicted. Approximately 72 percent of the tenants in our estimation sample have received a judgment against them, but just 18 percent are evicted

[10] Trials are extremely rare, typically less than 1 percent of cases. We drop cases originating in trial parts.

[11] We include "residential possessions" in our definition of eviction. New York City distinguishes an eviction from a possession by whether a tenant's belongings remain in the unit. In residential possessions, which are the vast majority of colloquial "evictions" in New York City, the tenant's belongings remain in the unit and the locks are changed (locking out the tenant), whereas an eviction involves transferring a tenant's belongings to a storage unit.

by a city Marshal executing a warrant. A judgment is a necessary but not sufficient step to a completed eviction. If a tenant is able to satisfy the terms of the stipulation, then the judgment goes away and the tenant can remain in the unit. Similarly, many more warrants are issued for an eviction than are actually executed, with warrants typically totaling more than 100,000 in a year and evictions numbering 20,000-30,000 in a year. Issued warrants are not executed either because a tenant resolves their dispute with the landlord (such as by meeting the terms of a stipulation), because they are able to get a judge to stay or delay the eviction, or because they move preemptively. Tenants will frequently seek judge approval of an Order to Show Cause (OSC), which can halt an eviction. If the judge agrees to grant the OSC, the tenant is given the opportunity to explain the reason for breaching the terms of the stipulation and may receive an extension of time to comply. In our sample, we find that issued warrants have essentially no independent effect on the odds that a household moves, confirming that executed warrants, our measure of treatment, is the most relevant one. [12]

II.1 Random Assignment to Courtrooms

Importantly for our primary research design, cases in housing court are randomly assigned to courtrooms. This is done by the Housing Court Information System (HCIS) computers within the tenant's county of residence when they file an Answer in a non-payment case. Judges are typically assigned to courtrooms on a pseudo-random rotation for terms of approximately one year. Court attorneys follow particular judges and typically remain in the same courtroom for a year. Deviations from random assignment occur in a few select instances, including cases involving public housing (New York City Housing Authority); condo or co-op disputes; cases where the respondent is in the military; cases brought by the District Attorney or in which the only allegation involves drug-related activity; cases brought by tenants against landlords for repairs (HP cases); lock-out cases; and cases arising in select zip codes as a part of the expansion of legal access to tenants, which began in August 2017. We exclude these non-randomly-assigned cases from our analytical sample and describe them in more detail in Appendix E.

We impose a handful of other restrictions on our sample, also described in detail in Appendix E. Our analytical sample consists of residential non-payment filings in 28 court-

[12] Using our most accurate move information, derived from linked school records, we run the following regression where $\mathbf{1}(\text{Move}_i)$ equals 1 if the household changed addresses 3 years after the housing court filing, and $\lambda_{c,t}$ is a court-by-time of filing fixed effect :

$$\mathbf{1}(\text{Move}_i) = \gamma_1 \mathbf{1}(\text{Executed Warrant}_i) + \gamma_2 \mathbf{1}(\text{Issued Warrant}_i) + \lambda_{c,t} + \nu_i$$

The coefficient $\gamma_2 = -0.00012$ (s.e. =0.0032). Using this sample of households with school-age children, we estimate that at least 43 percent of non-evicted households remain in their unit 3 years after initial filing.

rooms from the Bronx, Brooklyn, Manhattan, and Queens housing courts. We drop cases in Staten Island because there is only a single courtroom. We drop two parts in the Bronx to which cases from certain zip codes were non-randomly assigned for various years. We also exclude several isolated incidents of deliberate non-random assignment stemming from city-specific policy changes. Finally, and importantly, we restrict our estimation sample to cases that are assigned to a part in the HCIS system. About 40 percent of cases that are filed never result in an actual housing court appearance by the landlord or tenant. Based on conversations with court administrators, these are cases that are not pursued further by the landlord. We exclude these cases because we cannot construct our instrument for them, as they do not generate further interaction with the court system.

III Data

We begin with data on the universe of housing courts records in New York City from 2007-2016, which provides, among other things, information on the date of filing; the type of case (non-payment, holdover, housing repairs, or lock-out); the courtroom that the case is assigned to; information on each appearance for the case; the claim amount; whether a judgment was issued; and whether an eviction warrant was issued and returned by a City Marshal.[13] Unfortunately, housing court records contain limited identifiers: only names and addresses. To obtain additional identifiers that we can use to reliably match housing court records to outcomes, we first link housing court records to pre-case-filing administrative data on historical benefits receipt from New York City's Human Resources Administration. This benefits data include individuals receiving Medicaid, Temporary Assistance for Needy Families (TANF), Supplemental Nutrition Assistance Program (SNAP; food stamps), or other city-specific cash subsidies through HRA from 2004-2016.[14] This data covers more than 2 million unique adults and children each year. Importantly, the data include key individual identifiers such as first name, last name, Social Security Number (SSN), and date of birth. They also provide basic demographic information such as race, ethnicity, gender, age, and household composition for eligible members.[15] We merge on information on neighborhood characteristics using the address of the unit being disputed by the case.

To preserve the integrity of the research design, we must link housing court records to benefits receipt using only pre-filing information. For example, we cannot match individuals

[13] Where available, we also validate our court measures with records collected directly from City Marshals by New York City's Department of Investigations.

[14] This data does not capture Medicaid clients receiving Medicaid from the state Department of Health.

[15] Though we only know about other household members to the extent that they are eligible and listed on a case.

in the courts data to themselves in the benefits data after the case begins because whether they appear in the benefits data could be affected by the case. Our probabilistic record linking process therefore restricts matches to only those matches made prior to the filing date. The housing court data are linked to benefits records using first name, last name, and location. Our algorithm incorporates name rarity, robustness to misspelling, and the number of days between the benefits record and the housing court filing. For each year of our housing courts data, we have a minimum of three years of prior benefits data, and on average we have 8 prior years. The algorithm is described in full detail in Appendix D. We limit our matches to the most recent case for a given person-address-year. We are able to match roughly 40 percent of all housing court cases in our sample. We cannot be certain whether records fail to match due to errors in the data, problems with the matching procedure, or insufficient coverage of the data (not all households in housing court are likely to be eligible for and receiving benefits). Using observations from the 2013 American Housing Survey, we estimate that roughly 43 percent of households in New York City that report being threatened with an eviction notice had received either Food Stamps or public assistance in the past 12 months, suggesting that our matched sample covers a high proportion of low-income households in housing court.[16]

While our sample population represents a subset of all cases, we do not consider this to be a significant limitation for several reasons. First, we match a minority of cases, but our matched cases represent a majority of actual evictions. That is, the matched population is a population at greater risk of eviction and actually accounts for more than half of all evictions in housing court. Second, our sample, by virtue of being more likely to be low-income and receiving benefits, is also the population most likely to be targeted with any eviction prevention policies.[17] The somewhat higher-income population in housing court who are unlikely to be in the benefits data are more likely to be younger, male and without children (NYC Civil Justice Survey Report). Third, since we restrict our sample to those we can match at the listed address *before* their filing, we must drop a substantial fraction of households who appear in the benefits data at the filing address *after* the filing date. If we included these households, it would boost our match rate to about 57 percent of cases. Fourth, the age and gender characteristics of our matched sample are very similar a random sample of unrepresented tenants surveyed in NYC Housing Court in 2015.[18] Fifth, our matched sample originates from similar neighborhoods and is roughly as likely to be rent

[16]It should be noted that self-reported benefits receipt in household surveys suffers from well-known measurement error issues (Meyer and Wu 2018).

[17]For example, the expansion of legal aid to tenants in housing court in NYC provided legal representation for tenants earning less than 200 percent of the poverty line.

[18]We compare our population to the housing court population surveyed in Appendix A.

stabilized and owe similar amounts of rental arrears as the unmatched sample.

III.1 Outcome Data

We then link our matched housing court-benefits sample to a variety of outcome data sources using a combination of SSN, first name, last name, and date of birth (depending on availability) derived from the benefits records. Since this paper is focused on the effects of eviction on adults, we construct our outcome measures for the individual identified as the household head. Roughly 62 percent of our sample has only a single adult identified in the household, based on the benefits case. We identify household heads as the name appearing on both the housing court filing as the party name and in the benefits data. In instances when the housing court lists multiple tenants in the party and we are able to match multiple party names to the benefits records, we assign the oldest working-age (less than 62) adult to be the head of household. We focus on linking our outcomes to the adult heads to minimize possible measurement error introduced by matching to the historical benefits cases.[19]

To measure the effects of eviction on formal labor market outcomes, we link our sample to New York State quarterly earnings records from 2004-2016. Records were linked using social security numbers by the New York State Department of Labor (NYSDOL). NSYDOL data include quarterly earnings and detailed industry codes (six digit NAICS) and cover approximately 97 percent of New York State's non-farm employment, but they do not capture private household workers, student workers, the self-employed, or unpaid family workers. A crucial advantage of these data is the quarterly frequency, which allows us to condition on pre-filing earnings losses that develop only in the quarters immediately prior to filing. An important limitation is that we do not have similar earnings data for the bordering states of New Jersey or Connecticut. If eviction increases the probability that individuals leave New York State but not their true employment and earnings in the new state, then our employment and earnings estimates will be biased downward. We evaluate whether attrition out of state poses a significant threat to internal validity in Appendix Table AIX and find no evidence of differential attrition in our sample.

To quantify effects on homelessness, we match our records to administrative data covering applications to homeless shelters from the New York City Department of Homeless Services from 2003 to 2017. These data contain information on applications to and stays in New York City homeless shelters. New York City has a "right to shelter," and so all single adults applying to shelter are eligible for shelter. However, some families are diverted or

[19] We also repeat our analysis using all members from the cases where we are able to match multiple members on a housing court record, and our results do not change materially.

found ineligible. Since a family's eligibility for shelter can be affected by having an eviction in Housing Court, we focus on application to shelter to measure housing need rather than shelter eligibility. We also measure time spent in shelter.

We also link our housing court-benefits sample to public assistance records to measure the effects of eviction on receipt of public assistance. Recall that the original housing courts-benefits match was done on pre-filing characteristics to obtain key identifiers such as SSN and date of birth. By contrast, in this match we use name, SSN, and date of birth to match individuals in the housing courts-benefits sample to public assistance records for all years from 2000 to 2016. This allows us to construct quarterly histories of public assistance, before and after the housing court case filing, for all individuals in our housing courts-benefits sample. We construct three binary measures of benefits: receipt of cash assistance, emergency assistance, and food stamps. Cash assistance includes TANF and New York's Safety Net Assistance program. Emergency assistance refers to one-time cash grants, or "one shots," provided by the city. Formal evictions or generally "forced moves" can make an individual eligible for these grants.

We also use the public assistance data to construct a quarterly history of individuals' locations before and after the housing court case filing. We do this using address information in the public assistance data that is supposed to be updated any time a client has a change of address. This allows us to construct two residential mobility outcomes. The first is a binary indicator equal to 1 if an individual moved from the census block of the housing court case filing to any other block post filing. The second outcome is a running sum of the number of individual moves made since the filing date. This allows us to test whether eviction affects housing instability beyond the initial move-out.

An important caveat is that we only observe whether people move if their address is updated in the public assistance system. Thus, if eviction affects the probability that they receive any benefits, this could bias our residential mobility results. The direction of the bias is unclear: eviction could increase or decrease these interactions. We directly test this concern and find that our instrument is not significantly related to whether the household head has a post-filing address observation.[20]

Finally, we measure the effects of eviction on health by linking our sample to the New York State Department of Health's Statewide Planning And Research Cooperative System (SPARCS). This data set includes all inpatient and outpatient (including Emergency

[20] See Appendix Table AIX. We also find that our residential mobility treatment effect estimates are robust to restricting the sample to individuals having a case update (and thus a potential address update) within the past 1, 3, 6, or 12 months.

Department) hospitalizations in New York State from 2004 to 2016. An advantage of these data is that we can observe any hospitalization in New York State regardless of payer. A drawback is that we don't observe interactions with the health system that do not result in a hospitalization, such as primary care visits or fulfilling prescriptions.

For each hospitalization, the data include the date of intake, a primary diagnosis code, and a code for any procedure. Many of the individual ICD-9 codes are quite specific and rarely observed in our data. Similar to Currie and Tekin (2015), we use the Clinical Classifications Software (CCS) groupings produced by the Agency for Healthcare Research and Quality (AHRQ) to construct more general categories of diagnosed conditions, such as mental health or asthma-related conditions. By consolidating conditions, we reduce the number of separate hypotheses tested. Our groupings are documented in further detail in Appendix G. We construct hospitalization histories for a range of health conditions before and after filing.

III.2 Descriptive Statistics

Table I provides summary statistics for our estimation sample along with a randomization check discussed in the next section. Our analytical sample includes nearly 200,000 separate non-payment cases and more than 170,000 unique household heads. The sample of adult household heads is 70 percent female and is predominantly black and/or Hispanic. The average household head in our sample is approximately 44 years old. On average, there are about 0.86 children per household. 52 percent of households have received food stamps in the year prior to filing, and 24 percent have received some type of cash assistance during the year prior to filing. 18 percent of household heads have previously applied to or stayed in homeless shelter at some point in the past.

The average amount claimed by the landlord for non-payment cases is $3,900, which we estimate to be two to three months of back rent. 47 percent of tenants have had a previous filing in housing court (though this could be an earlier filing at the same address). The average tenant lives in a fairly impoverished neighborhood, with a poverty rate close to 30 percent. 67 percent of cases are in buildings with at least some rent stabilization, though we cannot determine if the actual unit itself is rent stabilized. 44 percent of household heads are employed in the quarter of filing, and average quarterly earnings before filing is roughly $3,000.

In terms of health conditions, 81 percent of household heads have experienced some hospitalization in the years prior to filing. The most common causes for previous hospitalizations are respiratory infections, mental health issues, and other types of infections.

IV Research Designs

In this section we describe our two complementary research designs: instrumental variables and panel difference-in-differences. Our primary design uses the tendency of a randomly assigned courtroom to evict as an instrument for whether individuals are actually evicted. We show that the instrument is uncorrelated with baseline characteristics and a good predictor of eviction status. However, for labor market outcomes particularly, the instrumental variables estimates are imprecise because only a modest fraction of the sample are compliers. We therefore also include a complementary panel difference-in-difference specification, which is very precise. Overall, we find that the two designs yield similar results, and we interpret the effects of evictions accordingly.

IV.1 Instrumental Variables Design

Our primary research design exploits the random assignment of housing court cases to courtrooms to identify the causal effect of court-ordered evictions. The basic idea is to use the overall eviction rate in an individual's randomly assigned courtroom as an instrument for whether they are actually evicted.[21] This strategy amounts to comparing the outcomes of households randomly assigned to courtrooms where ex ante risk of eviction is higher to the outcomes of households randomly assigned to courtrooms where risk of eviction is lower. Because tenants are assigned to courtrooms through a random, computer-generated process, tenants assigned to different courtrooms are statistically equivalent. Differences in eviction rates arise only through differences in the courtroom actors, and differences in courtroom actors are unrelated to individual tenants' characteristics. This method has been used persuasively to study the effects of other treatments that could not ethically or practically be randomly assigned, including juvenile incarceration (Aizer and Doyle 2015), adult incarceration (Kling 2006; Mueller-Smith 2015), pre-trial detention (Dobbie et al. 2017), consumer bankruptcy protections (Dobbie and Song 2015), and disability insurance claims (French and Song 2014; Maestas et al. 2013). We are the first to apply this research design to study evictions.

[21] We exclude each individual's own eviction outcome when calculating the overall courtroom eviction rate.

Since randomization occurs on a rolling basis as cases originate in housing court, we follow the approach of Dobbie et al. (2017) and Bhuller et al. (2016) and define our instrument as the leave-one-out (excluding each individual's own case) eviction rate after removing time effects. In our case, we remove court-year-month-day-of-week effects since randomization is done within each borough courthouse. We then de-mean by court-year average (again leaving out the individual's own case). We construct our instrument using the courtroom to which the case is first assigned.[22] This captures the relative eviction rate for a courtroom compared to other courtrooms in the same court (Bronx, Brooklyn, Manhattan, or Queens) at the same time. Formally, for individual i assigned to courtroom j in court c in year y and handled at time t, their instrument value is calculated as:

$$Z_{icjt} = \frac{1}{n_{cjy} - 1} \left(\sum_{k \neq i}^{n_{cjy}} \widetilde{\text{Evicted}}_{kcjt} \right) - \frac{1}{n_{cy} - 1} \left(\sum_{k \neq i}^{n_{cy}} \widetilde{\text{Evicted}}_{kct} \right) \tag{1}$$

$$\widetilde{\text{Evicted}}_{ict} = \text{Evicted}_{ict} - \hat{\gamma} \boldsymbol{X}_{ct}$$

where n_{cjy} is the number of cases heard in court c in year y and courtroom j, and n_{cy} is the number of cases heard in court c and year y.[23] $\widetilde{\text{Evicted}}_{ict}$ is the eviction residual after removing the court-time fixed effects: $\boldsymbol{X}_{ct}$.

IV.2 Instrument Relevance

We implement our first stage in equation 2. Evicted_i is an indicator equal to one if individual i is ever evicted and zero otherwise. X_i includes pre-filing tenant characteristics such as race/ethnicity, age, and numerous other characteristics listed below. We include year-by-month-by-day-of-week fixed effects (λ_{ct}) to mitigate concern about compositional changes over time:[24]

$$\text{Evicted}_i = \alpha + \gamma Z_{ijct} + \lambda_{ct} + \delta \boldsymbol{X}_i + \eta_i \tag{2}$$

We find a strong statistical relationship between the relative leave-one-out rate of eviction in the courtroom and the probability of eviction. Our first stage results appear in Table II. Column (1) contains our court-time fixed effects but no controls. Column (2) adds our full list of controls. A move from the most lenient courtroom ($Z = -0.03$) to

[22] In rare instances, cases may be re-assigned to a specialized part (drug, military, night, etc.) if the case's initial assignment is not correct or to a trial part if the landlord or tenant agree to go to trial.

[23] In the typical year, our analytical sample contains 28 unique courtrooms across the four boroughs/courts.

[24] Throughout, we will refer to court by year by month by day of week of filing fixed effects as simply "court by time of filing."

the least lenient courtroom ($Z = 0.05$) raises the probability of eviction by 6.3 percentage points ($0.79 \times (0.05 + 0.03)$), a 33 percent increase over the mean (19.2 percent). The residualized eviction rate is a good predictor of eviction, with a large F-statistic on the excluded instrument of nearly 100.

We plot a local-linear version of our first stage in Figure I along with the distribution of the instrument. The range of the instrument is narrow, with a standard deviation of 1 percentage point, or about a 6 percent change in eviction risk, but is not dissimilar to the literature (Dobbie et al. 2017; Bhuller et al. 2016). However, it suggests that our IV estimates, which attempt to extrapolate from this narrow range to the effect of moving from 0 percent to 100 percent eviction risk, are likely to be imprecise. We therefore interpret our IV estimates with this caveat in mind and note the similarity of the sign and size of our IV point estimates to the OLS and difference-in-differences point estimates, which are much more precisely estimated.

We construct our instrument according to the courtroom ("part") to which a case is first assigned. Judges rotate through parts on year-long terms, but each case is assigned to the part rather than the judge, so if the judge rotates out the case remains in the originally assigned part. Our data do not include the judge, making it difficult to determine whether it is the judge alone influencing cases. In practice, the judge, court attorney, courtroom configuration, and space for negotiation could all impact the likelihood and timing of eviction, which our instrument reflects. We assess whether the instrument is correlated with court actions that reflect relatively more or less leniency in Appendix Table AIII. Our instrument is positively correlated with a judgment against the tenant and is positively correlated with whether the judge denies an Order to Show Cause (OSC). While unsurprising, this gives us confidence that the instrument is related to court actions that increase the probability of eviction. Our instrument is strongly negatively correlated with the average time to eviction for evicted tenants, consistent with anecdotal evidence that tougher judges may be associated with a more accelerated eviction process. Thus, being assigned to a courtroom with a high value of the instrument means that the tenant is more likely to be evicted, more likely to have the eviction occur more quickly, more likely to receive a judgment against them, and more likely to be denied a request to halt the eviction.

We estimate our IV model via two-stage least squares using equation 3, where $\widehat{\text{Evicted}}_i$ represents the fitted-values from equation 2 and β is the parameter of interest, the causal effect of being evicted. Our regressions are at the case level, where y_i is the outcome for the head of household on case i. Since randomization to courtrooms is within county/borough, we include court by time of filing fixed effects, λ_{ct}, in all of our models. Finally, we include a rich set of pre-filing characteristics in $\boldsymbol{X}_i$, including race/ethnicity; age; gender; number of adults and number of children on the matched benefit case; two years of quarterly earnings history (including earnings and employment in the quarter of filing); two years of benefits history; an indicator for previous applications to homeless shelter; poverty rate of the census tract of filing address; hospitalization history since 2004; rent amount owed (as claimed by the landlord); an indicator for legal representation; rent stabilization status of the building; and indicators for previous housing court cases. We also include zip code-by-year fixed effects for 2015 and 2016 to account for a zip code-based roll out of legal aid to tenants in housing court, though it has little impact on our estimated effects. Throughout, we report Eicher-White robust standard errors two-way clustered at the courtroom-year and individual levels. This clustering reflects the design of housing court, where tenants assigned to the same courtroom at the same time receive a common shock to eviction risk (Abadie et al. 2017).

$$y_i = \alpha + \beta \widehat{\text{Evicted}}_i + \lambda_{ct} + \phi \boldsymbol{X}_i + \varepsilon_i \tag{3}$$

IV.3 Instrument Exogeneity

If our instrument is exogenous, it should be as good as randomly assigned conditional on court and time of filing. We show in Table I that the instrument is in fact uncorrelated with a range of baseline individual characteristics. We split the sample into individuals randomly assigned to courtrooms with above and below median values of the instrument and report the average characteristics of these two groups.[25] Consistent with randomization, the average characteristics are extremely similar across more and less lenient courtrooms. In Column 3 we report the p-values from a series of separate bivariate regressions of a given characteristic on our instrument.[26] Across more than 30 characteristics, only one is significant at the 5% level, and the sizes of the differences are not economically meaningful. We also conduct an omnibus test of these differences and fail to reject the null hypothesis that that there are no differences between the two groups.[27]

We also show in Appendix Table AII that our instrument is uncorrelated with baseline characteristics that are themselves important predictors of eviction. In Column (1) we regress the endogenous treatment variable, eviction, against our pre-filing characteris-

[25]Comparisons are always within court-month-day by construction of the instrument.

[26]Specifically, we regress $x_i = \gamma Z + \lambda_{ct} + \varepsilon$ and report the p-value for the coefficient γ. For the instrument, we report the t-statistic for the coefficient β from a regression of $Z = \beta \mathbf{1}(\text{Above Median}) + \nu$.

[27]To conduct this test, we stack each characteristic as an outcome Y in a seemingly unrelated regression framework (SUR) and conduct a joint F test on the instrument, clustering standard errors at the household level. This yields a p-value of 0.31.

tics. Households that have applied to homeless shelters before, who have mental health or substance abuse hospitalizations, who are black, who live in buildings with some rent stabilization, and who reportedly owe more back rent to their landlord are at greater risk of eviction. Households with legal representation, those with a female-head, and older households are less likely to be evicted. In Column (2) we repeat this regression but replace the eviction flag with our continuous instrument. We again find that baseline characteristics are unrelated to the leniency of the courtroom that is initially assigned.

IV.4 LATE Interpretation

In order for our IV estimates to be interpreted as Local Average Treatment Effects (LATE) of Imbens and Angrist (1994), our instrument must also meet an exclusion restriction and be monotonic in eviction risk. The exclusion restriction implies that the randomized courtroom assignments should have no effect on the outcome except through its effect on actual evictions. This assumption could fail if stipulation terms or other actions taken by a judge affect households through channels other than eviction.

Monotonicity requires that if a given courtroom has a higher risk of eviction than other courtrooms, then it must (weakly) raise risk of eviction for everyone assigned to that courtroom relative to their risk if they had been assigned to a lower risk courtroom. This can fail in randomized screener designs if, as in Mueller-Smith (2015), judges are relatively harsh on some types of cases (or for certain types of individuals) while relatively more lenient on others. In our setting, for example, monotonicity would fail if courtroom A is more lenient than courtroom B in cases where the tenant is rent stabilized but courtroom B is more lenient than courtroom A in cases where the tenant is not rent stabilized. This could be more likely in circumstances where multiple courtroom actors can influence a case outcomes, such as ours. To test for non-monotonicity, we construct our instrument within ten different sub-samples based on individual or case characteristics. We then re-estimate our first stage using the within-group instrument over the excluded group sub-sample to check for a positive relationship. Our approach is akin to the "reverse instruments" constructed in a recent paper by Norris et al. (2018). The results are reported in Appendix Table AIV. In column 1, we test whether courtrooms that are relatively more lenient for male respondents are relatively less lenient for female respondents and we find this is not the case: being assigned to a courtroom with an eviction rate that is one percentage point higher for male tenants raises a female tenant's risk of eviction by 0.4 percentage points. Across all characteristics and case types, we find that less lenient courtrooms raise everyone's risk of eviction (positive, significant results across all groups), consistent with monotonicity. Stated differently, we find no evidence that courtrooms that raise eviction risk for some types of individuals lower eviction risk for other types of individuals. We find similar results when constructing our instrument within each of these categories, consistent with monotonicity holding across them.

To facilitate comparison of our IV LATE estimates to our OLS estimates, we present results where we re-weight our OLS estimates to better match the characteristics of the complier population. Similar to Bhuller et al. (2016) and Dobbie et al. (2017), we first evaluate which characteristics are disproportionately represented in our complier population relative to the sample overall.[28] We find that younger households (those with a head of household under 45), households with a male head, and those with a history of shelter use are more likely to be compliers. Thus, our IV estimates are more indicative of the effects of eviction for this population. We therefore re-weight our OLS regressions so that the estimated effects more closely reflect those of the complier population.

IV.5 Difference-in-Differences

We complement our IV strategy with a panel difference-in-differences specification. The specification appears in equation 4, where i denotes individual cases, c denotes court, s is calendar time, and t is event-time relative to filing (which ranges from $[-8, 16]$) :

$$y_{ict} = \alpha_i + \alpha_{c,t} + \alpha_{c,s} + \left[\sum_{t \neq 0} \beta_t \mathbf{1}\left(\text{Evicted}_i\right)\right] + \pi_{x,t} + \varepsilon_{it} \tag{4}$$

We include individual fixed effects α_i, a court-by-*calendar time* fixed effect $\alpha_{c,s}$, a court-by-*event time* fixed effect $\alpha_{c,t}$, and an age-by-calendar time fixed effect $\pi_{x,t}$. We exclude the base quarter of filing ($t = 0$), β_0, since almost no households are actually evicted in the quarter of filing.[29] We construct plots of the effects of eviction from this specification, which we discuss in the results section. The identifying assumption is the standard parallel trends condition: that in the absence of eviction, outcomes would have evolved similarly before and

[28] We are interested in which characteristics are more represented in the complier population, $\frac{P[X=x|\text{complier}|]}{P[X=x]}$, which by the definition of conditional probability is equivalent to $\frac{P[\text{complier}|X=x]}{P[\text{complier}]}$, which we can estimate as our first stage for the group with $X = x$ over the first stage for the sample overall. We estimate this ratio for mutually exclusive discrete variables for race, ethnicity, gender, age, prior employment, amount owed, and prior homelessness. We select the characteristics that are most over- (or under-) represented in our complier population. These are age (< 45 and $>= 45$), male-headed household (or not), and prior applications to shelter (or not). We construct our complier weights using these characteristics. To do this, we divide our sample into eight mutually exclusive age-household composition-prior homelessness groups, estimate the first stage for each of the eight group, then construct our weights as the share of compliers relative to the share of the estimation sample in each group. See Appendix B.

[29] The estimates are extremely similar if we define $t = -1$ as the base year.

after filing for evicted and non-evicted households. While this assumption is not directly testable, we find generally similar pre-trends for evicted and non-evicted households in the lead up to filing for most of our outcomes.[30] To more easily summarize these effects in tables, we adjust the specification in equation 4 by grouping the post-filing quarters into four annual dummies ($\beta_1 \in [1,4], \beta_2 \in [5,8], \beta_3 \in [9,12], \beta_4 \in [13,16]$) and estimate the difference-in-differences with these four dummies. To account for outcomes possibly being correlated across courts and time, we cluster our standard errors conservatively at the court-by-year-by-quarter level for our difference-in-differences specification.

V Graphical Evidence

Before turning to our model estimates, we provide graphical evidence on the effects of eviction using our detailed quarterly panel of outcomes. For each outcome, we construct plots of the mean outcomes for evicted and non-evicted households in each quarter. We residualize these plots by court by time of filing, court by calendar time, and calendar time by age fixed effects. This leaves just residual variation in time relative to filing and across eviction status. For each plot we add back the quarterly mean to aid interpretation.

These raw difference figures are instructive about the possible magnitude of effect sizes we might observe and secular trends through the housing court interaction. However, they could be misleading about the causal effects of eviction if the timing of evictions are strongly endogenous. Thus, we also include reduced form plots that compare the trajectory of households randomly assigned to a courtroom where they are more likely to be evicted (top quintile) to those assigned to a court room where they are less likely to be evicted (bottom quintile). These are a visual complement to our IV strategy. In addition to providing prima facie evidence of the causal effects of eviction, they also provide further validation of the randomization by illustrating not just similar levels before filing but also similar trends. For these reduced form plots, the difference in eviction rates between the two groups is a modest 4 percentage points (or about 20 percent of the base rate). We therefore do not impose uniform scaling across the two types of figures.

Figure II displays the probability of applying to homeless shelters by quarter relative to the quarter of filing in housing court. There is a modest decline in the rate of shelter application approaching the quarter of filing. In the quarter after filing there is an immediate, large jump in the probability of shelter application among evicted households, while non-

[30] We also estimated an event study design using only variation in the timing of eviction (rather than case filing) and found very similar effects. Results are available upon request.

evicted households see only a modest increase in shelter application. The spike in shelter application peaks in the second quarter after filing for households that are evicted. The vast majority of evictions occur within the two quarters after the quarter of filing, consistent with evictions having a direct causal effect on homeless shelter use. The reduced form plot again shows nearly identical levels and trends leading up to the filing date. After filing, those assigned to courtrooms that are more likely to evict (top quintile) have elevated rates of shelter application relative to those assigned to courtrooms that are less likely to evict, though the magnitudes are small given the small difference in courtroom stringency between the two groups (4 percentage points). Nevertheless, this provides suggestive graphical evidence that eviction causally increases the probability of shelter application.

Next, we explore the health impacts of eviction by plotting hospitalizations by quarter relative to filing. The probability of being hospitalized in any given quarter is low, and as a consequence some of the series are quite noisy. The most visually suggestive effect is for mental health, with hospitalizations jumping and peaking around the time of eviction. We plot this separately in Figure III along with the reduced form in the lower panel. We also plot event study series for a range of other hospitalization conditions separately for evicted and non-evicted households in Figures AV and AVI. Several of the raw comparison plots suggests that eviction might be associated with increased probability of hospitalization for certain conditions including mental health, asthma, and hypertension. However, the reduced form plots suggest many of the relationships may not be causal.

Figure IV shows mean earnings by quarter relative to filing. Unsurprisingly, the onset of a non-payment filing in housing court is preceded by a dip in earnings. This loss in earnings appears for households regardless of eventual eviction status, but evicted households experience a sharper drop in earnings prior to filing. Our reduced form plot (bottom panel of Figure IV) compares the households randomly assigned to a more stringent courtroom (top quintile) to those randomly assigned to a less stringent courtroom (bottom quintile). The reduced form plot suggests similar pre-filing trends, consistent with randomization. It also shows modest earnings declines in the quarters immediately following filing for both groups, but no clear pattern of differential earnings trajectories between households assigned to more and less lenient courtrooms.

We plot quarterly employment in Figure V. The patterns are similar to those for earnings. There is a slight drop in employment for all households prior to filing, but a steeper drop in employment (though still modest in magnitude) for evicted households after filing. Because employment is measured as positive earnings, it is likely that it lags actual job loss by up to one quarter. The reduced form plot again points to similar pre-filing trends and small declines in employment immediately after filing, with little discernible difference in the pattern of employment between households assigned to more and less lenient courtrooms.

Figure AI shows receipt of cash assistance over time. Raw trends show that receipt trends upward leading up to court filing, peaks one to two quarters after filing, then decreases. The reduced form shows slight evidence that eviction increases cash assistance receipt. Figure AII shows similar patterns for food stamps and Figure AIII for emergency assistance, though there is less evidence of causal effects from the reduced forms (note the scale of the y axes). Overall, there is little graphical evidence that evictions substantially affect benefits receipt.

VI Results

In this section we present OLS, IV, and panel difference-in-differences results on the effects of eviction on employment and earnings, public assistance, homelessness, and hospitalizations. Our data set is an unbalanced panel covering cases with filing dates from 2007 to 2016, and our outcomes data run through the fourth quarter of 2016.[31] We do not have a consistent number of post-filing quarters for each case: earlier cases will have more post filing quarters, and more recent cases will have fewer. However, our inclusion of time-of-filing fixed effects means all comparisons are between individuals filing at the same time. For each outcome, we first investigate the effects over post-filing quarters 1-8 (Years 1-2) to capture any immediate impacts of eviction. We also estimate the effect of eviction over all post-filing quarters (All Years). For benefits receipt, homelessness, and hospitalizations, we estimate the effect on the likelihood that an individual ever appears with the particular state during any of the post-filing quarters we observe them. For labor market outcomes, we focus on earnings and employment in the short run (Years 1-2) and in five years of post filing quarters (Years 1-5).[32] We follow Bhuller et al. (2016) and report the effect on cumulative post-filing earnings since we care primarily about the total effect of eviction on income, but we also report the effects on averaged quarterly earnings over the selected windows. For employment, we focus on the share of quarters employed so as to capture the stability of employment.

Before turning to our outcomes, we evaluate whether our treatment - a completed eviction - results in a meaningful difference in housing stability. If, for example, everyone entering housing court is de facto evicted, such as informally forced out through landlord pressure, then the "treatment" we are evaluating is unlikely to be important. We first test whether those that avoid eviction appear to remain at their initial address post-filing. We

[31] Homelessness runs through Q1 2017.

[32] We find similar patterns, though less precise, when we estimate effects of earnings across all post-quarter filings.

also test whether evictions cause individuals to move more often in the period after eviction. Persistent housing instability has been hypothesized as a key mechanism through which evictions might lead to adverse outcomes (Desmond 2016). As described in Section 3.1, we rely on our public assistance records to measure mobility, so we restrict the sample to cases for which we have a post-filing public assistance case, which is slightly more than half our sample.

Our results on moving appear in Appendix Table AI. Column 1 presents outcome means for those not evicted to aid interpretation of point estimates. OLS results with full controls (Column 3) suggest that evicted individuals are 25 percentage points more likely to make any move in the two years after filing, on a base of 28 percentage points. IV point estimates suggest that these associations are causal: eviction increases the probability of moving within two years of filing by 40 percentage points and increases the number of moves made in the first two years after filing by 0.8 to 0.9. The fact that OLS results are smaller than IV estimates suggests that unobservables that increase the probability of eviction actually decrease the probability of moving and number of moves. Using all post-filing moves, we test whether the coefficient on the number of moves is indeed larger than the coefficient on moving once (Pr(Moving)), and this difference is statistically significant. This suggests to us that eviction increases residential mobility beyond what would be expected by the immediate move from the housing unit disputed in court. This is consistent with eviction leading to persistent housing instability as described by Desmond (2016).

VI.1 Effects on Homelessness

Evictions are frequently cited as an important cause of homelessness, but previous work has been unable to separate the role of evictions from other factors of disadvantage. Table III reports our estimates of the effect of eviction on shelter application and time spent in shelter. OLS results with full controls (Column 3) suggest that evicted individuals are 16 percentage points more likely to apply to shelter in the first two years after case filing, on a baseline shelter application rate of 3.4 percent. Evicted individuals also spend a greater share of days in shelter: around 6 percent more days in shelter on a base of 1 percent. Our complier-weighted OLS estimates are higher than OLS unweighted, suggesting compliers may be somewhat more affected by eviction than our sample as a whole. The difference-in-differences estimates point to similarly large increases in homelessness when the dependent variable is defined as quarterly applications to shelter. Since we don't expect households to continually apply for shelter each quarter, we estimate a cumulative effect on applications to shelter by summing the quarterly coefficient in each year. This is closer to our "ever applied" dependent variable, though it will capture some reapplications. It again finds very large effects, with eviction causing a 19 percentage point increase in applications to shelter in the year after filing.

Turning to our IV estimates, we find large effects on shelter application, with eviction causing application to increase in the 1-2 years after filing by about 14 percentage points. These are similar to our unweighted OLS estimates with controls, but notably smaller (about 40 percent) than our complier-weighted OLS estimates, suggesting that selection may explain some of our OLS results. We find that eviction also increases the share of days spent in shelter during the first two years after filing by 5 percent, or about 36 days.[33] Across all post-filing quarters, we find eviction increases the probability of applying to shelter by 12 percentage points and share of days in shelter by 4 percentage points, though these estimates are less precise. Importantly, this suggests that averting evictions isn't simply delaying an inevitable bout of homeless but leading to persistently different housing stability. Thus, interventions targeted near the time of eviction could be effective in keeping individuals out of shelter long-term. We view this finding as consistent with economic models of homelessness that emphasize the transitory dynamics of homelessness.

VI.2 Effects on Health

To examine whether evictions have a causal effect on adult health, we link our sample to data covering all hospitalizations in New York City. Evictions could conceivably affect adult health through several different channels: stress brought about by housing instability, changes in housing quality and exposure to health hazards, and changes in neighborhood environment. Evictions could also worsen health if evicted households are more financially constrained and cut back on preventative care or healthy behavior to afford new moving costs, such as a security deposit or broker's fee.

Table IV reports the effects of evictions on select health outcomes, where we focus on summary measures and more common conditions in our sample. Appendix Table AV reports results across the remaining diagnosis codes. Our OLS estimates imply that an eviction in housing court raises the probability of being hospitalized in an emergency room in the 1-2 years after filing by roughly 3.5 percentage points. Our IV estimates, while imprecise, imply that eviction increases the number of emergency room visits by about 0.38 visits in the 1-2 years after filing, an increase of about 70 percent over the mean for non-evicted households.

[33]The average number of post-filing days we observe shelter status is 640. Given our estimate of 0.055, we can infer that eviction increases days in shelter by about 36 days.

This is largely driven by sizable increases in the probability of a mental health hospitalization. Both OLS and IV estimates point to large effects of eviction on mental health hospitalizations. These estimates are not sensitive to the inclusion of a range of controls that predict hospitalizations. For the marginal case, being evicted in housing court increases the probability of being hospitalized for a mental health diagnosis in the 1-2 years after filing by 9 percentage points. This effect is large but imprecisely estimated. The OLS estimate of 2.6 percentage points is smaller than IV, but substantively large, representing a 68 percent increase relative to non-evicted households. The OLS estimate is larger when we weight the observable characteristics to be more similar to our IV compliers, which suggests the complier population may be more susceptible to mental health stress. We cannot rule out that our estimated effects simply reflect increases in utilization rather than increases in the underlying incidence of mental health distress. That said, the increase in probability of mental health hospitalizations is composed almost entirely of emergency department visits, with the most frequent mental health diagnosis being anxiety-related, suggesting the presence of acute increases in mental health distress. Further, this effect is concentrated primarily in the year after filing.

We plot the effect of eviction on the probability of quarterly mental health hospitalization at the bottom of Figure VII from estimating equation 4. There is no differential pattern in hospitalizations in the lead up to filing. After filing, we find a statistically significant jump in the effect of eviction. These effects peak in quarters 2 and 3 after filing, when most households have been evicted. Corresponding regression results are in Table VII. Since our dependent variable is quarterly probability of mental health hospitalization (rather than indicator for any mental health hospitalization after filing), it is not directly comparable to our OLS specification in Table IV, so we also include a cumulative effect using the sum of the quarterly coefficients in each year. Both tables imply large increases in risk of mental health distress.

Finally, to capture overall health we construct an index of health that sums standardized values of hospitalizations for all the conditions listed in Appendix Table AV. Eviction lowers the health index by one tenth of a standard deviation for the average case (OLS) and nearly one-half a standard deviation for marginal cases. Taken together, our estimates suggest evictions worsen health, particularly mental health, and substantially increase emergency room use.

VI.3 Effects on Earnings and Employment

To assess whether evictions cause an economically significant increase in poverty among low-income adults, we estimate its effect on earnings, employment, and public assistance receipt. Theoretically, evictions could be expected to reduce or increase labor supply. They could reduce labor supply by limiting the mental bandwidth of low-income renters. Mullainathan and Shafir (2013) argue that when individuals are mentally taxed, they are less likely to engage in careful, deliberate planning and decision-making. Evictions could directly tax the bandwidth of low-income households by forcing them to search for new housing, hence limiting mental reserves that be could used to hold down a job, manage work hours, or find a new job. However, they might also be expected to increase labor supply. In some ways, non-payment of rent is equivalent to a line of credit, with housing court resembling a debt collection process and late fees an interest rate. The removal of this de facto line of credit through eviction could increase labor supply through the removal of a credit smoothing channel. An analogous example is foreclosure delay for defaulting homeowners. Herkenhoff and Ohanian (2015) examine the relationship between foreclosure delay and labor supply and find that foreclosure increases the probability of employment but lowers earnings. In their model, households forced out through foreclosure find jobs more quickly, but match quality and earnings are lower. Their model would similarly suggest that evictions could increase labor supply along the extensive margin in the short run but produce lower earnings.

Table V reports our estimates of the effect of evictions on earnings and employment. Controlling only for court-by-time of filing fixed effects (Column 2), we find that eviction is associated with an approximately 3,000 dollar reduction in total earnings in the 1-2 years after filing, or about 13 percent of the mean earnings of non-evicted households.[34] Adding a host of controls, including quarterly earnings and employment history up to and including the quarter of filing, shrinks these OLS earnings estimates by about one half, to -1,760 dollars, or about 8 percent. Our IV point estimates are similar, suggesting a reduction in cumulative earnings of about $1,000 in years 1-2. A similar pattern exists for employment: OLS estimates with full controls suggest that evicted individuals are around 1 percentage point less likely to be employed. Our IV point estimates for the first 1-2 years after filing are in general quite similar to the OLS estimates, suggesting that these results may be causal, though they are considerably more imprecise than the OLS estimates.[35] Patterns are similar

[34] All dollar amounts are in 2016 dollars.

[35] One concern is that our IV results are exhibiting the well-known "weak instrument bias," where IV is biased towards OLS in the finite sample. There are several reasons to think this is not the case. First, in just-identified cases such as ours, IV is not necessarily biased toward OLS. Second, when we estimate our reduced form effects using standard OLS, we get very similar point estimates and standard errors. Third, we

when looking at effects on earnings 1 to 5 years post filing.

We plot the effects of eviction by quarter from our non-parametric difference-in-differences specification (equation 4) in Figure VIII. Our estimates from the panel difference-in-differences specification appear in Table VII. These are similar to our cross-sectional estimates from OLS and IV, suggesting that eviction results in a percentage point reduction in quarterly employment and a reduction in earnings of roughly 200 dollars.

Overall, we find evidence that eviction results in modest reductions in cumulative earnings of about 5-7 percent, lowers quarterly earnings, and reduces employment by around a percentage point. This contrasts with evidence from Desmond and Gershenson (2016), who use matching methods and cross sectional survey data asking retrospectively about housing instability to evaluate the relationship between forced moves from housing and job loss. They find that forced moves substantially raise the probability of job loss, with estimated increases in job loss as large as 22 percentage points. We measure only formal sector employment and therefore could be missing changes in informal sector work patterns. The sensitivity of our OLS results to controls suggests the importance of capturing detailed earnings and employment histories leading up to eviction, which they are unable to do with their data. We also construct a measure of job loss as employed in the filing quarter following a quarter with zero earnings so that we can as closely possible compare our own estimates to Desmond and Gershenson (2016). The results appear in Figure VI. While our respective parameter estimands are not necessarily equivalent, our OLS, Difference-in-Differences and IV estimates suggest much smaller effects of housing instability on job loss than this previous estimate.

VI.4 Effects on Public Assistance

Table VI reports results on the effects of evictions on receipt of public assistance. OLS with only fixed effects (Column 2) suggests that evicted individuals are a few percentage points more likely to receive cash assistance, food stamps, and emergency assistance in the first two years after filing. However, adding individual controls (Column 3) reduces these estimates substantially: associations with cash assistance and food stamps are reduced by one half to two thirds, and associations with emergency assistance fall to zero.[36] IV point

have a large first stage F-statistic that far exceeds the thresholds for weak instruments suggested by (Stock and Yogo 2005). Fourth, if we are concerned that our F-statistic is masking an underlying problem of many weak instruments and we instead estimate our first stage using all our courtroom-by-filing-year dummies entered separately (235 in total) then we get a resulting F-statistic of 14.

[36] We emphasize that emergency assistance is only available once to households, so if they receive it sometime before formal eviction then it is unsurprising that we find no effect of eviction on its receipt.

estimates range from slightly positive to slightly negative, with large standard errors. Our panel difference-in-differences estimates show small effects that vary in sign.

When we look at the effect of eviction on *any* benefits receipt across all years after filing, we find few important differences in the receipt of public assistance. Our OLS estimates point to very small differences. Moreover, the IV estimates are generally similar in sign and magnitude, though with large standard errors. Overall, these results, combined with the graphical evidence described above, suggest that evictions may have small, if any, effects on benefits receipt.

VI.5 Robustness

In the previous subsections we document that eviction increases applications to homeless shelters and hospitalizations for mental health diagnoses. In this section we explore the robustness of these results. First, we examine the relationship between our instrument and our outcome variables. We generate predicted values for each of several different outcome measures: applications to homeless shelters, mental health hospitalizations, and earnings over the two years after filing and across all post-filing quarters. We separately regress each variable on lagged values and the other baseline regressors and form our prediction as the fitted values for each outcome. Table VIII reports the results from a series of bi-variate regressions of these predictions on our instrument alongside our reduced form estimates using our instrument without any controls. Our instrument is uncorrelated with the predicted value of each outcome: each coefficient is approximately zero. By contrast, our reduced form estimates suggest sizable effects of the instrument on actual mental health hospitalizations and homeless shelter applications. This gives us confidence that our results are not driven by spurious correlation between our instrument and individual characteristics that predict our outcomes.

In Appendix Table AVI we re-estimate our primary results with different empirical specifications and find that the results are not particularly sensitive to our instrument construction or precise specification. In Columns (2)-(6) we present our key results with different constructions of our instrument and different combination of court-by-time fixed effects. In Column (7), we include zip code-year fixed effects across all years, which has little effect on our results.

Finally, a concern is that by examining so many outcomes we mechanically raise the risk of false positives. We address this multiple hypothesis testing in three ways. First, we have grouped our health measures to reduce the number of outcomes. Second, we construct a simple health index as in Kling et al. (2007) that equally weights all of our health measures and re-estimate our main specification. Finally, we calculate the family-wise error rate, which accounts for the increase in probability of incorrectly rejecting one or more null hypotheses when multiple hypotheses are tested from the same "family" of hypotheses (where family can refer to models, subgroups, or outcomes). For the family-wise error rates, we follow Jones et al. (2018) and define our families in terms of the data set / outcome domain (homelessness, health) and use the re-sampling procedure of Westfall and Young (1993). We only calculate the family-wise error rate adjusted p-values from our IV design because our OLS and DiD results are extremely precise such that the adjusted p-values are only trivially different from conventional p-values. Appendix Table AX reports p-values from the Westfall and Young (1993) re-sampling method ("FWER p-value") and the conventional unadjusted p-values. As expected, the FWER p-values are somewhat higher, but they do not materially alter the interpretation of any of our results.

VI.6 Heterogeneity

In Appendix Table AVII we examine heterogeneity in our OLS results by sub-group. The effects of eviction appear to be quite similar across sub-groups. Eviction raises the risk of homelessness for everyone, with women, families with children, and those previously homeless experiencing the largest raw increase in application to shelter. Similarly, every group experiences an increase in mental health hospitalizations. The largest reductions in earnings occur among those who were employed in the year before filing. We also report our IV subgroup results in Appendix Table AVIII. We interpret these estimates with caution given the imprecision for smaller sub-groups. The results are broadly similar to our OLS results. Notably, the IV estimates suggest effects on homelessness even for households with no history of homelessness.

VII Conclusion

In this paper we provide the first causal estimates of the effects of eviction on several important adult outcomes. To do so, we construct a sample of nearly 200,000 non-payment cases from the New York City housing court and match them to administrative data covering employment, earnings, public assistance receipt, homeless shelter use, and hospitalizations. We use the tendency of a randomly assigned courtroom to evict to identify the causal effect of eviction. We find that evictions cause a large and persistent increase in risk of homelessness. Evictions also increase emergency room use and raise the risk of mental health hospitalizations, particularly for cases on the margin.

Given that eviction cases assigned to more or less lenient courtrooms are often decided by modest differences in allowable repayment timelines, these results suggest that short-term liquidity constraints may play an important role for households in housing court. Since the costs of eviction are considerable - in Appendix C, we describe a back-of-the-envelope cost-benefit calculation that shows that the ex post value of avoiding an eviction is approximately $8,000 - policies aimed at insuring low-income renters against adverse shocks may be welfare-enhancing.

On the question of whether formal evictions contribute substantially to poverty, we find muted effects on employment, modest reductions in earnings, and little effect on receipt of public assistance. These results, combined with the volume of low-income households evicted each year, suggest that formal evictions are probably not a principal driver of overall poverty in New York City. Those experiencing eviction are mostly already living in poverty, and their labor market outcomes are not dramatically different when they are evicted. However, it is possible that evictions could have a larger effect on poverty through significant negative effects on children. In concurrent work, we are studying the effects of eviction on child schooling, incarceration, and health outcomes.

There are a few limitations of this work. First, our results are only informative about the ex post, partial equilibrium effects of changing the probability that a tenant is evicted for non-payment. There could be important ex ante, general equilibrium effects on rental delinquency and rents more generally from changing the overall likelihood that delinquent tenants are evicted or end up in housing court. Second, we are unable to study "informal evictions," or those that may occur without a judgment for a Marshall eviction. Third, while we capture a number of important adult outcomes in the short and medium run, our data limit our ability to precisely estimate the long-run effects of evictions. Fourth, we are unable to say anything about how eviction affects landlord outcomes, such as their post filing rental income.Finally, our research design exploits shocks to housing stability but is not necessarily informative about the effects of long-term changes to housing affordability.

There are many unexplored areas for future research. Remarkably little is known about what triggers non-payment of rent. In future work we will use the data assembled here to examine drivers of non-payment and housing court appearances, such as drops in earnings, health expense shocks, and rent increases. There is also little evidence on how effective different policies, such as providing legal representation or short-term emergency cash transfers to renters in housing court, are at resolving financial distress and avoiding eviction. We are studying the effectiveness of legal representation in future work. Finally, it would be beneficial for policy to better understand the bargaining that takes place in housing court between tenants and landlords in the negotiation of settlements. This is a unique bargaining setting involving high stakes for disadvantaged households, who must make quick decisions under significant pressure and often with limited information. This setting offers a number of interesting avenues for behavioral economics.

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Table I: Summary Statistics and Randomization Check

	IV Sample		
	(1) More Lenient	(2) Less Lenient	(3) P-value
Instrument:			
Residualized Eviction Rate	-0.006	0.006	[14.53]
Pr(Evicted | X)	0.196	0.196	[0.14]
Demographics:			
Female	0.703	0.703	[0.15]
Black	0.589	0.594	[0.22]
Hispanic	0.441	0.439	[0.42]
Asian	0.056	0.056	[0.91]
White Not-Hispanic	0.083	0.083	[1.00]
Total Members	1.507	1.500	[0.06]
Total Children	0.861	0.868	[0.94]
Married	0.089	0.088	[0.14]
Age	43.918	43.915	[0.84]
Benefits:			
Food Stamps in Past Year	0.520	0.521	[0.38]
Cash Assist in Past Year	0.238	0.237	[0.20]
One-Shot Assistance in Past Year	0.213	0.215	[0.26]
Ever Homeless	0.182	0.183	[0.91]
Housing Court, Building, Neighborhood:			
Rent Owed (Thousands)	3.926	3.933	[0.76]
Had Previous Case	0.469	0.468	[0.72]
Tract Poverty Rate	0.287	0.286	[0.81]
Rent Stabilized	0.672	0.671	[0.87]
Labor Supply:			
Employed Prior to Filing	0.444	0.442	[0.35]
Earnings Prior to Filing (Thousands)	2.991	2.966	[0.41]
Predicted Earnings (Thousands)	2.764	2.746	[0.39]
Health (Hospitalizations):			
Any Visit	0.814	0.812	[0.10]
Infection	0.121	0.120	[0.19]
Cancer	0.041	0.041	[0.81]
Diabetes	0.032	0.031	[0.04]
Metabolic	0.039	0.038	[0.76]
Nervous System	0.055	0.054	[0.42]
Heart/Stroke	0.049	0.050	[0.59]
Heart Valve	0.031	0.030	[0.17]
Hypertension	0.042	0.043	[0.93]
Respritory Infection	0.280	0.278	[0.06]
Mental Health	0.132	0.134	[0.73]
Asthma	0.075	0.074	[0.90]
N	93330	93261	197575

This table reports the average characteristics for our sample divided by values of our instrument. Column (1) reports the mean value of each characteristic for the cases assigned to a more lenient courtroom: one that has a below median value of the instrument. Column (2) reports the mean value of each characteristics for the cases assigned to a less lenient courtroom: one that has an above median value of the instrument. Column (3) reports the p-value for the regression coefficient γ from a regression of the characteristic on our instrument: $x_{ict} = \gamma Z_{ict} + \lambda_{ct} + \varepsilon$, with standard errors two-way clustered at the courtroom-year and individual level. The exception is for differences in the instrument, for which we report a t-statistic from a regression of the continuous instrument on a dummy for being above median. We report the t-statistic because the p-value is exceedingly small. We also conduct an omnibus F-test of differences across all characteristics (excluding the instrument) and fail to reject the null (p-value: 0.31). This uses our combined housing court-benefits sample from 2007-2016. $Pr(\text{Evicted}|X)$ reports a propensity score estimated using the characteristics listed in the table (excluding the instrument). Dollar values are reported in thousands of 2016 dollars. Earnings Prior to Filing are quarterly earnings in the quarter before filing, as is employment status. Predicted earnings report the predicted post-filing earnings using the full set of covariates listed in Section 4 of the text. Demographics uses data from the matched benefits case file. Benefits uses receipt of public assistance from HRA and Shelter Applications from DHS. Housing, Building and Neighborhood uses the housing court records and filing address information linked to ACS 2006-2010 data (poverty rate) and rent stabilization data from DHCR. Labor Supply uses data from NYSDOL. Health reports data on hospitalizations from the SPARCS data system 2004-2016. See the text for additional details on the specification, outcome measures, and sample.

Table II: First Stage

	(1)	(2)
	Eviction	Eviction
Residualized Eviction Rate (Z)	0.805***	0.798***
	(0.0885)	(0.0833)
Observations	197541	197541
Controls	No	Yes
Court-Time-FE	Yes	Yes

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table reports our first stage: the effect of our instrument on an indicator for whether the case results in an eviction. Our instrument is the residualized eviction rate of the courtroom that a case is assigned to, leaving out the individual's own case. Column 1 includes only court-time of filing fixed effects. Column 2 adds the extensive controls listed in Section 4.1. Standard errors are robust and two-way clustered at the courtroom-year and individual level.

Table III: The Effects of Eviction on Homelessness

	Mean Not-Evicted	OLS Results			IV Results	
	(1)	(2)	(3)	(4)	(5)	(6)
Years 1-2:						
Pr(Apply to Shelter)	0.034	0.189***	0.160***	0.222***	0.127**	0.138**
		(0.004)	(0.003)	(0.006)	(0.062)	(0.062)
Share of Days in Shelter	0.010	0.070***	0.060***	0.095***	0.049	0.055*
		(0.002)	(0.002)	(0.004)	(0.032)	(0.033)
All Years:						
Any App to Shelter	0.050	0.217***	0.180***	0.237***	0.112	0.122*
		(0.005)	(0.003)	(0.006)	(0.068)	(0.071)
Share of Days in Shelter	0.015	0.081***	0.068***	0.096***	0.039	0.043
		(0.003)	(0.002)	(0.004)	(0.042)	(0.044)
Observations	197541	197541	197541	197541	197541	197541
Court-Time-FE		Yes	Yes	Yes	Yes	Yes
Controls		No	Yes	Yes	No	Yes
Complier Weights		No	No	Yes	No	No

This table reports estimates of the effect of eviction on measures of homelessness: the probability of application to shelter and the share of days spent in shelter. Column (1) reports the mean dependent variable for households not evicted to aid interpretation. The dependent variable is reported in each row. The dependent variable Pr(Apply to Shelter) takes the value of 1 if the household head on the case applied to stay in a homeless shelter during the time window after filing and zero otherwise. Share of Days in Shelter is the share of available post-filing days that the household resided in a homeless shelter. We report in two time windows for outcomes as described in Section 3: outcome data for available post filing quarters 1-8 (Years 1-2) and outcome data from all available post-filing quarters 1-40 (All Years). We present OLS in Columns (2)-(4) and IV estimates in Columns (5)-(6). For our IV models, the parameters are estimated via 2SLS as in Equation 3. All specifications include court-by-time fixed effects and fixed effects for 2015 and 2016 zipcode-year. When estimated with controls, the controls include race, ethnicity, age, gender, household composition, prior earnings and employment history, prior benefits history, previous applications to homeless shelters, tract poverty rate, hospitalization history since 2004, rent amount owed, legal representation, rent stabilization status of the building, and indicator for previous housing court case. Complier weights are described in Section 4 of the text. Standard errors, in parentheses, are clustered at the courtroom-year level. See the text for additional details on the specification, outcome measures, and sample.

Table IV: The Effects of Eviction on Health and Hospitalizations

	Mean Not-Evicted	OLS Results			IV Results	
	(1)	(2)	(3)	(4)	(5)	(6)
Years 1-2:						
Health Index	0.033	-0.061***	-0.068***	-0.094***	-0.370	-0.452*
		(0.007)	(0.006)	(0.015)	(0.252)	(0.261)
Any Emergency Visit	0.347	0.044***	0.016***	0.035***	0.047	0.165
		(0.003)	(0.003)	(0.006)	(0.117)	(0.113)
Num Emergency Visits	0.543	0.094***	0.036***	0.036***	0.139	0.377*
		(0.006)	(0.005)	(0.005)	(0.208)	(0.202)
Any Hypertension	0.013	0.002***	0.004***	0.002	0.024	0.026
		(0.001)	(0.001)	(0.002)	(0.028)	(0.028)
Any Mental Health	0.037	0.033***	0.025***	0.029***	0.088*	0.091*
		(0.002)	(0.001)	(0.004)	(0.053)	(0.054)
All Years:						
Health Index	0.055	-0.069***	-0.074***	-0.122***	-0.330	-0.414*
		(0.008)	(0.007)	(0.017)	(0.223)	(0.235)
Any Emergency Visit	0.347	0.044***	0.016***	0.035***	0.047	0.165
		(0.003)	(0.003)	(0.006)	(0.117)	(0.113)
Num Emergency Visits	0.543	0.094***	0.036***	0.036***	0.139	0.377*
		(0.006)	(0.005)	(0.005)	(0.208)	(0.202)
Any Hypertension	0.013	0.002***	0.004***	0.002	0.024	0.026
		(0.001)	(0.001)	(0.002)	(0.028)	(0.028)
Any Mental Health	0.037	0.033***	0.025***	0.029***	0.088*	0.091*
		(0.002)	(0.001)	(0.004)	(0.053)	(0.054)
Observations	197541	197591	197541	197541	197591	197541
Court-Time-FE		Yes	Yes	Yes	Yes	Yes
Controls		No	Yes	Yes	No	Yes
Complier Weights		No	No	Yes	No	No

This table reports estimates of the effect of eviction on inpatient and outpatient hospitalizations. Column (1) reports the mean dependent variable for households not evicted to aid interpretation. The dependent variable is reported in each row. The first row of each panel is a health index (mean: 0, SD: 1) constructed as the average of standardized hospitalization counts for diagnosis, where higher values of the index indicate "better" health (fewer hospitalizations for listed conditions) and lower values suggest "worse" health (more hospitalizations for listed conditions). Each subsequent dependent variable is an indicator for having any hospitalization in the time period with a primary diagnosis in the listed group. The diagnoses that make up each grouping are listed in Appendix E. We report two time windows for outcomes, as described in Section 6: outcome data for available post filing quarters 1-8 (Years 1-2) and outcome data from all available post-filing quarters 1-40 (All Years). We present OLS in Columns (2)-(4) and IV estimates in Columns (5)-(6). For our IV models, the parameters are estimated via 2SLS as in equation 3. All specifications include court-by-time fixed effects and fixed effects for 2015 and 2016 zipcode-year. When estimated with controls, the controls include race, ethnicity, age, gender, household composition, prior earnings and employment history, prior benefits history, previous applications to homeless shelters, tract poverty rate, hospitalization history since 2004, rent amount owed, legal representation, rent stabilization status of the building, and indicator for previous housing court case. Complier weights are described in Section 4 of the text. Standard errors, in

Table V: The Effects of Eviction on Labor Market Outcomes

	Mean Not-Evicted	OLS Results			IV Results	
	(1)	(2)	(3)	(4)	(5)	(6)
Years 1-2:						
Quarterly Earnings	3381	-399***	-210***	-169***	-717	-795
		(30)	(15)	(22)	(1381)	(628)
Cumulative Earnings	21179	-3009***	-1758***	-1545***	-1971	-995
		(220)	(118)	(186)	(8522)	(4949)
Employment	0.459	-0.022***	-0.013***	-0.009***	0.043	-0.003
		(0.003)	(0.002)	(0.003)	(0.112)	(0.070)
Years 1-5:						
Quarterly Earnings	3430	-409***	-247***	-187***	-995	-1131
		(30)	(17)	(25)	(1411)	(687)
Cumulative Earnings	37029	-6429***	-4411***	-4081***	-11772	-11586
		(502)	(322)	(451)	(17662)	(12883)
Employment	0.455	-0.023***	-0.016***	-0.011***	0.024	-0.025
		(0.003)	(0.002)	(0.003)	(0.114)	(0.071)
Observations	168163	168200	168163	168163	168200	168163
Court-Time-FE		Yes	Yes	Yes	Yes	Yes
Controls		No	Yes	Yes	No	Yes
Complier Weights		No	No	Yes	No	No

This table reports estimates of the effect of eviction on cumulative earnings ("Cumulative Earnings"), average quarterly earnings ("Quarterly Earnings"), and the percentage of quarters employed ("Employed"). Column (1) reports the mean dependent variable for households not evicted to aid interpretation. The dependent variable is reported in each row. We report two time windows for outcomes, as described in Section 6: outcome data for available post filing quarters 1-8 (Years 1-2) and outcome data from quarters 1-20 post-filing (Years 1-5). We present OLS in Columns (2)-(4) and IV estimates in Columns (5)-(6). For our IV models, the parameters are estimated via 2SLS as in Equation 3. All specifications include court-by-time fixed effects and fixed effects for 2015 and 2016 zipcode-year. When estimated with controls, the controls include race, ethnicity, age, gender, household composition, prior earnings and employment history, prior benefits history, previous applications to homeless shelters, tract poverty rate, hospitalization history since 2004, rent amount owed, legal representation, rent stabilization status of the building, and indicator for previous housing court case. Complier weights are described in Section 4 of the text. Standard errors, in parentheses, are clustered at the courtroom-year level. The sample is limited to cases with a household head age 18-65 and with a valid SSN. We verify that the instrument is uncorrelated with having a valid post-filing address record. See the text for additional details on the specification, outcome measures, and sample.

Table VI: The Effects of Eviction on Receipt of Public Assistance

	Mean Not-Evicted	OLS Results			IV Results	
	(1)	(2)	(3)	(4)	(5)	(6)
Years 1-2:						
Emergency Assistance	0.066	0.015***	0.005***	0.004**	0.051	0.044
		(0.001)	(0.001)	(0.002)	(0.045)	(0.041)
Cash Assistance	0.111	0.064***	0.018***	0.019***	-0.061	-0.029
		(0.003)	(0.002)	(0.004)	(0.070)	(0.048)
Food Stamps	0.321	0.066***	0.017***	0.029***	-0.007	0.004
		(0.003)	(0.002)	(0.004)	(0.108)	(0.081)
All Years:						
Any Emergency Assistance	0.260	0.034***	0.006***	0.007***	-0.008	0.007
		(0.001)	(0.001)	(0.001)	(0.033)	(0.022)
Any Cash Assistance	0.290	0.035***	0.006***	0.007***	-0.011	0.006
		(0.001)	(0.001)	(0.001)	(0.034)	(0.021)
Any Food Stamps	0.747	0.033***	0.007***	0.010***	-0.003	0.042
		(0.001)	(0.001)	(0.001)	(0.058)	(0.040)
Observations	125919	197541	197541	197541	197541	197541
Court-Time-FE		Yes	Yes	Yes	Yes	Yes
Controls		No	Yes	Yes	No	Yes
Complier Weights		No	No	Yes	No	No

This table reports estimates of the effect of eviction on measures of benefits use. Column (1) reports the mean dependent variable for households not evicted to aid interpretation. The dependent variable is reported in each row. Cash Assistance is the average quarterly rate of Cash Assistance receipt for the available post-filing quarters in the relevant time window. Any Cash Assistance takes the value 1 if the household head receives cash assistance in the available post-filing quarters in the relevant time window. We report two time windows for outcomes, as described in Section 6: outcome data for available post filing quarters 1-8 (Years 1-2) and outcome data from all available post-filing quarters 1-40 (All Years). We present OLS in Columns (2)-(4) and IV estimates in Columns (5)-(6). For our IV models, the parameters are estimated via 2SLS as in Equation 3. All specifications include court-by-time fixed effects and fixed effects for 2015 and 2016 zipcode-year. When estimated with controls, the controls include race, ethnicity, age, gender, household composition, prior earnings and employment history, prior benefits history, previous applications to homeless shelters, tract poverty rate, hospitalization history since 2004, rent amount owed, legal representation, rent stabilization status of the building, and indicator for previous housing court case. Complier weights are described in Section 4 of the text. Standard errors, in parentheses, are clustered at the courtroom-year level. We verify that the instrument is uncorrelated with having a valid post-filing address record. See the text for additional details on the specification, outcome measures, and sample.

Table VII: Difference-in-Differences: Effects of Eviction

	Mean $t = 0$	Year 1	Year 2	Year 3	Year 4
Labor Market:					
Earnings	3123.7***	-131.0***	-155.0***	-198.5***	-216.5***
	(4713.5)	(15.87)	(15.32)	(16.43)	(18.44)
Employed	0.463	-0.00933***	-0.0112***	-0.0137***	-0.0124***
	(0.499)	(0.00219)	(0.00214)	(0.00235)	(0.00216)
Public Assistance:					
Emergency Assistance	0.0846	0.00798***	-0.000145	-0.00544***	-0.00710***
	(0.278)	(0.00201)	(0.00248)	(0.00201)	(0.00208)
Cash Assistance	0.155	0.0141***	0.0104***	0.00290	0.0000951
	(0.362)	(0.00187)	(0.00233)	(0.00263)	(0.00300)
Food Stamps	0.411	0.00646***	-0.0108***	-0.0102***	-0.0115***
	(0.492)	(0.00218)	(0.00293)	(0.00316)	(0.00364)
Other Outcomes:					
Pr(Apply to Shelter)	0.00378	0.0477***	0.0175***	0.0107***	0.00856***
	(0.0614)	(0.00175)	(0.000837)	(0.000895)	(0.000989)
$\beta_{q=1,t} + \ldots + \beta_{4,t}$		0.190***	0.0691***	0.0424***	0.0336***
		(0.00693)	(0.00333)	(0.00362)	(0.00400)
Pr(Mental Health)	0.0114	0.00433***	0.00284***	0.00198***	0.000867
	(0.106)	(0.000869)	(0.000784)	(0.000858)	(0.000886)
$\beta_{q=1,t} + \ldots + \beta_{4,t}$		0.0174***	0.0114***	0.00786**	0.00343
		(0.00348)	(0.00314)	(0.00344)	(0.00354)

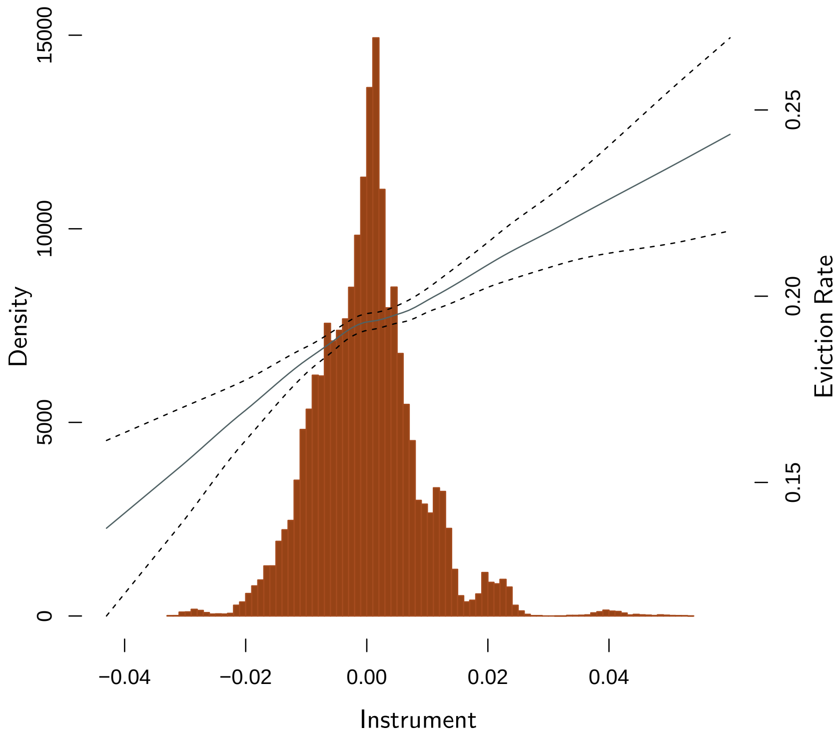
This table reports panel difference-in-difference estimates from equation 4, where post-filing quarter dummies are replaced with annual dummies. The regression includes 197,541 unique cases and 3,818,574 case-quarters. Column 1 reports the dependent variable mean and standard deviation in the quarter of filing. Columns 2-4 report coefficients on the effect of eviction from quarters 1-4, 5-8, 9-12 and 13-16 quarters. 197,541 unique cases and 3,818,574 case-quarters. Robust standard errors, in parentheses, are clustered at the court-by-year-by-quarter level.

Table VIII: Reduced Form Results

	Predicted Mental Health Hosp	Mental Health Hosp	*Predicted* Shelter App	Shelter App	*Predicted* Earnings	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
Years 1-2:						
Instrument	0.001	0.071*	-0.004	0.101**	-0.278	-0.615
	(0.016)	(0.042)	(0.019)	(0.051)	(1.054)	(1.193)
All Years:						
Instrument	-0.001	0.089	-0.003	0.089	0.010	-0.976
	(0.020)	(0.054)	(0.023)	(0.057)	(1.025)	(1.228)
Observations	197541	197591	197541	197591	168200	168200
Court-Time-FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No

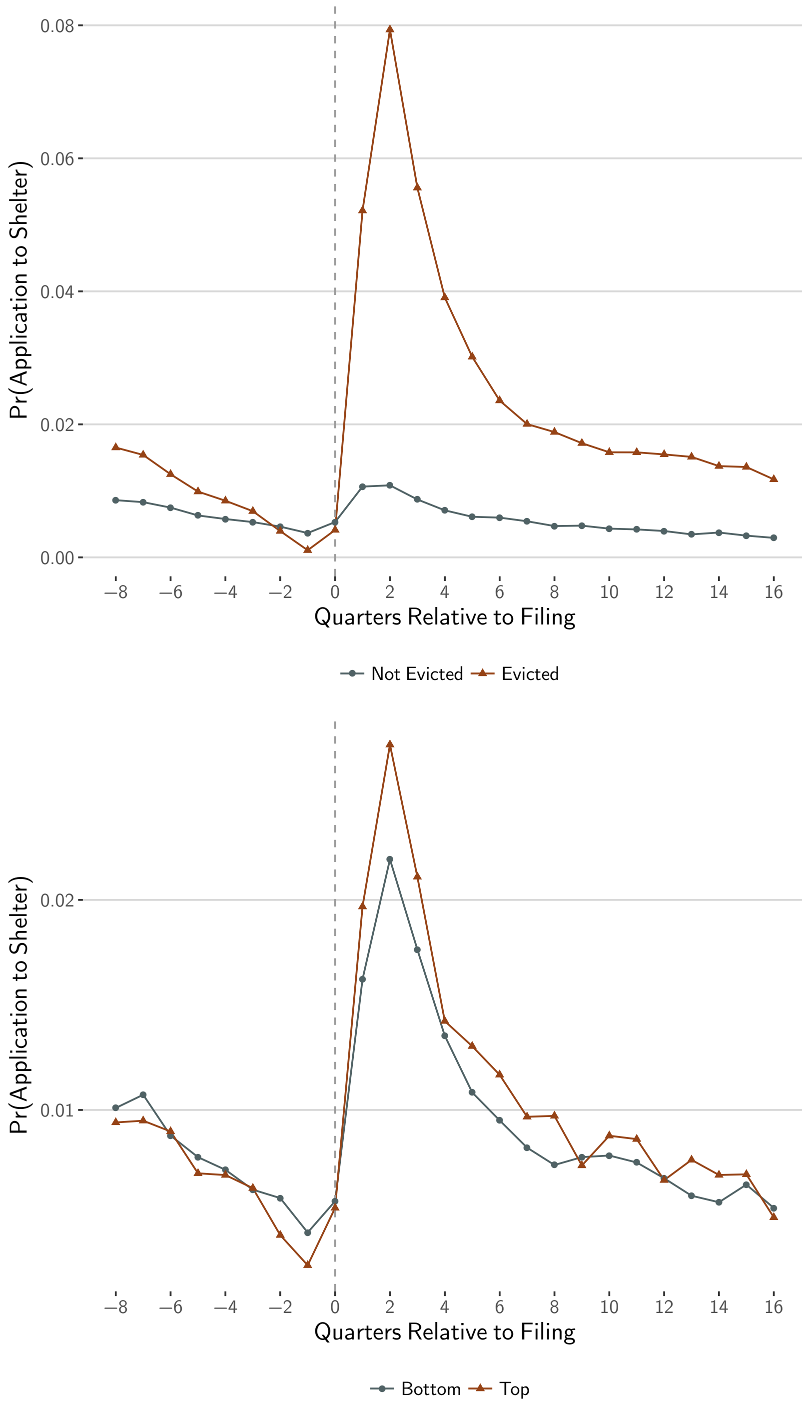
This table reports reduced form estimates of the relationship between our instrument and predicted and observed values of our key outcomes: applications to shelter, mental health hospitalizations, and averaged quarterly earnings (in thousands). The dependent variable for each regression is listed in the column header. In Column (1), the dependent variable is predicted mental health hospitalizations using all controls (excluding the instrument and eviction status) during the listed time window. In Column (2), the dependent variable is actual hospitalizations for mental health diagnoses in the listed time period. In Column (3), the dependent variable is predicted applications to shelter using all controls (excluding the instrument and eviction status) during the listed time window. In Column (4) the dependent variable is actual applications to shelter in the listed time period. Columns (5)-(6) use our labor force sample and report the relationship between the instrument and predicted mean quarterly earnings and actual mean quarterly earnings, each measured in thousands of 2016 dollars. We report two time windows for outcomes, as described in Section 6: outcome data for available post filing quarters 1-8 (Years 1-2) and outcome data from all available post-filing quarters 1-40 (All Years). All specifications include court-by-time fixed effects and fixed effects for 2015 and 2016 zipcode-year. We exclude controls in all the listed regressions. See the text for additional details on the specification, outcome measures, and sample.

Figure I: First Stage



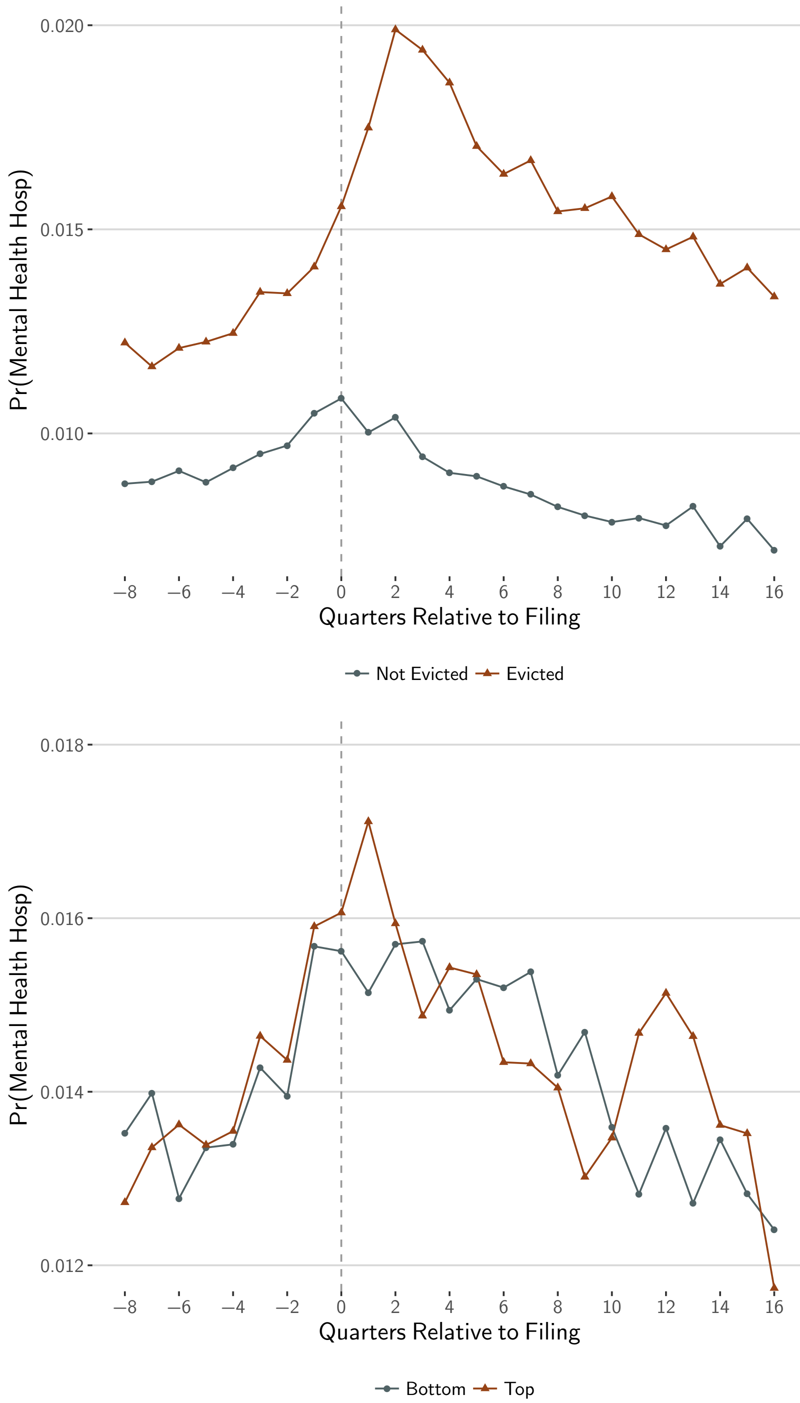
This figure shows a local-linear version of our first stage overlaid on top of a histogram of our instrument. All the data used in the plot have been residualized by court-by-time of filing fixed effects. See Section 4 for details on the instrument construction.

Figure II: Applications to Homeless Shelters



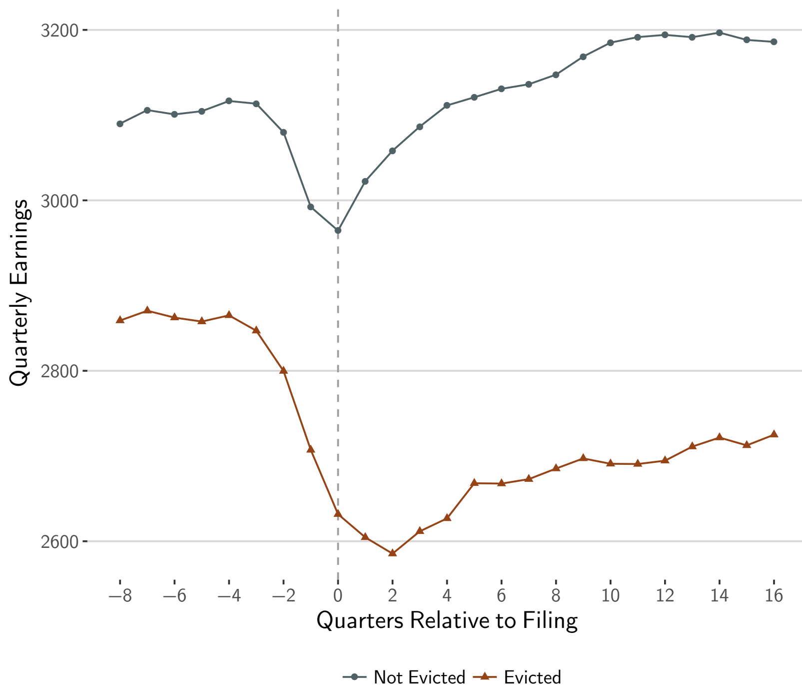
The top figure plots the mean rate of applications to New York City homeless shelter in our sample by quarter relative to filing, separately for households that experience eviction and households that avoid eviction. The bottom figure plots the mean rate of applications to New York City homeless shelter in our sample by quarter relative to filing separately for households assigned to a a courtroom that is more likely to evict (top quintile of instrument) and households assigned to a courtroom that is less likely to evict (bottom quintile of instrument). See Section 4 for details on the instrument construction. Each plot residualizes the shelter application rate by court-year-month-day-of-the-week, age-time, and court-time fixed effects, then adds back the mean for interpretation.

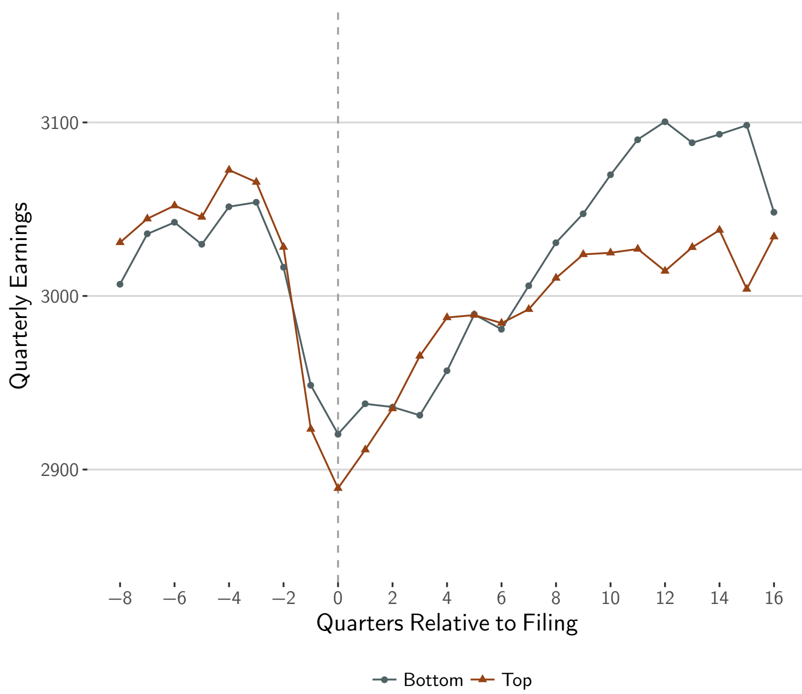
Figure III: Mental Health Hospitalizations



The top figure plots the probability of hospitalizations for mental health diagnoses for household heads in our sample by quarter relative to filing, separately for households that experience eviction and households that avoid eviction. The bottom figure plots the probability of hospitalizations in our sample by quarter relative to filing separately for households assigned to a courtroom that is more likely to evict (top quintile of instrument) and households assigned to a courtroom that is less likely to evict (bottom quintile of instrument). See Section 4 for details on the instrument construction. Each plot residualizes the probability of hospitalizations for mental health diagnoses by court-year-month-day-of-the-week, age-time, and court-time fixed effects, then adds back the mean for interpretation.

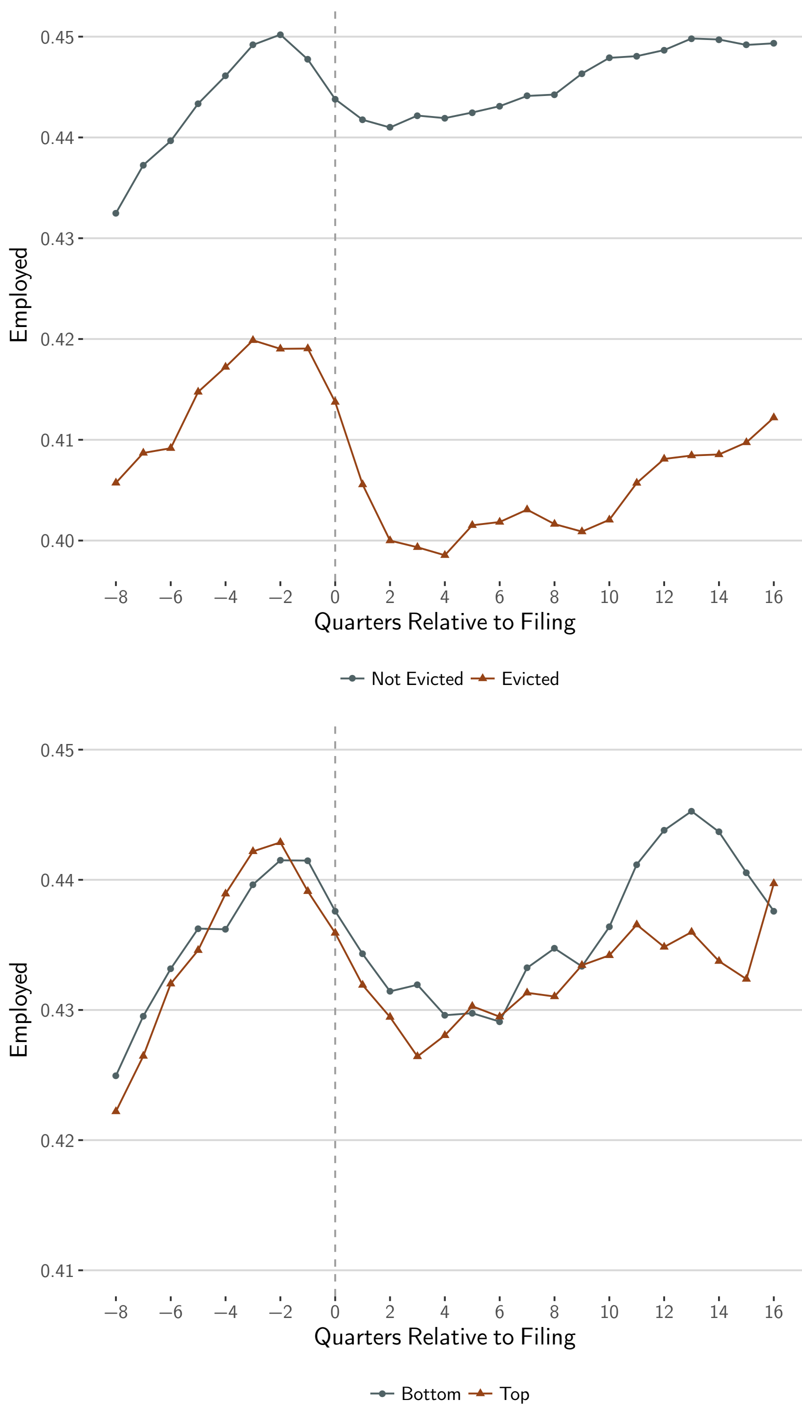
Figure IV: Quarterly Earnings





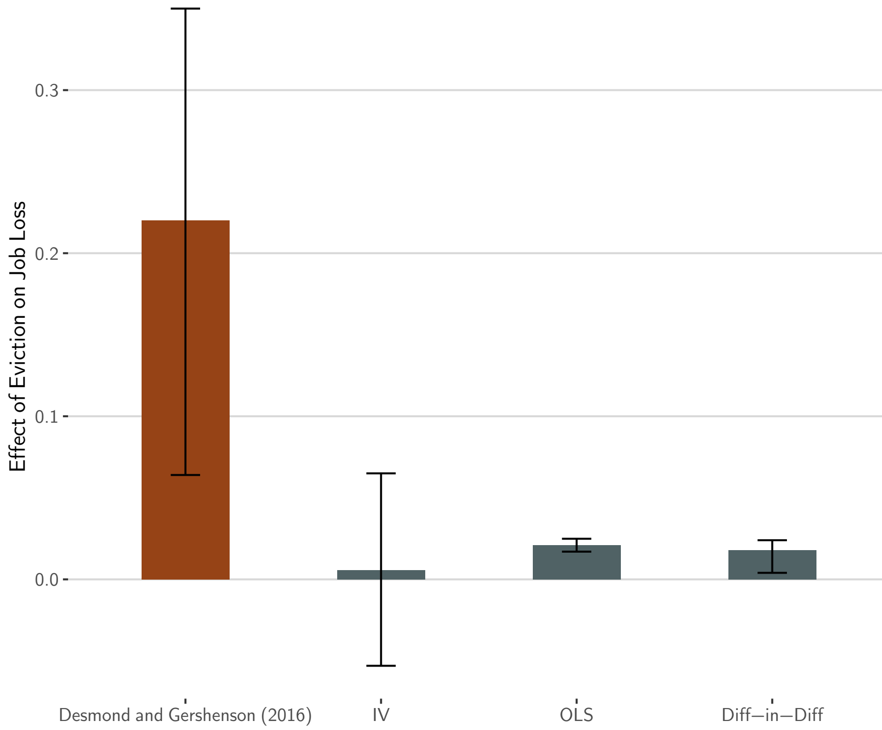
The top figure plots the mean quarterly earnings for household heads in our sample by quarter relative to filing, separately for households that experience eviction and households that avoid eviction. The bottom figure plots the mean quarterly earnings in our sample by quarter relative to filing separately for households assigned to a courtroom that is more likely to evict (top quintile of instrument) and households assigned to a courtroom that is less likely to evict (bottom quintile of instrument). See Section 4 for details on the instrument construction. Each plot residualizes earnings by court-year-month-day-of-the-week, age-time, and court-time fixed effects, then adds back the mean for interpretation.

Figure V: Quarterly Employment



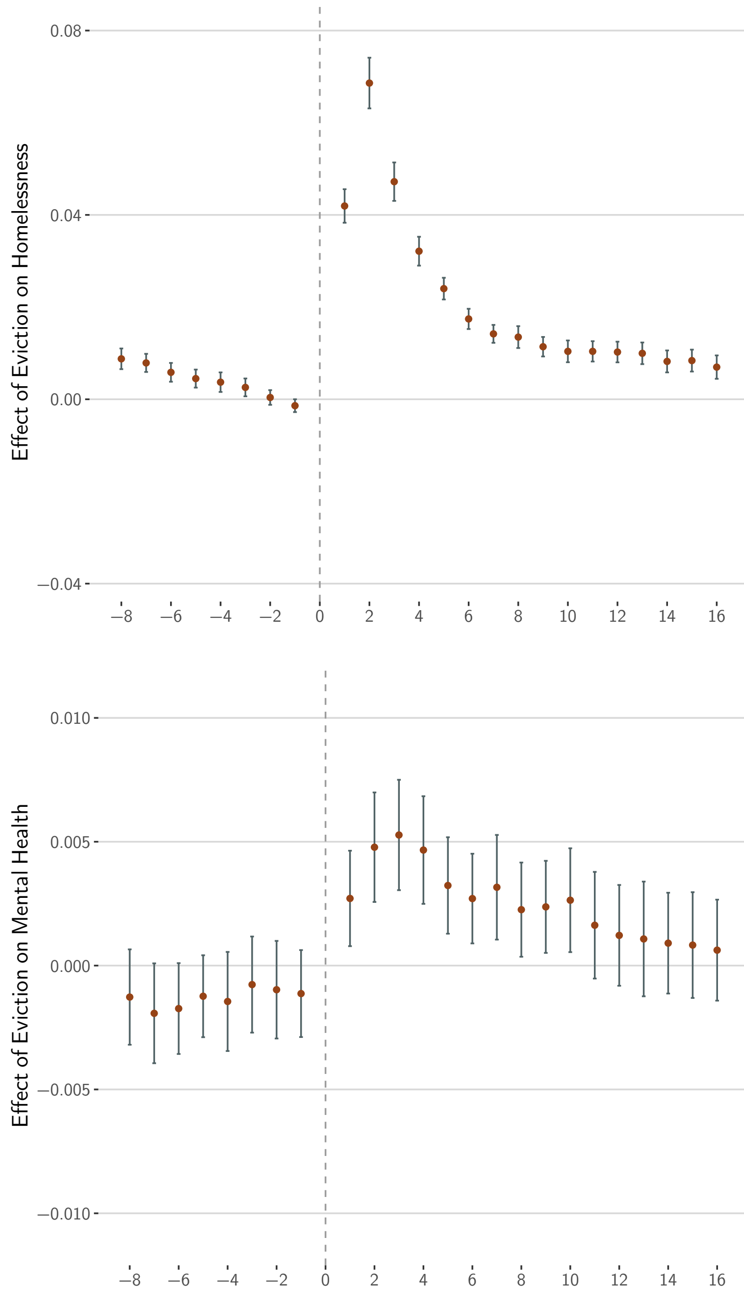
The top figure plots the mean quarterly employment rate for household heads in our sample by quarter relative to filing, separately for households that experience eviction and households that avoid eviction. The bottom figure plots the mean quarterly employment rate in our sample by quarter relative to filing separately for households assigned to a courtroom that is more likely to evict (top quintile of instrument) and households assigned to a courtroom that is less likely to evict (bottom quintile of instrument). See Section 4 for details on the instrument construction. Each plot residualizes the employment rate by court-year-month-day-of-the-week, age-time, and court-time fixed effects, then adds back the mean for interpretation.

Figure VI: Job-Loss Estimates Comparison



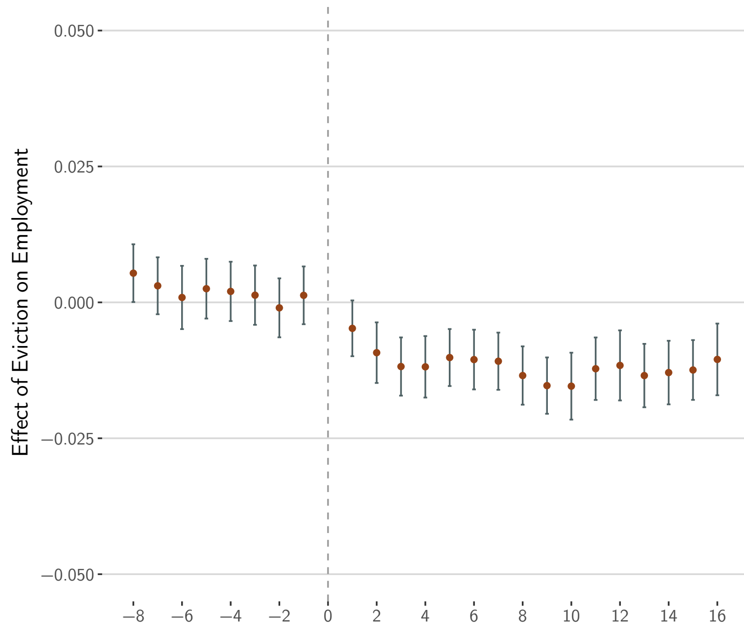
This figure plots estimates of the effect of forced moves on reported job loss from Desmond and Gershenson (2016) against our IV , OLS, and Diff-in-Diff estimates of the effect of evictions on a measure of job loss using our quarterly earnings records. Whiskers are for 95 percent confidence intervals.

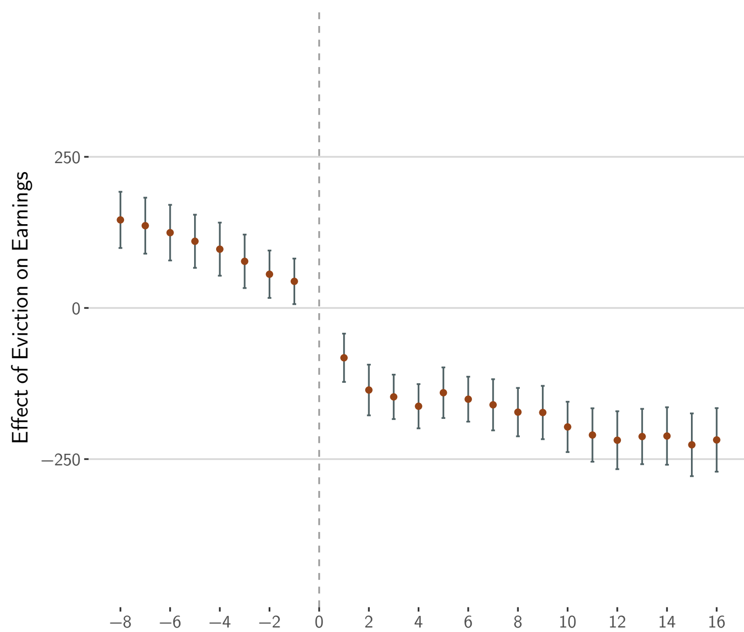
Figure VII: Event Study Estimates: Homelessness and Mental Health



These figures plot the coefficients from our event-study/difference-in-differences specification for the effect of eviction on the probability of homeless shelter application (top) and the probability of mental health hospitalization (bottom). The reported estimates come from the specification in equation 4. The horizontal axis denotes time relative to non-payment filing. Our estimates are normalized to be zero in the quarter of filing (dashed vertical line). Plot whiskers are 95 percent confidence intervals from standard errors clustered at the individual level.

Figure VIII: Event Study Estimates: Labor Market Outcomes





These figures plot the coefficients from our event-study/difference-in-differences specification for the effect of eviction on labor market outcomes: quarterly employment (top) and quarterly earnings (bottom). The reported estimates come from the specification in equation 4. The horizontal axis denotes time relative to non-payment filing. Our estimates are normalized to be zero in the quarter of filing (dashed vertical line). Plot whiskers are 95 percent confidence intervals from standard errors clustered at the individual level.

Appendix A: Sample Representativeness

In the table below we compare the characteristics in our matched court-benefits sample to the characteristics of a random sample of housing court tenants from a 2016 survey conducted by the New York City Human Resource Administration's Office of Evaluation and Research. Our sample is quite similar in age and gender distribution, but we appear to slightly undercount household members, which we can only infer indirectly from public assistance cases.

Sample Comparison

Variable	Matched Sample (2007-2016) (1)	Housing Court Survey (2016) (2)
Female	0.70	0.66
Male	0.30	0.34
Age (Mean)	44.3	44.1
Age Distribution:		
19-24	0.05	0.03
25-34	0.24	0.22
35-44	0.25	0.29
45-54	0.25	0.25
55-64	0.14	0.14
65+	0.07	0.06
Has Children	0.47	0.51
Household Size:		
1	0.39	0.26
2	0.22	0.24
3	0.18	0.25
4	0.11	0.15
5+	0.10	0.11

This table reports characteristics for our sample in Column (1) and the characteristics of a random sample of tenants in housing court in 2016 (source: Office of Evaluation and Research, Housing Court Survey, May 2016)

Next, we use the location and limited baseline information from the housing court filings to compare the characteristics of the matched and unmatched cases (reported below). Our matched records are very similar in terms of the neighborhood minority share, poverty rate, and median tract rent. Matched records owe slightly more in rent, on average, but are slightly less likely to be in a building with rent stabilization. Finally, our matched cases are less likely to have a previous case for the same tenant. Overall, our sample originates from similar neighborhoods and buildings, is very comparable in age and gender to the housing court population overall, but owes slightly more to their landlords and is less likely to have a history of cases.

Comparison, Matched to Unmatched Cases

	(1) Unmatched Cases	(2) Matched Cases
Log(Rent Owed)	7.957	8.030
Share Previous Case	0.566	0.465
Rent Stabilized	0.725	0.675
Log(Median Tract Rent)	6.881	6.856
Tract Share Non-White	0.854	0.876
Tract Share Poverty	0.268	0.287

This table reports mean baseline characteristics of cases in our sample 2007-2016 for cases that we match to a benefits record (Matched Cases) and cases we do not match to a benefits record (Unmatched Cases).

Appendix B: Complier Weighting

To facilitate comparisons between our IV and OLS estimates, we re-weight OLS so that it more closely matches the characteristics of the complier population. We follow the approach of Bhuller et al. (2016) and estimate our first stage equation separately for a set of mutually exclusive sub-groups, identified below, which allows us to calculate the proportion of compliers by sub-group. We then weight our OLS estimates so that the share of compliers in a subgroup matches the share of the full sample in that subgroup. The table below reports the first stage for each group. The groups over-represented in our complier population include younger households (age<45), male-headed households, and households with a history of homelessness.

Complier Characteristics

	$P[X = x]$	$P[X = x \mid \text{Complier}]$
Younger	0.541	0.661
Older	0.459	0.339
Female	0.703	0.584
Male	0.297	0.416
Ever Homeless	0.183	0.212
Not Ever Homeless	0.817	0.788
Black	0.592	0.604
Not Black	0.408	0.396
Hispanic	0.439	0.332
Not Hispanic	0.561	0.668
Employed	0.548	0.556
Not Employed	0.452	0.444
With Child	0.464	0.478
No Child	0.536	0.522
Rent Owed > 3,000	0.524	0.561
Rent Owed < 3,000	0.476	0.439

This table reports mean characteristics for our sample in Column (1) and the estimated mean among our compliers in Column (2).

Appendix C: Cost-Benefit Calculation

We combine estimates on the cost of shelter use and emergency room usage with our point estimates for the effects of eviction on our outcomes to generate estimates for the ex post costs of eviction. We focus on costs and benefits within the first two years of court filing, where most of our estimates are most precisely estimated. When converting some of our estimates into equivalents over this two year period, we necessarily assume that our estimated effects are reasonably linear.

Costs associated with lost earnings are straightforward to calculate. Our OLS estimates of the effect of eviction on cumulative earnings within the first two years of court filing are around -\$1500, and the IV estimates, though imprecisely estimated and not statistically distinguishable, are slightly smaller at -\$1,000 per eviction.

Costs associated with shelter use require an estimate of the cost of a stay in shelter. We focus on the financial cost to the City of New York of housing one family in shelter for one night, which is \$170 (Mayor's Management Fund 2017). We focus on administrative costs of staying in shelter and do not consider any physical, psychological, or economic costs associated with being in shelter. On average, we observe 630 days post filing. Our OLS estimate of the effect of eviction on the share of days spent in shelter (Years 1-2) is 0.06, implying 38 additional days in shelter, or about \$6,400 (38×170) in costs. Similarly, our IV estimates imply \$6,100 in administrative shelter costs.

We generally find no statistically significant or economically meaningful effects of eviction on public assistance, so we assume the costs and benefits with regard to public assistance are zero.

We next consider health outcomes. We focus here on the financial costs of the hospitalization itself, which was \$2,000 per hospitalization in 2016 based on research by the Health Care Cost Institute. Our OLS estimates imply that eviction increases the number of emergency room visit by about 0.036, for a cost of about \$72. Our IV estimate is larger, suggesting an increase in emergency room visits by roughly 0.38 visits, which translates to a cost of \$760.

The table below summarizes these results. Aggregating these different costs yields estimates of around \$8,000 for OLS and IV. These numbers should be interpreted with caution, as many of our point estimates, particularly our IV estimates, have large confidence intervals.

Cost-Benefit Calculation

Outcome	Cost Per Unit	Using OLS Estimates (1)	Using IV Estimates (2)
Cumulative Earnings	-	-$1,500	-$1,000
Days in Shelter	$170	-$6,400	-$6,100
Emergency Room Visits	$2,000	-$70	-$760
Total		$-7,970	$-7,860

This table reports estimates of the costs of eviction in the 1-2 years after filing. Column (1) uses our OLS estimates and Column (2) uses our IV estimates. Cost of shelter comes from a report of the Mayor's Management Fund, 2017. The cost of emergency room hospitalization comes from the Health Care Cost Institute, 2016.

Appendix D: Linking Procedure

This appendix describes our procedure to link housing court records to administrative benefits files. Our housing court records contain only first name, last name, and address. The administrative benefits data include every address that an individual has provided for the years covered in our benefits data. For each address record we have a client ID and case number.[37] We use the client ID to add the first name, last name, date of birth, and Social Security Number. After cleaning the names in the courts data, including removing non-numeric characters and obvious aliases ("John/Jane Doe"), we have 1.7 million distinct name-address pairs. The benefits file is quite large, with nearly 70 million person-case-address-date combinations.

We "block" our matching algorithm on borough/county and phonic similarity (same soundex transformation of first and last name) due to the size of the data sets and the computational capacity required by the record-linking procedures. This blocking establishes the most general requirements that must be met in order to be in the universe of possible matches. After narrowing to phonically similar records in the same borough/county, we drop any match for which the date of the benefits record is after the housing court filing date in order to ensure that the match is not endogenous to our treatment. We are left with matrices of all possible pairwise combinations of housing court records with a benefits record that meets these criteria (e.g. within borough pairs with similar names), with a median of 16 cases per court record.

We then apply a modified version of the common EM (expectation-maximization) algorithm described by Fellegi-Sunter (1969), which can also be thought of as a naive Bayes classifier. We modify this conventional probabilistic matching algorithm by replacing binary string agreement with an indicator function applied to string distance measure (in this case, the Jaro-Winkler string distance J_{ij} for record pair i, j). If the Jaro-Winkler distance exceeds 0.85 it is considered a "match" in the EM algorithm. This is a common threshold that yields matches that appear valid but is also robust to misspelling and incorporates name complexity. The algorithm calculates a separate "Name Score," where M_a is the probability that the field matches given that the match is true: $P(M_i = M_j|\text{Match=True})$. This of course is not known, but we use a common value for names: 0.95. U_a is the probability that the field matches when the true match is false $P(M_i = M_j|\text{Match}\neq\text{True})$. This is akin to how common or rare the name is. We estimate these quantities in the benefits data directly, which contains over 9 million unique persons.[38]

[37] These are 2001-2016 for Cash Assistance, 2004-2016 for Food Stamps, and 2006-2016 for Medicaid.

[38] We set a lower bound of 0.0000002.

$$\text{Name Score}_{ij} = log\left(\frac{M_{first}}{U_{first}}\right)\mathbf{1}\left(J_{ij}\left(\text{First Name}\right) \geq 0.85\right) + log\left(\frac{M_{last}}{U_{last}}\right)\mathbf{1}\left(J_{ij}\left(\text{Last Name}\right) \geq 0.85\right) + \ldots$$
$$+ log\left(\frac{1-M_{first}}{1-U_{first}}\right)\mathbf{1}\left(J_{ij}\left(\text{First Name}\right) < 0.85\right) + log\left(\frac{1-M_{last}}{1-U_{last}}\right)\mathbf{1}\left(J_{ij}\left(\text{Last Name}\right) < 0.85\right)$$

Because large buildings in New York City often have multiple entrances and multiple valid addresses, we geocode all of our data to the borough-block-lot (BBL), which is equivalent to a parcel. The linking geo-fields are BBL and census block. These receive different M and U probabilities to account for the likelihood of matching on BBL (unlikely) versus census block (slightly more likely). We list these probabilities directly in the formula below:

$$\begin{aligned} \text{Geo Score}_{ij} = log\left(\frac{0.975}{U_{BBL}}\right)&\mathbf{1}\left(\text{BBL Courts=BBL Benefits}_{ij}\right) + \ldots \\ log\left(\frac{0.95}{0.05}\right)&\mathbf{1}\left(\text{Block Court=Block Benefits}_{ij}\right) + \ldots \\ + log\left(\frac{1-0.975}{1-U_{BBL}}\right)&\left(1-\mathbf{1}\left(\text{BBL Courts=BBL Benefits}_{ij}\right)\right) + \\ log\left(\frac{1-0.95}{1-0.05}\right)&\left(1-\mathbf{1}\left(\text{Block Court=Block Benefits}_{ij}\right)\right) \end{aligned}$$

The algorithm then proceeds as follows:

1. Rank Name Score (ties are broken by relative closeness to filing date)

2. Rank Geo Score (ties are broken by relative closeness to filing date)

3. Assign a match to any exact matches and set aside (ties are broken by relative closeness to filing date)

4. For non-exact matches, keep pairs with same top name and top geo records, assign as best available record

5. For non-exact matches with disagreeing top name and top geo record, sum the Name Score and Geo Score and rank the combined score (ties are broken by relative closeness to filing date), assign as best available record

6. From best available records keep as a true match if score exceeds a threshold

Appendix E: Sample Restrictions

We make several sample restrictions in instances of non-random assignment to court parts. In each case, these sample restrictions are applied before we calculate the instrument. We start with a sample of 496,000 matched non-payment cases. As noted in Section 3, we limit our sample to case filings that produce some interaction with the court system. If there is no subsequent activity after filing by either party, then the case is not assigned to a part and there is no substantive interaction in housing court. Dropping these cases reduces our sample by approximately 33 percent, to 331,000.

Specialized Courts

We drop cases in the specialized courts serving Harlem and Red Hook, as well as the court in Staten Island, which has only a single courtroom. This accounts for roughly 9,000 cases dropped.

Condo and Co-op Cases

Cases involving condos or co-ops are not randomly assigned to parts. Within each court, these cases are assigned to a single part. In all boroughs except the Bronx, this designated part handles some non-condo/co-op cases: the average share of condo/co-op cases in these parts is 27 percent. We use annual administrative data from the New York City Department of Finance to identify buildings with condos or co-ops in each year. We drop any case arising from a building with condos or co-op units, which accounts for 28,000 matched cases.

Public Housing Cases

Cases involving units in public housing are not randomly assigned to parts. In each court there is a designated part that manages public housing cases. We drop each of these parts in the Bronx, Brooklyn, and Manhattan. In Queens, there are just four resolution parts in total, and the public housing part handles non-public housing cases three days out of the week. We drop the public housing part on these public housing days in our primary sample, but our results are robust to excluding the part entirely. Since some cases are incorrectly assigned to the wrong part initially by the court's information system, public housing cases are sometimes assigned to non-public housing parts. To avoid including these cases, we also further drop cases arising from block faces with large concentrations of public housing. These cases are less than 2 percent of the sample. Taken together, our public housing restriction drops 67,000 cases.

Military and Drug Cases

Cases involving a member or spouse of an active member of the military are also assigned to a single part within each court. These cases are identified in the Court data and are quite rare, with less than 1,000. We drop them.

Zip Assignment and Policy Initiatives

The Housing Court has in select instances used zip code-based assignment to court-parts. There are two parts in the Bronx that operated with zip code assignment for several years (Part I from 2011-2016 and Part E from 2011-2016). More recently, in tandem with expanded subsidized legal services for low-income tenants in housing court, the housing court has begun to assign cases arising from certain low-income zip codes to particular parts. We can identify these zip code-based assignments using the property address and part. We drop these parts. As discussed in Section 4, we also include zip code-by-year fixed effects beginning in 2015 to account for the expanded legal assistance. Additionally, in two isolated years, clients receiving public assistance with a history of homelessness were systematically directed to two designated parts in the Bronx and Brooklyn. These cases are immediately apparent, with rates of prior homelessness more than triple the other parts in the same period. We drop both parts. In total, these restrictions comprise about 25,000 cases.

Age Restriction

Finally, we restrict our sample to households with an identified head age greater than 18 and less than 85. This results in about 2,000 cases being dropped.

Sample Restrictions

Restriction	Matched Cases Dropped	Remaining Non-Pay Cases
-	-	496,000
No Appearance of Either Party	165,000	331,000
Specialized Courts	9,000	322,000
Condo and Coop	28,000	294,000
Public Housing (NYCHA)	67,000	226,000
Military & Drug	<1,000	225,000
Zip-Code Assign/Other Policy	25,000	200,000
Age Restriction	2,000	198,000

Appendix F: Measuring Evictions

Our measure of evictions comes from the New York Housing Court 2007-2016 combined with records from the New York City Department of Investigations (DOI) 2014-2016. We identify evictions in the Housing Court data using warrants that were executed with eviction or possession. Possessions are when the tenant is evicted but their belongings are locked in the apartment, rather than put in storage (as with an eviction). While this measure covers most completed evictions, it misses a small share of all evictions (and a smaller share in our estimation sample, where we estimate that we capture around 98 percent of evictions) when Marshals fail to return the warrant to court. The probability of missing a true eviction is unrelated to person characteristics but appears relatively more likely for holdover cases (which are not included in our primary estimation sample). When available, we use records directly from the City Marshals, overseen by the DOI, which captures all evictions. We link DOI records to the housing court data using the case index number. To evaluate how sensitive our results are to using the court measure, we re-estimate our key OLS and IV models for years that we have both eviction measures separately using only the courts measure "Courts-Only" and using the courts and DOI records "DOI + Courts." Our point estimates change minimally with the source of eviction data used.

Eviction Measure Sensitivity

	OLS		IV	
	Courts-Only (1)	DOI+Courts (2)	Courts-Only (3)	DOI+Courts (4)
Homeless				
Pr(Apply to Shelter)	0.11***	0.11***	0.21***	0.19***
	(0.0054)	(0.0047)	(0.069)	(0.061)
Mental Health				
Any Mental Health Hosp	0.018***	0.017***	0.14*	0.13*
	(0.0037)	(0.0032)	(0.084)	(0.076)
Earnings				
Quarterly Earnings	-275.1***	-288.9***	-89.3	-82.0
	(44.6)	(39.8)	(1316.0)	(1207.3)
Controls	Yes	Yes	Yes	Yes
Court-Time-FE	Yes	Yes	Yes	Yes
Observations	49911	49911	49911	49911

This table reports results from estimating our central results using data from 2015 and 2016 with two different measures of eviction. "Courts-Only" results use only our eviction measure available in the housing court records (described above) in measuring both eviction and constructing the instrument. "Courts+DOI" uses our linked courts-DOI measure of eviction when constructing the instrument and measuring evictions. Standard errors, in parentheses, are Eicher-White robust SE. See the text for additional details on the specification, outcome measures, and sample.

Appendix G: Hospitalization Diagnosis Groupings

Our hospitalization data contain individual ICD-9 codes, which are quite detailed, as well as the Clinical Classifications Software (CCS) diagnosis category developed by the Agency for Healthcare Research and Quality (AHRQ). We follow Currie and Tekin (2015) and group these CCS diagnosis categories into more general groups, which are listed below.

Sample Restrictions	
Category	CCS Diagnoses Codes
Asthma	128
Cancer	11-47
Diabetes	49,50
Heart/Stroke	97,100,101,104, 107-117
Heart Valve	12,96,105,106
Hypertension	98,99
Hematological	59-64,188-121
Infection	1-10
Mental Health	650-661, 663, 670
Metabolic	48, 51-58
Nervous System	76-83,85,95
Respiratory Infection	122-126

Table AI: The Effects of Eviction on Residential Mobility

	Mean Not-Evicted	OLS Results			IV Results	
	(1)	(2)	(3)	(4)	(5)	(6)
Years 1-2:						
Pr(Moving)	0.276	0.292***	0.253***	0.253***	0.413***	0.405***
		(0.005)	(0.005)	(0.008)	(0.136)	(0.137)
Number of Moves	0.398	0.608***	0.526***	0.560***	0.892***	0.851***
		(0.013)	(0.011)	(0.020)	(0.271)	(0.270)
All Years:						
Pr(Moving)	0.379	0.277***	0.240***	0.227***	0.440***	0.440***
		(0.006)	(0.006)	(0.008)	(0.138)	(0.144)
Number of Moves	0.963	1.085***	0.925***	0.959***	1.529***	1.456***
		(0.031)	(0.023)	(0.037)	(0.542)	(0.536)
Observations	106785	106785	106785	106785	106785	106785
Court-Time-FE		Yes	Yes	Yes	Yes	Yes
Controls		No	Yes	Yes	No	Yes
Complier Weights		No	No	Yes	No	No

This table reports estimates of the effect of eviction on measures of residential mobility derived from benefits records, described in Section 3.1. Column (1) reports the mean dependent variable for households not evicted to aid interpretation. The dependent variable is reported in each row. They are an indicator for any move, Pr(Moves), and the number of changes of address (Number of Moves). We report two time windows for outcomes, as described in Section 6: outcome data for available post filing quarters 1-8 (Years 1-2), and outcome data from all available post-filing quarters 1-40 (All Years). We present OLS in Columns (2)-(4) and IV estimates in Columns (5)-(6). For our IV models, the parameters are estimated via 2SLS as in equation 3. All specifications include court-by-time fixed effects and fixed effects for 2015 and 2016 zipcode-year. When estimated with controls, the controls include race, ethnicity, age, gender, household composition, prior earnings and employment history, prior benefits history, previous applications to homeless shelters, tract poverty rate, hospitalization history since 2004, rent amount owed, legal representation, rent stabilization status of the building, and indicator for previous housing court case. Complier weights are described in Section 4 of the text. Standard errors, in parentheses, are clustered at the courtroom-year level. The sample is limited to cases with valid post-filing address record. We verify that the instrument is uncorrelated with having a valid post-filing address record Appendix Table 8. See the text for additional details on the specification, outcome measures, and sample.

Table AII: Randomization Test

	Eviction (1)		Residualized Evict Rate (Z) (2)	
Black	0.00994***	(0.00204)	0.0000543	(0.0000431)
Hispanic	-0.0264***	(0.00260)	-0.00000362	(0.0000440)
Female	-0.0459***	(0.00242)	-0.0000540	(0.0000335)
Total Adults	-0.000620	(0.00104)	-0.0000302	(0.0000186)
Total Children	-0.00608***	(0.000938)	0.0000134	(0.0000190)
Married	-0.0248***	(0.00305)	-0.000101	(0.0000688)
Age	-0.00304***	(0.000123)	-0.000000338	(0.00000160)
Food Stamps	0.0104***	(0.00214)	-0.0000155	(0.0000448)
Cash Assistance	0.0390***	(0.00297)	-0.0000558	(0.0000516)
Emergency Assistance	0.0467***	(0.00286)	0.0000586	(0.0000448)
Applied to Shelter Before	0.105***	(0.00639)	0.00000937	(0.0000476)
Rent Amount Owed	0.0000130***	(0.000000435)	1.45e-09	(6.54e-09)
Has Legal Representation	-0.0858***	(0.00894)	0.000621*	(0.000337)
Previous Case	0.0376***	(0.00340)	0.0000209	(0.0000392)
Tract Poverty Rate	0.00382	(0.0115)	-0.0000501	(0.000232)
Rent Stabilized	0.0269***	(0.00233)	-0.00000984	(0.0000392)
Employed	-0.00543**	(0.00256)	-0.0000106	(0.0000526)
Earnings	-0.000000550**	(0.000000271)	-4.47e-09	(7.23e-09)
Has SSN	-0.00109	(0.00415)	-0.0000616	(0.0000742)
Any Hospitalization	0.00292	(0.00237)	-0.0000531	(0.0000560)
Infection	-0.00509*	(0.00280)	-0.0000375	(0.0000484)
Cancer	-0.0104***	(0.00375)	0.000000961	(0.0000949)
Diabetes	-0.00258	(0.00438)	-0.000201*	(0.000104)
Metabolic	-0.00995**	(0.00419)	0.0000678	(0.000109)
Nervous System	-0.000233	(0.00349)	-0.0000502	(0.0000820)
Heart/Stroke	-0.00546	(0.00382)	0.0000749	(0.0000808)
Heart Valve	-0.000200	(0.00429)	-0.000128	(0.000103)
Hypertension	0.0136***	(0.00425)	0.0000286	(0.000113)
Upper Respritory	-0.00428*	(0.00231)	-0.0000610	(0.0000413)
Mental Health	0.0239***	(0.00280)	0.0000411	(0.0000601)
Asthma	0.000235	(0.00344)	0.0000246	(0.0000807)
Observations	197575		197575	
Court-Time-FE	Yes		Yes	

This table reports the relationship between eviction and our baseline characteristics and the relationship between our instrument and these same characteristics. Column (1) presents results from regressing eviction on the listed characteristics. Column (2) shows the results from regressing the instrument on the listed characteristics. Both regressions include court-time of filing fixed effects. Reported standard errors are robust two-way clustered at the courtroom-year and individual level.

Table AIII: Instrument and Case Actions

	Eviction	Avg. Days Till Eviction	Judgment	OSC Denied
	(1)	(2)	(3)	(4)
Instrument	0.805***	-306.716***	1.213***	0.736***
	(0.089)	(85.259)	(0.165)	(0.112)
Observations	197591	197591	197591	197591
Controls	No	No	No	No
Court-Time-FE	Yes	Yes	Yes	Yes

This table reports the relationship between our instrument and select case actions or outcomes. Column (1) reproduces our first stage. In Column (2), the dependent variable is the average number of days between filing and eviction for the courtroom. The dependent variable in Column (3) is an indicator for whether a judgment was issued in favor of the landlord, and the dependent variable in Column (4) is an indicator for whether the tenant was denied an "Order to Show Cause" at any point in the housing court process. None of the regressions include controls. Each regression includes court-time of filing fixed effects. Reported standard errors are robust two-way clustered at the courtroom-year and individual level.

Table AIV: Monotonicity Across Characteristics

	Gender		Race		Ethnicity		Rent Owed		Rent Stabilized	
	Female	Male	Black	Not Black	Hispanic	Not Hispanic	Owes <2 Months	Owes>2 Months	Stabilized	Not Stabi-lized
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Instrument	0.41***	1.06***	0.61***	0.69***	0.46***	0.48***	0.62***	0.45***	0.53***	0.84***
	(0.083)	(0.16)	(0.13)	(0.098)	(0.096)	(0.11)	(0.14)	(0.14)	(0.082)	(0.19)
Constructed with	Male	Female	Not	Black	Not	Hispanic	More	Less	Not	Stabilized
Court-Time-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	136213	61370	112332	85245	86259	111311	63687	133890	133317	64255

This table reports first stage results for a variety of sub-samples. In each case the instrument is constructed over the excluded group indicated in the row, but the first stage is estimated over the sub-sample indicated in the column. As discussed in section 4.4, each coefficient is strongly positive suggesting no deviations from monotonicity. Column (1) the first stage is estimated over female households heads. In column (2) it is male head's of household. Column (3) it is black head's of household. Column (4) it is non-black head's of household. Column (5) is estimated over Hispanics. Column (6) is estimated over not Hispanics. Column (7) is estimated for a sub-sample that we estimate owes 2 month's rent or less, and column (8) is for those who owe more than 2 month's rent. Column (9) is a sub-sample with rent stabilized units in their building and Column (10) is for a sample without rent stabilization. All specifications include court-by-time fixed effects and fixed effects for 2015 and 2016 zipcode-year. Standard errors, in parentheses, are two-way clustered at the courtroom-year and individual level. See the text for additional details on the specification, outcome measures, and sample.

Table AV: The Effects of Eviction on Other Hospitalizations

	Mean Not-Evicted	OLS Results			IV Results	
	(1)	(2)	(3)	(4)	(5)	(6)
Years 1-2:						
Any Emergency Visit	0.347	0.044***	0.016***	0.035***	0.047	0.165
		(0.003)	(0.003)	(0.006)	(0.117)	(0.113)
Any Heart/Stroke	0.019	-0.004***	0.000	0.004**	0.002	-0.008
		(0.001)	(0.001)	(0.002)	(0.044)	(0.045)
Any Heart Valve	0.009	-0.001**	-0.000	0.001	0.025	0.027
		(0.001)	(0.001)	(0.001)	(0.023)	(0.024)
Any Hypertension	0.013	0.002***	0.004***	0.002	0.024	0.026
		(0.001)	(0.001)	(0.002)	(0.028)	(0.028)
Any Mental Health	0.037	0.033***	0.025***	0.029***	0.088*	0.091*
		(0.002)	(0.001)	(0.004)	(0.053)	(0.054)
Any Asthma	0.024	0.007***	0.005***	0.006***	-0.013	0.003
		(0.001)	(0.001)	(0.002)	(0.037)	(0.035)
Any Diabetes	0.012	0.000	0.002**	0.002	0.017	0.032
		(0.001)	(0.001)	(0.002)	(0.033)	(0.030)
Num Hospital Visit	1.212	0.418***	0.260***	0.338***	0.666	1.358
		(0.029)	(0.022)	(0.071)	(1.021)	(0.953)
Num Emergency Visit	0.543	0.094***	0.036***	0.036***	0.139	0.377*
		(0.006)	(0.005)	(0.005)	(0.208)	(0.202)
Num Heart/Stroke	0.029	-0.004**	0.002	0.009*	0.119	0.098
		(0.002)	(0.002)	(0.005)	(0.093)	(0.094)
Num Heart Valve	0.011	-0.001	-0.000	0.005*	0.031	0.034
		(0.001)	(0.001)	(0.003)	(0.035)	(0.036)
Num Hypertension	0.017	0.004***	0.005***	0.004	0.050	0.053
		(0.001)	(0.001)	(0.004)	(0.049)	(0.049)
Num Mental Health	0.081	0.112***	0.087***	0.092***	0.142	0.147
		(0.009)	(0.008)	(0.035)	(0.303)	(0.301)
Num Asthma	0.047	0.024***	0.018***	0.022***	-0.150	-0.105
		(0.005)	(0.004)	(0.007)	(0.136)	(0.134)
Num Diabetes	0.019	0.006**	0.006***	0.011**	0.028	0.067
		(0.003)	(0.002)	(0.004)	(0.083)	(0.072)
Observations	197541	197591	197541	197541	197591	197541
Court-Time-FE		Yes	Yes	Yes	Yes	Yes
Controls		No	Yes	Yes	No	Yes
Complier Weights		No	No	Yes	No	No

This table reports estimates of the effect of eviction on the number of inpatient and outpatient hospitalizations. Column (1) reports the mean dependent variable for households not evicted to aid interpretation. The dependent variable is reported in each row. Each dependent variable is the number of hospitalization in the time period with a primary diagnosis in the listed group. The diagnoses that make up each grouping are listed in Appendix E. We report two time windows for outcomes, as described in Section 6: outcome data for available post filing quarters 1-8 (Years 1-2) and outcome data from all available post-filing quarters 1-40 (All Years). We present OLS in Columns (2)-(4) and IV estimates in Columns (5)-(6). For our IV models, the parameters are estimated via 2SLS as in equation 3. All specifications include court-by-time fixed effects and fixed effects for 2015 and 2016 zipcode-year. When estimated with controls, the controls include race, ethnicity, age, gender, household composition, prior earnings and employment history, prior benefits history, previous applications to homeless shelters, tract poverty rate, hospitalization history since 2004, rent amount owed, legal representation, rent stabilization status of the building, and indicator for previous housing court case. Complier weights are described in Section 4 of the text. Standard errors, in parentheses, are clustered at the courtroom-year level. We verify that the instrument is uncorrelated with having a valid post-filing address record. See the text for additional details on the specification, outcome measures, and sample.

Table AVI: Alternate Specifications

	Main Specification (1)	No De-Mean (2)	Winsorized Instrument (3)	Leave One-Out Mean (4)	Day Residualized (5)	Quarter Residualized (6)	Zip-Year FE (7)
First Stage:							
Instrument	0.805***	0.705***	0.833***	0.811***	0.835***	0.805***	0.829***
	(0.088)	(0.094)	(0.098)	(0.094)	(0.102)	(0.091)	(0.088)
Homelessness							
Eviction	0.138**	0.124*	0.149**	0.131**	0.213***	0.131**	0.142**
	(0.062)	(0.068)	(0.063)	(0.067)	(0.070)	(0.061)	(0.061)
Mental Health							
Eviction	0.090*	0.103*	0.092*	0.110*	0.066	0.095*	0.087
	(0.054)	(0.060)	(0.055)	(0.058)	(0.060)	(0.053)	(0.054)
Observations	197541	197541	197541	197543	188494	197541	197497
Court-Time-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table reports first stage and 2SLS estimates for our key outcomes using alternative specifications and constructions of the instrument. Each outcome listed is for the quarters 1-8 (Years 1-2) time period. Column (1) is our preferred specification reported throughout the paper. Column (2) constructs the instrument without de-meaning the average of the court, e.g. $Z_i = \frac{1}{n_{cjy}-1}\left(\sum_{k \neq i}^{n_{cjy}} \widetilde{\text{Evicted}}_{kcjt}\right)$. Specifications in Column (3) use a version of the instrument where we winsorize values at the 1st and 99th percentiles. Column (4) uses the average eviction rate for the judge without residualizing by court-by-time of filing. Column (5) constructs the instrument by residualizing using exact day of filing and day of week of first appearance. Column (6) constructs the instrument by residualizing using quarter of filing (rather than month) and day of week of first appearance. All specifications use court-by-time of filing fixed effects. All second stage results include controls. Reported standard errors are robust two-way clustered at the courtroom-year and individual level.

Table AVII: Sub-group OLS Estimates

	Homeless Status		Employment		Gender		Family Status	
	Homeless (1)	Not Homeless (2)	Employed (3)	Unemployed (4)	Female (5)	Male (6)	Has Child (7)	No Child (8)
Homeless								
Years 1-2	0.30***	0.11***	0.14***	0.19***	0.18***	0.12***	0.20***	0.12***
	(0.0071)	(0.0027)	(0.0036)	(0.0045)	(0.0039)	(0.0036)	(0.0047)	(0.0031)
Any	0.30***	0.13***	0.16***	0.20***	0.20***	0.14***	0.21***	0.14***
	(0.0070)	(0.0034)	(0.0037)	(0.0044)	(0.0040)	(0.0039)	(0.0048)	(0.0034)
Mental Health								
Years 1-2	0.029***	0.023***	0.018***	0.034***	0.023***	0.029***	0.016***	0.034***
	(0.0033)	(0.0016)	(0.0016)	(0.0025)	(0.0017)	(0.0027)	(0.0015)	(0.0024)
Any	0.033***	0.032***	0.027***	0.044***	0.030***	0.039***	0.024***	0.043***
	(0.0044)	(0.0020)	(0.0023)	(0.0031)	(0.0022)	(0.0035)	(0.0023)	(0.0028)
Earnings								
Years 1-2	-0.95***	-1.86***	-3.09***	-0.26***	-1.64***	-2.03***	-1.62***	-1.94***
	(0.17)	(0.15)	(0.18)	(0.095)	(0.15)	(0.20)	(0.16)	(0.16)
Years 1-5	-2.71***	-4.30***	-7.53***	-0.95***	-4.28***	-4.84***	-4.90***	-3.96***
	(0.45)	(0.40)	(0.49)	(0.26)	(0.39)	(0.50)	(0.45)	(0.42)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-Time-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	33416	141946	100157	75407	124353	51213	85912	89644

This table reports estimates of the effect of eviction using our OLS specification for several sub-groups. Each row is an estimate of the effect of eviction on the outcome listed in the row groupings, where the time period is noted in the individual row. The homelessness variable is an indicator for applications to shelter. The mental health variable is an indicator for a mental health hospitalization. And the earnings variable is cumulative earnings in thousands (USD2016). The columns identify the corresponding sub-group. Columns (1)-(2) are for household heads who have previously applied to shelter ("Homeless") or never previously applied to shelter ("Not Homeless"), respectively. Columns (3)-(4) report the effect of eviction for those employed during the year before filing ("Employed") and those not employed in the year before filing ("Unemployed"). Columns (5)-(6) report the effect of eviction for household heads identified as female in the benefits data ("Female") and male in the benefits data ("Male"). Columns (7)-(8) report the effect of eviction separately for households with a minor member on the case ("Has Child") and without a minor listed on the case ("No Child"). We report results for two time windows: outcome data for available post filing quarters 1-8 (Years 1-2) and outcome data from all available post-filing quarters 1-40 (All Years). All parameters are estimated via OLS. All specifications include court-by-time fixed effects and fixed effects for 2015 and 2016 zipcode-year. The controls are listed in section IV.2. Standard errors, in parentheses, are two-way clustered at the courtroom-year and individual level. See the text for additional details on the specification, outcome measures, and sample.

Table AVIII: Sub-group IV Estimates

	Homeless Status		Employment		Gender		Family Status	
	Homeless	Not Homeless	Employed	Unemployed	Female	Male	Has Child	No Child
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Homeless								
Years 1-2	0.17	0.099*	0.080	0.25**	0.16*	0.11	0.20*	0.11
	(0.22)	(0.053)	(0.086)	(0.12)	(0.081)	(0.080)	(0.11)	(0.079)
Any	0.32	0.063	0.052	0.24	0.16	0.078	0.15	0.14*
	(0.24)	(0.063)	(0.099)	(0.15)	(0.11)	(0.090)	(0.12)	(0.082)
Mental Health								
Years 1-2	0.24	0.061	0.11	0.052	0.085	0.13	0.071	0.095
	(0.17)	(0.053)	(0.068)	(0.13)	(0.059)	(0.099)	(0.084)	(0.096)
Any	0.13	0.12*	0.093	0.13	0.093	0.18*	0.0061	0.21**
	(0.19)	(0.070)	(0.087)	(0.14)	(0.085)	(0.10)	(0.11)	(0.098)
Earnings								
Years 1-2	-13.6	1.12	-2.86	-1.11	-7.43	9.48	-5.59	2.04
	(9.36)	(6.13)	(7.67)	(5.24)	(8.01)	(8.57)	(8.23)	(7.63)
Years 1-5	-18.3	-13.9	-18.4	-11.7	-23.3	10.6	-17.8	-5.20
	(19.9)	(15.2)	(17.5)	(11.5)	(19.1)	(20.1)	(18.5)	(17.9)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-Time-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	33416	141946	100157	75407	124353	51213	85912	89644

This table reports estimates of the effect of eviction using our IV specification for several sub-groups. Each row is an estimate of the effect of eviction on the outcome listed in the row groupings, where the time period is noted in the individual row. The homelessness variable is an indicator for applications to shelter. The mental health variable is an indicator for a mental health hospitalization. And the earnings variable is cumulative earnings in thousands (USD2016). The columns identify the corresponding sub-group. Columns (1)-(2) are for household heads who have previously applied to shelter ("Homeless") or never previously applied to shelter ("Not Homeless"), respectively. Columns (3)-(4) report the effect of eviction for those employed during the year before filing ("Employed") and those not employed in the year before filing ("Unemployed"). Columns (5)-(6) report the effect of eviction for household heads identified as female in the benefits data ("Female") and male in the benefits data ("Male"). Columns (7)-(8) report the effect of eviction separately for households with a minor member on the case ("Has Child") and without a minor listed on the case ("No Child"). We report results for two time windows: outcome data for available post filing quarters 1-8 (Years 1-2) and outcome data from all available post-filing quarters 1-40 (All Years). All parameters are estimated via 2SLS as in Equation 3. All specifications include court-by-time fixed effects and fixed effects for 2015 and 2016 zipcode-year. The controls are listed in section IV.2. Standard errors, in parentheses, are two-way clustered at the courtroom-year and individual level. See the text for additional details on the specification, outcome measures, and sample.

Table AIX: Matching and Attrition

	Match to Benefits	Missing SSN	Any Post Labor Record	Any Post Labor Record Q4+	Any Post Benefits Record	Any Post Hosp Record	Any Post Record
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Instrument	-0.026	0.019	0.055	0.050	0.130	0.023	0.068
	(0.058)	(0.046)	(0.097)	(0.093)	(0.110)	(0.096)	(0.083)
Observations	734165	177012	169607	169607	169607	169607	169607
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Court-Time-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

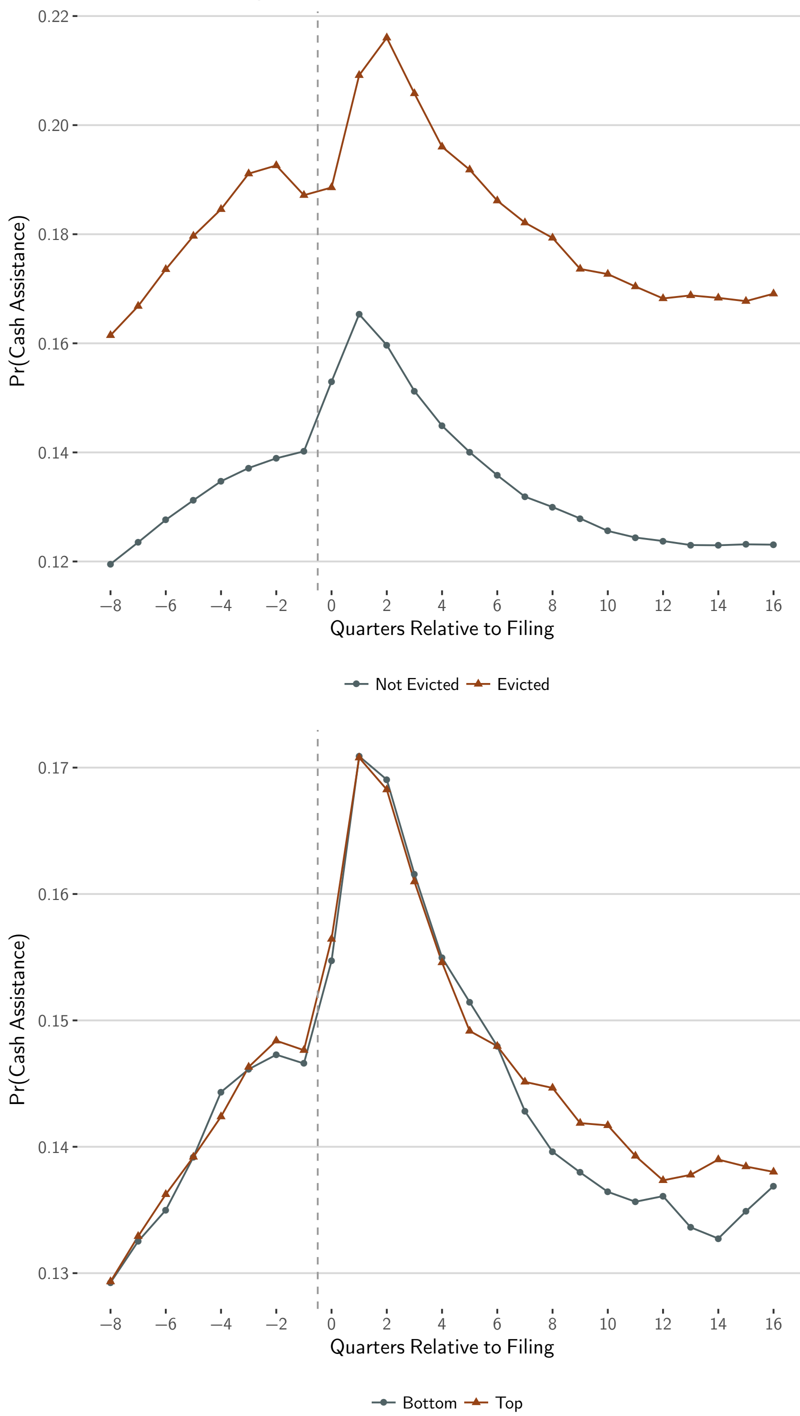
This table reports the relationship between our instrument and indicators of attrition or data matching success. In Column (1), we use all the court records and regress an indicator for matching to the benefits data (and thus being included in our estimation sample) on our instrument. In Column (2), we regress an indicator for having a missing social security number on our instrument. In Column (3), we regress an indicator for having all zeros for labor records in quarters 1-39 post-filing on our instrument. In Column (4), we regress an indicator for having all zeros for earnings record from the 8-39 quarter after filing on our instrument. In Column (5), we regress an indicator for having any benefits record after filing on our instrument. In Column (6), we regress an indicator for any hospitalization after filing on our instrument. In Column (7), we regress an indicator for having any earnings, hospitalization, or benefits record post-filing on our instrument. None of the specifications include controls. All specifications include court-by-time of filing fixed effects. Standard errors, in parentheses, are two-way clustered at the courtroom-year and individual level except in Column (1), where they are clustered at the courtroom-by-year level only.

Table AX: P-Values and Family-Wise Error Rates

	P-Value (1)	FWER P-Value (2)
Pr(Apply to Shelter)	0.026	0.052
Share of Days	0.091	0.104
Asthma	0.935	0.966
Heart-Stroke	0.858	0.942
Heart-Valve	0.257	0.542
Diabetes	0.266	0.532
Mental Health	0.084	0.132
Hypertension	0.353	0.531

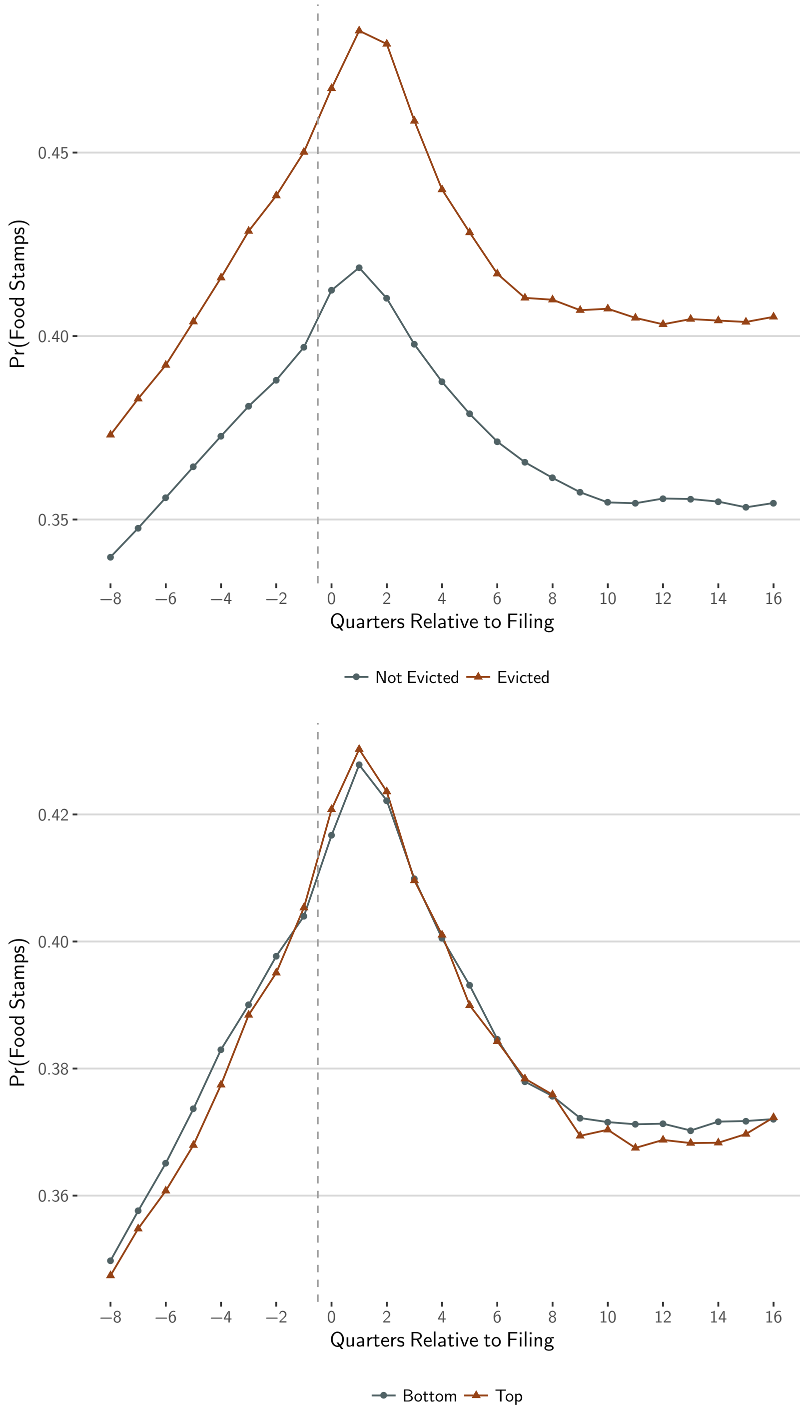
This table reports conventional p-values from our reduced form regression along with Family-Wise Error Rates calculated using the step-down method used in Jones et al. (2018) and developed by Westfall and Young (1993).

Figure AI: Cash Assistance



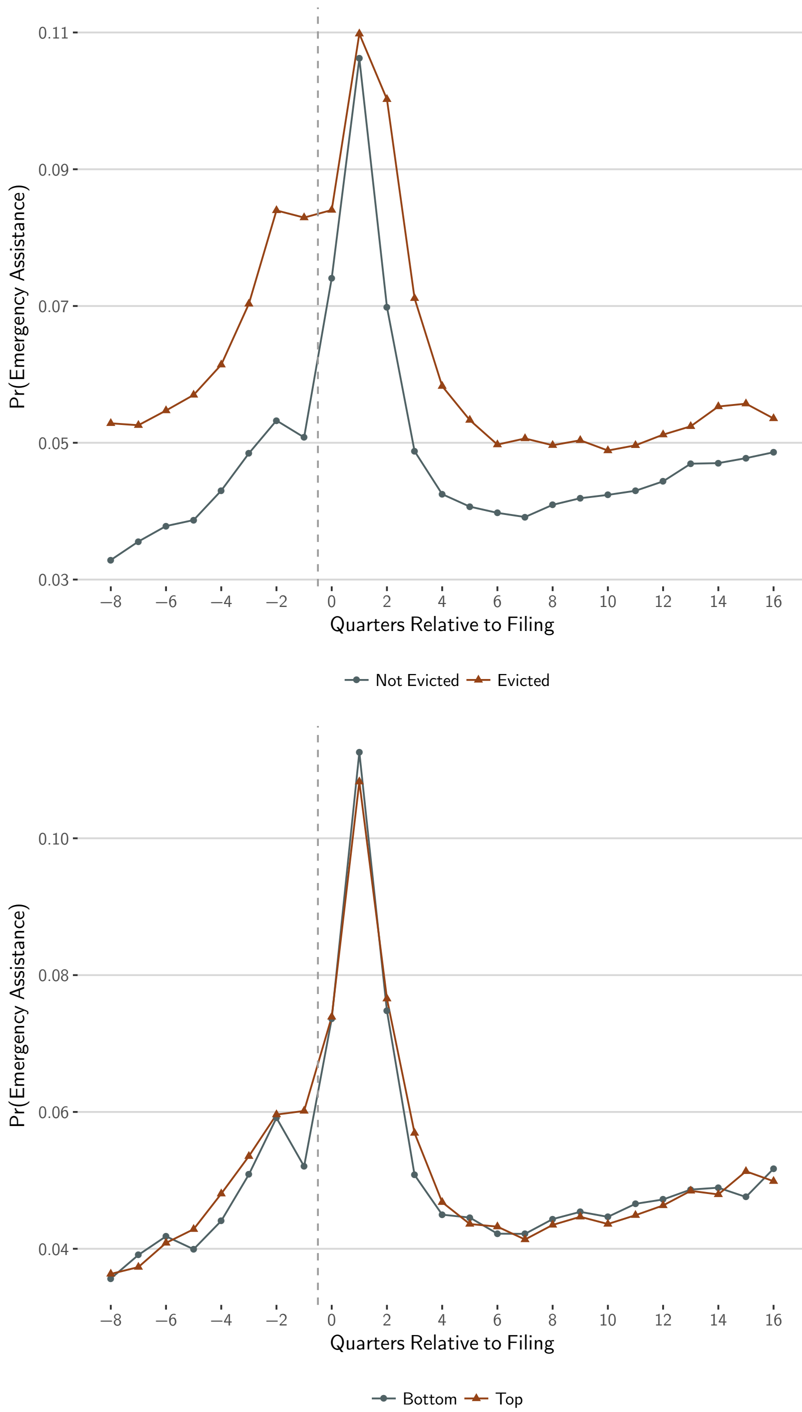
The top figure plots the probability of receiving Cash Assistance (TANF or Safety Net Assistance) for household heads in our sample by quarter relative to filing, separately for households that experience eviction and households that avoid eviction. The bottom figure plots the probability of receiving Cash Assistance (TANF or Safety Net Assistance) in our sample by quarter relative to filing separately for households assigned to a courtroom that is more likely to evict (top quintile of instrument) and households assigned to a courtroom that is less likely to evict (bottom quintile of instrument). See Section 4 for details on the instrument construction. Each plot residualizes the probability of receiving cash assistance by court-year-month-day-of-the-week, age-time, and court-time fixed effects, then adds back the mean for interpretation.

Figure AII: Food Stamps



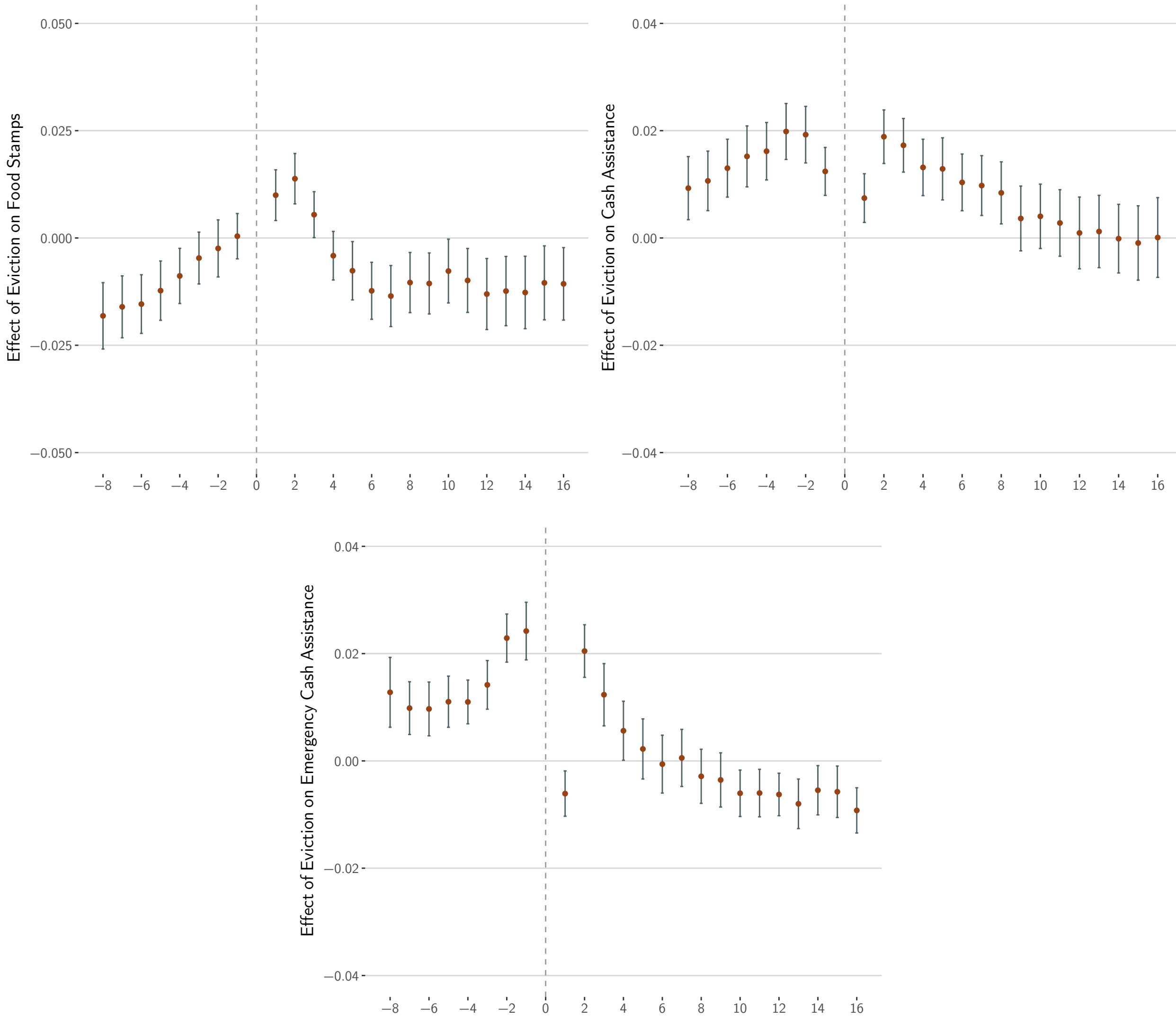
The top figure plots the probability of receiving Food Stamps (SNAP) for household heads in our sample by quarter relative to filing, separately for households that experience eviction and households that avoid eviction. The bottom figure plots the mean probability of receiving Food Stamps (SNAP) in our sample by quarter relative to filing separately for households assigned to courtroom that is more likely to evict (top quintile of instrument) and households assigned to a courtroom that is less likely to evict (bottom quintile of instrument). See Section 4 for details on the instrument construction. Each plot residualizes the probability of receiving SNAP by court-year-month-day-of-the-week, age-time, and court-time fixed effects, then adds back the mean for interpretation.

Figure AIII: Emergency Assistance



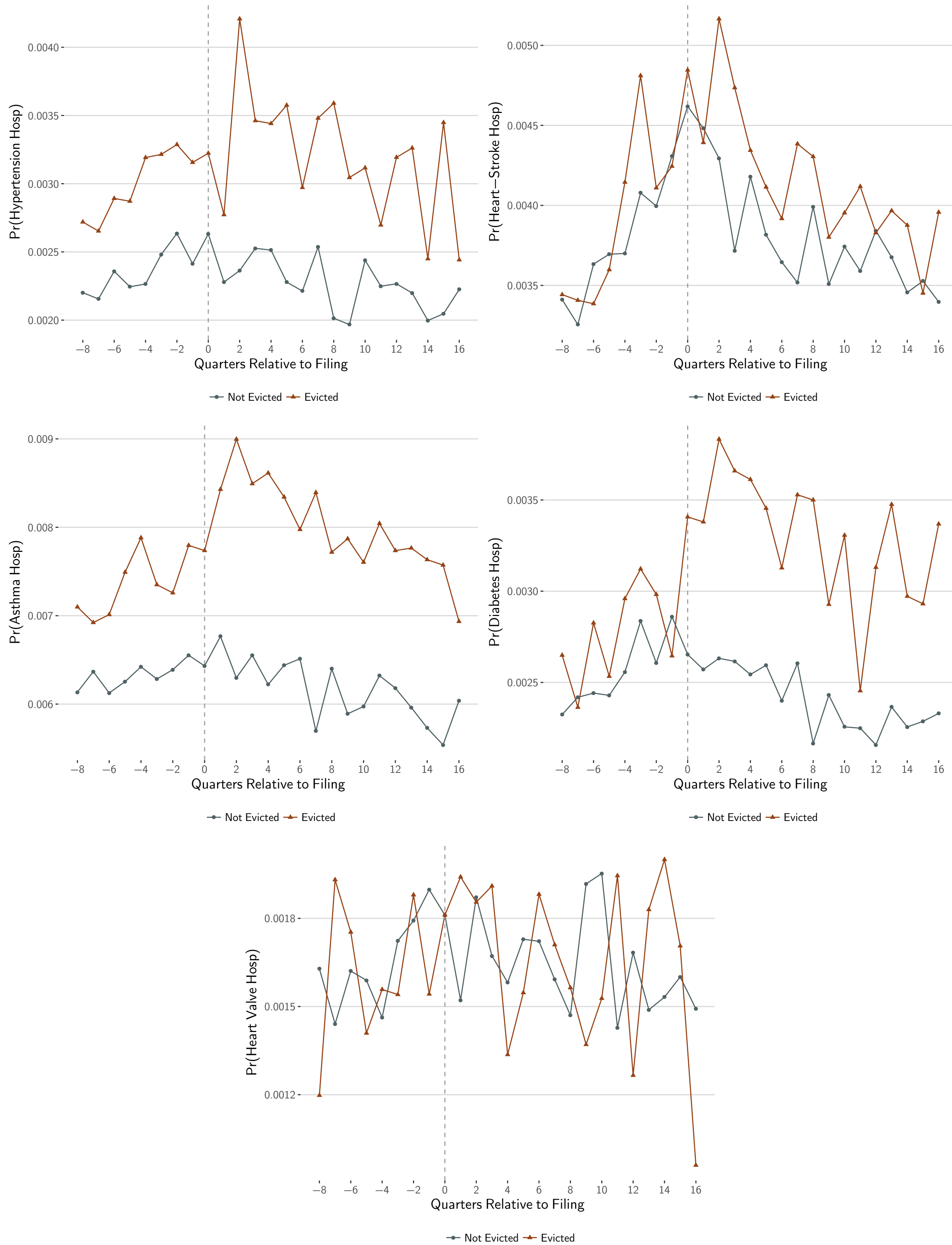
The top figure plots the probability of receiving Emergency Assistance ("One Shot") grants for household heads in our sample by quarter relative to filing, separately for households that experience eviction and households that avoid eviction. The bottom figure plots the mean quarterly employment rate in our sample by quarter relative to filing separately for households assigned to a courtroom that is more likely to evict (top quintile of instrument) and households assigned to a courtroom that is less likely to evict (bottom quintile of instrument). See Section 4 for details on the instrument construction. Each plot residualizes the shelter application rate by court-year-month-day-of-the-week, age-time, and court-time fixed effects, then adds back the mean for interpretation.

Figure AIV: Event Study Estimates: Public Assistance



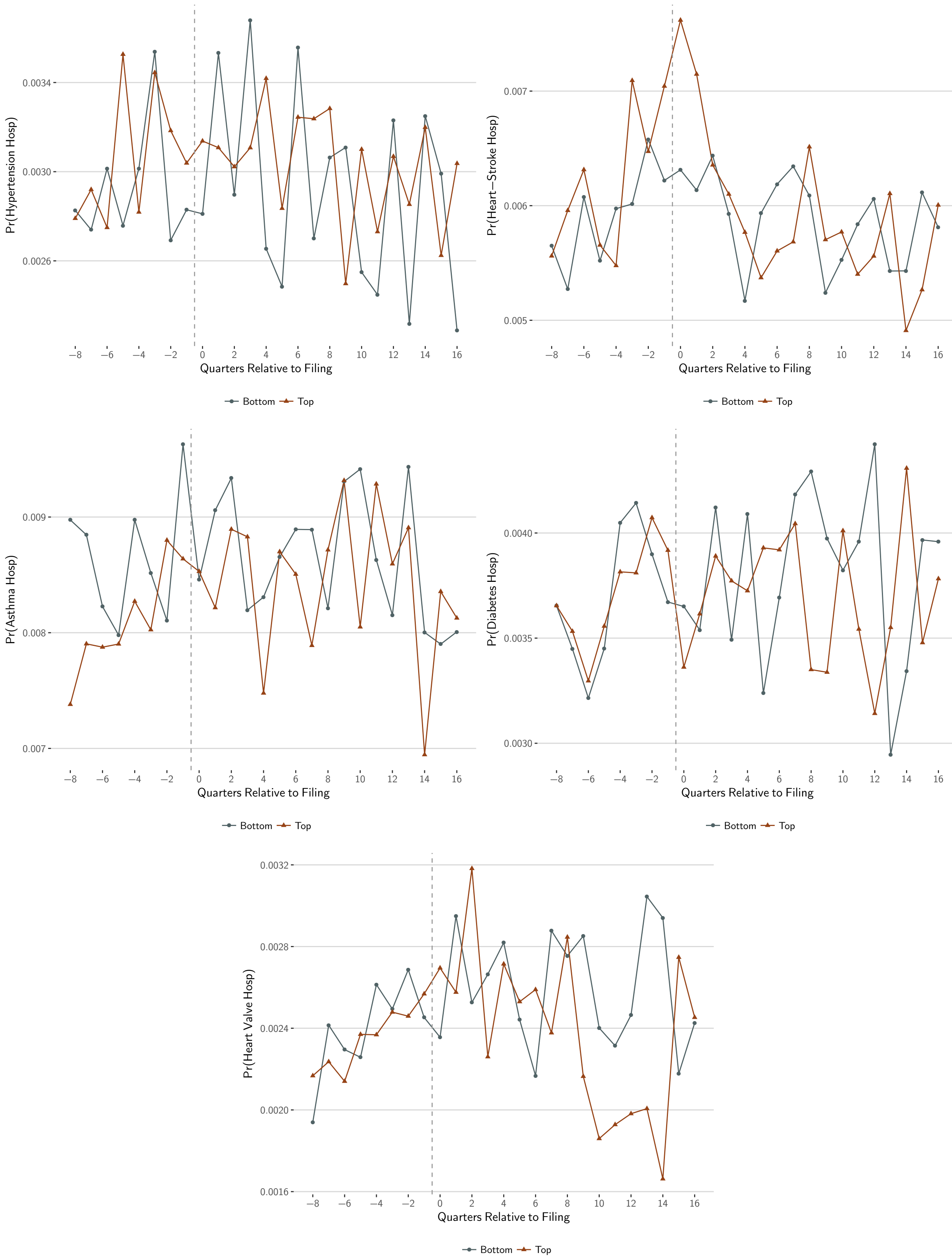
These figures plot the coefficients from our event-study/difference-in-differences specification for the effect of eviction on several outcomes: food stamps (top left); cash assistance (top right); and emergency assistance (bottom). The reported estimates come from the specification in equation 4. The horizontal axis denotes time relative to non-payment filing. Our estimates are normalized to be zero in the quarter of filing (dashed vertical line). Plot whiskers are 95 percent confidence intervals from standard errors clustered at the individual level.

Figure AV: Other Hospitalizations



These figures plot the probability of hospitalization for different conditions for household heads in our sample by quarter relative to filing, separately for households that experience eviction and households that avoid eviction. Each plot residualizes the probability of hospitalization by court-year-month-day-of-the-week, age-time, and court-time fixed effects, then adds back the mean for interpretation. The diagnosis groupings are described in Appendix G.

Figure AVI: Other Hospitalizations (Reduced Form)



These figures plot the probability of hospitalization for different conditions by quarter relative to filing, separately for households assigned to a courtroom that is more likely to evict (top quintile of instrument) and households assigned to a courtroom that is less likely to evict (bottom quintile of instrument). See Section 4 for details on the instrument construction. Each plot residualizes the probability of hospitalizations for mental health diagnoses by court-year-month-day-of-the-week, age-time, and court-time fixed effects, then adds back the mean for interpretation. The diagnosis groupings are described in Appendix t G.