Eviction and Poverty in American Cities: Evidence from Chicago and New York*

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Abstract

More than two million U.S. households have an eviction case filed against them each year, and local governments are increasingly pursuing policies to reduce the number of evictions. We study the consequences of eviction for tenants using newly linked administrative data from two major cities. We document that prior to housing court, tenants experience declines in earnings and employment and increases in financial distress and hospital visits. These pre-trends are more pronounced for tenants who are evicted, which poses a challenge for disentangling correlation and causation. To address this selection problem, we use an instrumental variables approach based on cases randomly assigned to judges of varying leniency. We find that receiving an eviction order increases housing instability—as measured by residential mobility, homeless shelter use, and interactions with homeless services—and reduces earnings, credit access, and durable goods consumption. Effects on housing and labor market outcomes are driven by impacts for female and Black tenants.

Keywords: eviction, homelessness, poverty, tenant protections, rental housing markets

JEL codes: J01, H00, R38, I30

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1 Introduction

More than two million eviction court cases are filed in the United States each year, predominantly involving low-income and minority households.[1] About half of these proceedings end in a court order for eviction: a judgment authorizing the landlord to repossess the property by obtaining a lockout. Recent research has argued that eviction is a cause of poverty, homelessness, poor health, and other forms of physical and material hardship (e.g., Desmond, 2012, 2016). In response, local and state governments have made reducing the number of evictions a policy priority.[2] Measuring the costs of an eviction to tenants is crucial for evaluating these policies and, more broadly, for understanding the role of housing instability as a driver of urban poverty and inequalities in wealth, socioeconomic mobility, and health, which have been documented in recent literature (Saez and Piketty, 2003; Chetty et al., 2014; Case and Deaton, 2015).

Despite the large number of tenants who interact with housing court each year, the consequences of eviction for households are not well documented or understood. Empirical research in this area is challenging due to both a lack of data linking households facing eviction to subsequent outcomes and the difficulty of separating the impact of eviction from correlated sources of distress such as job loss or declining health. This paper overcomes both of these challenges to provide new evidence on the effect of eviction on earnings, employment, residential mobility, homelessness, financial distress, and health. We link newly constructed data sets based on housing court records from New York, NY, and Cook County, IL, to a broad range of administrative data sets.[3] These linked data allow us to document and characterize tenants' outcome trajectories several years before and after their eviction case. To identify the causal impact of the eviction order, we use an instrumental variables (IV) research design that relies on the random assignment of cases to judges. We document that there is systematic variation in the rate at which different judges' cases end in eviction, and we use this stringency rate as an instrument for the case outcome. We complement this research design with panel data methods that allow us to bound the effects of eviction under alternative assumptions.

We begin by providing new descriptive evidence on the long-run dynamics of labor market,

[1] The Princeton Eviction Lab estimates that more than two million eviction cases were filed in each year since 2002, and about one million cases end in an eviction order (Desmond et al., 2018a). These numbers are lower bounds because the underlying data do not have national coverage. In the 2017 American Housing Survey, about 800,000 renter households reported being threatened with an eviction notice within the past three months, which extrapolates to 3.2 million over the year (U.S. Census Bureau, 2017). Although we must be careful about drawing conclusions from cross-country comparisons that use different types of data sources, a recent OECD report provides suggestive evidence that the United States is an outlier in the number of eviction cases filed per renter household, with a filing rate 1.5 times as high as the next-highest country (Canada) and at least 3.8 times as high as the remaining 10 countries for which data are available (OECD, 2020).

[2] Appendix A provides an overview of recently passed or proposed reforms, including expansions of financial assistance and eviction diversion programs and increases in legal protections for tenants.

[3] The City of Chicago is located entirely within Cook County, IL, and eviction cases filed in the city represent about 75 percent of the county's case volume.

housing, financial, and health outcomes in the years preceding and following the court case. Compared to a random sample of tenants from the same neighborhoods, tenants in housing court have lower earnings, lower employment, less access to credit, and more debt in collections. Similar but smaller disparities exist between evicted tenants and tenants in housing court who are not evicted. These disparities are present several years prior to the filing of the case and persist after the case is resolved. Both evicted and non-evicted tenants in housing court experience striking negative trends in earnings, employment, and credit scores, and increasing trends in hospital visits, unpaid bills, and payday loan inquiries in the two years leading up to the case filing. These pre-filing trends are steeper for cases ending in an eviction order. Taken together, these findings highlight that there is both selection into court and selection into court outcomes, motivating our quasi-experimental approach using the random assignment of judges.

Using the IV approach, we first study the effect of an eviction order on tenants' housing situation in the immediate aftermath of the court case. Within the first year, an eviction order increases the probability of observing the tenant at a new address by 10pp (31% of the non-evicted mean, $p = 0.008$) and increases the probability of experiencing at least one emergency shelter stay by 3pp (200% of the non-evicted mean, $p = 0.018$). We interpret these results as showing that an eviction order disrupts the tenant's housing situation in the period immediately following the court case, which could have direct costs for tenants and could adversely affect tenants' labor market, health, and credit outcomes. These effects persist over the next two years, with estimated increases in the probability of moving of 10pp ($p = 0.016$) and in the probability of interacting with homeless services of 3.5pp ($p = 0.065$).

Eviction also negatively impacts labor market outcomes. Our IV estimates imply that eviction lowers earnings by \$352 per quarter (8% of the non-evicted mean, $p = 0.081$) in the two years after filing. While this estimate is smaller than the estimate from a simple OLS regression with demographic controls ($-\$842$, $p = 0.000$), it suggests that eviction has economically meaningful effects on earnings—indeed, the IV estimate is very similar to evicted tenants' average drop in quarterly earnings in the year leading up to case filing (-\$337). Impacts for female and Black tenants drive the consequences of eviction for labor market outcomes, residential mobility, and homelessness. Among women, eviction reduces earnings by an estimated \$532 per quarter (12.6% of the non-evicted mean, $p = 0.008$). Point estimates for women are also larger for the effect of eviction on longer-run residential mobility (12.4pp, $p = 0.008$), and especially the probability of entering emergency shelter (4.5pp, $p = 0.018$) and interacting with homeless services (7.1pp, $p = 0.001$). This finding is consistent with ethnographic research that suggests eviction may have a larger impact on women (Desmond, 2012; Desmond et al., 2013; Desmond, 2016). For Black tenants, we find a negative employment estimate of 6.3pp (10.9% of the non-evicted mean, $p = 0.042$) over the two years after filing, about 2.5 times as large as the overall estimate of the impact on employment (-2.3pp, $p = 0.250$). Black households may experience more adverse impacts of eviction because of discrimination while searching for new housing (Bayer et al., 2017;

Christiansen and Timmins, 2019).

Next, we examine the effect of eviction on measures of financial strain using data from credit reports.[4] Eviction reduces access to credit, as proxied by credit scores and open revolving accounts. We estimate that eviction lowers credit scores by 12 points ($p = 0.045$) and increases the probability of having no source of revolving credit by 7.5pp (18%, $p = 0.018$). Eviction also negatively impacts the probability of having an auto loan, which may be interpreted as a proxy for durable goods consumption, by 4.5pp (27%, $p = 0.061$).[5]

Finally, we consider the impact of eviction on physical and mental health outcomes. These outcome data are only available for New York. Hospital visits and emergency room trips rise in the lead-up to filing, peaking in the quarter of filing. Eviction then increases the number of hospital visits in the year following court filing by 0.193 (29%, $p = 0.041$) and increases visits for mental health-related problems during the same period by 0.05 (100%, $p = 0.074$). These effects on mental health and hospital use are concentrated in the first year after the case.

The IV estimates are relevant for policy changes that affect cases that would have received a different outcome had they been assigned a different judge ("complier cases"). While this set of cases is of direct interest, some policy changes might affect non-complier cases. To understand whether our findings might extend to such cases, we compare the IV estimates to estimated bounds on the average causal impact of an eviction for all those who are evicted (i.e., the average treatment effect on those who are treated, or ATT). We derive a set of assumptions that do not require the parallel trends assumption but which allow us to use a set of difference-in-differences estimates to place bounds on the ATT parameter. Across different outcomes, these estimates suggest that impacts for compliers are qualitatively similar although in some cases larger than they are for the average evicted tenant. This result suggests that interventions that help tenants avoid eviction are likely to be most impactful when they are targeted at marginal cases.

This paper contributes to research that finds that eviction is negatively associated with tenants' physical and mental health (Burgard et al., 2012; Desmond and Kimbro, 2015; Sandel et al., 2018) and positively associated with depression, stress, material hardship (Desmond and Kimbro, 2015), suicide (Fowler et al., 2015; Rojas and Stenberg, 2016), job loss (Desmond and Gershenson, 2016a), and homelessness (Crane and Warnes, 2000; Phinney et al., 2007). Desmond and Bell (2015) provide an overview of this literature. These studies largely use ethnographic methods and relatively short-term surveys of households at risk of eviction. We make three main contributions to this literature. First, we characterize the labor market and housing experiences of tenants who appear in housing court using large-scale and linked administrative data. These

[4]Several studies have used credit bureau data to measure financial strain, including studies of the consequences of health shocks (Mazumder and Miller, 2016; Dobkin et al., 2018) and bankruptcy (Dobbie et al., 2017). Our data additionally include information on payday loans in Cook County, which are common among low-income households (Bhutta et al., 2015; Skiba and Tobacman, 2019).

[5]Dobkin et al. (2018) and Agarwal et al. (2020) are examples of other studies that use auto loans and leases to proxy for durables goods consumption.

data allow us to avoid concerns with survey data such as selective non-response and misreporting of income (Meyer et al., 2015). We are also able to follow individuals for several years and, for many outcomes, across state lines. Second, we provide a unified analysis across two major U.S. urban areas not previously studied and for a wide range of outcomes. Finally, this paper is the first to study the impact of eviction using a quasi-experimental research design: random variation in judge stringency allows us to separate the causal effect of eviction from correlated sources of distress, which is new to the literature on eviction.[6]

While there is relatively little work on eviction in economics, related work examines the impact of homeowners' foreclosure on health outcomes (Currie and Tekin, 2015), subsequent homeownership, housing and neighborhood conditions (Molloy and Shan, 2013), and credit scores (Brevoort and Cooper, 2013). A related study by Diamond et al. (2019) examines the impact of foreclosure on residential mobility, homeownership, divorce, measures of neighborhood quality, and credit reports using a randomized judge design. As part of their analysis, Diamond et al. (2019) consider the impact of a landlord's foreclosure on tenants. We view our work as complementary, since eviction and foreclosure are different court processes and affect different populations.[7] We consider several additional dimensions that eviction is likely to impact, including employment, earnings, homeless shelter entries, and health outcomes.

Lastly, we contribute to recent work studying the incidence and drivers of eviction filings. Gallagher et al. (2019) find that expansions of ACA Marketplace subsidies substantially reduced eviction filing rates, and Zewde et al. (2019) find that Medicaid expansions were associated with reductions in county-level filing rates and eviction rates. These results are consistent with our findings that adverse health, labor market, and credit outcomes precede, and may contribute to, appearing in housing court and being evicted. Desmond et al. (2013) point to children as a risk factor for eviction, consistent with our finding that women are over-represented in housing court relative to the general low-income renter population. Desmond and Gershenson (2016b) find that family size, job loss, neighborhood crime, eviction rates, and network disadvantage are additional risk factors. Kroeger and La Mattina (2020) find that criminal nuisance ordinances substantially increase eviction filing rates and eviction rates. Finally, Fetzer et al. (2020) study the effect of cuts to rental subsidies in the UK and find that these substantially increased rent arrears and evictions.

The remainder of this paper is organized as follows. Section 2 provides institutional details relevant for understanding the eviction process in Cook County and New York. Section 3

[6] In other settings, many papers have used the random assignment of judges to study the impact of court orders, including incarceration (Kling, 2006; Aizer and Doyle, 2015; Mueller-Smith, 2015; Bhuller et al., 2018, 2020; Norris et al., 2019), bankruptcy protection (Dobbie and Song, 2015), disability claims (Maestas et al., 2013; Dahl et al., 2014; French and Song, 2014), and foster care placement (Doyle, 2007; Bald et al., 2019).

[7] One distinction is that a landlord's foreclosure need not lead to the eviction of their tenants. Under the Protecting Tenants at Foreclosure Act (2009), the new owner of a foreclosed property is required to continue the lease agreed upon by the previous landlord.

describes data collection and record linkage processes. Section 4 describes our sample, provides new descriptive evidence on the evolution of outcomes among evicted and non-evicted tenants around a court filing, and explores selection into eviction. Section 5 formalizes our empirical framework and tests the key underlying assumptions. Section 6 presents the main results of our analysis. Section 7 concludes.

2 Institutional context

This section describes the legal process of eviction and other relevant institutional details in Cook County and New York. Appendix B contains additional details.

2.1 The housing court process

In Cook County and New York, as in most jurisdictions, the housing court process begins with a notice served to the tenant by the landlord, followed by a court filing, one or more court hearings, and finally a judge's decision on whether to issue an order authorizing the landlord to repossess the property by asking an enforcement officer to perform a lockout.

A landlord must serve the tenant a written notice to begin an eviction. The notice typically includes the reason for terminating the lease and the number of days until termination. Permissible causes for filing an eviction are nonpayment of rent or violation of any lease terms. In Cook County, ending a tenancy at the end of the lease is also a permissible cause. If the number of days in the notice elapses without resolution, the landlord or their attorney has the right to file a case in housing court. Landlords file different types of eviction cases based on their desired type of legal action. They may seek only possession of the property (*single action* in Cook County, *holdover* in New York), or both possession and a money judgment (*joint action* in Cook County, *non-payment* in New York). Cases where the landlord requests a money judgment are the majority of housing court cases in both cities: according to our data, 86 percent of New York cases are non-payment proceedings, and 80 percent of Cook County cases are joint action. Although it is difficult to establish whether tenants in fact have rental arrears in cases where the landlord is requesting a money judgment, studies of housing court in other cities have found that non-payment of rent is the most common reason for eviction.[8]

In Cook County and New York, the landlord has no discretion over which court will handle their case, since the court where the landlord can file the case is determined by the location of the property under dispute. Cases are randomly assigned to courtrooms within a court by a computer algorithm. Judge assignments to courtrooms are set in advance, and therefore random assignment to a courtroom is effectively random assignment to a judge. Cases may be ruled on

[8]See, e.g., Desmond et al. (2013), who study evictions in Milwaukee. In the 2013 American Housing Survey, 75 percent of households who reported being threatened with an eviction reported that the reason for the threat was failure or inability to pay rent.

by a different judge than the one initially assigned, for example if the schedule for the initially assigned judge changes. Either party has the right to request a trial before a jury, but jury trials are rare (within our samples of court records: 3 percent of cases in Cook County and less than 1 percent in New York).

When the landlord and tenant meet in a courtroom, the hearing is typically brief. Court observation studies have found that an eviction hearing lasts only a few minutes on average (e.g., Doran et al., 2003). Tenants are much less likely than landlords to be represented by attorneys: in our data, 3 percent of tenants in Cook County and 1 percent of tenants in New York are represented by an attorney.[9] This stands in contrast to the high proportion of cases with an attorney on the landlord side (75 percent in Cook County and 95 to 99 percent in New York). We define an eviction as a case ending with an eviction order: a judgment that returns possession of the property to the landlord.[10] In New York, the fraction of all cases that ends in an eviction order is 35 percent, while the fraction in Cook County is 66 percent.

The alternative to an eviction order is often a formal agreement between landlord and tenant that is approved by the judge. Such agreements typically include a payment plan, and they may also set terms for continued occupancy of the unit.[11] The landlord may return to court to pursue an eviction order if the tenant doesn't satisfy the terms of the agreement. Among cases without an eviction order in New York, approximately 64 percent end with such an agreement. For Cook County, we estimate the share of not-evicted cases with a formal agreement to be upwards of 39 percent.[12] Cases can also be discontinued, which happens if the landlord decides not to pursue the case further (29 percent of not-evicted cases in New York and at most 45 percent in Cook County). In a small fraction of cases, there is a dismissal that bars the landlord from bringing another eviction case with the same allegations against the tenant (five percent of not-evicted cases in New York and Cook County).

An eviction order may or may not be followed by the execution of a warrant, during which an enforcement officer performs a lockout.[13] A lockout involves changing the locks and may involve the removal of the tenant's possessions. Whether a lockout occurs depends on several

[9]While not relevant for our sample period, the rate of representation for tenants has grown significantly in New York City since universal access to legal representation for low-income tenants facing eviction was enacted in August of 2017.

[10]This definition of an eviction is used by Desmond et al. (2018b), who compile the most complete national database of eviction filings and orders to date, based on court records. We discuss how we process the court records to determine whether a case ended with an eviction order in Appendix C.3.

[11]For example, Summers (2020) studies housing court cases in New York and finds that among cases with agreements, they are almost always payment plans, with only 1% of these cases involving a move-out agreement. In Section 4.2 we study the probability that evicted and non-evicted tenants actually move using our linked dataset.

[12]The electronic court docket, from which we collect our court data for Cook County, does not record whether there was a formal agreement. We hand-collected and coded court microfilm records for a random sample of court cases ending in dismissal. In Appendix C.4 we provide details on how we process the microfilm information to arrive at our estimates for outcomes in not-evicted cases.

[13]Lockouts are performed by a Sheriff's deputy in Cook County and typically by a City Marshal in New York. In other jurisdictions, the enforcement officer is sometimes referred to as a Constable.

factors. For example, the landlord may choose not to request a lockout because they must pay an additional fee. The landlord and tenant may also come to an informal agreement. Finally, the tenant may choose to vacate the unit before a Sheriff or Marshal is scheduled to conduct the lockout, and the landlord may cancel the lockout in that case. Lockouts occur in 26 percent of cases ending with an eviction order in Cook County and 31 percent of cases ending with an eviction order in New York.[14]

There are several reasons an eviction order may affect tenants' future outcomes. First, an eviction order may prompt a tenant to move following a lockout or in expectation of a lockout and to incur the costs associated with searching for new housing, relocating, and reorienting the household's work and schooling arrangements. Second, eviction orders and filings are public records in most jurisdictions, and an order can also be recorded as a civil judgment on the tenant's credit report. Eviction filings and eviction orders are commonly used in background screenings by landlords, employers, and creditors, and therefore an eviction can make it harder for tenants to secure future rental contracts, employment, or loans. Finally, a money judgment may be used by the landlord to obtain an order for garnishment of wages, tax refunds, or other assets. Though, wage garnishment requires getting an additional court judgment, and is rare in practice.

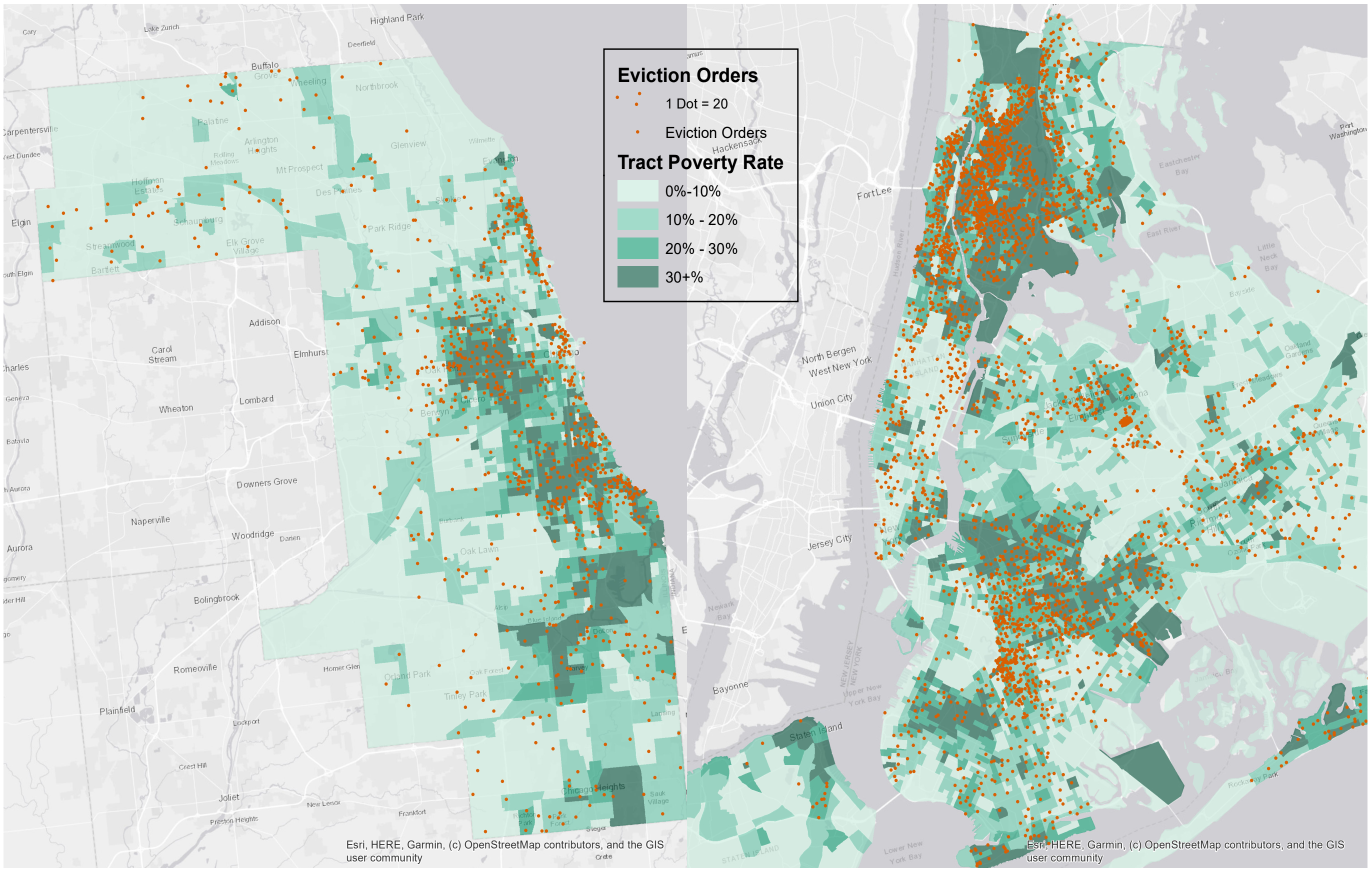
2.2 Eviction rates and geographic concentration

Each year, around 240,000 cases are filed in New York and around 33,000 cases are filed in Cook County.[15] Figure 1 maps the 2010 eviction rate (the number of evictions relative to the number of occupied rental units) by census tract for each location. While evictions occur across all of Cook County and New York, Figure 1 shows that they are concentrated in neighborhoods with higher poverty rates: 58 percent of evictions in New York and 46 percent of evictions in Cook County occur in high-poverty neighborhoods, which are defined as census tracts with more than 20 percent of residents living below the poverty line. This spatial concentration is consistent with Desmond (2012), Desmond and Shollenberger (2015), and Desmond and Gershenson (2016b), who find that eviction is common in poor communities in Milwaukee. Appendix Figure B.2 shows how eviction filing rates vary across neighborhoods. Some neighborhoods have annual eviction filing rates as high as 1 in 10 renter households in Cook County and as high as 1 in 5 renter households in New York.

[14]Lockout rates reported here are not based on our main court record data in Cook County. The data set used to calculate these rates for Cook County is obtained from the Sheriff's Office and only covers the years 2011 to 2016. For New York, the court data contains information on executed warrants, which we supplement with records on executed lockouts from the Department of Investigations.

[15]Appendix Figure B.1 provides trends in eviction filing and evictions over time and shows that the number of evictions per year has been relatively stable over the period we study.

Figure 1: Evictions and Neighborhood Poverty



Notes: This figure depicts the approximate locations of court ordered evictions in Cook County (left) and New York (right) in 2010 (each dot represents 20 eviction orders in the census tract), along with the poverty rate of the census tract (based on 2006–2010 American Community Survey 5-year averages).

2.3 Rental housing markets and local housing policies

We note a few salient features of the broader institutional settings in Cook County and New York that may be important for contextualizing our estimates of the impact of eviction. First, while most U.S. cities do not have rent control or rent stabilization policies, New York is among the few that do, while Cook County does not.[16] The presence of rent control or rent stabilization may affect landlords' incentives to evict as well as tenants' ability to find new housing if evicted (Diamond et al., 2019). By studying these two cities, we capture both a more- and a less-regulated rental housing market and can examine differences in estimates across the two locations (which we do in Section 6.5). Second, some housing assistance policies differ between the two jurisdictions. In particular, homeless shelter capacity or eligibility rules may play a role in determining post-eviction outcomes. New York has a right-to-shelter policy, which guarantees all individuals determined to be homeless access to shelter accommodations. In contrast, Cook County does

[16]Other cities with some form of rent control or stabilization are Washington, D.C., and several cities in California, Maryland, and New Jersey.

not have right-to-shelter, and homeless individuals may be turned away from shelters that are at capacity.[17] In New York, less than 5 percent of the homeless are unsheltered, while in Cook County about a quarter of the homeless are unsheltered (Henry et al., 2019). As such, homeless shelter use may be more common among individuals who are evicted in New York, while in Cook County evicted tenants may be more likely to be unsheltered if homeless.

3 Data and linkage

Our empirical analysis uses court records from Cook County, IL, and New York, NY, linked to administrative data sets measuring earnings and employment, residential address histories, and interactions with the homelessness services system. In addition, we link the Cook County and New York court data to credit bureau records, and we link the New York court data to records of hospital visits. This section summarizes our data sources, sample construction, data linkage, and key outcomes. We provide additional details in Appendix C.

3.1 Court data

The basis of our linked data set is the near-universe of court records for Cook County for the years 2000–2016 and for New York for 2007–2016. Each court record includes the residential address of the disputed housing unit and the names of one or more tenants. The unit of analysis is therefore the case-individual, so that each tenant who appears as a defendant in the case will have a separate record.[18] Other key elements we observe in the court records are case type (single action or joint action in Cook County, holdover or nonpayment in New York), filing date, courtroom and date assignment, name of the landlord, attorneys' names, the amount claimed by the landlord (*ad damnum* amount), and whether an eviction order was granted. We also observe other judge decisions throughout the case, such as whether to grant a continuance and whether to grant an appeal or a stay of the eviction order. In Cook County, the data include the value of the money judgment awarded, if any, and the name of the judge associated with each action in the court record. We link court records to data held by the Sheriff's office (Cook County) or Marshal's office (New York) so that we know in each case whether a lockout is executed.

We impose several restrictions on the court samples. We drop eviction cases associated with businesses, cases associated with condominiums (or co-ops in New York), and cases with a missing defendant name or address. We also drop cases involving more than $100,000 in claimed damages. In New York, some types of cases are not randomly assigned to courtrooms and are excluded: cases involving public housing units, cases for family members of active military personnel, and

[17] As of this writing, New York, Massachusetts, and Washington, D.C., have a right to shelter.

[18] Individuals living in the unit who are not named in the case filing, which may include children, other family members, or cohabiting partners, are not included in the sample.

cases involving the District Attorney's office or the New York City Police Department. For the IV analysis, we further restrict the sample to cases in which the judge presided over a specified minimum number of cases. In Cook County, we restrict to cases where the judge presided over at least 100 cases that year and in which at least two judges presided over eviction cases in that week and court. In New York, we drop courtrooms with fewer than 500 cases in a year, and instances in which there are fewer than two courtrooms hearing cases in the week. The full IV sample, before linking to outcomes data, includes around 530,000 case-individuals for Cook County and 570,000 for New York.[19] In both jurisdictions, we define an eviction as a case ending with an eviction order. Appendix C.3 explains in more detail how we construct eviction orders from the housing court data.

3.2 Outcomes data

We link the court records to multiple administrative data sets. Below, we describe these data sets and define the outcomes we consider in our analysis. We separately analyze linked records for Cook County and New York because of data security restrictions. Additional details on data linkage and sample construction are provided in Appendix C.

Earnings and employment. In both settings, we measure earnings and employment using quarterly records derived from state Unemployment Insurance (UI) data systems. Our main earnings outcome is quarterly earnings, and the main employment variable is an indicator for positive employment income. We restrict the analysis to tenants who are 18 to 55 years old at the time of case filing to exclude individuals aging into retirement. Earnings and all other dollar amounts are expressed in 2016 USD using the CPI-U for the two metropolitan areas we study. Employment and earnings records only cover formal employment and exclude individuals not covered by UI benefits, such as the self-employed.

We obtain employment and earnings data linked to the Cook County court records from the Longitudinal Employer-Household Dynamics (LEHD) Employer History File, a restricted Census Bureau data set (see Abowd et al., 2004; Vilhuber, 2018, for more details on the LEHD). We measure employment using the LEHD file that contains a flag for any positive earnings in any of the fifty states or the District of Columbia. We observe quarterly earnings for Illinois, the District of Columbia, and eleven other states for which we were granted access to earnings

[19] Appendix Table C.1 details how the number of observations changes by imposing these restrictions for the Cook County sample. Appendix Table C.2 does the same for New York.

data.[20] The years available vary by state, but all states have data from 1995 to 2014.[21] The New York court records are linked to data from the New York State Department of Labor and hence do not include states other than New York. These data cover the years 2004 to 2016.

Residential mobility. In Cook County, our primary data source for measuring residential address changes is the Census Bureau's Master Address File Auxiliary Reference File (MAFARF), which provides addresses of residence and associated Census geographic location identifiers by year.[22] We use this data set to construct measures of whether a tenant is observed at the filing address at a given point in time. For New York, we combine two sources of address histories: consumer reference data from Infutor Data Solutions and administrative benefits records.[23] Similar to the Cook County data, we define a tenant as not at their eviction address if we observe them at a different address than the one listed on the court filing according to either the benefits data or the Infutor data in the relevant outcome window. A concern with these sources of address data is that the availability of address information could be affected by an eviction. However, Appendix Table C.3 shows that eviction is only weakly correlated with the probability of having an address from either the Infutor data or the benefits data. Additionally, Appendix Table C.4 shows that estimates of the impact of eviction on residential mobility in New York are not particularly sensitive to using either data source on its own in cases when both are available.

[20]The eleven states are the LEHD "Option A" states: Arizona, Arkansas, Delaware, Indiana, Iowa, Kansas, Maine, Maryland, Nevada, Oklahoma, and Tennessee. We find little evidence of eviction orders affecting migration out of the observed set of states. Using the national employment indicators, we find that two to three years after the eviction case, evicted tenants are 0.6 percentage points more likely to be employed in one of the states for which we do not observe earnings, relative to non-evicted tenants.

[21]The quarterly earnings variable is set to zero when the national indicator for positive earnings is zero. It is set to missing and excluded from the analysis when the national employment indicator is one but we do not observe earnings.

[22]The MAFARF provides a link between unique individuals from various administrative records (identified by Protected Identification Keys, or PIKs) and unique addresses (identified by Master Address File Identifiers, or MAFIDs). Its source data include "the Census Numident, the 2010 Census Unedited File, the IRS 1040 and 1099 files, the Medicare Enrollment Database (MEDB), Indian Health Service database (IHS), Selective Service System (SSS), and Public and Indian Housing (PIC) and Tenant Rental Assistance Certification System (TRACS) data from the Department of Housing and Urban Development, and National Change of Address data from the US Postal Service" (Finlay, 2016). The unique addresses are in the Census Bureau's Master Address File (MAF), which is an "accurate, up-to-date inventory of all known living quarters in the United States, Puerto Rico, and associated island areas" and is used to support Census surveys such as the Decennial Census and American Community Survey (U.S. Census Bureau, 2020).

[23]Infutor compiles data from several sources including public and private telephone billing data, deed and property information, customer information from utility companies, subscription services, and other sources. The data have been used to track housing instability among low-income tenants but may miss households with more limited paper trails (Phillips, 2020). The benefits records contain address histories for households as long as they continued to receive or apply for assistance from any of the covered programs from the New York City Human Resources Administration: Medicaid, the Supplemental Nutrition Assistance Program (SNAP), Temporary Assistance for Needy Families (TANF), and other city-specific cash subsidies.

Homelessness. We measure homelessness in both settings using local Homeless Management Information System (HMIS) data.[24] The Cook County HMIS database is managed by All Chicago and is similar to the data set used in Evans et al. (2016). The HMIS records are linked to Census identifiers and are studied within the Census RDC. They capture the years 2014 to 2018 and include individual-level data on stays in emergency shelters as well as other interactions with homelessness prevention services. Similarly, the HMIS data in New York capture individual-level applications to and stays in the city's vast shelter system, as well as "diversions" through homeless prevention programs. These data come from the New York City Department of Homeless Services and cover the years 2003 to 2017. We use these data to construct two outcomes. The "Emergency Shelter" outcome is an indicator for whether the individual has any stay at an emergency shelter. "Any Homeless Services" is an indicator for whether the individual has interacted with any homeless services. In Cook County, homeless services include emergency shelters use, permanent supportive housing, coordinated assessment of need, rapid rehousing, and street outreach. In New York, this indicator additionally includes applications to shelter, which cover instances where families are diverted or deemed ineligible.[25]

Financial distress. To measure financial distress, we draw from credit records held by Experian, one of the three major credit bureaus in the United States.[26] For Cook County, the linked credit report data are biennial snapshots from March 2005 to March 2017 and an additional snapshot for September 2010. For New York, we have quarterly credit report data from March 2014 to September 2019. For both locations, we measure overall financial health using VantageScore 3.0, which is on a scale of 300–850; scores under 600 are considered subprime. We measure unpaid bills as the total balance in collections. Collections represent unpaid debt, such as credit card balances, which the original lender decides to turn over to a collections agency following a period of delinquency (typically at least 30 days). We construct an indicator for any positive balance on an auto loan or lease, which may proxy for durable goods consumption (Dobkin et al., 2018; Agarwal et al., 2020).[27] We measure whether the tenant has no open source of revolving credit, such as a credit card, which serves as a proxy for having limited access to

[24] Maintaining an HMIS database is a data collection requirement imposed by the U.S. Department of Housing and Urban Development for participation in the Continuum of Care and Emergency Solutions Grant programs.

[25] New York City has a "right to shelter," and therefore all single adults applying to shelter are eligible for shelter accommodations. However, families, unlike individuals, can be ineligible for shelter. Families are also occasionally diverted from shelter, meaning they are directed to benefits or relocation assistance or otherwise helped to find other housing options.

[26] Avery et al. (2003) provide a detailed description of these data. We follow existing literature in the selection of credit bureau outcomes (Dobbie et al., 2017; Dobkin et al., 2018; Miller and Soo, 2020a).

[27] The majority of auto purchases in the U.S. are financed with debt (in 2018, 85% of new vehicle purchases and 54% of used vehicles were financed with a loan or lease according to Experian, 2019).

credit. We also observe payday loan account inquiries for individuals in Cook County.[28]

Health. For our New York sample, we measure health outcomes using data from the New York State Department of Health's Statewide Planning and Research Cooperative System. This data set includes all inpatient and outpatient (including Emergency Department) hospital visits in New York State from 2004 to 2016.[29] For each hospital visit, the data include the date of intake and a primary diagnosis code (ICD-9 code). We focus on the total number of hospital visits, including inpatient or outpatient visits, the total number of emergency department visits, and the total number of hospital visits for mental health conditions.[30]

3.3 Data linkage

We link court records to other administrative data sets using tenant names and addresses. To link Cook County court records to Census Bureau-held data sets, the Census Bureau used names and addresses to link individuals to their unique Protected Identification Key (PIK). PIKs are assigned through the Person Identification Validation System (PVS), which uses probabilistic matching to link individuals to a reference file constructed from the Social Security Administration Numerical Identification File and other federal administrative data (Wagner and Layne, 2014). The PIK rate for the Cook County sample is 52 percent. PIKs are then used to link to other restricted data sets held in the Census Bureau Research Data Centers (RDCs).

To link New York court records to outcomes, we first use names and addresses to link individuals to historical benefits data from the New York City Human Resource Administration, which include individuals receiving Medicaid, the Supplemental Nutrition Assistance Program (SNAP), Temporary Assistance for Needy Families (TANF), or other city-specific cash subsidies.[31] Appendix C.7 describes this process in detail. The benefits data, which are available from 2004 to 2016, have additional identifiers including Social Security Number (SSN) and date of birth, which we use to link to the outcomes data, and basic demographic information such as age, gender, race, and ethnicity. The benefits data capture roughly 2 million unique New Yorkers each year. Because receiving benefits may be endogenous to the eviction court outcome, we restrict the sample to court records that match the benefits data prior to an eviction filing. Roughly 40

[28] The payday loans data come from Clarity, a credit reporting agency that maintains the largest subprime database (over 62 million unique consumers) and is owned by Experian. We study inquiries into single payment and installment microloans that originate primarily from online and storefront subprime lenders. We observe payday loan inquiries for all months between September 2011 and November 2018. We only observe payday loan information for consumers who have a record in our main credit file.

[29] An advantage of the data is that we can observe any hospital visits in New York State regardless of payer. A limitation is that we do not observe primary care visits or prescription fulfillments.

[30] We follow Currie and Tekin (2015) and use the Clinical Classification Software (CCS) to group ICD-9 diagnosis codes into broader categories. We define mental health visits as CCS codes 650–661, 663, and 670. Appendix Table C.5 provides the category labels associated with these codes.

[31] These data do not capture Medicaid clients receiving Medicaid from the state Department of Health.

percent of the court records have a match in the benefits data. Individuals in the linked data have somewhat lower incomes and are more likely to be older, female, and have children when compared to the overall population in housing court (NYC Office of Civil Justice, 2016).

Lastly, we link the Cook County and New York court records to credit outcomes by Experian. This yields a match rate of 61.3 percent in Cook County and 68 percent in New York, which is comparable to match rates in previous studies that use data linked by one of the major credit bureaus.[32] We restrict the sample to individuals who match to a credit record prior to the eviction filing date. The linked credit sample consists of those who are "credit visible," meaning they have a credit record.[33]

Appendix D compares the court record populations in Cook County and New York to the sub-populations successfully linked to outcomes and also examines court record characteristics predictive of a match. In Cook County, tenants without legal representation are 4 percentage points less likely to be linked to Census records, and evicted tenants are 5 percentage points less likely to be linked. Results are similar for linking Cook County data to Experian records, though the magnitudes are smaller. In New York, tenants without legal representation are 4.3 percentage points more likely, and evicted tenants 5.7 percentage points more likely to be linked to benefits data. Evicted tenants in New York are slightly less likely to be linked to Experian records. The appendix provides evidence that judge stringency is uncorrelated with the probability that a case is successfully linked.

4 Trends and evidence of selection

This section provides new descriptive facts about the earnings, employment, housing, health, and financial circumstances of tenants in housing court. The linked panel data reveal substantial selection into housing court as well as selection into eviction conditional on being in housing court. Selection occurs on both levels and trends leading up to an appearance in court for nearly all outcomes considered. These patterns motivate our causal framework described in Section 5.

4.1 Tenants in housing court

Table 1 presents summary statistics for tenants successfully linked to outcomes alongside representative samples of renters from the same counties using the American Community Survey

[32] Dobbie et al. (2017), perhaps the most closely related example, links bankruptcy filings to the same identifiers we use and has a match rate of 68.9 percent. Dobkin et al. (2018), using additional identifiers unavailable to us here (SSNs), are able to match 72 percent of their Medicaid sample to credit reports, and the linked data used to study the Oregon Health Experiment have a match rate of 68.5 percent (Finkelstein et al., 2012).

[33] The Consumer Financial Protection Bureau reports that in low-income neighborhoods, slightly more than 70 percent of adults have a credit record (Brevoort et al., 2015).

(ACS).[34] Relative to the ACS sample, both evicted and non-evicted tenants are more likely to be female (62 percent in Cook County and around 72 percent in New York) or Black (68 percent in Cook County and 58 percent in New York), and they are less likely to be non-Hispanic white (17 percent in Cook County and only 7 percent in New York). By contrast, the demographic characteristics of evicted and non-evicted tenants within housing court are quite similar. For the New York sample, we also include a comparison group of benefits recipients since the data linkage for the New York sample is conditional on having received public benefits at some point in time before the eviction filing.

Tenants in housing court have lower levels of earnings and employment after housing court than renters in the ACS samples. These differences are also present between evicted and non-evicted tenants *within* housing court. In Cook County, average earnings in the eight quarters after case filing are \$5,050 for non-evicted tenants and \$3,709 for evicted tenants, and in New York these numbers are \$3,832 and \$2,982, respectively.[35]

Table 1 describes key case characteristics. The average ad damnum amount—the judgment amount the landlord is seeking from the court—for evicted tenants is around \$2,100 in Cook County and \$4,590 in New York, which are a few hundred dollars more than for non-evicted tenants in the same city. In Cook County, evicted tenants are less likely than non-evicted tenants to have no prior case (63 percent vs. 67 percent) and somewhat more likely to be unrepresented (97 percent vs. 94 percent), while in New York evicted and non-evicted tenants are more similar in these respects (53 to 54 percent have no prior case and 99 to 100 percent are unrepresented at the time of answering the filing).[36]

[34]For Cook County, we reweight the ACS sample to represent a sample of tenants from the same ZIP codes and years as the housing court sample. For New York, we reweight the ACS Public Use Microdata Samples to match the distribution of housing court cases across Public Use Microdata Areas (PUMAs) in New York City. All dollar amounts are expressed in 2016 USD using the CPI-U for the two metropolitan areas we study.

[35]The lower earnings levels in New York are consistent with conditioning the sample on initial benefits receipt.

[36]Our measure of whether the tenant is self-represented for NYC captures this at the time of first appearance in court, and therefore may understate the level of representation somewhat, since some tenants pursue representation after their initial hearing

Table 1: Linked Sample Summary Statistics

	Cook County			New York			
	Evicted (1)	Not Evicted (2)	ACS Sample (3)	Evicted (4)	Not Evicted (5)	Benefits Sample (6)	ACS Sample (7)
Individual-specific characteristics							
Age	37.52	37.45	35.68	38.55	40.49	33.55	35.03
	(10.36)	(10.22)	(11.20)	(9.50)	(9.22)	(10.60)	(10.16)
Female	0.62	0.62	0.54	0.71	0.74	0.52	0.52
	(0.49)	(0.49)	(0.50)	(0.46)	(0.44)	(0.50)	(0.50)
Black	0.69	0.66	0.32	0.58	0.58	0.47	0.31
	(0.46)	(0.47)	(0.47)	(0.49)	(0.49)	(0.50)	(0.42)
Non-Hispanic White	0.17	0.20	0.42	0.07	0.07	0.10	0.17
	(0.38)	(0.40)	(0.49)	(0.26)	(0.26)	(0.30)	(0.47)
Earnings (1-8 quarters after filing)	3,709.00	5,050.00	7,420.00	2,982.23	3,831.71	3,315.87	7,284.44
	(5,069.00)	(8,515.00)	(11,130.00)	(4,295.89)	(4,901.89)	(4,468.40)	(15,602.02)
Prop. of quarters employed (1-8 quarters after filing)	0.58	0.62	0.66	0.44	0.51	0.49	0.71
	(0.41)	(0.41)	(0.42)	(0.43)	(0.45)	(0.44)	(0.44)
Neighborhood poverty rate (5yr avg)	0.20	0.20	0.19	0.29	0.29	0.30	0.24
	(0.14)	(0.15)	(0.13)	(0.12)	(0.12)	(0.12)	(0.24)
Neighborhood med. rent (5yr avg)	711.70	738.40	816.20	973.41	961.68	951.25	1,060.00
	(213.10)	(242.50)	(242.10)	(198.63)	(213.60)	(201.12)	(1,060.00)
Case-specific characteristics							
Ad damnum amount (1000s)	2.01	1.74		4.59	4.16		
	(3.00)	(3.09)		(27.80)	(31.45)		
No prior case	0.63	0.67		0.54	0.53		
	(0.48)	(0.47)		(0.50)	(0.50)		
No Attorney	0.97	0.94		1.00	0.99		
	(0.16)	(0.23)		(0.07)	(0.10)		
Observations	194,000	108,000		86,138	69,202		

Notes: The statistics in columns (1), (2), (4), and (5) are for the samples matched to earnings and employment records. The "Evicted" columns include summary statistics for those who are evicted. The "Not Evicted" columns include summary statistics for those in housing court who are not evicted. The "ACS Sample" columns includes summary statistics for tenants from the same counties. For Cook County, the ACS Sample is reweighted to match the distribution of ZIP codes seen in the housing court sample, and earnings outcomes for this sample are obtained from the LEHD. For New York, we rely on public-use files and cannot reweight by ZIP. Instead, we reweight the ACS Public Use Microdata Samples to match the distribution of housing court cases across Public Use Microdata Areas (PUMAs) in NYC. In addition, earnings and employment measures for the New York ACS sample are constructed directly from the ACS PUMS 2006–2010. Average quarterly earnings is constructed as annual wage income divided by four. The proportion of quarters employed is approximated by the proportion of people with any wage income. Cook County observation counts are rounded in accordance with Census Bureau disclosure requirements. Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

4.2 Trends around a court filing

Figure 2 documents trends in each of four outcomes—earnings, employment, residential moves, and homelessness—relative to the time of a housing court filing, separately for evicted and non-evicted tenants with cases filed against them. The figures are based on the regression

$$Y_{i,t} = \gamma_\tau + \alpha \times E_i + \beta_t + \delta_t \times E_i + \epsilon_{i,t} \ , \tag{4.1}$$

where t indexes time relative to the eviction filing, E_i is an indicator for the case outcome being an eviction order, β_t are coefficients on indicators for time relative to the case filing, and δ_t are coefficients on indicators for relative time interacted with the eviction outcome.[37] The labor market outcomes are measured at a quarterly frequency, while the housing outcomes are

[37] The time span covered by our data varies across outcomes. For labor market outcomes and residential moves, we consider trends up to four years before and up to six years after a filing. For homelessness, the available data is more limited, covering up to two years before filing and up to three years after.

measured at an annual frequency. The only controls are calendar year dummies (γ_τ). Figure 2 displays regression estimates of β_t and $\alpha + \delta_t + \beta_t$. For both sets of coefficients, we add the non-evicted group mean in the omitted period so that the magnitudes are easy to interpret.

Figure 2, Panel A depicts the typical earnings levels for our sample over time. Both evicted and non-evicted groups show signs of declining earnings in the year prior to case filing. The decline is steeper for tenants who are evicted. The magnitude of the decline in the three quarters prior to filing is about $400 for evicted tenants and $250 for non-evicted tenants in Cook County and about $250 for evicted tenants and $100 for non-evicted tenants in New York. The average drop in quarterly earnings for the evicted across both locations in the year lead-up to filing is $337, and for the not-evicted it is $186.

Panel B of Figure 2 shows a decline in the probability of being employed for both evicted and non-evicted tenants prior to filing. Following eviction, employment does not recover to its pre-filing peak for the next six years in either city. In Cook County, there is a slight tapering for the entire sample period following three quarters after filing, which is not due to aging into retirement since our sample is restricted to individuals who are between 18 and 55. In New York, there is a similar pattern of flat employment rates at a diminished level relative to the pre-filing peak. Earnings and employment rates are lower on average in New York relative to Cook County, which is not surprising given that the New York sample is limited to individuals receiving food stamps, Medicaid, or cash assistance at some point before their case.

Figure 2, Panel C shows the probability that we observe a tenant at an address different from the filing address. The mean in the year of filing (year 0) is around 45 percent in Cook County and 20 percent in New York, partly reflecting moves in the year of filing and also reflecting noise in the mobility data.[38] These values are the same for evicted and non-evicted tenants in the same city, suggesting these groups may not be differentially affected by mismeasurement in the mobility data. In Cook County, the probability of observing the tenant at a new addresses increases from 45 percent to about 55 percent in the first year after filing and eventually rises to over 90 percent six years after filing. The probabilities rise slightly faster for evicted than for non-evicted tenants and eventually yield a gap of about 10 percentage points. In New York, the gap between evicted and non-evicted tenants increases more dramatically: for evicted tenants the probability of being at a different address increases from 25 percent to almost 40 percent in the first year after filing and to 70 percent in five years, while for non-evicted tenants it barely increases in the year after filing and only reaches 40 percent after five years.[39]

[38]Measuring residential moves at an annual frequency in the United States is challenging, and particularly so for our population of unstably-housed tenants. We have reproduced these results using alternative sources of residential address data including Infutor, InfoUSA, and credit bureau data (which measures addresses at the zip code level), and all of these sources lead to similar findings.

[39]High mobility among non-evicted tenants is consistent with Brummet and Reed (2020), who find with linked Census Bureau microdata (from the Census 2000 and American Community Survey 2010–2014) that 70 percent of high school-educated renters living in low-income, central city neighborhoods in 2000 are in a different neighborhood 10 to 14 years later.

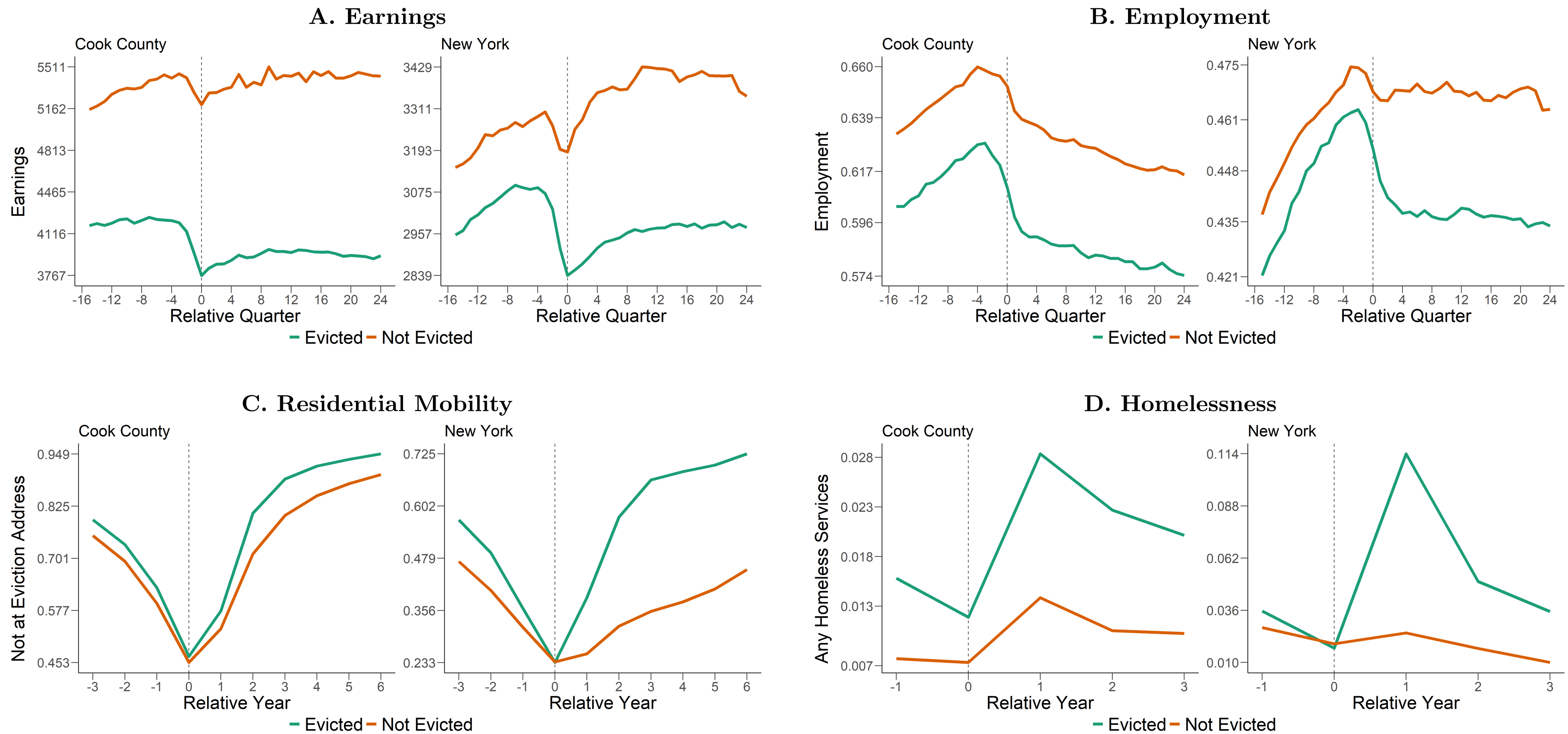
Figure 2, Panel D shows that use of homeless services spikes in the year after housing court, particularly for the evicted group. The magnitudes of these changes are striking: the probability of using homeless services doubles for the evicted group in Cook County and increases by 5 times in New York.

Figure 3 displays trends in credit report outcomes around the filing of an eviction case and reveals patterns of distress similar to those in Figure 2, with tenants' credit scores falling, collections balances rising, and payday loan inquiries rising in the two years before case filing.[40] Tenants have elevated indebtedness and diminished credit access for several years after the case. Panel A shows trends in credit scores in Cook County and New York. Both evicted and non-evicted tenants have, on average, low credit scores in the years prior to housing court and would be considered subprime borrowers. Evicted tenants' credit scores are approximately 10 points lower. Both groups experience declines in the two years before the case: roughly 12 points in Cook County and about 6 points in New York. In both settings, credit scores reach their nadir 1–2 quarters after filing and recover slowly, taking between three and five years to return to their pre-filing peak. The two groups' credit scores remain broadly parallel throughout the sample period.

Figure 3, Panel B reveals limited access to credit before housing court: in Cook County, more than 50 percent of non-evicted tenants and nearly 60 percent of evicted tenants have no source of revolving credit at the time of filing, and in New York the respective estimates are 34 percent and 38 percent. Across both locations, this rate increases for both groups following the case filing, and the gap between evicted and non-evicted tenants widens.

[40]Trends are shown for fewer quarters in New York because we only have Experian data there for five years.

Figure 2: Labor Market and Housing Outcomes Relative to Time of Eviction Filing



Notes: This figure plots estimates of $\{\beta_t\}$ and $\{\alpha + \delta_t + \beta_t\}$ from the regression $Y_{i,t} = \gamma_\tau + \alpha \times E_i + \beta_t + \delta_t \times E_i + \epsilon_{i,t}$. The employment and earnings outcomes are measured at a quarterly frequency, while the housing outcomes are measured at an annual frequency. The only controls are calendar year dummies (γ_τ). For both sets of coefficients we add in the non-evicted group mean in the omitted period so that the magnitudes are easy to interpret. Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

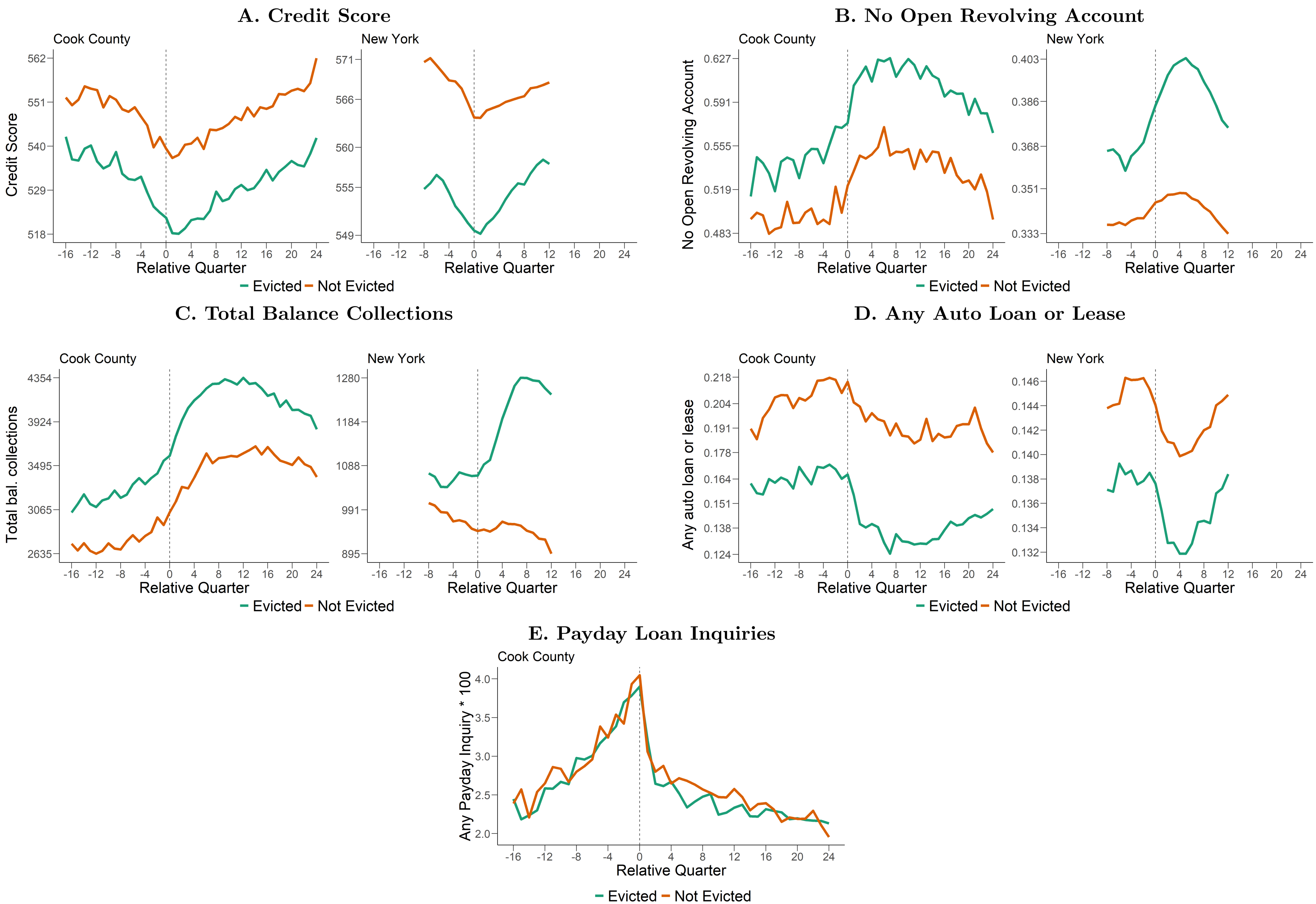
Figure 3, Panel C shows total balances in collections. In both locations, the levels of indebtedness two years prior to filing are high: evicted and non-evicted tenants in Cook County have approximately \$3,000 and \$2,600 in collections, respectively, while in New York the corresponding amounts are \$1,050 and \$950. Indebtedness is higher and rising for the evicted group and continues rising steeply after filing. In Cook County, the evicted group's balance increases by approximately \$1,000 in the two years after filing, while the non-evicted group's balance increases by \$750. In New York, the evicted group's balance increases by about \$200 in the two years after filing, while the non-evicted group's balance is flat or declining over the next several years.

Panel D in Figure 3 depicts the probability of having an open auto loan or lease. In both locations, auto loans exhibit flat or slightly increasing trends before filing. After filing, both evicted and non-evicted tenants are less likely to have an auto loan and the gap between evicted and non-evicted tenants widens, particularly in Cook County.

Figure 3, Panel E shows trends in the probability of making an inquiry into a payday loan. Tenants in housing court have high levels of payday loan inquiries, even three years prior to filing, when about 2.5 percent of tenants have an inquiry each quarter. For comparison, from a random sample of Cook County residents age 18–55, 2.3 percent have a payday inquiry each quarter. In the three years leading up to the eviction filing, there is a striking increase in demand for payday loans by both evicted and non-evicted tenants, with inquiries increasing from 2.5 percent per quarter to 4.0 percent per quarter. After the filing quarter, there is a sharp and immediate drop-off in payday loan inquiries, which may reflect less demand for liquidity—such as tenants finding less costly housing arrangements—or reduced supply of loans from creditors. Overall, these results suggest that the run-up to eviction filing coincides with a moment of acute financial strain on households, with tenants seeking short-run liquidity even at high interest rates.

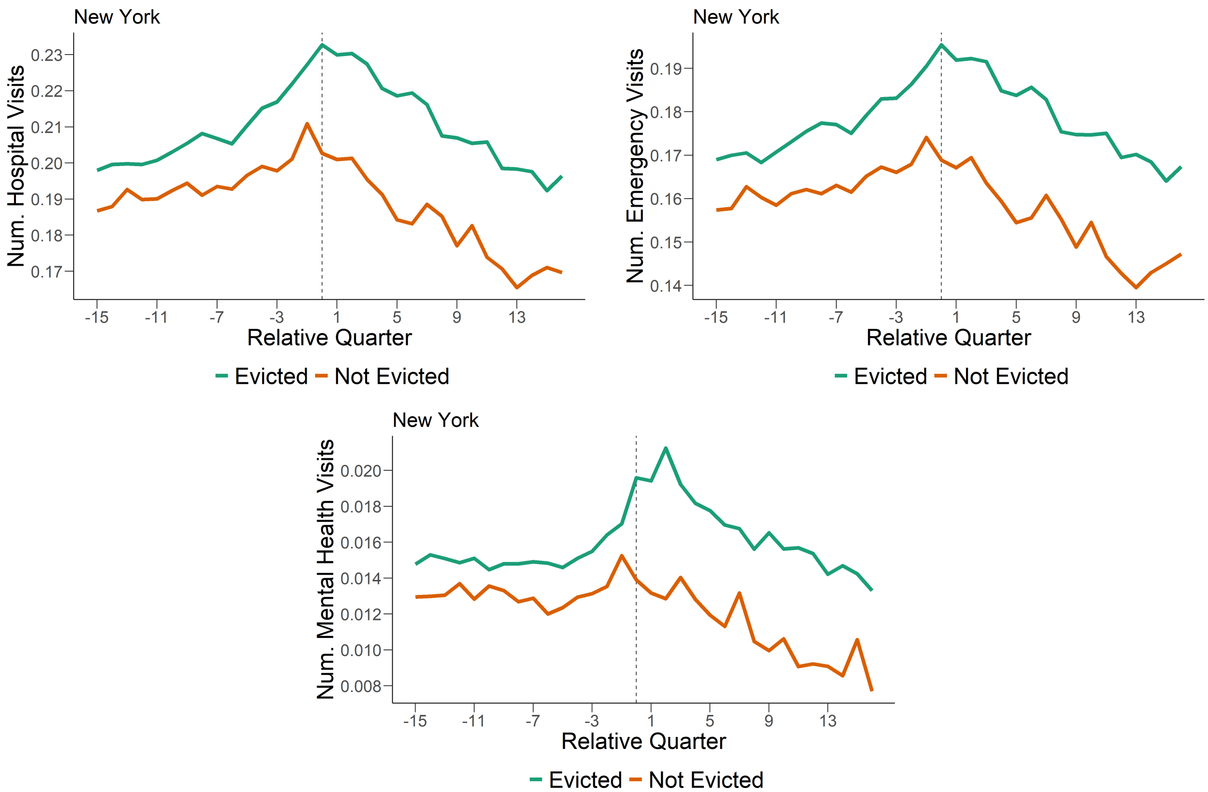
Figure 4 shows trends around the time of the housing court filing in total hospital visits, total emergency room visits, and the total hospital visits related to mental health in New York. Total hospital visits increase in the two years leading up to eviction filing. They peak during the quarter in which the housing court filing occurs, which coincides with the point where earnings are at their lowest. The increase preceding housing court hints at the possibility that health shocks could be a source of earnings losses that lead to non-payment of rent, although it is not clear in which direction causality runs. The vast majority of these hospital visits are trips to the emergency room, which appear in the middle panel of Figure 4. The right panel of Figure 4 shows that the total number of mental health-related hospital visits also increases during the period leading up to housing court. Note that the gap between evicted and non-evicted tenants in hospital visits widens following eviction in all three panels.

Figure 3: Credit Report Outcomes Relative to Time of Eviction Filing



Notes: This figure plots estimates of $\{\beta_t\}$ and $\{\alpha + \delta_t + \beta_t\}$ from the regression $Y_{i,t} = \gamma_\tau + \alpha \times E_i + \beta_t + \delta_t \times E_i + \epsilon_{i,t}$. The outcomes are measured at a quarterly frequency. The only controls are calendar year dummies (γ_τ). For both sets of coefficients we add in the non-evicted group mean in the omitted period so that the magnitudes are easy to interpret. Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

Figure 4: Health Outcomes Relative to Time of Eviction Filing



Notes: This figure plots estimates of $\{\beta_t\}$ and $\{\alpha + \delta_t + \beta_t\}$ from the regression $Y_{i,t} = \gamma_\tau + \alpha \times E_i + \beta_t + \delta_t \times E_i + \epsilon_{i,t}$. The outcomes are measured at a quarterly frequency. We only observe health outcomes in the New York sample. The only controls are calendar year dummies (γ_τ). For both sets of coefficients we add in the non-evicted group mean in the omitted period so that the magnitudes are easy to interpret.

4.3 Considerations for empirical design

The analysis up to this point has revealed two key patterns. First, there are clear cross-sectional differences between evicted and non-evicted tenants, not just after eviction but also years before the case is filed: evicted tenants have lower levels of earnings, labor market participation, and residential stability and higher levels of healthcare utilization and financial distress. Second, for many of the outcomes, differences in pre-trends between evicted and non-evicted tenants start to develop as tenants approach the court filing: evicted tenants see larger drops in earnings, employment, and credit scores and larger increases in healthcare utilization in the periods prior to the court case. These patterns are consistent with changes to pre-filing earnings, health, or financial circumstances being correlated with both the case filing and with receiving an eviction order.

These findings guide our methodological choices in analyzing the consequences of eviction.

Differences in levels and pre-trends caution against making cross-sectional comparisons of post-court outcomes between evicted tenants and observationally-similar tenants who are not evicted. Such comparisons will likely overstate the effect of eviction because they will incorrectly attribute both the pre-existing level difference as well as any lingering differences due to pre-court shocks to the eviction itself.[41] While difference-in-differences estimators would address bias due to permanent differences in levels, they would not in general address bias due to differential trends.[42] For these reasons, we use an instrumental variables design, which addresses both potential sources of bias and allows us to identify a local average treatment effect. In addition, we explore whether our findings are specific to complier cases by developing a supplementary analysis to draw inference about the average treatment effect for evicted tenants. This analysis uses assumptions about the underlying economic processes to sign the bias in difference-in-differences estimators (without relying on the parallel trends assumption), allowing us to bound the average effect for evicted tenants.

5 Empirical framework

This section lays out our instrumental variables approach based on judges' tendency to evict in cases randomly assigned to them. We discuss how the assumptions that underlie this identification strategy are supported by the institutional environment and test these assumptions. Next, we develop a supplementary identification strategy to explore whether our IV-based findings are specific to the set of complier cases. Guided by our observations in the previous section, we outline assumptions that allow us to provide bounds on the average causal impact of an eviction for all those who are evicted (i.e., the average treatment effect on those who are treated), as opposed to only the compliers.

5.1 Instrumental variables

The evidence in Section 4 suggests that whether a tenant is evicted may depend on unobserved characteristics as well as unobserved shocks that affect both eviction and subsequent outcomes. If a suitable instrument is available, it can be used to solve this endogeneity problem and

[41]Previous studies of the consequences of eviction have typically used observationally-similar tenants without a case filing as a comparison group (e.g., Desmond and Kimbro, 2015; Desmond and Gershenson, 2016b). In Appendix E, we use our data to explore the potential magnitude of selection bias in estimates based on such comparisons. We find, for example, that using a comparison group of observationally-similar tenants who are not in court yields estimates roughly double the size of estimates based on a comparison group of non-evicted tenants in court.

[42]Previous work has shown that in the presence of differential trends, certain difference-in-differences specifications still produce unbiased estimates under specific assumptions on the nature of shocks to the outcome process (Heckman and Robb, 1985; Ashenfelter and Card, 1985; Chabé-Ferret, 2015). In general, it is not known whether these assumptions hold, and the researcher must judge their credibility based on intuition or knowledge about the underlying economic processes.

estimate causal effects of eviction. A common approach in court settings is to exploit the random assignment of judges to eviction cases and use $Z_{j(i)}$ as an instrumental variable, where $Z_{j(i)}$ is the leave-one-out estimate of stringency for judge j assigned to individual i's case. This approach estimates the following two-stage least squares model:

$$E_i = \gamma Z_{j(i)} + X_i'\alpha + \epsilon_i \tag{5.1}$$

$$Y_{i,t} = \beta E_i + X_i'\delta + \nu_i. \tag{5.2}$$

Here, E_i is an indicator for whether case-individual combination i has an eviction, $Y_{i,t}$ is the observed outcome t years after treatment, and X_i is a set of controls including court-year fixed effects and individual characteristics such as age, gender, and race or ethnicity, as well as case characteristics.[43] If the IV assumptions are satisfied, this analysis will recover a positive weighted average effect of eviction among compliers, where compliers are defined as tenants who would have received a different eviction outcome had their case been assigned to a different judge (Imbens and Angrist, 1994).

5.1.1 The judge stringency instrument

We measure judge stringency using the yearly leave-one-out mean eviction rate for the initial judge (Cook County) or courtroom (New York) assignment. Using the sample described in Section 3, we calculate the stringency of the judge to which tenant i's case is assigned, $Z_{j(i)}$, as the leave-one-out mean eviction rate (omitting i) for judge $j(i)$ in the same year.

Figure 5 shows the distribution of judge stringency (residualized by court-year-quarter) across cases in Cook County and New York. The variation in judge stringency is substantial and similar across settings: a 7 percentage point difference between the 10th percentile and 90th percentile of judge stringency in Cook County and a 6 percentage point difference in New York. Appendix F.6.2 shows that the first stage is robust to alternative ways of measuring stringency, including using an alternative procedure for assigning judges to cases, controlling for additional judge characteristics, or using alternative criteria for which cases are included in the IV sample.

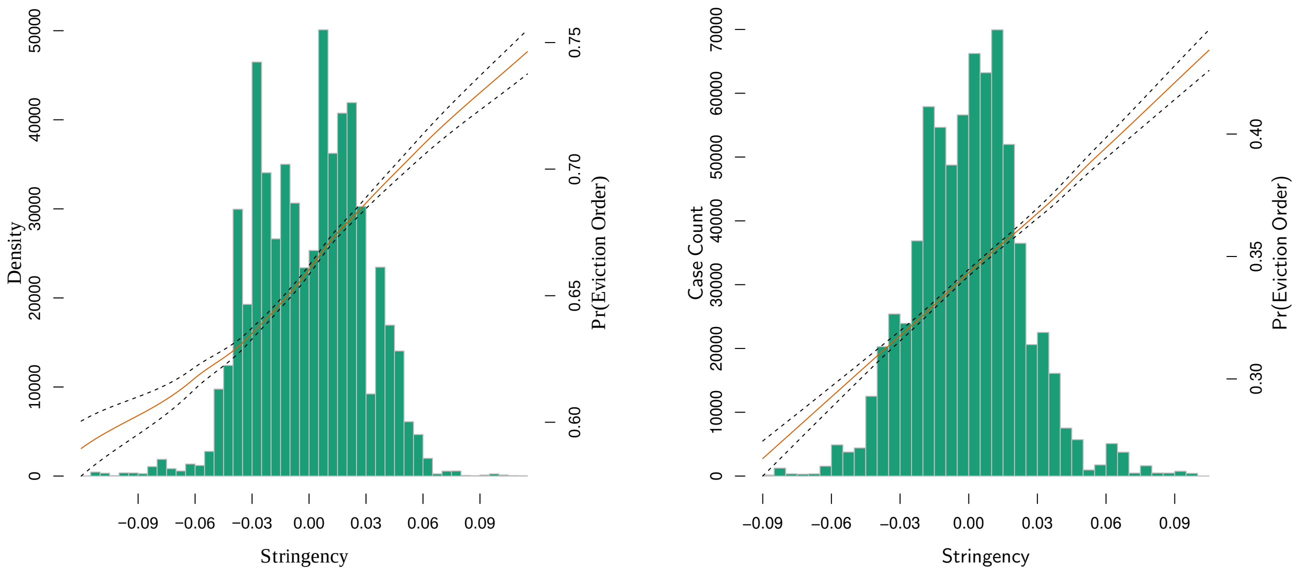
5.1.2 Validating the IV design

This section discusses sufficient conditions for judge stringency to be a valid instrument and for the IV estimand to be interpretable as a positive weighted average of local treatment effects on compliers: relevance, exogeneity, exclusion, and monotonicity. We discuss each of these assumptions below and support them with arguments based on institutional details and empirical evidence.

[43] See the notes to Table 4 for a detailed description of the controls.

Figure 5: Judge Stringency

(a) Cook County **(b) New York**



Notes: For each location, this figure shows a histogram of the mean-standardized distribution of judge stringency, $Z_{j(i)}$, with the number of cases indicated along the left vertical axis. Each panel also depicts fitted values of the first-stage regression of eviction on judge stringency and court-year-quarter fixed effects (solid line, plotted along the right vertical axis). Dotted lines indicate 95 percent confidence intervals. Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

Relevance. Column 1 in Table 2 reports first-stage estimates from Equation (5.1) for Cook County and New York. Judge stringency has a large and statistically significant impact on evictions, with an F-statistic for the full first stage of 149.4 in Cook County and 152.3 in New York, providing strong evidence against the null of weak instruments. Column 2 shows that the first stage remains largely the same when controlling for other observable characteristics about the judge: judge stringency in their judgment amounts in Cook County and judge stringency in granting stays in New York. Appendix Table F.18 provides additional evidence that the first stage remains relevant and largely similar when changing which variables are controlled for, using an alternative method for constructing judge stringency, and using different sample selection criteria.

Exogeneity. Table 3 shows the result of a standard balance test of random assignment. Columns (1) and (3) show that case and tenant characteristics predict receiving an eviction order in both cities. However, columns (2) and (4) show that they do not predict the stringency of the judge randomly assigned to the case. We also conduct an omnibus test of the differences between cases assigned to more and less stringent judges and fail to reject the null hypothesis that there are no differences between them.

Table 2: First Stage

	Cook County		New York	
	(1)	(2)	(1)	(2)
Judge stringency	0.741***	0.753***	0.836***	0.845***
	(0.025)	(0.034)	(0.049)	(0.050)
Alternative stringency		0.004		0.008
		(0.003)		(0.024)
N obs	269,000	267,000	174,437	174,437

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$.This table reports results for a first stage regression of eviction on judge stringency for Cook County and New York. The first column (1) shows the regression including only judge stringency. The second column (2) controls for another potential measure of judge stringency, and the estimate for this alternative stringency measure is also displayed under this specification. The regression for Cook County adds controls for judgment amount stringency, while the regression for New York controls for judge stringency in granting stays. Judgment amount stringency is constructed using joint action cases that end in an eviction order. For each case, we construct the leave-one-out mean for the difference between the judgment amount and the ad damnum amount for each judge in each year (for judges who see at least 100 cases). Stay stringency is the leave-one-out mean of the indicator for if the judge allowed a stay of the eviction order, which extends how long the city must wait before they can perform the lockout associated with the eviction. Table F.18 in the Appendix provides additional evidence on the robustness of the first-stage regression. Regressions include court-year fixed effects. Standard errors are included in parentheses and are clustered at the judge-year level. Cook County observation counts are rounded in accordance with Census Bureau disclosure requirements. Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

Exclusion. Our estimation strategy relies on the assumption that judge stringency affects tenant outcomes only through the eviction order. As discussed in Section 2, judges determine whether to issue an eviction ruling but may also influence other aspects of the case such as the judgment amount (for cases involving money judgments), the amount of time tenants have to find legal aid, or allowing the case to be rescheduled. This multi-dimensional sentencing can make it challenging to isolate the impact of the eviction order on outcomes (Mueller-Smith, 2015; Bhuller et al., 2020). In particular, exclusion will be violated if judge stringency is correlated with other dimensions of judge discretion that affect tenant outcomes but are not directly controlled for. For example, we might be concerned that judges affect tenant outcomes by granting a stay of the eviction order, which grants extra time for the tenant to move before the order can be executed, or by adjusting the judgment amount, which is the amount the judge orders the tenant to pay the landlord.

To explore the importance of these other channels, we construct additional judge stringency measures with respect to the judgment amount and the propensity to grant a stay of the eviction order.[44] The second column of Table 2 shows that controlling for judges' stringency of the judgment amount has little impact on the first stage in Cook County, and controlling for their stringency of granting a stay has little impact on the first stage in New York.

Appendix F.6 provides additional evidence that other aspects of judges' behavior are unlikely to affect our analysis. Using evidence from Cook County and New York, we show that the

[44] In New York, we do not observe the judgment amount, so it is not possible to construct a measure of amount stringency.

various judge stringency measures have low correlations, that judge eviction stringency does not predict the money judgment amount for those who are evicted, and that including other stringency measures has little impact on the first stage. We also show that the IV results are largely unchanged when controlling for these other stringency measures or when including judgment amount as a second endogenous variable and instrumenting for it with judgment amount stringency. Overall, these results suggest that these other channels are not a threat to the exclusion restriction.

Monotonicity. For the IV estimates to be interpreted as a positively weighted average of local average treatment effects (LATEs), it is sufficient for monotonicity to also hold. In our setting, monotonicity requires that evicted tenants would also have been evicted by a more stringent judge, while non-evicted tenants would not have been evicted by a less stringent judge. This condition can fail in randomized examiner designs if judges are relatively harsh for some types of cases or individuals and relatively lenient for others. We perform two tests of this assumption. First, under monotonicity, the first-stage estimates should be non-negative for any subsample of tenants. Appendix Tables F.20 and F.21 show non-negative first-stage estimates for various subsamples in Cook County and New York. A second test is to calculate judge stringency on one sub-population (for example women) and then use that stringency measure in the first stage for the complementing sub-population (for example men), as in Bhuller et al. (2020) and Norris et al. (2019). Appendix Table F.22 shows that we find no evidence of switching signs and find similar-sized first-stage parameters across specifications. Hence, neither of these tests produce evidence against the monotonicity assumption.

5.2 What can we learn from difference-in-differences estimates?

The IV estimates are relevant for policy changes that affect cases that would have received a different outcome had they been assigned a different judge. However, other types of policy changes might affect non-complier cases. To understand whether our findings might extend to such cases, we would like to compare the IV estimates to estimates of the average causal impact of an eviction for all those who are evicted (i.e., the average treatment effect on those who are treated), as opposed to only the compliers to the instrument. We now turn our attention to developing a supplementary identification strategy that explores whether our IV-based findings are specific to the set of complier cases.

In panel settings, a common empirical strategy for estimating the average treatment effect on those who are treated is to rely on a panel specification of the following form:

$$Y_{i,t} = \lambda_i + \mu_\tau + \alpha_t + \sum_{k \neq -\ell} \gamma_k^{-\ell} E_i \times \mathbf{1}\{k = t\} + \nu_{i,t}, \tag{5.3}$$

where λ_i is a unit fixed effect, μ_τ are calendar-time (year-quarter) dummies, α_t are dummies for time (quarter) relative to the treatment period, and $-\ell$ is the reference period, which we refer to as the pre-period. The parameter of interest in these analyses is the average treatment effect on the treated k periods after treatment:

$$\Delta_k^{ATT} := \mathbb{E}\left[Y_{i,k}(1) - Y_{i,k}(0) | E_i = 1\right].$$

We refer to the estimated $\hat{\gamma}_k^{-\ell}$ from the regression in Equation 5.3 as difference-in-differences (DiD) estimates with post-period k and pre-period $-\ell$.

Consider the probability limit of $\hat{\gamma}_k^{-\ell}$:

$$DiD_{k,\ell} \equiv \Delta_k^{ATT} + \mathbb{E}[Y_{i,k}(0) - Y_{i,-\ell}(0)|E_i = 1] - \mathbb{E}[Y_{i,k}(0) - Y_{i,-\ell}(0)|E_i = 0], \tag{5.4}$$

where the last two terms capture the difference in trends in $Y_{i,t}(0)$ between tenants who are evicted and those who are not. Under the usual parallel trends assumption, the last two terms cancel out, and $\hat{\gamma}_k^{-\ell}$ is an unbiased estimator of Δ_k^{ATT}.

In our setting, the parallel trends assumption is unlikely to hold. As shown in Section 4.3, evicted and non-evicted tenants differ in their pre-court trends in outcomes. These differences are most apparent in Figure 2.A, where the earnings for tenants who are eventually evicted drop more sharply in the period before treatment than for those who avoid eviction. Given this pattern, we might expect the counterfactual trajectories for evicted tenants not to be parallel to trajectories for non-evicted tenants, which would violate the parallel trends assumption. However, as demonstrated by Manski and Pepper (2018), it is possible to learn about Δ_k^{ATT} under assumptions that are weaker than parallel trends. For example, assumptions on the degree of mean reversion in counterfactual trajectories for the non-evicted group allow us to sign the bias in DiD estimates. Moreover, in some cases it is possible to provide both upward- and downward-biased estimates, producing bounds on Δ_k^{ATT}. In what follows, we use the earnings outcome as our leading example.

We can rewrite the bias resulting from non-parallel trends in terms of the differences in potential outcome trajectories for the evicted and non-evicted, in the pre-treatment and post-treatment periods:

$$\begin{aligned} DiD_{k,\ell} - \Delta_k^{ATT} &= \Big(\underbrace{\mathbb{E}[Y_{i,k}(0) - Y_{i,0}(0)|E_i = 1]}_{\tau_{0,k,E=1}} - \underbrace{\mathbb{E}[Y_{i,k}(0) - Y_{i,0}(0)|E_i = 0]}_{\tau_{0,k,E=0}}\Big) \\ &\quad + \Big(\underbrace{\mathbb{E}[Y_{i,0}(0) - Y_{i,-\ell}(0)|E_i = 1]}_{\tau_{-\ell,0,E=1}} - \underbrace{\mathbb{E}[Y_{i,0}(0) - Y_{i,-\ell}(0)|E_i = 0]}_{\tau_{-\ell,0,E=0}}\Big) \\ &= (\ \tau_{0,k,E=1} \ - \ \tau_{0,k,E=0}\ ) \ + \ (\ \tau_{-\ell,0,E=1} \ - \ \tau_{-\ell,0,E=0}\ ). \end{aligned} \tag{5.5}$$

We observe the second term in parentheses, and it captures the difference in potential outcome trends in the pre-period. As discussed in Section 4, this term is negative for earnings. Therefore, the sign of the overall bias of the DiD estimator will depend on the sign and relative magnitude of the first term in parentheses: the difference between the counterfactual outcome trend in the post-period for the evicted group and the observed trend for the non-evicted group.

For example, suppose the evicted group would have experienced a sharper post-filing earnings recovery had they not been evicted than the recovery observed for the non-evicted group. Then, the first term in parentheses will be positive. If this differential recovery in the post-period is smaller than the differential deterioration in the pre-period, i.e. $\tau_{0,k,E=1} - \tau_{0,k,E=0} < \tau_{-\ell,0,E=1} - \tau_{-\ell,0,E=0}$, then the overall bias will be negative.

There are a number of realistic types of earnings processes for which the bias of the DiD estimator can be signed, which we detail in Appendix G. For example, in a model where (i) a negative earnings shock at the time of filing (at $t = 0$) increases the odds that a tenant is evicted and (ii) earnings shocks follow a transitory and covariance-stationary process without oscillation, the DiD estimator will be unbiased when the pre- and post-period are chosen symmetrically around the treatment period (Heckman and Robb, 1985). More generally, under the same set of assumptions but with the pre-period window further from $t = 0$ than the post-period window ($\ell > k$), the bias will be negative.[45] One potential concern with these assumptions is that earnings shocks may have a permanent component. If we assume that earnings follow a random walk, then the DiD estimator will still be biased downward if $\ell \neq 0$.[46]

As discussed in Appendix G, the sign of the bias depends on how outcomes or shocks to outcomes affect evictions and on how outcomes evolve over time. We show that under plausible economic assumptions, the DiD estimates we report are biased away from zero, providing a lower bound on the treatment effect on the treated for earnings and employment.[47] Under somewhat stronger assumptions that rule out persistent innovations in the outcome process, we show that the DiD can provide both an upper and lower bound on the treatment effect on the treated, depending on the choice of the pre- and post-periods. Under these assumptions, we may learn about the effects on everyone who was evicted, not just the compliers.

[45] These assumptions are similar to those used in studies that non-experimentally estimate the returns to job training programs discussed in Ashenfelter and Card (1985), Heckman and Robb (1985), and, more recently, Chabé-Ferret (2015).

[46] In other work, researchers adjust for non-parallel trends by assuming the differential trends in the pre-period can be extrapolated to the post-period. Under this assumption, the DiD estimates would again be biased away from zero. Given the divergence in trends observed prior to eviction, we think this assumption is unlikely to hold in our setting and instead we directly consider restrictions on selection into court and the earnings process.

[47] Chabé-Ferret (2015) further evaluates the bias from DiD and matching estimators for evaluating job training programs. The paper considers several combinations of assumptions on the earnings and selection process and argues that symmetric DiD estimates outperform matching.

5.3 Combining estimates across cities

Due to restrictions on data sharing, we are unable to pool individual observations from Cook County and New York City. We therefore separately estimate each specifications by city and then report simple average point estimates in the tables in Section 6. The combined point estimates weight results from the two locations equally, so we calculate the standard errors for the combined estimates as

$$\widehat{SE}_{\text{combined}} = \sqrt{\omega^2 \times \widehat{SE}^2_{NYC} + (1-\omega)^2 \times \widehat{SE}^2_{CC}},$$

where $\omega = 0.5$. Under the assumptions outlined in Section 5.1, the combined estimates can be interpreted as the simple average of the effect of evictions among compliers in Cook County and New York City.

Table 3: Testing Balance

	Cook County		New York	
	Evicted (1)	Judge Stringency (2)	Evicted (3)	Judge Stringency (4)
Age at case	-0.0006*	0.0000	-0.0119***	-0.0002
	(0.0003)	(0.0000)	(0.0027)	(0.0001)
Female	0.0105***	-0.0000	-0.0461***	-0.0001
	(0.0028)	(0.0002)	(0.0026)	(0.0001)
Black	0.0307***	-0.0004	0.0178***	0.0001
	(0.0042)	(0.0002)	(0.0033)	(0.0001)
White	-0.0237***	-0.0003	-0.0033	-0.0002
	(0.0038)	(0.0002)	(0.0048)	(0.0002)
Neighborhood poverty rate (5yr avg)	0.4327***	0.0007	-0.0021	-0.0005
	(0.0292)	(0.0018)	(0.0151)	(0.0006)
Ad damnum amount (in 1000s)	0.0181***	0.0000	0.0001***	-0.0000
	(0.0006)	(0.0000)	(0.0000)	(0.0000)
Joint action	-0.0100*	-0.0006**		
	(0.0053)	(0.0003)		
No prior case	-0.0382***	-0.0001	-0.0151***	-0.0001
	(0.0022)	(0.0001)	(0.0040)	(0.0001)
No attorney (plaintiff)	-0.0204***	-0.0001	0.2174***	0.0001
	(0.0048)	(0.0002)	(0.0125)	(0.0009)
Joint F-Test stat.	149.4000	1.1640	224.8136	1.0073
p-value	0.0000	0.3020	0.0000	0.4427
Observations	302,000	269,000	174,000	174,000

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$.For each city, the left column presents results for a regression of eviction on case and defendant characteristics. The right column shows results for a regression of our measure of judge stringency on case and defendant characteristics. "Neighborhood poverty rate" is a rolling 5-year average poverty rate for the defendant's eviction address census tract. "Ad damnum amount" is the amount the landlord listed as owed by the defendant at the time of filing. "Joint action" is an indicator for if the case was a joint action case seeking an eviction order and a money judgment rather than a single action case seeking only an eviction order, and is specific to CC. "No prior case" is an indicator for the defendant having no prior eviction case. "No attorney" is an indicator for the plaintiff (landlord) having no attorney. Both regressions include court-year fixed effects. Standard errors are depicted in parentheses and are clustered at the judge-year level. Cook County observation counts are rounded in accordance with Census Bureau disclosure requirements. Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

6 Results

This section presents estimates of the causal effects of eviction on tenants' housing situation, labor market outcomes, financial strain, and hospital use.

6.1 Residential mobility and homelessness

A key question to answer in order to understand the consequences of eviction is whether and to what extent eviction disrupts tenants' housing situations. As documented in Section 4, tenants in housing court move frequently both before and after a court filing, and it is not a priori clear how much additional mobility is caused by an eviction. Moreover, while it is often suggested that eviction is a key driver of homelessness, there is currently no causal evidence on this relationship. In this section, we consider the impact of eviction on residential moves, emergency shelter use, and any interactions with the homelessness system in the first year after a case is filed. We also study the likelihood that tenants whose cases do not end in eviction are evicted over the next year to characterize the extent to which future eviction plays a role in tenants' counterfactual outcomes. We consider longer-run impacts on tenants' housing situations and labor market outcomes in Section 6.2.

Table 4 reports IV estimates of the effects of eviction on housing outcomes alongside estimates from OLS, lagged dependent variable (LDV), and reduced form (RF) specifications. The IV estimates identify a treatment effect for compliers—the group of tenants whose case outcome depends on judge assignment. For these tenants, receiving an eviction order increases the probability of appearing at a new address in the year after filing by 10 percentage points, relative to a mobility rate of 31 percent in the non-evicted group. We also report the reduced-form estimates, the effect of judge stringency on the outcomes, which do not require the exclusion restriction or monotonicity to hold in order to be interpreted as causal. The reduced-form estimates are very similar to the IV estimates.

As discussed in Section 2, a lockout occurs when the Sheriff's Office or the City Marshal physically removes a tenant and/or their belongings from a property and changes the locks. Not all eviction orders are followed by a lockout, either because a tenant moves before a scheduled lockout or because the landlord does not file a request for the Sheriff or City Marshal to carry out a lockout. Table 4 shows that receiving an eviction order substantially increases the probability of being subjected to a lockout, with an IV estimate of 39 percentage points.[48]

The timing of a lockout may be difficult for tenants to predict, and tenants may move out more quickly after an eviction order in anticipation of a lockout. Thus, moves following an eviction order may occur under more time pressure or at an unexpected time, leaving less time to secure new accommodation. This increased pressure is potentially reflected in our estimates of

[48] Because of misclassification in our processing of the court records, we observe a very small number of lockouts in cases that don't end in eviction.

the impact of an eviction order on homelessness outcomes. Table 4 shows that an eviction order increases homelessness. The IV estimate indicates that receiving an eviction order increases the probability of staying in emergency shelter by roughly 3 percentage points, which is a large relative increase since only 1 percent of tenants who avoid eviction end up in shelter. The reduced-form estimate of 0.026 implies that being assigned a judge with a 10 percentage point higher stringency (within the full range of stringency in both settings) increases the chances of ending up in a homeless shelter by 0.26 percentage points, an increase of 28 percent relative to the non-evicted mean. The effects on use of any homeless services are similar in magnitude, although the IV estimate is not statistically significant. These results suggest that evicted tenants experience difficulty finding alternative housing, which could in turn affect other outcomes such as earnings or employment.

The last row of Table 4 reports estimated effects on the likelihood of receiving a future eviction order at the same address during the year following the eviction case filing. During this year, 7.4 percent of those who avoid an eviction receive an eviction order in a future case. The IV estimate indicates that eviction decreases this probability by 5 percentage points. The IV estimates are larger, suggesting that complier cases would be somewhat more likely to experience an eviction order the following year had an eviction order not been issued. When interpreting the effects on longer-run outcomes, it is important to note that tenants who avoid eviction in a given case may still be in financial distress and thus at risk of future eviction; nevertheless, over 90 percent of these tenants are not evicted within the next year.

Taken together, these findings show that an eviction order triggers a disruption in tenants' housing situation that may take the form of a residential move or stays in emergency shelter. While move rates are high even among those who avoid eviction, moves among the evicted may occur less predictably or under greater time pressure, explaining their increased rate of stays in emergency shelter.

6.2 Earnings, employment, and longer-run housing instability

We now shift attention to estimates of the causal effects of an eviction order on earnings and employment in the two years after filing. We report estimates for the full sample and separately for female tenants and Black tenants. As shown in Section 4.1, female and Black tenants are over-represented in housing court in Cook County and New York and comprise the two largest single groups within each city. These groups are of particular interest because prior ethnographic work has argued that they are more likely to face eviction and that they experience greater adversity as a result of eviction. Qualitative research (Desmond et al., 2013; Desmond, 2016) points to two central reasons for more severe impacts of eviction on women, both revolving around children in the household. First, as a result of both greater childcare responsibilities and larger household size, women may face more difficulties securing and maintaining new accommodation. Second, landlords may be reluctant to rent to households with children because

Table 4: Impact on Housing Situation, One Year After Court

	$\mathbb{E}[Y\|E=0]$ (1)	OLS (2)	LDV (3)	RF (4)	IV (5)
Not at Eviction Address	0.311	0.079***	0.070***	0.073***	0.096***
	(0.321)	(0.003)	(0.003)	(0.027)	(0.036)
Lockout	0.002	0.287***		0.314***	0.387***
	(0.031)	(0.005)		(0.026)	(0.033)
Any Homeless Services	0.013	0.038***	0.034***	0.024*	0.030
	(0.082)	(0.002)	(0.002)	(0.015)	(0.019)
Emergency Shelter	0.009	0.033***	0.030***	0.026**	0.033**
	(0.066)	(0.001)	(0.001)	(0.011)	(0.014)
Future Eviction at Same Address	0.074	-0.010***		-0.039***	-0.052***
	(0.184)	(0.001)		(0.011)	(0.013)

Notes: This table reports equally-weighted averages of Cook County and New York non-evicted sample means ($\mathbb{E}[Y|E=0]$), as well as equally-weighted averages of location-specific OLS, lagged dependent variable OLS (LDV), reduced-form (RF), and two-stage least squares (IV) estimates of the impact of eviction on outcomes related to the tenant's housing situation. Outcomes are listed on the left of each row. "Not at Eviction Address" is defined for the calendar year containing the court proceedings and the subsequent calendar year. "Lockout" is defined as a sheriff-executed lockout following court proceedings that occurs within the first year after court proceedings. "Any Homeless Services," "Emergency Shelter," and "Future Eviction at Same Address" are defined for the year following court proceedings. "Future Eviction at Same Address" is restricted to the first case seen for a specific tenant at a specific address. "Future Eviction at Same Address" and "Lockout" do not include LDV specifications as there is no variation in lagged outcomes given the nature and construction of the variables. The main set of controls, included in all model specifications, are: ad damnum amount, gender, race, average neighborhood household income, average neighborhood rent, a cubic in age at filing date, dummies for missing controls, and court-by-year fixed effects. In Cook County regressions only, we also include an indicator for case type; in NYC regressions only, we include controls for the timing of prior benefits receipt. Additionally, for LDV, RF, and IV specifications, we also control for average lagged outcome values for the two years prior to filing year. Standard errors for regression model coefficients are included in parentheses and are clustered at the judge-year level. Median number of observations is 229,342 (max: 942,476, min: 210,842). Observation counts for all specifications and outcomes can be found in Appendix Table F.1. Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

children may cause nuisances to neighbors or attract inspections by Child Protective Services or the city's health department for lead hazards. Black households may experience more adverse impacts of eviction because of discrimination while searching for new housing (Bayer et al., 2017; Christiansen and Timmins, 2019), which would exacerbate the disruptive effects of eviction (Desmond and Gershenson, 2016a).

Table 5 reports estimates for labor market and longer-run housing outcomes. For each sample, we report the estimates from the LDV and IV specifications. For the full sample, we also report OLS (without controls for lagged values of the dependent variable) and reduced-form estimates. The first row reports the estimates for earnings. Column 1 shows that in the full sample, average quarterly earnings in the two years after filing among non-evicted tenants are \$4,441. The OLS estimate suggests that evicted individuals earn approximately \$842 dollars (19 percent) less per quarter in the two years after filing compared to non-evicted individuals. After controlling for lagged values of earnings, the estimate shrinks to \$266 (6 percent) per quarter, consistent with selection into eviction based on negative pre-filing labor market trends. The IV estimate, which is the causal effect for compliers, is a \$352 reduction in quarterly earnings in the full sample, 8 percent of the non-evicted mean. The IV estimates suggest that eviction has a negative impact on earnings similar in magnitude to the earnings drop among evicted tenants prior to filing.

Estimates of the impact of eviction on employment during the two years after filing are also negative across specifications. The employment rate among the non-evicted is 56 percent, and the OLS estimate shows that eviction reduces employment by 3.6 percentage points. Controlling for lagged labor market outcomes shrinks the OLS estimate to 1.6 percentage points, again consistent with negative selection into eviction. The IV estimate suggests that, for marginal tenants, eviction causes a 2.3 percentage point reduction in employment, though the estimate is not statistically significant.

The overall effects of eviction on earnings and employment are driven by large negative effects for women. The IV estimates imply that eviction lowers women's quarterly earnings by $532 (13 percent of the non-evicted mean) and reduces employment by 2.7 percentage points (4.6 percent), though the reduction in employment is not statistically significant. For Black tenants, the IV earnings estimate of a $398 (9 percent) reduction is similar to the full sample estimate and not statistically significant. However, IV also suggests that effects on employment are largest for Black tenants, who experience a 6.3 percentage point (11 percent) reduction in employment over the two years after filing.

The bottom panel of Table 5 examines persistence—during the two years after the year of case filing—in the effects of eviction on the measures of housing instability considered in Section 6.1. The effect on residential mobility persists, with eviction increasing the probability of not being at the eviction address by around 10 percentage points (22 percent) in all specifications. The effect on use of any homeless services also persists, with the IV estimate showing a 3.5 percentage point (194 percent) increase in the two years after the year of case filing. The IV estimate for emergency shelter use is smaller than the IV estimates for the first year after the case (see Table 4) and is not statistically significant. We view our results as consistent with economic models of homelessness that emphasize the transitory dynamics of homelessness (O'Flaherty, 2004).

The effects on housing instability, particularly homelessness, are also driven by larger effects for women. The IV estimates for female tenants suggest a 7 percentage point (373 percent) increase in use of any homeless services and a 4.5 percentage point (375 percent) increase in use of emergency shelters. Both of these estimates are more than twice as large as for the full sample. The effects of emergency shelter use for Black tenants are not reported due to confidentiality restrictions related to cell size.

While overall rates of homelessness are in the single digits for both evicted and non-evicted tenants, our estimates suggest that evictions in Cook County and New York create a large number of homelessness spells. Given there are approximately 90,000 eviction orders per year, if we assume constant effects our estimates imply that an additional 3,000 individuals interact with homeless services in the next year and another 3,100 do so two to three years after the case. Similarly, 2,700 more individuals stay at emergency shelters in the next year and another 600 do so in the two to three years after the case.

Overall, this section has shown that eviction lowers earnings and employment and increases housing instability and homelessness beyond the immediate aftermath of the case. Consistent with ethnographic work suggesting that women and Black renters have more difficulty navigating the housing market because of discrimination and the presence of children, we find that women and Black tenants experience more acute effects from eviction: women appear to experience more severe earnings losses and are more likely to end up homeless, while Black tenants appear to experience larger reductions in employment.

Table 5: Impact on Earnings, Employment, and Longer-Run Housing Situation

	All					Female			Black		
	$\mathbb{E}[Y\|E=0]$ (1)	OLS (2)	LDV (3)	RF (4)	IV (5)	$\mathbb{E}[Y\|E=0]$ (6)	LDV (7)	IV (8)	$\mathbb{E}[Y\|E=0]$ (9)	LDV (10)	IV (11)
Labor:											
Earnings	4,441	-842***	-266***	-278*	-352*	4,212	-220***	-532***	4,386	-221***	-398
	(4,913)	(20)	(19)	(159)	(202)	(3,817)	(17)	(200)	(3,757)	(14)	(247)
Employment	0.564	-0.036***	-0.016***	-0.018	-0.023	0.584	-0.017***	-0.027	0.582	-0.014***	-0.063**
	(0.306)	(0.002)	(0.001)	(0.016)	(0.020)	(0.303)	(0.001)	(0.023)	(0.304)	(0.002)	(0.031)
Housing:											
Not at Eviction Address	0.462	0.117***	0.108***	0.075**	0.099**	0.447	0.112***	0.124**	0.443	0.110***	0.108*
	(0.333)	(0.003)	(0.003)	(0.032)	(0.041)	(0.325)	(0.003)	(0.050)	(0.326)	(0.003)	(0.061)
Any Homeless Services	0.018	0.028***	0.026***	0.025*	0.035*	0.019	0.027***	0.071***	0.023	0.029***	0.033
	(0.094)	(0.001)	(0.001)	(0.015)	(0.019)	(0.096)	(0.001)	(0.021)	(0.105)	(0.001)	(0.027)
Emergency Shelter	0.012	0.022***	0.020***	0.006	0.007	0.012	0.021***	0.045**			
	(0.078)	(0.001)	(0.001)	(0.013)	(0.017)	(0.077)	(0.001)	(0.019)			
Future Eviction at Same Address	0.177	-0.073***		-0.067***	-0.086***	0.178		-0.104***	0.204		-0.112***
	(0.266)	(0.002)		(0.015)	(0.018)	(0.265)		(0.021)	(0.280)		(0.030)

Notes: This table reports equally-weighted averages of Cook County and New York non-evicted sample means ($\mathbb{E}[Y|E=0]$), as well as equally-weighted averages of location-specific OLS, lagged dependent variable OLS (LDV), reduced-form (RF), and two-stage least squares (IV) estimates of the impact of eviction on outcomes related to the tenant's labor and housing situation. Outcomes are listed on the left of each row. Labor outcomes are defined for the two years following the court proceedings. Housing outcomes are defined for 1–2 years following the court proceedings. Because we observe addresses annually, "Not at Eviction Address" is defined for the two calendar years after the court proceedings year. "Future Eviction at Same Address" is an indicator for whether an eviction order was given against the same tenant at the same address within the next three years and is restricted to the first observed case for the tenant at that address. LDV estimates are not reported for "Future Eviction at Same Address" as the analysis restricts to the first observed case for the tenant at that address. We report estimates for the full sample in columns 1–5, the sample of female tenants in columns 6–8, and the sample of Black tenants in columns 9–11. Controls for all model specifications are the same as those described in Table 4, with the omission of gender for "Female" and race for "Black". The effects of emergency shelter use for Black tenants are not reported due to confidentiality restrictions related to cell size. Standard errors for regression model coefficients are included in parentheses and clustered at the judge-year level. Median number of observations is 259,626 (max: 861,743, min: 117,108). Observation counts for all specifications and outcomes can be found in Appendix Table F.2. Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

6.3 Financial distress

Table 6 examines the effect of eviction on financial distress. We focus on outcomes averaged over quarters 5–12 because it may take longer for eviction to affect outcomes measured in credit reports, such as indebtedness and access to credit, but we report short-run effects in Appendix F.5. Table 6 does not report results by race or gender as race is not included in the data provided by the credit bureau.

The first two rows report estimates of the effects on outcomes that proxy for credit access: credit score and the presence of an open revolving line of credit, such as a credit card. The IV estimate suggests that, for the complier population, eviction reduces credit scores by 12 points over quarters 5–12 after filing. This estimate is similar to the decline in credit scores in the lead-up to filing that we document in Section 4, and it is slightly larger than the LDV estimate of an 8 point drop. We find additional evidence that eviction reduces credit access, increasing the likelihood of having an no open source of revolving credit by 7.5 percentage points (16 percent of the non-evicted mean). This estimate is larger than the LDV estimate, suggesting that compliers are more likely to be on the margin of access to conventional credit sources.

Next, we explore whether eviction increases the amount of debt sent to collections agencies. The IV estimate implies that receiving an eviction order increases the total balance in collections by roughly $314 dollars (14 percent), which is not statistically significant.[49]

We then report the effects of eviction on having an auto lease or loan, which may proxy for durable goods consumption (Dobkin et al., 2018; Agarwal et al., 2020). The IV estimate is a 4.5 percentage point reduction in the probability of having an auto lease or loan (a 22 percent decrease relative to the non-evicted mean). This estimate is somewhat larger than the OLS estimate of 3.5 percentage points. While the estimates for credit bureau outcomes are generally similar across locations (Appendix Table F.12), the reduction in auto loans is driven by large effects in Cook County. Finally, we explore effects on payday loan inquiries, which are for the sample of Cook County cases only. The IV estimates for the impacts on payday loan inquiries are imprecise. However, the effects are negative across specifications, providing suggestive but inconclusive evidence that eviction may lessen demand for high interest loans.

Taken together, we find that eviction reduces access to credit up to three years after filing, as measured by both credit scores and having an open revolving line of credit. Eviction also appears to reduce durable consumption during this period, as measured by having an auto lease or loan. Our estimates imply that, for compliers, avoiding an eviction has an effect on credit scores that is similar in magnitude to the effect that moving to a low-poverty neighborhood has on children's future credit scores (Miller and Soo, 2020a), or the effect of removing a bankruptcy flag from a

[49] An eviction may be mechanically related to collections debt if the defendant does not pay the money judgment associated with the eviction case. In this situation, the plaintiff can use the court process to collect the money, including obtaining a citation to discover assets, wage garnishment, and using a collections agency. But in practice this rarely occurs.

credit report (Gross et al. (2020), Dobbie et al. (2020)). While the economic interpretation of these measures is not entirely clear, the reduction in credit scores from eviction could adversely impact tenants through several channels including: reducing liquidity, increasing borrowing costs, and increasing the difficulty of securing housing in the future.

Table 6: Impact on Financial Distress

	$\mathbb{E}[Y\|E=0]$	OLS	LDV	RF	IV
	(1)	(2)	(3)	(4)	(5)
Credit Score	554.35	-13.24***	-8.04***	-8.98**	-12.33**
	(66.58)	(0.40)	(0.36)	(4.45)	(6.15)
No Open Revolving Account	0.459	0.052***	0.041***	0.054**	0.075**
	(0.324)	(0.002)	(0.002)	(0.024)	(0.032)
Total Bal. Collections	2,164.41	471.30***	420.74***	162.47	313.97
	(2,572.07)	(16.60)	(16.50)	(193.22)	(263.88)
Any Auto Loan or Lease	0.200	-0.035***	-0.027***	-0.027	-0.045*
	(0.282)	(0.002)	(0.002)	(0.018)	(0.024)
Any Payday Inquiry (x100)	15.387	-1.182***	-1.004***	-1.229	-3.153
	(36.083)	(0.232)	(0.216)	(3.271)	(4.478)

Notes: This table reports equally-weighted averages of Cook County and New York non-evicted sample means ($\mathbb{E}[Y|E=0]$), as well as equally-weighted averages of location-specific OLS, lagged dependent variable OLS (LDV), reduced-form (RF), and two-stage least squares (IV) estimates of the impact of eviction on outcomes related to the tenant's financial health. The only exceptions are estimates for the outcome "Any Payday Inquiry (x100)," which are specific to Cook County. All outcomes are for five to twelve quarters after the court proceedings. Controls for all model specifications are those described in Table 4, except we do not control for race, which is not included in the data provided by the credit bureau. Standard errors for regression model coefficients are included in parentheses and are clustered at the judge-year level. Median number of observations is 316,669 (max: 350,328, min: 42,985). Observation counts for all specifications and outcomes can be found in Appendix Table F.3.

6.4 Hospital visits

In this section, we investigate the effects of eviction on hospital use in New York, the only city for which we are able to link our eviction data to hospital data. Table 7 reports estimates for three measures of hospital use: the total number of (non-pregnancy related) hospital visits, the total number of emergency room visits, and the total number of hospital visits for mental health conditions. The table includes results for the two time periods considered in the previous sections: the first year following a case and the two subsequent years.

Eviction orders increase total hospital visits by 0.19 visits in the first year following the case (29 percent relative to the non-evicted mean). Estimates for the total number of emergency room visits are similar, though the IV point estimates are not statistically different from zero. Eviction also increases the number of visits to a hospital for mental health conditions in the immediate wake of a case by about 0.05 visits, a more than 100 percent increase over the non-evicted mean.[50] Beyond the first year after the case, the IV estimates are mostly negative and none are

[50]The most common category of mental health conditions among the evicted is anxiety-related diagnoses.

statistically significant. Overall, the effects of eviction on hospital use appear concentrated in the period shortly after the case filing. These impacts may reflect a deterioration in tenants' health, but they may also reflect the use of hospitals as an alternative temporary source of shelter.

Table 7: Impact on Hospital Use

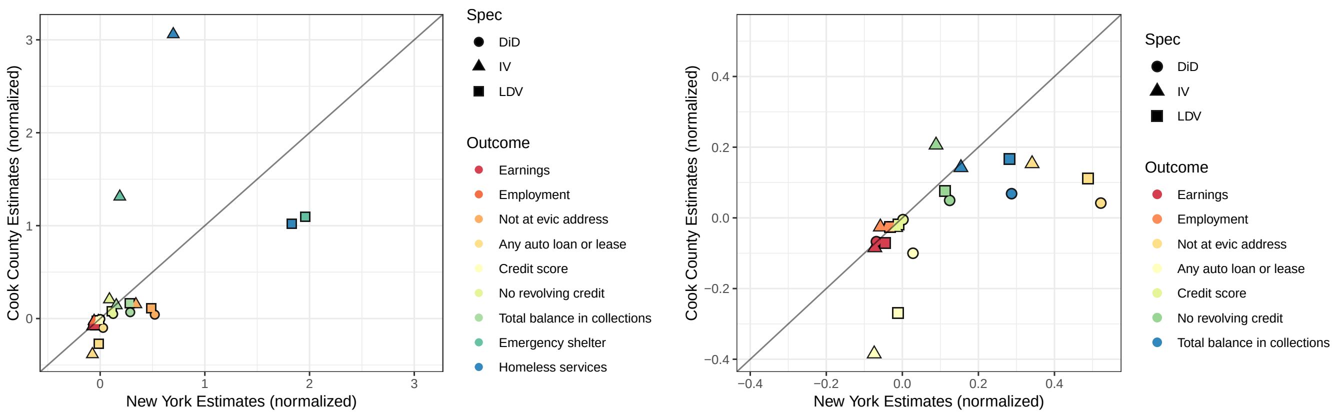
	$\mathbb{E}[Y\|E=0]$ (1)	OLS (2)	LDV (3)	RF (4)	IV (5)
Q1-Q4:					
Num. Hosp. Visits	0.655	0.067***	0.043***	0.164**	0.193**
	(1.237)	(0.008)	(0.006)	(0.073)	(0.094)
Num. Emerg. Visits	0.538	0.053***	0.034***	0.092	0.111
	(1.044)	(0.006)	(0.005)	(0.070)	(0.090)
Num. Mental Health Visits	0.044	0.019***	0.014***	0.043*	0.049*
	(0.290)	(0.002)	(0.001)	(0.023)	(0.029)
Q5-Q12:					
Num. Hosp. Visits	0.685	0.070***	0.040***	-0.050	-0.088
	(1.548)	(0.009)	(0.008)	(0.113)	(0.151)
Num. Emerg. Visits	0.555	0.055***	0.031***	-0.046	-0.073
	(1.262)	(0.007)	(0.007)	(0.090)	(0.122)
Num. Mental Health Visits	0.074	0.045***	0.029***	-0.254	-0.331
	(0.829)	(0.006)	(0.006)	(0.160)	(0.223)

Notes: This table reports New York non-evicted sample means ($\mathbb{E}[Y|E=0]$), as well as New York specific OLS, lagged dependent variable OLS (LDV), reduced-form (RF), and two-stage least squares (IV) estimates of the impact of eviction on outcomes related to the tenant's health. Outcomes are listed on the left of each row. Outcomes are defined for the year following court proceedings (year 0) and for the first and second year following court proceedings (years 1–2). Controls for all model specifications are the same as those described in Table 4. Standard errors for regression model coefficients are included in parentheses and clustered at the judge-year level. Median number of observations is 179,026 (max: 181,887, min: 50,893). Observation counts for all specifications and outcomes can be found in Appendix Table F.4.

6.5 Comparisons across locations

Given that our estimates combine effects across two different locations, it is natural to ask whether the combined estimates are driven by especially large effects in one location. Figure 6 explores this possibility by comparing estimates for Cook County and New York. Estimates for Cook County are plotted along the vertical axis, while estimates for New York are plotted along the horizontal axis. We normalize the estimates by dividing them by the corresponding non-evicted mean. The left panel shows the full set of estimates, while the right panel removes homelessness-related outcomes. The estimated effects on employment, earnings, and credit score are remarkably similar across cities. The normalized estimates for the effects of moving from the eviction address are somewhat larger for New York, but these differences are driven largely by lower rates of moving among those not evicted in New York (30% one to two years after the case) compared to Cook County (63% one to two years after the case) rather than large

Figure 6: Comparison Across Locations



Notes: Left panels show results including HMIS outcomes related to homelessness. Right panel excludes HMIS outcomes. All coefficients have been normalized by dividing by the mean of the outcome for tenants who are not evicted. All results are for 1–2 years after the case filing. Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

differences in point estimates. Estimates for total balances in collection are moderately larger for New York, while estimates for any auto loan or lease are larger for Cook County. Estimates for homelessness are less consistent across cities—though both point to substantial increases—which may be expected given the sizeable differences in rates of shelter use in New York and Cook County.

6.6 What about non-complier cases?

The IV results above are estimates of weighted averages of local average treatment effects, which are relevant for compliers: tenants whose case would have had a different outcome if they had been assigned a different judge. As discussed in Section 5.2, under certain assumptions on the outcome process and the relationship between outcomes and eviction at the time of filing, difference-in-differences estimates provide either a downward-biased or upward-biased estimate of the average treatment effect on the treated.

Table 8 compares the IV estimates discussed above with the two sets of DiD estimates. Column 4 reports DiD estimates with a pre-period that is further from treatment than the post period, while Column 5 reports DiD estimates with a pre-period that is closer to treatment than the post period. For earnings one to two years after filing, the DiD estimates are −\$301 and −\$119, suggesting that the average treatment effect on the treated is somewhat smaller than our IV point estimates of −\$353. For employment, the DiD estimators yield estimates of −.017 and −.013, which is again slightly smaller than the IV estimate of −.023.

Appendix Tables F.6 and F.7 provide similar comparisons between the IV estimates and the two DiD estimates for financial distress outcomes and health outcomes in years 1–2 after case filing.

For financial distress, we find that the DiD estimates have the same sign as IV but are consistently smaller. For health-related outcomes, the IV estimates are all statistically insignificant, while the DiD estimates are all statistically significant and suggest moderate increases in hospital visits and emergency department visits and relatively large effects on hospital visits for mental-health related diagnoses.

Overall, the DiD estimates consistently show results that are broadly similar to the IV estimates but smaller in magnitude. This suggests that the average treatment effect for the treated is smaller than the local average treatment effect, which relies on complier cases.

Table 8: Comparison of IV and DiD estimates

Labor Market	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	DiD$_{-8}$ (3)	DiD$_{-1}$ (4)
Earnings	4,441	-352*	-301***	-119***
	(4,913)	(202)	(34)	(33)
Employment	0.564	-0.023	-0.017***	-0.013***
	(0.306)	(0.020)	(0.002)	(0.002)

Housing	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	DiD$_{-12}$ (3)	DiD$_{-5}$ (4)
Not at Eviction Address	0.462	0.099**	0.091***	0.097***
	(0.333)	(0.041)	(0.004)	(0.004)
Any Homeless Services	0.018	0.035*		
	(0.094)	(0.019)		
Emergency Shelter	0.012	0.007		
	(0.078)	(0.017)		
Future Eviction at Same Address	0.177	-0.086***		
	(0.266)	(0.018)		

Notes: This table reports equally-weighted averages of Cook County and New York non-evicted sample means ($\mathbb{E}[Y|E=0]$), as well as equally-weighted averages of location-specific two-stage least squares (IV) and difference-in-differences (DiD) estimates of the impact of eviction on outcomes related to the tenant's labor and housing situation. For the DiD estimates, the subscript denotes the reference pre-period quarter relative to quarter of court proceedings. The dependent variable is listed in each row. Outcomes are listed on the left of each row. Outcomes are defined as in Table 5: labor outcomes are defined for the two years following the court proceedings, and housing outcomes are defined for 1–2 years following the court proceedings. IV controls are the same as those described in Table 4, and DiD controls are the same as for OLS controls as described in Table 4 but also include individual-level fixed effects. Standard errors for regression model coefficients are included in parentheses and are clustered at the judge-year level in all specifications. Median number of observations is 248,907 (max: 861,743, min: 117,108). Observation counts for all specifications and outcomes can be found in Appendix Table F.5. Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

7 Conclusions

This paper provides new evidence on the consequences of a court-ordered eviction for tenants by linking eviction case records to a range of administrative data sets. We study two major urban areas where many low-income households interact with housing court, with annual case filing rates over 10 percent in some zip codes. We document signs of increasing economic distress in the lead-up to case filing across a broad set of tenant outcomes: falling earnings, decreased attachment to the labor market, rising unpaid bills, and increases in hospital visits. These patterns motivate our IV design, which uses the random assignment of judges to estimate the impact of an eviction order for complier cases that would be affected by a marginal change in judge leniency. We complement this research design with panel data methods that allow us to bound the effects of eviction on all evicted tenants—as opposed to compliers to the instrument—under alternative assumptions.

Our findings suggest that eviction compounds the rising economic distress experienced by tenants in the lead-up to a court filing. Eviction increases residential mobility, hospital visits, and use of emergency shelters in the first year after the case is filed, suggesting that it contributes to physical and material hardship. Eviction also reduces earnings, credit access, and durable goods consumption for at least two years after filing, and it increases residential mobility and interactions with homelessness services systems for up to three years after filing. Impacts on earnings, residential mobility, and homelessness are driven by deleterious effects on female and Black tenants, as suggested by previous ethnographic studies.

This research speaks to an active policy debate on how, if at all, governments should address evictions. Our results suggest that averting an eviction order may yield considerable benefits for tenants, particularly women and Black tenants, two groups that are over-represented in housing court. Beyond the pecuniary costs of eviction to the tenant from reduced earnings and worsened credit, the increases in hospital and homeless services use suggest that eviction has meaningful non-pecuniary costs to tenants. The high cost to local governments of providing healthcare and homeless services (Evans et al., 2019) suggest that there are also considerable spillover costs for society. These costs are important considerations when evaluating eviction-related policies, prominent examples of which are emergency financial assistance for renters, subsidized legal aid in housing court, and increased legal protections for tenants. Such policies merit further study to fully assess their costs and benefits, including their general equilibrium effects. Finally, the broad-based increase in economic distress that we document in the run-up to filing suggests that case filings—which are typically public records—might provide a useful proxy to help target assistance to low-income renters who have recently experienced potentially-destabilizing life events.

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APPENDIX

Eviction and Poverty in American Cities: Evidence from Chicago and New York

A Appendix: Recent eviction reforms in U.S. cities, counties, and states

This section describes a selection of recent reforms to legal frameworks for eviction and tenant-landlord relations, as well as publicly-funded programs aiming to support tenants facing eviction at the city, county, and state level. Table A.1 provides a summary of these reforms, with the most recent changes listed first. This section was last updated on July 13, 2019, and may not include the latest developments for ongoing legislative initiatives.

2019

New York On June 14, 2019, Governor Andrew Cuomo signed the "Housing Stability and Tenant Protection Act of 2019" (S.6458), which increases protections for tenants facing eviction and strengthens rent control statewide. Beyond making rent regulation permanent, the omnibus bill strengthens rent control in several ways, including: repealing policy that previously allowed landlords to significantly increase rent for vacant units, including high rent units in the scope of rent regulation, and restricting the permissible rent increase when landlords renovate an apartment or unit. The bill also expands tenant and eviction protections by banning tenant blacklists, establishing illegal eviction (e.g., locking tenants out), extending the time allotted for tenants to find a lawyer or pay unpaid rent, and allowing judges to stay eviction orders for a maximum of one year.

California In May 2019, the California State Assembly passed a bill (AB 1482) to strengthen rent control, and the California State Senate last amended the bill on July 11, 2019, to add restrictions on permissible causes for eviction. If passed, the bill will prohibit landlords from raising rent more than once each year. Also, the allowed rent increase would be capped at the lower of either 7 percent plus inflation (annual percentage change in regional CPI), or 10 percent of the lowest rental rate for the unit during the previous year. In addition to rent control, the bill includes a clause that prohibits landlords from evicting tenants without a "just cause" (AB 1481). Current state laws do not require landlords to have a specific cause for eviction, but 17 cities have already enacted city-wide provisions on "just-cause" eviction.

Washington On May 9, 2019, Governor Jay Inslee signed HB 5600, a bill aimed to protect tenants facing eviction. Once the bill is implemented on July 28, 2019, landlords will be required to provide tenants with a 14-day, instead of a 3-day notice when they default on rent payment. The notice must be written in plain language and include information on legal aid resources and court interpreter services. The bill also mandates that a tenant's right to possession of his unit is conditional only on rent and not other monetary amounts (e.g., costs incurred by late payments, attorney fees, etc). Importantly, under HB 5600, judges will be given discretion to stay eviction

Table A.1: Recent changes to eviction policy

Location	Year	Summary	Implemented?
New York	2019	Bill 6458 extends rent control statewide; establishes stronger tenant protections (e.g., defining illegal eviction and allowing judges to stay eviction orders up to one year).	Yes
California	2019	Bill 1482 establishes Universal Rent Control; prohibits landlords from eviction without "just cause".	No
Washington	2019	Bill 5600 requires landlords to notify tenants 14 days in advance when there is a default in rent payment; Bill 1440 requires landlords to notify their tenants 60 days before rent increase.	Yes
Mississippi	2019	Bill 2716 eliminates the ten day grace period tenants were originally given to vacate their home.	Yes
Virginia	2019	Bill 2655 establishes a pilot eviction diversion program.	No
Oregon	2019	Bill 608 implements Universal Rent Control.	Yes
Philadelphia, PA	2019	Bill 170854 requires "good cause" for evictions; tenants must be notified 30 days in advance.	Yes
Richmond, VA	2018	Eviction Diversion Program	No
California	2018	AB2343 extends the number of days tenants are given to remedy the cause for eviction and to respond to eviction court filings.	Yes
Oakland, CA	2018	Measure Y extends "just cause" eviction protections to tenants living in owner-occupied duplexes and triplexes.	Yes
North Carolina	2018	S.224 allows landlords to recover attorney's fees and filing fees incurred from a tenant during the eviction process.	Yes
Washington, D.C.	2018	Eviction notices must have a set date, at least 2 weeks in advance; evictions will occur by changing the locks.	Yes
San Francisco, CA	2018	Proposition F gives all tenants the right to tax-funded legal assistance.	Yes
Durham, NC	2018	Eviction Diversion Program.	Yes
Santa Monica, CA	2018	Provides protection from eviction during the school year for educators and families with school age children.	Yes
Portland, OR	2018	Ordinance 188849 requires landlords to pay renters' moving costs when evicted without cause or due to a rent increase.	Yes
Philadelphia, PA	2018	Philadelphia Eviction Project provides legal services for tenants facing eviction.	Yes
Denver, CO	2018	Eviction legal defense program.	Yes
Denver, CO	2017	Mediation services, Landlord-Tenant Guide, and financial support to low- and moderate-income households in crisis.	Yes
Detroit, MI	2017	Ordinance No. 33-17 prevents landlords from collecting rent if they haven't passed city inspections.	Yes
New York, NY	2017	Intro. 214-B provides all low-income tenants facing eviction with legal representation.	Yes
Berkeley, CA	2017	Tenant Protection Ordinance prohibits landlords from conducting evictions using misleading information or coercive conduct.	Yes

Notes: This table summarizes proposed and implemented changes to eviction policy.

orders up to 90 days after the judgment, for considerations such as whether the tenant defaulted on rent due to extraordinary circumstances. Separately, the governor signed HB 1440, which will also be implemented on July 28, 2019. This bill will require landlords to provide a 60-day, rather than 30-day notice if they plan to increase the rental rate.

Mississippi On March 22, 2019, Governor Phil Bryant signed SB 2716, a bill that amends the Mississippi Landlord-Tenant Act to reduce protections for tenants in eviction court. This bill will eliminate the ten day grace period tenants were previously given to vacate their homes once they were issued an eviction order. Prior to the amendment, tenants used this time to move out of their residences, or negotiate payment schedules with their landlords. Under the new law, tenants may petition for three days to vacate as long as the request is just and equal for both parties involved. If the tenants do not petition, they will be forced to move directly after the eviction judgment.

Virginia On March 12, 2019, Governor Ralph Northam signed HB 2655 into law, which aims to reduce the number of evictions at district courts in Danville, Hampton, Petersburg, and Richmond. Under the eviction diversion program, the court will order eligible tenants to pay back their landlords through monthly installments. The court will then dismiss the eviction order if and when the tenant satistfies the payment plan. To qualify for the program, tenants must not be in another eviction diversion program, and must not have missed their rent payment more than two times in 6 months or three times in 12 months. Proponents of HB 2655 argue that the program will help tenants who fall behind in their rent payments due to sudden job loss or medical emergencies. The program is scheduled to run on a trial basis from July 1, 2020 to July 1, 2023.

Oregon On February 28, 2019, Governor Kate Brown signed SB 608 into law, making Oregon the first state to implement Universal Rent Control. Now, landlords can only increase rent once a year, up to seven percent plus inflation, with some exceptions. Additionally, if a tenant lived in the unit for over a year, his landlord is prohibited from evicting him without cause. If a tenant has lived in a unit for less than a year, the landlord is able to end the month-to-month tenancy without cause, provided he or she gives the tenant a 30-day notice. Finally, to increase public accountability, the Oregon Department of Administrative Services is required to publish the maximum rent increase percentage annually.

Philadelphia, PA On January 22, 2019, Mayor Jim Kenney signed Bill 170854, which went into effect on April 22, 2019. The new law requires there to be a "good cause" to evict a tenant if the residential lease is less than a year. A few "good cause" reasons include: if the renter has not paid rent, has not followed the terms of the lease, or if there has been property damage. Additionally, even if the landlord has "good cause," he or she must notify the tenant at least

30 days before the eviction date. Finally, the tenants then have the right to contest the "good cause" by filing a complaint with the Fair Housing Commission.

Richmond, VA In January 2019, Mayor Levar Stoney announced the initiation of the Richmond Eviction Diversion Program. Led by the Central Virginia Legal Aid Society, Housing Opportunities Made Equal of Virginia, and the city courts, the program promises to provide an array of services to tenants facing eviction. The planned initiatives include pro-bono legal representation in court, financial assistance for qualifying households, and a financial literacy campaign. This program is similar to existing ones in Durham, NC and Kalamazoo, MI.

2018

California Governor Jerry Brown signed AB 2343 into law on September 5, 2018. This bill amends the California Code of Civil Procedure Sections 1161 and 1167. It gives tenants three court days, instead of calendar days, to pay rent or comply with the other terms of the lease before landlords can proceed with eviction court filing. Additionally, tenants will have five court days to respond to the landlord's eviction court filing, after which the landlord can obtain an eviction order by default. This bill uses court days instead of calendar days to ensure that holidays and weekends are not counted under the tenants timeline to respond to the landlords eviction notice or breach of lease notice.

Oakland, CA On July 24, 2018, the Oakland City Council voted unanimously to add to the local ballot a measure aimed to amend limitations on Oakland's eviction law (Measure Y). With 58 percent voter approval, Measure Y was passed on November 6, 2018. The effects are twofold: first, it extends "just cause" eviction protections to tenants living in owner-occupied duplexes and triplexes. Second, it allows the city council to pass further limitations on landlords' right to evict without another election.

North Carolina SB 224 became law in June 2018, allowing landlords to recover "reasonable" attorney's fees incurred from a tenant during the eviction process. It also allows landlords to recover filing fees charged by the court, which is the cost to issue a summons for the tenant to appear in court. There are some restrictions on this measure, however. If the tenant owes back rent, the amount the landlord can recover must not be more than 15% of the rent owed. If they don't owe back rent, the amount recovered cannot be more than 15% of the monthly rent.

Washington, D.C. On July 10, 2018, the Council of the District of Columbia passed the Eviction Reform Emergency Amendment Act of 2018, which was enacted on July 26, 2018. The emergency act amends prior laws that required eviction notices to include a scheduled eviction date and be delivered to the tenant two weeks prior to that date. The act also places limitations on how the landlord handles and disposes of the tenant's personal possessions. For instance, rather than placing the tenant's property outside of the unit during the eviction process, the landlord is required to keep those belongings for at least seven days (excluding Sundays and federal holidays). Finally, the act prohibits evictions when rain or snow is forecast.

Note that the emergency act expired on October 24, 2018. A temporary act with identical content was enacted on October 10, 2018 and became effective on November 27, 2018 (D.C. Law 22-183). Given the nature of temporary acts, the law is set to expire on July 10, 2019.

San Francisco, CA On June 5, 2018, San Francisco County voters passed Proposition F, a local ballot measure that gives tenants facing eviction lawsuits the right to tax-funded legal assistance. This program is estimated to cost the city $4.2 million to $5.6 million a year. Legal services are available to tenants either 30 days after they are served an eviction notice, or when they are served an unlawful detainer complaint. The program applies to renters of all income levels, not just low-income households.

Durham, NC On May 31, 2018, the Durham City Council voted to allocated $200,000 to the Eviction Diversion Program led by the Civil Justice Clinic. The organization is a collaborative effort between Duke Law and Legal Aid of North Carolina. The program was launched earlier in 2017 and provides low-income tenants with legal representation in eviction court.

Santa Monica, CA On May 8, 2018, the Santa Monica City Council approved an ordinance that strengthens protections for educators or households with school-age children facing potential eviction. The ordinance prohibits a court from granting a no-fault eviction during the school year to the aforementioned types of tenants. A no-fault eviction usually occurs when a landlord wishes to occupy, renovate, or demolish the unit. This aims to prevent evictions from disrupting the school year for both students and teachers.

Portland, OR In March 2018, the Portland City Council passed Ordinance 188849 to permanently establish the tenant relocation assistance program. Under this amendment to the Residential Landlord and Tenant Act, landlords must pay their tenants' moving costs either if they are evicted without cause, or if they are forced to move due to a rent increase of 10 percent or more. The program existed for a year on a trial basis prior to March 2018.

Philadelphia, PA The Philadelphia Eviction Protection Project launched in January 2018. It provides new and improved legal services for tenants facing eviction, including legal assistance in the courtroom, a new tenant aid hotline, a website answering common legal questions, full-time service in a Landlord-Tenant Help Center in the courtroom, and financial counseling. Community Legal Services, along with a team of other local organizations, has been selected to implement the program. The program is a product of the Eviction Task Force, which was formed in 2017 to help come up with solutions to solve the city's eviction problem. The City Council allocated $400,000 for the project, while the Department of Planning and Development allocated $100,000.

Denver, CO In January 2018, thirteen Denver City Council members, through donations from office budgets and personal contributions, pooled together $131,500 to help start the Eviction Legal Defense Pilot. Led by Colorado Legal Services, this program provides full legal representation for tenants who fall below 200 percent of the federal poverty standard. Attorneys are available either on site at the Denver County Court or at Colorado Legal Services. This pilot program was funded to last for six to nine months, but has been continued.

2017

Denver, CO In October 2017, Mayor Michael B. Hancock launched a series of programs aimed at reducing evictions, through several government departments and county courts. They created a Landlord-Tenant Guide, which clearly outlines the rights and responsibilities of both parties and provides a list of resources for conflict resolution before court action. The city also put mediation services in place to resolve landlord-tenant conflicts before and after the eviction process. Finally, the Temporary Rent and Utility Assistance (TRUA) program provides low- to middle-income tenants in danger of eviction with funds for utility payments and rent.

Detroit, MI In October 2017, the Detroit City Council passed Ordinance No. 33-17, which prevents landlords from collecting rent if they have not passed city inspections. The motivation for this amendment came from the low level of landlord compliance with lead inspection laws. Under the law, after a six-month phase-in period, tenants who live in units that have not passed inspections can put their rent in an escrow account for 90 days. If the landlord continues to refuse city inspection, the tenant can collect the escrowed rent after 90 days. Although most rental units must undergo annual inspection by law, the ordinance provides exceptions to compliant landlords who meet certain criteria.

New York, NY On August 11, 2017, New York Mayor Bill de Blasio signed Int. No. 214-B into law. The new law requires the implementation of programs to provide low-income tenants facing eviction with legal representation. Low-income is defined as households with gross incomes at or lower than 200 percent of the federal poverty standard. In addition, tenants of all income levels would be entitled to one legal consultation.

Berkeley, CA In March 2017, the Berkeley City Council passed the Tenant Protection Ordinance, which prohibits landlords from conducting illegal evictions using fraudulent/misleading information or intimidating/coercive conduct. Landlords are also prohibited from exploiting tenants on the basis of their immigration status and disabilities. Finally, landlords must now give a copy of the ordinance to tenants when they move in, and must also include it with any eviction notice.

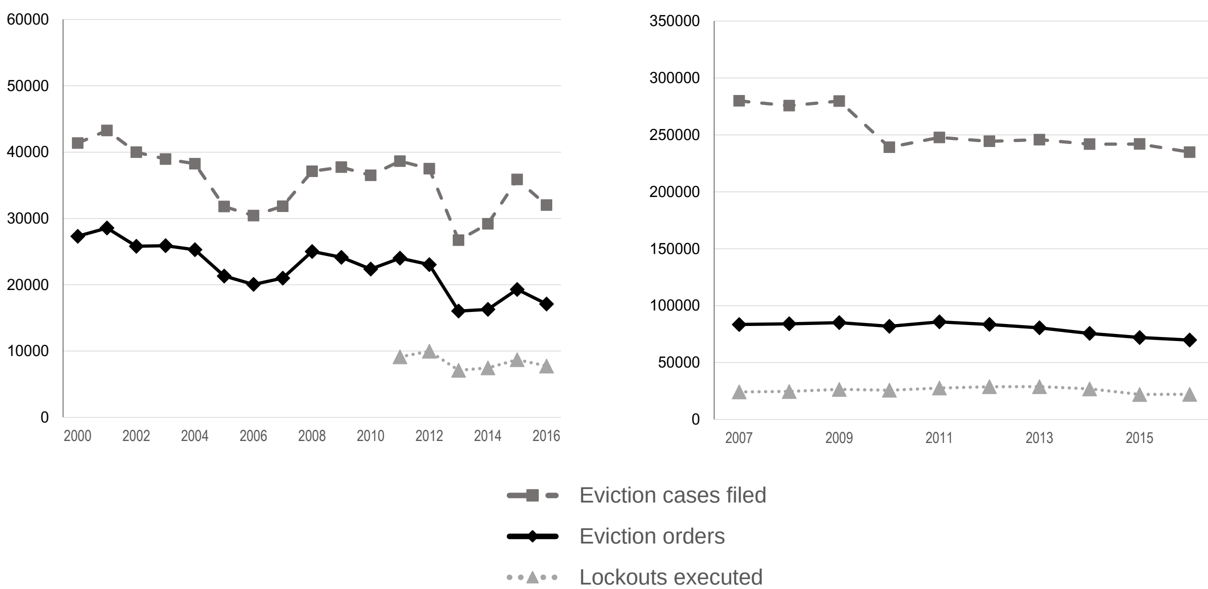
B Appendix: Institutional details

B.1 Time trends in eviction filings, orders, and lockouts

Figure B.1 plots the number of eviction cases filed (top line), eviction orders (middle line), and lockouts executed (bottom line) in Cook County (left panel) and New York (right panel).

Figure B.1: Time Trends in Eviction Filings, Orders, and Lockouts

(a) Cook County **(b) New York**

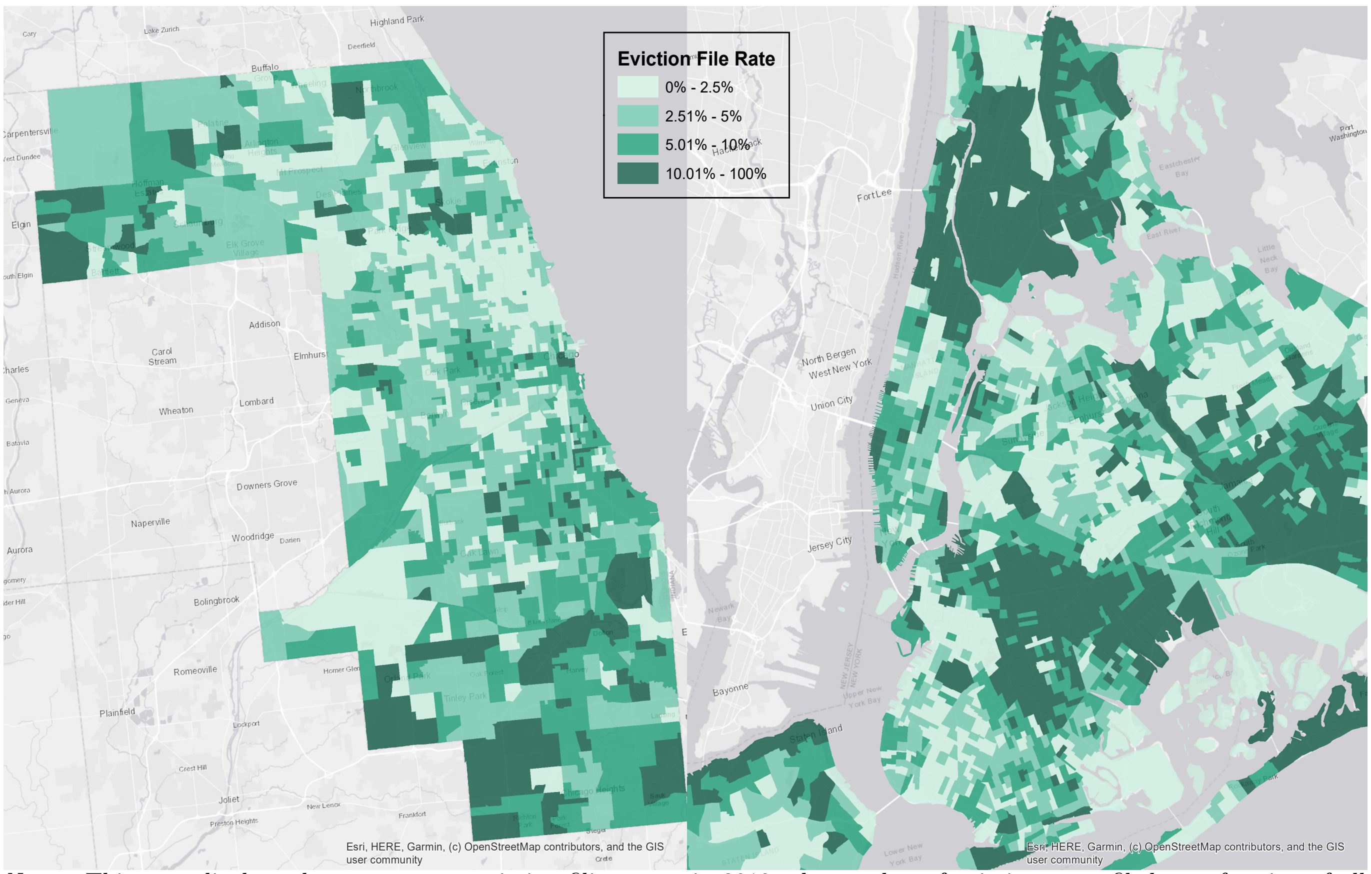


Notes: These figures display time trends in eviction filings, eviction orders, and lockouts in Cook County (left) and New York (right). The counts of filings and eviction orders are based on the full, unrestricted samples of court records for both jurisdictions. The lockout counts are based on data from the Sheriff's Office (Cook County) and the Marshal's office (New York).

B.2 Eviction rates across neighborhoods

Figure B.2 maps the eviction filing rate in 2010 by census tract. The eviction filing rate is calculated as the number of eviction cases filed in the census tract divided by the number of occupied rental housing units calculated from the American Community Survey.

Figure B.2: Neighborhood Eviction Filing Rates



Notes: This map displays the census tract eviction filing rates in 2010: the number of eviction cases filed as a fraction of all rental occupied housing units (from the ACS 2006–2010). The overall eviction filing rate was 4 percent in Cook County and 11 percent in New York.

B.3 Court procedures in Cook County

Relevant legislation for Cook County. The relevant legislation is recorded in two sources, the Municipal Code of Chicago Residential Landlords and Tenants Ordinance (RLTO), and the Illinois Compiled Statutes (ILCS). The RLTO applies only to Chicago (i.e., the first court district), while the ILCS apply to Cook County and thus also to Chicago. The RLTO trumps the ILCS in Chicago, but only when it is more strict towards landlords. For our data period, the most important parts of the legislation are the Forcible Entry and Detainer Act (735

ILCS 5/9) and the Civil Practice Act (735 ILCS 5/2).[51]

Cook County court districts. The Forcible Entry and Detainer Section of the Circuit Court of Cook County handles eviction cases. The court divides the county into six districts. Each district has its own court house with evictions courtrooms, and its own set of judges who handle eviction cases. Landlords must file eviction cases in the district in which the property is located. The vast majority of cases in our data come from the first court district, which handles cases relating to properties located in the City of Chicago. Figure B.3 presents a map of the court districts. Our data set spans all six districts. In the paper and the remainder of this appendix, we refer to the Forcible Entry and Detainer Section of the Circuit Court of Cook County simply as 'Cook County eviction court' or 'Cook County housing court'.

Filing an eviction case. After serving the proper notice to the tenant and waiting the required number of days, if the tenant has not yet vacated the premises the landlord may file for an eviction case. To file, the landlord (the plaintiff) or his attorney must provide the clerk of the Circuit Court of Cook County with a complaint form and a summons form and pay the filing fee.

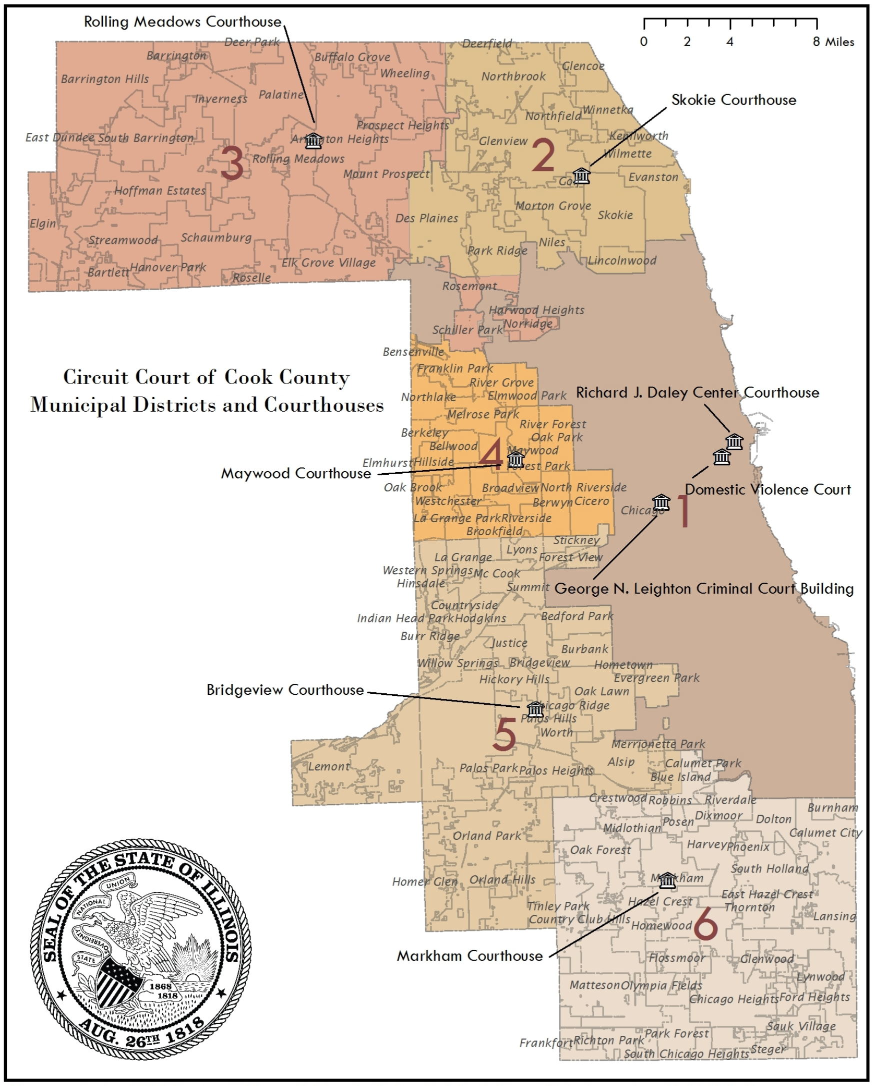
On the complaint form, the plaintiff must provide the address of the tenant, the reason for claiming action, and, for joint action court cases, the amount of rent and/or compensation claimed for damages. Then, the sheriff serves the summons form to the tenant, which alerts him of the eviction court case as well as the date, time, and location of the hearing.

The filing fee depends on the court case type and varies over time. For joint action cases (for possession and rent) with claims for over \$15,000 in compensation, the cost was \$255 in 2000 and \$463 in 2016. For joint action cases with claims under \$15,000 or single action cases (for possession only), the cost was \$106 in 2000 and \$268 in 2016.

Randomized case assignment. Once the plaintiff submits the required eviction filing forms and pays the filing fee, they are given a range of dates from which to choose. These dates are usually between 2–4 weeks after the filing date and always on weekdays. Once the clerk enters the date selected by the plaintiff, a computer program randomly assigns a courtroom and time to the case. Since each judge is designated to a specific courtroom, the random selection of courtroom and time effectively randomizes judge assignment. The process is analogous for plaintiffs who use e-filing. It is possible for the plaintiff to determine the judge who will be presiding over the assigned courtroom either by looking it up on the court website or by asking the clerk (either in person or by phone call). However, they cannot change the assignment by attempting to re-file or requesting a new date prior to the first hearing.

[51] The Forcible Entry and Detainer Act was replaced by the Eviction Act on January 1st, 2018. Our data set does not cover the Eviction Act's start date.

Figure B.3: Administrative Districts of the Cook County Circuit Court



Map prepared on Aug. 8, 2012; Department of Geographic Information Systems, Cook County Bureau of Technology; cook_muniJudicl_2012.pdf;

Notes: This figure shows the six Municipal Districts that determine where landlords in our sample must file eviction court cases. District 1 serves the City of Chicago, district 2 serves the northern suburbs of Cook County, district 3 serves the northwestern suburbs, district 4 serves the western suburbs, district 5 serves the southwestern suburbs, and district 6 serves the southern suburbs. Source: http://www.cookcountycourt.org/ABOUTTHECOURT/OrganizationoftheCircuitCourt.aspx.

Court proceedings. Except under rare circumstances, the landlord and/or his attorney will be present on the return date provided at the time of filing. Depending on whether the tenant was successfully served the court summons, the tenant may or may not show up on the return date. The landlord only finds out whether the defendant was successfully served on the return date. If the tenant is not present, the court will re-attempt to serve the tenant, usually through a special process server, and the landlord is given a new date to return to court. The judge will usually authorize multiple attempts at serving the tenant before deciding that a good-faith attempt at serving the tenant has been made and granting a default order for possession to the landlord.

If and when the tenant shows up to court, there are several courses of action they can pursue. They can request a continuance which delays the start of the case to give the tenant additional time to find an attorney or seek legal advice. If granted, the tenant is usually given one week to find legal assistance. At any point prior to the bench trial, the tenant can also request a trial by jury, and the case may be moved to a jury courtroom, which takes additional time. Alternatively, before moving to the bench trial, the landlord and tenant may agree to a settlement order.[52] This allows the landlord and tenant to negotiate certain binding conditions, which, if adhered to, result in the eviction case being dismissed. Typically, this involves the tenant agreeing to vacate the premises by a certain date and the landlord agreeing to dismiss the case, or the tenant agreeing to pay a certain amount by a certain date. If the tenant fails to fulfill the settlement conditions, the landlord can return to court and receive an immediate order for possession.

Finally, the landlord may dismiss the case for a variety of reasons. Common reasons include: the landlord realizes they made a mistake in the filing of the case, the tenant left the premises so the landlord no longer needs to obtain an order for possession, or the landlord and tenant came to an understanding outside of court. This typically results in the case being recorded as Dismissed by Plaintiff. If the landlord doesn't dismiss the case but simply fails to show up, the case is recorded as Dismissed for Want of Prosecution.

If none of the above occur, the case usually moves to a bench trial, in which both sides present their arguments and evidence in front of the judge. At that point, the judge makes a ruling to either grant an order for possession (and a money judgment for joint action cases) or to dismiss the case in favor of the tenant. Dismissal in favor of the tenant usually results in a dismissal with prejudice, which does not allow the landlord to re-file for the same reasons.

After a judge grants an order for eviction. After a judge grants an order for eviction, the judge can grant a "stay," which gives the tenant a certain number of days before the landlord can file the order for eviction with the sheriff. Judges usually give a one-week stay. Additionally, before the eviction is carried out, the tenant may submit a motion to vacate to the Court asking the judge to vacate the eviction order, though this is rare.

Once the order has been entered and any stay periods have expired, the landlord may file the order for possession with the Sheriff's Office for a fee of $60.50. The sheriff may enforce the eviction order by executing a lockout as soon as 24 hours after the landlord's filing. However, the median time between an order and a lockout, if it was executed, was 71 days in 2011–2016, based on data from the Cook County Sheriff's Office.

At any point leading up to the eviction, the landlord can cancel the eviction, for example

[52] An example of a settlement form is available online, but not all settlement forms follow this format: http://www.cookcountyclerkofcourt.org/Forms/pdf_files/CCMN027.pdf.

because the tenant already left the premises.

On the day of eviction, the landlord (or his representative) is required to greet the sheriff's deputy at the property with a locksmith alongside. Once papers authorizing the deputy's use of force (if necessary) are signed, the deputy enters the property and removes any occupants listed on the order.[53] Once the tenants have been removed, the landlord will change the locks to the door(s), completing the eviction process.

Money judgments. If the landlord filed a joint action case, the judge must also decide if and how much the tenant owes the landlord for back rent and claimed damages.

In joint action cases, it is possible for the judge to grant the landlord an order for possession but no money order. In contrast, it is very rare for the landlord to obtain a money order but no order for possession. If the tenant does not show up to court after being served the summons several times, the judge can often grant an order for possession, but the ILCS generally forbids the judge from making a money judgment in such situations.

Landlords can use a money judgment to obtain an order for garnishment of wages, tax refunds, or other assets, though wage garnishment requires getting an additional court judgment and is rare in practice.

B.4 Court procedures in New York

Filing an eviction case. Nonpayment cases in New York City's housing court begin with a "Demand" by the landlord to the tenant for unpaid rent. The demand can be made verbally or in writing. If the demand is in writing, the landlord must wait three days before filing a case. After making a rent demand, the landlord must then file a notice of petition and purchase an index number with the Court to initiate a case. The cost of purchasing an index number was $45 in New York City over our study period.

After the tenant has been served the notice by the landlord, the Court Clerk mails the tenant a postcard informing the tenant that they need to go to court to "Answer" the petition. The tenant has five days to answer, which involves coming to the clerk's window at the courthouse, at any time during business hours, and submitting an answer form. The answer form provides the opportunity for the tenant to list possible defenses for nonpayment. Acceptable defenses include disputes about the rent claimed, improper service of the petition, or incorrect parties listed on the petition. Examples of rent defenses include: the tenant was not properly notified of the rent demand, the rent or a portion of the rent has already been paid, the requested rent is an overcharge because the tenant paid for necessary repairs or services, the tenant tried to pay

[53] If an occupant not listed on the order is on the premises, the deputy has to stop the eviction process and the landlord may have to file a new complaint seeking to evict the previously unnamed occupants. To avoid this, plaintiffs will commonly include "any and all unknown occupants" when filing an eviction case.

but the landlord refused to accept it, and the requested amount of back rent is not the legal rent on the lease.

Randomized case assignment. After the tenant answers the petition at the courthouse, the case is assigned at random to a Resolution Part (the term given to courtrooms) by the Housing Court Information System (HCIS) computers. The assigned date is typically a week after the Answer is logged with the court. Judges rotate through courtrooms for year-long terms on a predetermined rotation system. Cases are assigned to courtrooms rather than judges, such that if the judge rotates out of a courtroom during an active case, the case will remain in the assigned courtroom.

Court proceedings. If the tenant and landlord both appear on the initial court date, they typically first negotiate an initial settlement agreement known as a Stipulation of Settlement and log it in court records. The tenant may request to reschedule (adjourn) the case in order to procure legal counsel or buy themselves additional time to come up with money to pay off rental arrears. In the case of a Stipulation agreement, the landlord or, more typically, the landlord's attorney and the tenant negotiate the terms of a possible settlement, haggling over "time"—length of repayment period—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. Tenants in New York City rarely agree to vacate the unit in a Stipulation agreement.[54] Negotiations between the tenant and the attorney may occur 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) or in the hall outside the courtroom.

Once an initial Stipulation agreement has been reached, the landlord's attorney and the tenant appear before the judge presiding over that courtroom to present the settlement. The judge reviews the terms with the tenant and landlord (or landlord's attorney), which may include discussing the tenant's ability to meet the terms of the agreement or raising questions about the agreement, 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. Trials are extremely rare, making up less than 1 percent of cases.

If the tenant fails to appear in court but the landlord appears, the judge may choose to issue a default judgment against the tenant, which, along with a warrant, can be used to evict the tenant.

A Stipulation 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

[54] Summers (2020) finds that just 1 percent of New York City cases result in a Stipulation where the tenant has voluntarily agreed to move out.

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.

After a judge grants an order for eviction. With a possessory judgment and warrant, the landlord can hire a Marshal to execute a lockout.[55] The Marshal must serve the tenant a Notice of Eviction before conducting a lockout. The tenant has three days after receiving a Notice of Eviction before a Marshal can return to perform a lockout. However, the tenant still has recourse to avoid an eviction by filing a post-judgment Order to Show Cause (OSC), a request to halt an eviction and reopen the case. If the tenant files an OSC, the judge can choose to grant the request or deny the request to reopen the case.

The cost of hiring a Marshal to conduct a lockout is $140 plus 5% of any money judgment collected. In New York City, a lockout can take two forms: an eviction or a legal possession ("possession"). Both involve removing the tenant and returning the property to the landlord. An eviction involves the removal of a tenant's belongings (into private storage), and in the case of a possession, the tenant's belongings remains under the care and control of the landlord until the tenant can arrange to retrieve them.

[55] In New York, County Sheriffs or City Marshals can conduct an eviction. However, in landlord-tenant cases nearly all landlords use City Marshals.

C Appendix: Detailed data descriptions

C.1 Court records: data cleaning and construction of variables

C.1.1 Cook County

Identifying cases involving businesses and unnamed occupants. Eviction court records include evictions involving tenants that are businesses as well as cases where the names of the occupants are not known. Similar to Desmond et al. (2018b), these cases are identified using regular expressions to select records in which the defendant's name includes strings such as "LLC", "LTD", "CORP", "INC", "ASSOCIATES", "DBA", and other phrases associated with being a business. Similarly, we exclude cases where the only listed name is a variation of "ALL UNKNOWN OCCUPANTS" or the last name is "DOE".

Deriving the assigned judge. When a case is filed, it is randomly assigned a court room and time, which determines the judge who will preside over the case. We assign judges to cases based on the room, time, and date assigned at the time of case filing. Note that this allows us to assign judges to cases even if the cases are withdrawn before the first hearing, which means our analysis is robust to strategic behavior, e.g., if experienced plaintiffs were to withdraw after observing the judge assignment.

As a robustness check, we construct an alternative measure of judge stringency using the first court record involving a judge after the defendant has been served and excluding procedural events handled by the presiding judge. We find that this alternative construction assigns the same judge in more than 90 percent of cases.

Standardizing addresses. We first checked addresses for common misspellings, typos, and formatting inconsistencies such as leading, lagging, or extra white space. We then processed addresses using the SmartyStreet address standardization API to return formatted and standardized addresses.

C.1.2 New York

Identifying cases involving businesses and unnamed occupants. New York housing court records include a field identifying whether the party involved is a business or person. We remove all cases where the respondent (i.e. the tenant) is coded as a business. We also exclude cases where the the last name of the tenant is "DOE".

Standardizing addresses. We standardized addresses using HUD's Geocoding Service Center, which uses Pitney and Bowes' Core-1 Plus address-standardizing software.

C.2 Court records: sample restrictions

C.2.1 Cook County

Table C.1 reports the number of cases and judges in the full sample, and how these numbers change as additional restrictions are imposed on the data. The first row reports the sample size for the full data set. Rows two through four impose that the case is not against a business, is not for a condo, is not missing names in the court docket, and has an ad damnum amount of less than \$100,000. The fifth row imposes that a single judge can be clearly identified from the randomly assigned room and time. The sixth row imposes that the assigned judge saw at least 10 cases that year, while the seventh row imposes that the district had at least two active active judges seeing cases during the week of the initial hearing. The seventh row corresponds to our "analysis sample" prior to linking to outcomes. The final row further restricts to cases that were successfully linked to Experian records prior to the filing date of the eviction case.

Table C.1: Sample Construction for Cook County

Sample	Cases	Judges
Full	583,874	313
No businesses or condos	555,167	313
Non-missing names	546,698	311
Damages $< \$100,000$	546,193	311
Non-missing judge	545,447	310
Judge sees more than 100 cases per year	483,200	149
Valid Courtrooms	413,976	127

C.2.2 New York

Table C.2 reports the number of cases and how these numbers change as additional restrictions are imposed on the data. The first row reports the sample size for the full data set of non-payment filings in NYC. We restrict the sample to filings that become "calendared cases," which are cases that are heard by a judge and have the potential to render an eviction order. Cases that are never calendared are never assigned to a courtroom and hence don't generate further court actions. We restrict the sample to cases involving residential property, excluding businesses. Next, we drop cases arising in courts with only one judge (Staten Island) or in one of two specialized courts in Red Hook and Harlem. We require that the courtroom hears at least 500 cases in a year. Cases involving condos or co-ops are not randomly assigned to courtrooms. Within each court, these cases are assigned to a single courtroom. In all boroughs except the Bronx, this designated courtroom handles some non-condo/co-op cases; the average share of condo/co-op cases in these courts is 27 percent. We use annual administrative data from the New York City Department of Finance to identify buildings with condos or co-ops. Finally, we drop cases where the courtroom (and hence the judge) are not randomly assigned. These include cases involving the public housing authority, cases assigned based on zip code through several policy initiatives, and cases involving drugs or members/family members of the active military. The final row reports the number of possible observations that could be linked to either the benefits data or Experian data. After linkages, we further restrict the linked samples based on age and availability of identifiers to link to other outcomes.

Table C.2: Sample Construction for New York

Restriction	Cases
None	1,826,672
Only calendared cases	987,320
No Businesses	958,814
No State Island / Red Hook/ Harlem	903,214
Minimum 500 cases per year	899,622
No condos and coops	830,944
No NYCHA (Public Housing)	638,286
No zip code assignment / other policy	579,084
No drug and military cases	577,851

C.3 Court records: construction of case outcomes

In both jurisdictions, we define an eviction as a case ending in the judge issuing an eviction order. Below, we describe how we use court records to construct this classification.

C.3.1 Cook County

The court dockets include a detailed history of events and rulings associated with each case. Some events are administrative, while others involve court hearings. For each case, we take the history of events and establish whether the case ended in eviction. We define cases as ending in eviction if the case has a judge rule for any of the docket entries listed below and there is no dismissal recorded afterwards:

- "ORDER FOR POSSESSION"
- "ORDER OF POSSESSION"
- "JUDGMENT FOR PLAINTIFF"
- "JUDGMENT FOR POSSESSION ONLY(NO MONEY) - ALLOWED"

- “SHERIFF EVICTION WORKSHEET FILED”
- “EX PARTE JUDGMENT-PLAINTIFF”
- “VERDICT FOR PLAINTIFF BY PROVE UP”
- “JUDGMENT ON PRIOR VERDICT - FAVOR OF PLAINTIFF -”
- “VERDICT FOR PLAINTIFF”

We code the following entries as dismissals:

- “VOLUNTARY DISMISSAL W/LEAVE TO REFILE-ALLOWED”
- “DISMISS ENTIRE CAUSE - PLAINTIFF -”
- “DISMISS BY STIPULATION OR AGREEMENT”
- “DISMISSED FOR WANT OF PROSECUTION”
- “VOLUNTARILY DISMISSED BY PLAINTIFF”
- “CASE DISMISSED WITH PREJUDICE - ALLOWED”
- “CASE DISMISSED WITHOUT PREJUDICE -ALLOWED”

Finally, we code the following entries as verdicts for the defendant:

- “VERDICT FOR DEFENDANT”,
- “JUDGMENT FOR DEFENDANT”,
- “JUDGMENT ON PRIOR VERDICT - FAVOR OF DEFENDANT -”

Over 99 percent of cases that we classify as an eviction have an “ORDER FOR POSSESSION” ruling, and our results are robust to using alternate definitions of eviction. The procedure described above leaves 3 percent of cases unclassified, because there is neither a dismissal nor an eviction order recorded. We therefore classify these cases as not evicted.

In Section C.4, we randomly sample court microfilms stratified by dismissal type. Specifically, we draw at random from cases where the tenant was successfully served and where the case was determined to end in one of the following dismissal categories:

- *Dismissed by stipulation or agreement* (“DISMISS BY STIPULATION OR AGREEMENT”);
- *Dismissed with prejudice* (“CASE DISMISSED WITH PREJUDICE - ALLOWED”);
- *Dismissed without prejudice* (“CASE DISMISSED WITHOUT PREJUDICE -ALLOWED”);
- *Dismissed by plaintiff* (“VOLUNTARY DISMISSAL W/LEAVE TO REFILE-ALLOWED”, “DISMISS ENTIRE CAUSE - PLAINTIFF -”, “VOLUNTARILY DISMISSED BY PLAINTIFF”);
- *Dismissed for want of prosecution* (“DISMISSED FOR WANT OF PROSECUTION”).

C.3.2 New York

Court records in New York include a detailed history of hearings, motions, judgments, and warrants. An eviction order is coded as cases that end with a recorded warrant for eviction and a possessory judgment ("Judgment with Possession"), where there are no records of successful appeal or satisfaction of the judgment afterwards.

Cases that do not produce an eviction order end with a discontinuance, a dismissal, or a settlement agreement. Discontinuances and dismissals most typically appear as cases ending with the outcomes:

- "Discontinued"
- "Withdrawn"
- "Dismissed No Appearance Plaintiff"
- "Dismissed No Appearance Either Side"
- "Dismissed via Conference"

Settlement agreements are common in New York non-payment cases. When cases end with a settlement, the settlement typically appears as:

- "Settled per Stipulation on record"
- "Settlement per Stipulation"
- "Settled Stip in File"

C.4 Court archive microfilms for Cook County

The electronic court docket for Cook County, from which we collect our court data, does not contain all information that is included in underlying court archival records, which are stored on paper and on microfilm. For example, the Cook County dockets do not record whether there was a formal agreement between the landlord and the tenant associated with a dismissal. To provide a richer description of dismissed cases in Cook County and to be able to make a comparison to court outcomes for not-evicted cases in New York (where the presence of an agreement *is* recorded in the court data), we hand-collected and coded court microfilm records for a random sample of court cases ending in dismissal. This sample contains cases from the first district, which is Cook County's largest—representing about 75 percent of case volume—and includes the City of Chicago.

For cases that do not end in an eviction order, the court docket records five main dismissal categories: *dismissed by stipulation or agreement*, *dismissed with prejudice*, *dismissed without prejudice*, *dismissed by plaintiff*, and *dismissed for want of prosecution*.[56] For each type of

[56] To determine the type of dismissal for each case, we group case outcomes according to the rules described in Section C.3.

dismissal, we collected the microfilms for 100 randomly-selected cases, except for *dismissed for want of prosecution*, where we only collected 50 cases. We also collected 45 microfilms for cases that didn't end in an eviction order but couldn't be classified into a dismissal category, which we labeled *other*. Some of the randomly selected files did not have associated microfilms. The composition of our sample is as follows, where "missing" means there were no microfilm records available:

- *Dismissed by stipulation or agreement*: 100 cases, 4 missing;
- *Dismissed with prejudice*: 100 cases, 7 missing;
- *Dismissed by plaintiff*: 100 cases, 26 missing;
- *Dismissed without prejudice*: 100 cases, 14 missing;
- *Dismissed for want of prosecution*: 50 cases, 18 missing;
- *Other*: 45 cases, 20 missing.

For each case, all documents relevant to the terms of the dismissal were photographed at the courthouse and then manually reviewed by two researchers, with a third reviewer added if the two initial researchers' classifications did not agree.

The fraction of not-evicted cases that involve a formal agreement. While the *dismissed by stipulation or agreement* category ostensibly records all cases that involve an agreement, the archival records show that there can be agreements for several of the other dismissal categories. To better compare between Cook County and New York, we compute the fraction of cases where the microfilms show some record of an agreement regarding either payment or moving out, for each of the six categories listed above. In cases where microfilms are missing, we assume they are missing at random. We then calculate a weighted average of these category-specific fractions with weights determined by the frequency of each dismissal category in the full sample of court records. This yields an estimate of 39 percent of not-evicted cases involving an agreement. This estimate likely understates the fraction of cases that have an agreement, since it is possible that documents related to an agreement were not included in the microfilms or were difficult to interpret and therefore coded as not showing evidence of an agreement.

The fraction of cases that are discontinued. In the New York data, cases are classified as discontinued when the landlord withdraws the case or fails to show up for court (see Section C.3). To estimate the fraction of cases in Cook County that ends in a similar way, we determine the fraction of cases in the *dismissed for want of prosecution* (the landlord or their lawyer fails to show up to court) and *dismissed by plaintiff* (the landlord asks the judge to dismiss the case) categories where the microfilms did not contain evidence of an agreement.[57] We also classify

[57]Cases that indicate the landlord failed to show up yet have evidence of an agreement are likely to have been misclassified. Cases that are dismissed at the request of the landlord yet have an agreement are conceptually more similar to cases that end with an agreement.

as discontinued cases where the tenant is never successfully served. Combined, this yields an estimate of 45 percent. This estimate likely overstates the number of discontinued cases, since our estimate of the fraction of cases with agreements is likely an underestimate, as explained above.

The fraction of cases with a verdict for the defendant.

In New York cases are coded as "dismissed" when the case does not end in an eviction order, and the listed outcome is "Dismissed". In Cook County, there are two types of cases that have outcomes that are conceptually similar. First, cases can be dismissed with prejudice and have no formal agreement.[58] Second, cases can end in a verdict for the defendant. In Cook County, 5 percent of cases fall into these two groups.

C.5 Address data sources

As discussed in Section 3, to measure mobility in New York we combine two sources of address histories: consumer reference data from Infutor Data Solutions and administrative benefits records. We examine whether the availability of the New York moves data sources is correlated with eviction status in Table C.3, which reports the results of regressing indicators of data source availability on an indicator for receiving an eviction order. Receiving an eviction order has almost no impact on the availability of any residential moves data ("Any Move Data"), which suggests that our primary mobility results from New York are unlikely to be affected by differential data availability. Households receiving an eviction order are slightly more likely to only have address data from benefits records but are slightly less likely to only have Infutor data. In both cases, the relationship is small.

Next, we evaluate the sensitivity of the New York residential mobility results to the sources of moves data. Table C.4 reports our IV estimates of the effects of eviction orders on an indicator for not being at the eviction address in years 1–2 after the year of filing separately by move data source. Columns (1)-(3) evaluate how sensitive the move results are to defining moves from the filing address as: (1) having a new address in either benefits or Infutor data (our baseline definition), (2) having a new address in the benefits data (but not Infutor), (3) having a new address in Infutor but not the benefits data. Column (4) examines moves for cases where we *only* have benefits data. Column (5) reports the estimate for cases where we *only* have Infutor data. Tenants in housing court are much more likely to appear in the benefits data than in the Infutor data during the study period. This is unsurprising given that our New York sample is restricted to tenants who have some history of benefits receipt. Thus, our "Infutor Only" estimates rely on a much smaller sample and are less precise. Still, both sources point to increases in residential mobility from eviction in the short run.

[58] A dismissal with prejudice bars the landlord from bringing another eviction case with the same allegations against the tenant. A dismissal without prejudice does not, is often accompanied by an agreement, and is therefore more similar to the stipulation agreement outcome in New York.

Table C.3: Move Data Source—New York

	Any Move Data (1)	Both Sources (2)	Infutor Data Only (3)	Benefits Data Only (4)
Eviction Order	0.004*	0.001	-0.006***	0.029***
	(0.002)	(0.002)	(0.002)	(0.003)
Observations	150,698	150,698	150,698	150,698

Notes: This table reports the results of separately regressing measures of move availability on an indicator for receiving an eviction order. "Any Move Data" takes the value 1 if we have move data from Infutor or benefits data for a given tenant and 0 otherwise. "Both Sources" takes the value 1 if we have both Infutor and benefits address information and 0 otherwise. "Infutor Data Only" and "Benefits Data Only" take the value 1 if we only have data on mobility for the listed source and 0 otherwise.

Table C.4: New York Residential Mobility Results by Move Data Source

	Move in Either (1)	Move in Benefits (2)	Move in Infutor (3)	Only Benefits Avail (4)	Only Infutor Avail (5)
Eviction Order	0.101	0.155*	0.132*	0.121*	0.261
	(0.058)	(0.064)	(0.066)	(0.059)	(0.245)
Observations	89027	89795	88362	93078	17820

Notes: This table reports the IV estimates in Year 1-2 after filing by different codings of address changes. "Move in Either" takes the value 1 if we the individual is observed at a new address in either data from Infutor or the benefits data, and 0 otherwise. "Move in Benefits" takes the value 1 if the individual is observed at a new address in the benefits data but not Infutor, and 0 otherwise. "Move in Infutor" takes the value 1 if the individual is observed at a new address in Infutor but not the benefits data, and 0 otherwise. "Infutor Data Only" and "Benefits Data Only" take the value 1 if we only have data on mobility for the listed source and 0 otherwise.

C.6 CCS codes for mental health

Table C.5: CCS Codes Related to Mental Health

CCS Code	Single-Level Diagnosis Category Name
650	Adjustment disorders
651	Anxiety disorders
652	Attention-deficit, conduct, and disruptive behavior disorders
653	Delirium, dementia, and amnestic and other cognitive disorders
654	Developmental disorders
655	Disorders usually diagnosed in infancy, childhood, or adolescence
656	Impulse control disorders, NEC
657	Mood disorders
658	Personality disorders
659	Schizophrenia and other psychotic disorders
660	Alcohol-related disorders
661	Substance-related disorders
663	Screening and history of mental health and substance abuse codes
670	Miscellaneous mental health disorders

Notes: This table lists the CCS Codes used to determine whether a hospital visit is coded as related to mental health in the data on hospital visits that we describe in Section 3, along with the category name. See https://www.hcup-us.ahrq.gov/toolssoftware/ccs/CCSCategoryNames_FullLabels.pdf for a complete list of CCS codes and their diagnosis category labels.

C.7 Data linkage procedure for New York

This appendix describes our procedure for linking 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. These are 2001–2016 for Cash Assistance, 2004–2016 for Food Stamps, and 2006–2016 for Medicaid. For each address record, we have a client ID and case number. 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 and Sunter (1969), which can also be thought of as a naïve 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: $P(M_i = M_j|\text{Match=True})$. Since we don't which matches are in fact "true", we must assume a value for M_a. We set $M_a = 0.95$, which is a common choice for names in the literature. 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 measure is akin to how common or rare a name is. We estimate these quantities in the benefits data directly, which contain over 9 million unique persons. We set a lower bound of 0.0000002.

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

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:

$$\begin{aligned}
\text{Geo Score}_{ij} = {} & log\left(\frac{0.975}{U_{BBL}}\right)\mathbf{1}\left(\text{BBL Courts=BBL Benefits}_{ij}\right) + \ldots \\
& + log\left(\frac{0.95}{0.05}\right)\mathbf{1}\left(\text{Block Court=Block Benefits}_{ij}\right) + \ldots \\
& + log\left(\frac{1-0.975}{1-U_{BBL}}\right)\left(1-\mathbf{1}\left(\text{BBL Courts=BBL Benefits}_{ij}\right)\right) \\
& + log\left(\frac{1-0.95}{1-0.05}\right)\left(1-\mathbf{1}\left(\text{Block Court=Block Benefits}_{ij}\right)\right)
\end{aligned} \tag{C.2}$$

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 to 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 to best available record

6. From best available records, discard the pair if the score is below the minimum matching threshold of 15.[59]

C.8 Baseline controls

We construct a number of baseline controls from the court records matched to Census or benefits data. The baseline controls in the restricted Census data are measured in the Numident file—age, gender, and race—and, if missing from Numident, we use information from the 2010 Decennial Census. We use the same set of controls in the New York sample, which are measured in the administrative benefits data. In the credit sample, we use a very similar but not identical set of controls because of data use restrictions. The main controls for the credit sample are: an indicator for female, a cubic in age at case filing, an indicator for the case being joint action (in Cook County only), ad damnum amount, an indicator for the plaintiff having no legal representation, Census tract median rent, Census tract percentage below the poverty line, and indicators for missing covariates.

C.9 Payday loans data

The payday loans data, observed for Cook County only, comes from Clarity, a credit reporting agency that maintains the largest subprime database of over 62 million unique consumers and is owned by Experian. Clarity's database includes only loans originating from lenders that use Clarity's underwriting services. Payday lenders are not required to report loans to the credit bureau under the Fair Credit Reporting Act. It is difficult to validate Clarity's data, since a representative national database of payday loans is not available for comparison, but Miller and Soo (2020b) provide evidence in support of the comprehensiveness and quality of the data.

Clarity collects data from alternative finance providers, including Online Installment, Online Small Dollar (Single Pay), Storefront Installment, Storefront Small Dollar (Single Pay), Title, Marketplace, Auto, Rent-to-Own, Telecom, Subprime Credit Card, and Collections Records.

In our analysis we study payday inquiries, which are the borrower's inquiry into getting a loan. Note that there may be one or many inquiries before an account is opened, or there may be an account opening with no associated inquiries, which may occur on roll-over loans or in cases in which a borrower is well known to the lender. From the inquiries file, we keep only inquiries

[59] This threshold was selected based on extensive clerical review of match quality

for new credit, which excludes a small number of inquiries due to collections or leases. In Table C.6 below, we show summary statistics of the payday loans data, for the Cook County linked eviction court sample, and for the 10 percent random sample of Cook County credit files, which is also linked to the payday loans data. Individuals must have a main Experian credit file in order to be observed in the Clarity database.

The summary statistics show that the vast majority of inquiries in the Clarity data are online rather than traditional storefront payday lenders. We note that online loans are likely over-represented in the Clarity database, since these lenders are more likely to require an external credit check prior to granting a loan. The average loan amount is lower for the eviction court sample, about $1500, relative to $2200 for the random sample. Most payday loans are for a short duration (less than one month), and the majority are short installment loans, in which the borrow makes multiple payments.

Table C.6: Payday Loans Data: Summary Statistics

	Random Sample	Eviction court sample
	(1)	(2)
I. *Inquiries*		
Online	0.85	0.89
	(0.36)	(0.31)
Storefront	0.04	0.03
	(0.21)	(0.18)
Other Type of Inquiry	0.11	0.07
	(0.31)	(0.26)
II. *Accounts*		
Amount	2,178.72	1,551.43
	(3432.28)	(2631.63)
Single Payment, Duration: $\leq$ 1 Month	0.40	0.46
	(0.49)	(0.50)
Single Payment, Duration: $>$ 1 Month	0.03	0.03
	(0.16)	(0.18)
Installment, Duration: $\leq$ 1 Month	0.54	0.48
	(0.50)	(0.50)
Installment, Duration: $>$ 1 Month	0.01	0.01
	(0.08)	(0.09)
Other Type of Account	0.02	0.02
	(0.14)	(0.14)

Notes: The table above provides sample means and standard deviations of key variables in the Clarity database, for both the linked Cook County eviction court sample and the linked Cook County random sample. Panel I presents the sample of payday loan inquiries. Panel II provides information on accounts opened, including the nominal amount of the loan, the duration of the loan, and whether the loan is to be repaid in one payment or in installments. The loan amount is the original amount of the loan and excludes fees and interest payments. Payday inquiries are available between September 2011 and November 2018, and account openings are available between January 2010 and November 2018.

D Appendix: Linked sample details

D.1 Sample linkage

Cook County. Earnings, employment, and homelessness outcomes for Cook County are linked to eviction records by the U.S. Census Bureau. Table D.1 reports which characteristics are associated with successfully linking the eviction case record to a PIK (an individual unique identifier used by the Census Bureau). The table includes covariates for: race predicted from Census tract and last name, gender predicted from first name, if the defendant had a lawyer, and if the case ended in an eviction order. We find that cases ending in an eviction and cases where the defendant does not have an attorney are less likely to be matched. Similarly, we find that cases with white or Black tenants are more likely to be matched, while cases with Hispanic tenants are less likely to be matched. Table D.2 provides similar results for the sample linked to credit bureau records. The second column of Table D.2 repeats the regression but replaces the indicator for eviction with judge stringency and finds that the stringency of the assigned judge does not predict a case being linked.

Table D.1: Linked to Census records—Cook County

	Has PIK (1)
Intercept	0.518***
	(0.006)
Evicted	-0.051***
	(0.001)
No attorney (defendant)	-0.040***
	(0.004)
Joint action case	0.018***
	(0.002)
Female (predicted)	0.058***
	(0.001)
White (predicted)	0.115***
	(0.005)
Black (predicted)	0.093***
	(0.005)
Hisp (predicted)	-0.042***
	(0.005)
Number of observations	548,000

Notes: This table shows results from the regression of an indicator for if the individual was linked to Census records regressed on covariates. Race is imputed using a Bayesian procedure using last names and racial composition of Census tracts as proposed in Imai and Khanna (2016). Gender is predicted using first names as described in Blevins and Mullen (2015). The regression also includes controls for court and year. Standard errors are in parentheses. * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. Cook County observation counts are rounded in accordance with Census Bureau disclosure requirements. Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

New York. Earnings, employment, residential mobility, homelessness, and hospital visit outcomes for New York are linked to public assistance records covering Medicaid, Food Stamps, and cash assistance. We limit our sample to records that link to a benefits case prior to the housing court filing. Table D.3 reports which characteristics are associated with successfully linking the eviction case record to a benefits record. The table includes covariates for: race predicted from Census tract and last name, gender predicted from first name, an indicator for whether the defendant had a lawyer, and an indicator for whether the case ended in an eviction order. In column 1, we regress an indicator for linking to the benefits data on individual, case, and neighborhood characteristics, as well as receiving an eviction order. In column 2, we repeat the exercise but replace eviction order with the stringency instrument. Importantly, we find that stringency is uncorrelated with matching to the benefits data.

Table D.2: Linked to Experian records—Cook County

Evicted	-0.004***	
	(0.001)	
Judge stringency		0.004
		(0.008)
Female	0.017***	0.017***
	(0.001)	(0.001)
Age at case	-0.000*	-0.000*
	(0.000)	(0.000)
Joint action	0.006***	0.006***
	(0.001)	(0.001)
Tenant without attorney	-0.006***	-0.006***
	(0.001)	(0.001)
Ad damnum (1000s)	-0.000***	-0.000***
	(0.000)	(0.000)
Median Rent	0.000***	0.000***
	(0.000)	(0.000)
Percent below poverty line	-0.011***	-0.011***
	(0.002)	(0.002)
Filing Year 2006	0.003**	0.003**
	(0.001)	(0.001)
Filing Year 2007	0.001	0.001
	(0.001)	(0.001)
Filing Year 2008	-0.008***	-0.008***
	(0.001)	(0.001)
Filing Year 2009	0.002	0.002
	(0.001)	(0.001)
Filing Year 2010	0.007***	0.007***
	(0.001)	(0.001)
Filing Year 2011	0.009***	0.009***
	(0.001)	(0.001)
Filing Year 2012	0.012***	0.013***
	(0.001)	(0.001)
Filing Year 2013	0.013***	0.013***
	(0.001)	(0.001)
Filing Year 2014	0.014***	0.014***
	(0.001)	(0.001)
Filing Year 2015	0.017***	0.017***
	(0.001)	(0.001)
Filing Year 2016	0.017***	0.017***
	(0.001)	(0.001)
Number of observations	444,565	444,565
R^2	0.8945	0.8945
Mean of dep. var.	0.5821	0.5821

Notes: This table shows results from the regression of an indicator for if the individual was linked to Experian records regressed on covariates and indicators for missing covariates (not reported). Standard errors are in parentheses. * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$.

Table D.3: Linked to Benefits—New York

	Matched to Benefits	
	(1)	(2)
Evicted	0.057***	
	(0.002)	
Stringency		-0.011
		(0.017)
Female (Predicted)	0.043***	0.017***
	(0.002)	(0.002)
Black (Predicted)	0.148***	0.163***
	(0.007)	(0.007)
Hispanic (Predicted)	0.095***	0.114***
	(0.004)	(0.004)
Amount Rent (1000's)	-0.000	0.000
	(0.000)	(0.000)
Tenant Without Attorney	0.043***	0.022***
	(0.007)	(0.007)
Tract Poverty Rate	0.200***	0.149***
	(0.008)	(0.008)
Tract Median Rent	-0.001	0.076***
	(0.005)	(0.004)
Observations	577,823	577,823

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table shows results in column 1 from the regression of an indicator for if the individual was linked to administrative benefits records in New York regressed on covariates and eviction order, and column 2 shows the results from a regression on covariates and the stringency instrument. Race is imputed using a Bayesian procedure using last names and racial composition of Census tracts as proposed in Imai and Khanna (2016). Gender is predicted using first names as described in Blevins and Mullen (2015). Regression also includes controls for court and time of filing. Standard errors are clustered at the courtroom/judge-year level . * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$.

Table D.4 reports the characteristics that predict linking to the Experian data for the New York City sample. Cases with a female tenant are more likely to match to Experian. Cases ending with an eviction order are slightly less likely to be successfully linked to Experian. Judge stringency is uncorrelated with the likelihood of matching to the Experian data.

Table D.4: Linked to Experian records—New York City

	Matched to Experian	
	(1)	(2)
Evicted	-0.025***	
	(0.002)	
Stringency		-0.006
		(0.029)
Female (Predicted)	0.198***	0.199***
	(0.003)	(0.003)
Amount Rent (1000's)	0.000	0.000
	(0.000)	(0.000)
Tenant Without Attorney	0.032***	0.029**
	(0.009)	(0.009)
Tract Poverty Rate	-0.077***	-0.078***
	(0.014)	(0.014)
Tract Median Rent	-0.055***	-0.056***
	(0.006)	(0.006)
Observations	278875	278875

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Column 1 shows results from the regressing an indicator for whether the individual was linked to Experian records on eviction order and other covariates. Column 2 repeats the specification, but replaces eviction order with the judge stringency instrument. All specification include court-by-time fixed effects. Standard errors are clustered at the judge/courtroom-year level. * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$.

The New York sample relies on outcomes data that are mostly limited to the state of New York. If judge stringency impacts the likelihood of moving out of New York state, then our IV estimates from New York will biased. In Table D.5, we regress outcome indicators for all post-filing values of the outcome being zero (a proxy for exiting the state) on our instrument. Judge stringency is unrelated to the likelihood of appearing in any of the outcome data for each data source.

Table D.5: Matching and Attrition—New York

	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)
Stringency	-0.013	0.006	0.015	0.028	0.006	-0.035	0.018
	(0.018)	(0.014)	(0.035)	(0.031)	(0.035)	(0.037)	(0.024)
Observations	577851	181887	150698	150698	150698	150698	150698
Controls	No	No	No	No	No	No	No
Court-Time-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the relationship between our instrument and indicators of attrition or data matching success in the New York City sample. 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 hospital visit after filing on our instrument. In Column (7), we regress an indicator for having any earnings, hospital visit, 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.

D.2 Comparing the linked sample to the overall population

Table D.6 compares the benefits-linked housing court sample in New York City to the general population in housing court in New York City. The characteristics of the New York City housing court population come from a survey conducted in 2016 (NYC Office of Civil Justice, 2016). The linked benefits sample is broadly similar to the larger housing court population in terms of age and gender. The linked sample is somewhat more likely to be female. However, household sizes for the linked sample appear smaller.

Table D.6: Linked Sample Compared to Housing Court Population—New York City

	Sample Comparison	
Variable	Matched Sample (2007-2016)	Housing Court Survey (2016)
	(1)	(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

Notes: This table reports characteristics for linked benefits-housing court sample in Column (1) (without the 18–55 age restriction) and the characteristics of a sample of tenants in housing court in 2016 (based on survey results reported in NYC Office of Civil Justice, 2016, p. 41).

E Appendix: Additional evidence of selection into eviction court

One challenge faced by prior research on eviction is finding an appropriate comparison group for evicted tenants. Studies based on survey data typically compare tenants who report being evicted to observationally similar tenants from the neighborhood.[60] We show that there is substantial selection into eviction court: tenants who are at risk of eviction are, on average, significantly more disadvantaged than tenants from the same neighborhood. The implication is that without an appropriate comparison group, studies are likely to find effects of eviction that are largely due to the composition of tenants who arrive at court. We also show selection *within* eviction court, into the eviction case outcome: tenants who are evicted are more disadvantaged than tenants who are not evicted.

Figure E.1 illustrates the extent of selection into eviction court and selection into the eviction case outcome within court with earnings as the outcome. (Figures E.2, E.3, and E.4 show similar results for earnings, residential mobility, and homelessness, respectively.) We begin with our linked court sample and append a random sample of comparison tenants.[61] Focusing first on selection into court, the two leftmost bars show the level differences between evicted tenants' post-eviction earnings, averaged one to eight quarters after the case filing, and earnings among renters who live in the same neighborhoods. Evicted tenants earn over $2,000 less per quarter in Cook County and approximately $950 less per quarter in New York. Controlling for age and demographics does not affect this conclusion, as shown in the second bar from the left. The magnitude of the gap differs somewhat between the two cities, which likely reflects that the New York sample consists of benefits recipients only. Despite the different populations, the patterns in Figure E.1 are remarkably similar across cities.

If we were to interpret the first two bars in the top panel of Figure E.1 as estimates of the causal impact of eviction, the results would suggest large effects of eviction on future earnings. Once we change the comparison group to non-evicted tenants in court, however, shown in the third bar, almost half of the difference in outcomes disappears, implying a large degree of selection into court. Selection into court may be a concern when interpreting much of the prior research on the consequences of eviction, which does not have a comparison group of non-evicted tenants in eviction court. To avoid bias due to selection into court, we restrict our sample to tenants in eviction court.

[60] See, for example, Burgard et al. (2012), Desmond and Shollenberger (2015), Desmond and Kimbro (2015), Desmond et al. (2015), Desmond and Gershenson (2016b).

[61] In Cook County, this is a sample of renters in the ACS. In New York, it is a representative sample of adults receiving public assistance. In New York we do not observe whether individuals are renters or homeowners, but they are likely majority renters since they are receiving public assistance. We assign these comparison individuals a placebo filing month, randomly drawn from the sample period, and re-weight the regression sample so that the random sample matches the court sample in its distribution across ZIP codes.

Figure E.1 also reveals the extent of selection *within* eviction court into the eviction case outcome. The fourth bar of Figure E.1 adds a control for lagged earnings. The difference between evicted tenants and the comparison group diminishes by approximately half in New York and by more than half in Cook County. Hence, prior to eviction court, tenants who are evicted are more economically distressed than non-evicted tenants, and controlling for prior distress can meaningfully affect conclusions about the consequences of eviction.

The extent of selection into eviction court reveals the importance of having a comparison group for evicted tenants; comparing evicted tenants to tenants from the same neighborhood is likely to overstate the impact of an eviction. The extent of selection within court into the eviction case outcome makes clear that empirically estimating the casual effect of an eviction requires a plausible source of random assignment into treatment. To deal with the first source of selection, we use our linked eviction court records, which allow us to compare evicted tenants to non-evicted tenants in eviction court. To deal with the second source of selection, we employ an IV strategy, which we describe in detail in Section 5.

Table E.1 compares our IV sample to a random sample from Cook County, which has been re-weighted to match the distribution of neighborhoods in the eviction sample. (Note we cannot do this exercise with New York because we do not have a random sample in New York.) We assign a placebo filing date for the random sample so that we can compare credit report outcomes at equivalent horizons to the IV sample. We restrict both samples to age 18–55. The results show that tenants in eviction court have lower credit scores, less access to credit, higher debt levels, and higher levels of payday loan inquiries than the random sample.

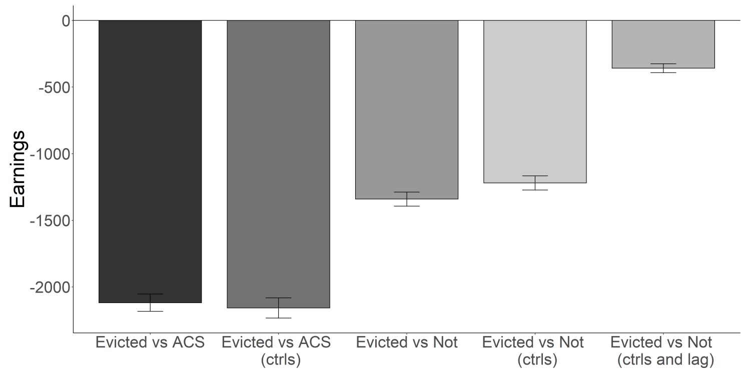
Table E.1: Summary Statistics: Comparing Credit Bureau Outcomes in Cook County to a Random Sample

	Evicted		Not Evicted		Random sample	
Person characteristics						
Age at filing	36.536	(9.300)	36.647	(9.178)	37.078	(10.240)
Female	0.653	(0.476)	0.649	(0.477)	0.489	(0.500)
Subsequent outcomes: Quarters 5-12						
Credit Score	521.886	(62.531)	538.623	(74.639)	610.059	(109.625)
No Open Revolving Account	0.650	(0.472)	0.586	(0.488)	0.370	(0.479)
Total bal. collections	3,997.179	(4,763.238)	3,296.041	(4,430.635)	1,510.220	(3,186.556)
Any Auto Loan or Lease	0.130	(0.332)	0.194	(0.391)	0.214	(0.406)
Any Payday Inquiry × 100	13.721	(34.407)	14.964	(35.672)	13.106	(33.747)

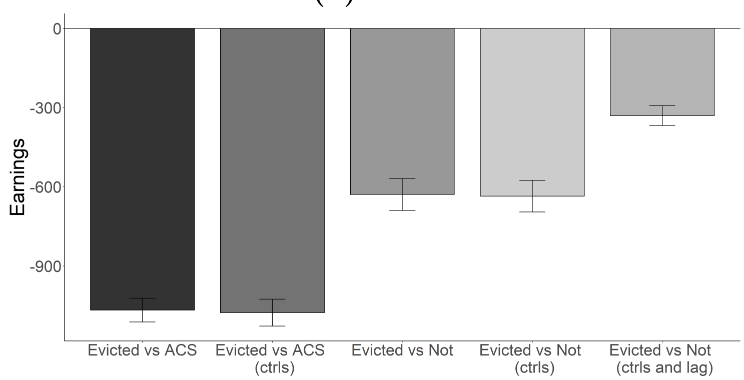
Notes: This table presents means and standard deviations (in parentheses) of key variables in our linked credit bureau sample used in the IV analysis for Cook County, compared to a random sample. (We do not have a random sample for New York.) For the random sample, we randomly assign a placebo eviction date to compare financial outcomes at quarters 5–12 relative to the eviction sample. Both samples are restricted to individuals age 18–55 at the time of filing. The random sample has been reweighted to match the distribution of individuals across zip codes in the eviction sample.

Figure E.1: Selection into Housing Court and Eviction - Earnings

(a) Cook County



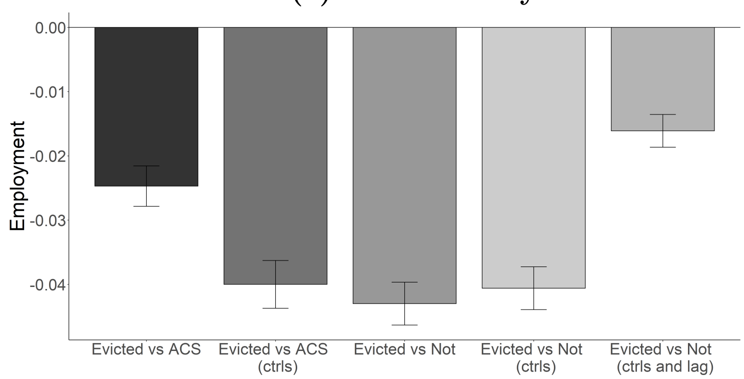
(b) New York



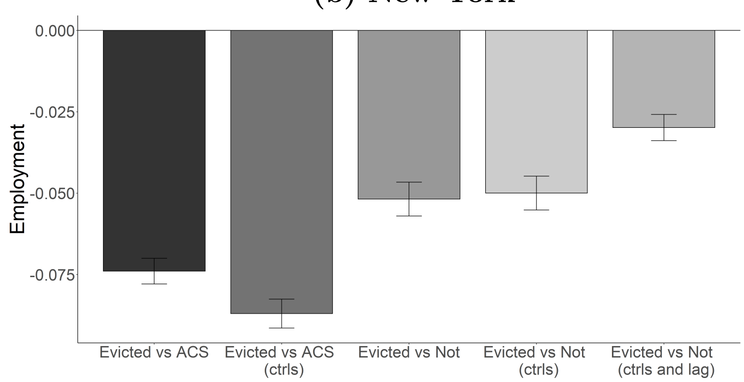
Notes: This table shows evidence of earnings-based selection into housing court and eviction for Cook County and New York. Each bar represents the coefficient from a regression of earnings on a dummy for being evicted in housing court where the reference group is the second group listed under the bar. Earnings is defined as average earnings in the two years following the court proceedings. For each regression, the sample is restricted to evicted individuals and the reference group. Specifically, "Evicted vs ACS" compares evicted individuals to an ACS weighted sample without any controls, "Evicted vs ACS (ctrls)" compares evicted individuals to an ACS weighted sample with controls, "Evicted vs Not" compares evicted individuals to non-evicted individuals in housing court without any controls, "Evicted vs Not (ctrls)" compares evicted individuals to non-evicted individuals in housing court with controls, and "Evicted vs Not (ctrls and lag)" compares evicted individuals to non-evicted individuals in housing court with controls and lagged controls. For all bars, the ACS sample is as described in Table 1. Regressions with controls use the same set of controls as in the OLS model of Table 4, and regressions with controls and lag use the same set of controls as in the LDV model of Table 4. Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

Figure E.2: Selection into Housing Court and Eviction—Employment

(a) Cook County



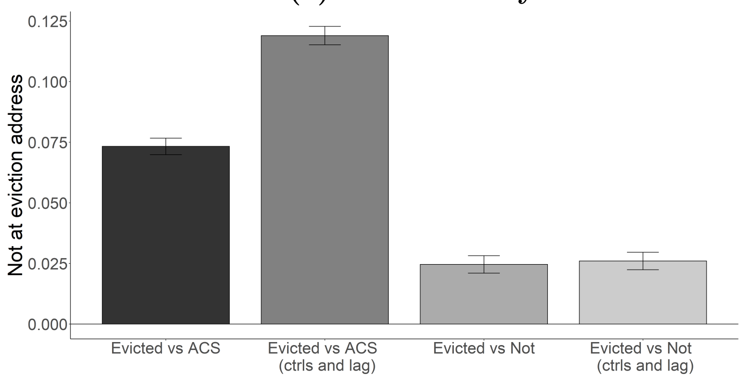
(b) New York



Notes: This table shows evidence of employment-based selection into housing court and eviction for Cook County and New York. Each bar represents the coefficient from a regression of employment on a dummy for being evicted in housing court where the reference group is the second group listed under the bar. Employment is defined as being observed as employed in the two years following the court proceedings. For each regression, the sample is restricted to evicted individuals and the reference group. See Figure E.1 for details about specifications and definitions. Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

Figure E.3: Selection into Housing Court and Eviction—Residential Mobility

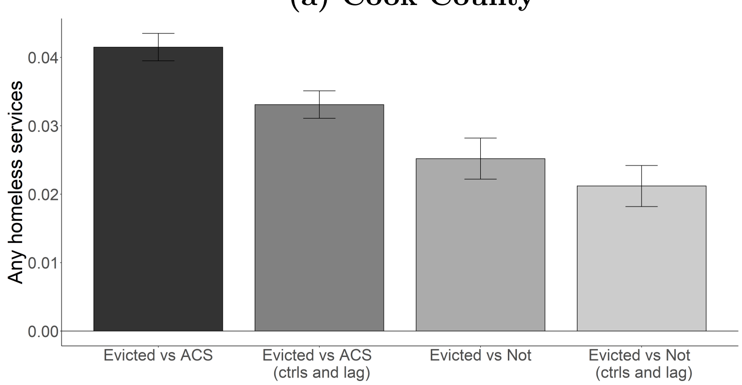
(a) Cook County



Notes: This table shows evidence of mobility-based selection into housing court and eviction for Cook County and New York. Each bar represents the coefficient from a regression of not being at eviction address on a dummy for being evicted in housing court where the reference group is the second group listed under the bar. Not at Eviction Address is defined as not being observed at the eviction address in the two calendar years including and following the court proceedings year. For each regression, the sample is restricted to evicted individuals and the reference group. See Figure E.1 for details about specifications and definitions. Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

Figure E.4: Selection into Housing Court and Eviction—Homelessness

(a) Cook County



Notes: This table shows evidence of homelessness-based selection into housing court and eviction for Cook County and New York. Each bar represents the coefficient from a regression of homelessness on a dummy for being evicted in housing court where the reference group is the second group listed under the bar. Homelessness is defined as being observed as homeless in the 1–2 years following court proceedings. For each regression, the sample is restricted to evicted individuals and the reference group. See Figure E.1 for details about specifications and definitions. Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

Table E.2: Summary Statistics: Credit Bureau Outcomes in NYC

	Evicted		Not Evicted	
	Mean	SD	Mean	SD
Female	0.686	(0.464)	0.726	(0.446)
Credit Score	557	(104)	570	(110)
No Revolving Account	0.386	(0.438)	0.332	(0.427)
Total Balance in Collections	1363	(3019)	1053	(2637)
Any Auto Loan	0.209	(0.407)	0.205	(0.404)

Notes: This table presents means and standard deviations (in parentheses) of key variables in our linked credit bureau sample used in the IV analysis for New York.

F Appendix: Additional estimates for main results

F.1 Number of observations

This subsection provides the number of observations for each regression result reported in the main text. Note that Cook County's observations are rounded following the procedure set by the U.S. Census Bureau.

Table F.1: Observation Counts for Table 4

	$\mathbb{E}[Y \mid E=0]$ (1)	OLS (2)	LDV (3)	RF (4)	IV (5)
Not at Eviction Address		259,230	259,230	218,230	218,230
Lockout		329,281		329,281	329,281
Any Homeless Services		229,342	229,342	210,842	210,842
Emergency Shelter		229,342	229,342	210,842	210,842
Future Eviction at Same Address		942,476		942,476	942,476

Notes: This table reports observation counts for each specification and outcome in Table 4. Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

Table F.2: Observation counts for Table 5

	All					Female			Black		
	$\mathbb{E}[Y\|E=0]$	OLS	LDV	RF	IV	$\mathbb{E}[Y\|E=0]$	LDV	IV	$\mathbb{E}[Y\|E=0]$	LDV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Labor:											
Earnings		398,429	398,401	376,401	376,401		261,907	247,907		259,626	246,626
Employment		399,429	399,401	377,401	377,401		261,907	248,907		260,626	247,626
Housing:											
Not at Eviction Address		244,027	244,027	203,027	203,027		158,132	135,132		147,935	130,435
Any Homeless Services		214,398	214,398	195,398	195,398		146,616	135,616		127,108	117,108
Emergency Shelter		214,398	214,398	195,398	195,398		146,616	135,616			
Future Eviction at Same Address		861,743		861,743	861,743			492,749			423,076

Notes: This table reports observation counts for each specification and outcome in Table 5. Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

Table F.3: Observation Counts for Table 6

	$\mathbb{E}[Y\|E=0]$ (1)	OLS (2)	LDV (3)	RF (4)	IV (5)
Credit Score	164,586	349,039	316,669	316,669	316,669
No Open Revolving Account	164,949	350,328	322,592	322,592	322,592
Total Bal. Collections	164,615	348,211	316,410	316,410	316,410
Any Auto Loan or Lease	164,949	348,982	317,690	317,690	317,690
Any Payday Inquiry (x100)	42,985	119,424	119,424	119,424	119,424

Notes: This table reports observation counts for each specification and outcome in Table 6. Note that the outcome "Any Payday Inquiry (x100)" is Cook County specific (hence its smaller sample size).

Table F.4: Observation Counts for Table 7

	$\mathbb{E}[Y\|E=0]$ (1)	OLS (2)	LDV (3)	RF (4)	IV (5)
Q1-Q4:					
Num. Hosp. Visits	82,323	179,026	179,026	179,026	179,026
Num. Emerg. Visits	82,323	179,026	179,026	179,026	179,026
Num. Mental Health Visits	50,893	181,887	181,842	181,842	181,842
Q5-Q12:					
Num. Hosp. Visits	82,323	179,026	179,026	179,026	179,026
Num. Emerg. Visits	82,323	179,026	179,026	179,026	179,026
Num. Mental Health Visits	50,893	145,820	145,795	145,795	145,795

Notes: This table reports observation counts for each specification and outcome in Table 7.

Table F.5: Observation Counts for Table 8

Labor Market	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	DiD$_{-8}$ (3)	DiD$_{-1}$ (4)
Earnings		376,401	481,401	481,401
Employment		377,401	490,401	490,401

Housing	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	DiD$_{-12}$ (3)	DiD$_{-5}$ (4)
Not at Eviction Address		203,027	273,590	273,590
Any Homeless Services		195,398		
Emergency Shelter		195,398		
Future Eviction at Same Address		861,743		

Notes: This table reports observation counts for each specification and outcome in Table 8. Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

F.2 Difference-in-differences estimates

This subsection reports additional difference-in-differences results. Table F.6 compares DiD estimates to IV estimates for credit bureau outcomes, while Table F.7 compares DiD estimates to IV estimates for hospitalization outcomes.

Table F.6: DiD: Impacts of Eviction on Financial Distress

	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	DiD_{-12} (3)	DiD_{-5} (4)
Credit Score	554.35	-12.33**	-0.88	0.33
	(66.58)	(6.15)	(1.14)	(0.63)
No Open Revolving Account	0.459	0.075**	0.035***	0.021***
	(0.324)	(0.032)	(0.005)	(0.004)
Total Bal. Collections	2,164.41	313.97	263.38***	148.17***
	(2,572.07)	(263.88)	(45.50)	(34.64)
Any Auto Loan or Lease	0.200	-0.045*	-0.007	-0.005
	(0.282)	(0.024)	(0.004)	(0.003)
Any Payday Inquiry (x100)	15.387	-3.153	-0.278*	-0.091
	(36.083)	(4.478)	(0.156)	(0.125)

Notes: This table reports equally-weighted averages of Cook County and New York non-evicted sample means ($\mathbb{E}[Y|E=0]$), as well as equally-weighted averages of location-specific two-stage least squares (IV) and difference-in-difference (DiD) estimates of the impact of eviction on outcomes related to the tenant's financial health. The only exceptions are estimates for the outcome "Any Payday Inquiry (x100)," which are Cook County specific. For the DiD estimates, the subscript denotes the reference pre-period quarter relative to quarter of court proceedings. The dependent variable is listed in each row. Outcomes are listed on the left of each row. Outcomes are defined as in Table 6: outcomes are defined for the second and third calendar years following the court proceedings. Controls for all model specifications are similar to those described in Table 4, except that gender and race controls are imputed based on name and address. Standard errors for regression model coefficients are included in parentheses and are clustered at the judge-year level. Median number of observations is 164,949 (max: 322,592, min: 42,985).

Table F.7: DiD: Impacts of Eviction on Hospital Utilization

	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	DiD_{-12} (3)	DiD_{-4} (4)
Q1-Q4:				
Num. Hosp. Visits	0.685	-0.088	0.059***	0.059***
	(1.548)	(0.151)	(0.006)	(0.006)
Num. Emerg. Visits	0.555	-0.073	0.055***	0.054***
	(1.262)	(0.122)	(0.006)	(0.006)
Num. Mental Health Visits	0.074	-0.331	0.015***	0.015***
	(0.829)	(0.223)	(0.001)	(0.001)

Notes: This table reports New York specific non-evicted sample means ($\mathbb{E}[Y|E=0]$), as well as New York specific two-stage least squares (IV) and difference-in-difference (DiD) estimates of the impact of eviction on outcomes related to the tenant's health. For the DiD estimates, the subscript denotes the reference pre-period quarter relative to the quarter of court proceedings. The dependent variable is listed in each row. Outcomes are listed on the left of each row. Outcomes are defined for the first and second year following court proceedings (years 1–2). IV controls are the same as those described in Table 4, and DiD controls are the same as the OLS controls described in Table 4 but additionally include individual-level fixed effects. Standard errors for regression model coefficients are included in parentheses and are clustered at the judge-year level. Median number of observations is 144,401 (max: 179,026, min: 50,893).

F.3 Heterogeneity on observables

This subsection reproduces the heterogeneity results by race and gender from the main draft, but additionally includes the results for tenants who are male and tenants who are not black.

Table F.8: Impacts on Earnings, Employment, and Longer-Run Housing Situation (heterogeneity by race and gender)

Labor Market	$\mathbb{E}[Y \mid E=0]$ (1)	OLS (2)	LDV (3)	RF (4)	IV (5)
All:					
Earnings	4,441	-842***	-266***	-278*	-352*
	(4,913)	(20)	(19)	(159)	(202)
Employment	0.564	-0.036***	-0.016***	-0.018	-0.023
	(0.306)	(0.002)	(0.001)	(0.016)	(0.020)
Female:					
Earnings	4,212		-220***		-532***
	(3,817)		(17)		(200)
Employment	0.584		-0.017***		-0.027
	(0.303)		(0.001)		(0.023)
Male:					
Earnings	4,798		-342***		95
	(6,316)		(33)		(485)
Employment	0.518		-0.015***		-0.016
	(0.307)		(0.002)		(0.040)
Black:					
Earnings	4,386		-221***		-398
	(3,757)		(14)		(247)
Employment	0.582		-0.014***		-0.063**
	(0.304)		(0.002)		(0.031)
Not Black:					
Earnings	4,650		-352***		-287
	(6,612)		(34)		(454)
Employment	0.536		-0.020***		0.034
	(0.307)		(0.002)		(0.036)

Housing	$\mathbb{E}[Y \mid E=0]$ (1)	OLS (2)	LDV (3)	RF (4)	IV (5)
All:					
Not at Eviction Address	0.462	0.117***	0.108***	0.075**	0.099**
	(0.333)	(0.003)	(0.003)	(0.032)	(0.041)
Any Homeless Services	0.018	0.028***	0.026***	0.025*	0.035*
	(0.094)	(0.001)	(0.001)	(0.015)	(0.019)
Emergency Shelter	0.012	0.022***	0.020***	0.006	0.007
	(0.078)	(0.001)	(0.001)	(0.013)	(0.017)
Female:					
Not at Eviction Address	0.447		0.112***		0.124**
	(0.325)		(0.003)		(0.050)
Any Homeless Services	0.019		0.027***		0.071***
	(0.096)		(0.001)		(0.021)
Emergency Shelter	0.012		0.021***		0.045**
	(0.077)		(0.001)		(0.019)
Male:					
Not at Eviction Address	0.453		0.098***		0.038
	(0.334)		(0.004)		(0.090)
Any Homeless Services	0.017		0.020***		-0.046
	(0.090)		(0.001)		(0.045)
Emergency Shelter	0.012		0.015***		-0.072**
	(0.078)		(0.001)		(0.036)
Black:					
Not at Eviction Address	0.443		0.110***		0.108*
	(0.326)		(0.003)		(0.061)
Any Homeless Services	0.023		0.029***		0.033
	(0.105)		(0.001)		(0.027)
Emergency Shelter					
Not Black:					
Not at Eviction Address	0.453		0.104***		0.100
	(0.331)		(0.004)		(0.078)
Any Homeless Services	0.011		0.019***		0.031
	(0.075)		(0.001)		(0.023)
Emergency Shelter					

Notes: This table reports equally-weighted averages of Cook County and New York non-evicted sample means ($\mathbb{E}[Y|E=0]$), as well as equally-weighted averages of location-specific OLS, lagged dependent variable OLS (LDV), reduced form (RF), and two-stage least squares (IV) estimates of the impact of eviction on outcomes related to the tenant's labor and housing situation. Outcomes are listed on the left of each row. Labor outcomes are defined for the two years following the court proceedings. Housing outcomes are defined for 1–2 years following the court proceedings. Because addresses are observed annually, "Not at Eviction Address" is defined for the two calendar years after the court proceedings year. Estimates are reported for the full sample "All", and separately for women "Female", men "Male", Black "Black", and not Black tenants "Not Black". Controls for all model specifications are the same as those described in Table 4, with the obvious omission of gender for "Female" and "Male" and race for "Black". Standard errors for regression model coefficients are included in parentheses and are clustered at the judge-year level. Median number of observations is 203,027 (max: 861,743, min: 60,249). Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

F.4 Location-specific estimates

This section reproduces the main regression results from the main text for Cook County and New York separately.

Table F.9: Location-specific Estimates: Impact on Housing Situation, One Year After Court

	Cook County					New York				
	$\mathbb{E}[Y\|E=0]$	OLS	LDV	RF	IV	$\mathbb{E}[Y\|E=0]$	OLS	LDV	RF	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Not at Eviction Address	0.401	0.028***	0.028***	0.079**	0.112**	0.222	0.130***	0.112***	0.068*	0.080*
	(0.490)	(0.003)	(0.003)	(0.039)	(0.057)	(0.415)	(0.006)	(0.006)	(0.038)	(0.045)
Lockout	0.004	0.273***		0.348***	0.440***	0.000	0.301***		0.279***	0.333***
	(0.063)	(0.004)		(0.041)	(0.049)	(0.000)	(0.010)		(0.032)	(0.043)
Any Homeless Services	0.014	0.015***	0.014***	0.012	0.016	0.013	0.060***	0.055***	0.037**	0.044**
	(0.117)	(0.002)	(0.002)	(0.025)	(0.033)	(0.114)	(0.003)	(0.002)	(0.015)	(0.019)
Emergency Shelter	0.006	0.010***	0.010***	0.015	0.021	0.011	0.055***	0.050***	0.037**	0.044**
	(0.079)	(0.001)	(0.001)	(0.016)	(0.021)	(0.105)	(0.003)	(0.002)	(0.016)	(0.019)
Future Eviction at Same Address	0.095	-0.011***		-0.112***	-0.141***	0.052	-0.009***		0.033**	0.037**
	(0.294)	(0.001)		(0.015)	(0.018)	(0.222)	(0.001)		(0.017)	(0.019)

Notes: This table reports Cook County and New York specific non-evicted sample means ($\mathbb{E}[Y|E=0]$), as well as Cook County and New York specific OLS, lagged dependent variable OLS (LDV), reduced form (RF), and two-stage least squares (IV) estimates of the impact of eviction on outcomes related to the tenant's housing situation. Outcomes are listed on the left of each row. Outcomes and controls for all model specifications are the same as those in Table 4. Standard errors for regression model coefficients are included in parentheses and are clustered at the judge-year level. Median number of observations for Cook County is 114,000 (max: 551,603, min: 29,000). Median number of observations for New York is 181,842 (max: 390,873, min: 45,365). Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

Table F.10: Cook County: Impacts on Earnings, Employment, and Longer-Run Housing Situation

	All					Female			Black		
	$\mathbb{E}[Y\|E=0]$	OLS	LDV	RF	IV	$\mathbb{E}[Y\|E=0]$	LDV	IV	$\mathbb{E}[Y\|E=0]$	LDV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Labor:											
Earnings	5,051	-1,249***	-360***	-319	-427	4,560	-273***	-510*	4,554	-257***	-220
	(8,516)	(32)	(34)	(244)	(325)	(5,938)	(29)	(308)	(5,517)	(18)	(295)
Employment	0.621	-0.042***	-0.016***	-0.012	-0.016	0.639	-0.016***	0.006	0.624	-0.011***	-0.010
	(0.410)	(0.002)	(0.001)	(0.020)	(0.027)	(0.403)	(0.002)	(0.029)	(0.408)	(0.002)	(0.032)
Housing:											
Not at Eviction Address	0.626	0.074***	0.070***	0.068	0.096	0.636	0.076***	0.086	0.628	0.073***	0.061
	(0.484)	(0.004)	(0.004)	(0.042)	(0.059)	(0.481)	(0.004)	(0.069)	(0.484)	(0.004)	(0.074)
Any Homeless Services	0.019	0.019***	0.019***	0.041*	0.057**	0.020	0.018***	0.098***	0.026	0.023***	0.073**
	(0.135)	(0.002)	(0.002)	(0.022)	(0.029)	(0.140)	(0.002)	(0.030)	(0.158)	(0.002)	(0.035)
Emergency Shelter	0.009	0.010***	0.010***	0.009	0.012	0.009	0.009***	0.061**			
	(0.095)	(0.001)	(0.001)	(0.019)	(0.026)	(0.095)	(0.001)	(0.027)			
Future Eviction at Same Address	0.239	-0.106***		-0.174***	-0.217***	0.252		-0.247***	0.277		-0.257***
	(0.426)	(0.003)		(0.019)	(0.023)	(0.434)		(0.027)	(0.448)		(0.028)

Notes: This table reports Cook County specific sample means ($\mathbb{E}[Y|E=0]$), as well as Cook County specific OLS, lagged dependent variable OLS (LDV), reduced form (RF), and two-stage least squares (IV) estimates of the impact of eviction on outcomes related to the tenant's labor and housing situation. Outcomes are listed on the left of each row. Labor outcomes are defined for the two years following the court proceedings. Housing outcomes are defined for 1–2 years following the court proceedings. Because addresses are observed annually, "Not at Eviction Address" is defined for the two calendar years after the court proceedings year. "Future Eviction at Same Address" is an indicator for if a case was filed against the same tenant at the same address within the next three years. Estimates are reported for the full sample "All" and separately for women "Female" and Black tenants "Black". Controls for all model specifications are the same as those described in Table 4, with the obvious omission of gender for "Female" and race for "Black". Standard errors for regression model coefficients are included in parentheses and are clustered at the judge-year level. Median number of observations is 155,000 (max: 514,659, min: 22,500). Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

Table F.11: New York: Impacts on Earnings, Employment, and Longer-Run Housing Situation

	All					Female			Black		
	$\mathbb{E}[Y\|E=0]$	OLS	LDV	RF	IV	$\mathbb{E}[Y\|E=0]$	LDV	IV	$\mathbb{E}[Y\|E=0]$	LDV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Labor:											
Earnings	3,832	-435***	-173***	-236	-277	3,865	-166***	-554**	4,218	-186***	-576
	(4,902)	(25)	(14)	(203)	(241)	(4,799)	(16)	(256)	(5,101)	(20)	(395)
Employment	0.507	-0.029***	-0.016***	-0.025	-0.029	0.528	-0.017***	-0.059*	0.540	-0.017***	-0.117**
	(0.453)	(0.002)	(0.002)	(0.024)	(0.029)	(0.452)	(0.002)	(0.035)	(0.450)	(0.002)	(0.053)
Housing:											
Not at Eviction Address	0.297	0.161***	0.145***	0.082*	0.101*	0.259	0.147***	0.161**	0.259	0.146***	0.154
	(0.457)	(0.005)	(0.005)	(0.048)	(0.058)	(0.438)	(0.006)	(0.073)	(0.438)	(0.006)	(0.098)
Any Homeless Services	0.018	0.037***	0.032***	0.010	0.012	0.018	0.037***	0.045	0.020	0.035***	-0.007
	(0.132)	(0.002)	(0.002)	(0.019)	(0.025)	(0.133)	(0.002)	(0.030)	(0.139)	(0.002)	(0.040)
Emergency Shelter	0.015	0.034***	0.030***	0.002	0.003	0.015	0.034***	0.030	0.017	0.033***	-0.017
	(0.123)	(0.002)	(0.002)	(0.018)	(0.023)	(0.122)	(0.002)	(0.027)	(0.130)	(0.002)	(0.036)
Future Eviction at Same Address	0.115	-0.040***		0.039	0.046	0.104		0.038	0.131		0.034
	(0.319)	(0.002)		(0.025)	(0.028)	(0.305)		(0.033)	(0.337)		(0.053)

Notes: This table reports New York specific sample means ($\mathbb{E}[Y|E=0]$), as well as New York specific OLS, lagged dependent variable OLS (LDV), reduced form (RF), and two-stage least squares (IV) estimates of the impact of eviction on outcomes related to the tenant's labor and housing situation. Outcomes are listed on the left of each row. Labor outcomes are defined for the two years following the court proceedings. Housing outcomes are defined for 1-2 years following the court proceedings. Because addresses are observed annually, "Not at Eviction Address" is defined for the two calendar years after the court proceedings year. "Future Eviction at Same Address" is an indicator for if a case was filed against the same tenant at the same address within the next three years. Estimates are reported for the full sample "All" and separately for women "Female" and Black tenants "Black". Controls for all model specifications are the same as those described in Table 4, with the obvious omission of gender for "Female" and race for "Black". Standard errors for regression model coefficients are included in parentheses and are clustered at the judge-year level. Median number of observations is 103,907 (max: 347,084, min: 27,393).

Table F.12: Location-specific Estimates: Impact on Financial Distress

	Cook County					New York				
	$\mathbb{E}[Y\|E=0]$	OLS	LDV	RF	IV	$\mathbb{E}[Y\|E=0]$	OLS	LDV	RF	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Credit Score	538.84	-14.76***	-10.22***	-9.05	-14.66	569.85	-11.71***	-5.86***	-8.91	-10.00
	(74.95)	(0.42)	(0.42)	(6.31)	(10.08)	(110.07)	(0.68)	(0.57)	(6.26)	(7.04)
No Open Revolving Account	0.585	0.056***	0.045***	0.081**	0.120**	0.332	0.049***	0.037***	0.026	0.029
	(0.488)	(0.003)	(0.003)	(0.040)	(0.056)	(0.427)	(0.003)	(0.003)	(0.026)	(0.030)
Total Bal. Collections	3,275.22	640.90***	544.15***	180.28	465.65	1,053.61	301.69***	297.33***	144.66	162.29
	(4,416.73)	(28.54)	(27.36)	(355.57)	(499.51)	(2,637.15)	(16.94)	(18.44)	(151.34)	(170.36)
Any Auto Loan or Lease	0.195	-0.061***	-0.053***	-0.040	-0.075**	0.205	-0.008***	-0.002	-0.014	-0.015
	(0.392)	(0.002)	(0.002)	(0.027)	(0.038)	(0.404)	(0.002)	(0.002)	(0.025)	(0.028)
Any Payday Inquiry (x100)	15.387	-1.182***	-1.004***	-1.229	-3.153					
	(36.083)	(0.232)	(0.216)	(3.271)	(4.478)					

Notes: This table reports Cook County and New York specific non-evicted sample means ($\mathbb{E}[Y|E=0]$), as well as New York and Cook County specific OLS, lagged dependent variable OLS (LDV), reduced form (RF), and two-stage least squares (IV) estimates of the impact of eviction on outcomes related to the tenant's financial health. All outcomes are defined for the second and third calendar years following the court proceedings. Controls for all model specifications are similar to those described in Table 4, except that gender and race controls are imputed based on name and address. Standard errors for regression model coefficients are included in parentheses and are clustered at the judge-year level. Median number of observations is 157,632 (max: 190,270, min: 42,985). .

F.5 Estimates for other time horizons

Figure F.13 provides credit bureau results one to four quarters after the case is filed.

Table F.13: Impact on Financial Distress, One Year After Court

	$\mathbb{E}[Y\|E=0]$ (1)	OLS (2)	LDV (3)	RF (4)	IV (5)
Credit Score	548.43	-15.58***	-9.81***	-8.27*	-10.33*
	(66.83)	(0.45)	(0.41)	(4.75)	(6.10)
No Open Revolving Account	0.465	0.052***	0.038***	-0.015	-0.011
	(0.331)	(0.002)	(0.002)	(0.028)	(0.035)
Total Bal. Collections	2,017.87	405.46***	332.79***	246.69	391.16
	(2,432.18)	(20.22)	(16.59)	(272.47)	(330.33)
Any Auto Loan or Lease	0.181	-0.035***	-0.026***	-0.049*	-0.073**
	(0.271)	(0.002)	(0.001)	(0.026)	(0.033)
Any Payday Inquiry (x100)	10.336	-0.714***	-0.552**	1.370	2.377
	(30.443)	(0.203)	(0.214)	(2.605)	(3.351)

Notes: This table reports equally-weighted averages of Cook County and New York non-evicted sample means ($\mathbb{E}[Y|E=0]$), as well as equally-weighted averages of location-specific OLS, lagged dependent variable OLS (LDV), reduced form (RF), and two-stage least squares (IV) estimates of the impact of eviction on outcomes related to the tenant's financial health. The only exceptions are estimates for the outcome "Any Payday Inquiry (x100)," which are Cook County specific. All outcomes are defined for the first calendar years following the court proceedings. Controls for all model specifications are similar to those described in Table 4, except that gender and race controls are imputed based on name and address. Standard errors for regression model coefficients are included in parentheses and are clustered at the judge-year level. * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$.Median number of observations is 248,533 (max: 282,088, min: 32,035).

F.6 Additional robustness tests

This section provides additional robustness results to support the main analysis in the paper.

F.6.1 Experian first stage

Table F.14 reports the coefficient on judge stringency in from the first stage in the linked Experian samples for Cook County and New York.

Table F.14: Experian First Stage

	Cook County		New York	
	(1)	(2)	(1)	(2)
Judge stringency	0.749***	0.747***	0.899***	0.892***
	(0.040)	(0.040)	(0.025)	(0.025)
N obs	168,558	168,558	157,700	157,700

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$.This table reports results for a first stage regression of eviction on judge stringency for Cook County and New York, restricted to the Experian samples. The first column (1) shows the regression including only judge stringency. The second column (2) controls for another potential measure of judge stringency, and the estimate for this alternative stringency measure is also displayed under this specification. The regression for Cook County adds controls for judgment amount stringency, while the regression for New York controls for judge stringency in granting stays. Judgment amount stringency is constructed using joint action cases that end in an eviction order. For each case, we construct the leave-one-out mean for the difference between the judgment amount and the ad damnum amount for each judge in each year (for judges who see at least 100 cases). Stay stringency is the leave-one-out mean of the indicator for if the judge allowed a stay of the eviction order, which extends how long the city must wait before they can perform the lockout associated with the eviction. Regressions include court-year fixed effects. Standard errors are included in parentheses and are clustered at the judge-year level.

F.6.2 Exclusion

Below, we evaluate whether there is any strong evidence against the exclusion restriction. Where possible, we provide evidence from both settings. However, since not all aspects of judge behavior are recorded in both settings, we rely on data from just one location when necessary. In Tables F.15 and F.16, we report correlations between our instrument (eviction order stringency) and other stringency measures in each location. Across both locations, all stringency measures are calculated as judge/courtroom-year leave-one-out averages. For Cook County, we construct stringency in granting continuances, stringency in judgment amount, and stringency in granting stays. For New York, we construct stringency in terms of granting stays and stringency for tenant receipt of emergency rental assistance in the 30 days after filing. The last stringency measure addresses concerns that judges could differentially urge tenants at risk of eviction to seek out emergency rental assistance. Across both locations, the eviction order stringency instrument is, at most, very weakly correlated with other types of judge actions, which gives us confidence that the exclusion restriction is plausible in our settings. In Cook County—where we are able to observe whether money judgments are included with an eviction order—we also regress judgment amounts on eviction order stringency. Table F.17 shows that eviction order stringency is a poor predictor of judgment amount. Taken together, we find no evidence suggesting the exclusion restriction is violated in our context.

Table F.15: Correlation between various judge stringency measures (Cook County)

	Eviction Order	Continuance	Amount	Stays
Stringency(eviction order)	1	−0.065	0.101	0.081
Stringency(continuance)	−0.065	1	0.025	0.015
Stringency(judgment amount)	0.101	0.025	1	0.097
Stringency(Stays)	0.081	0.015	0.097	1

Notes: This table reports the correlation between judges' eviction order stringency and three other stringency measures in Cook County: stringency in granting continuances, stringency in judgment amount, and stringency in granting stays. Stringency measures are judge-year leave-one out averages. Judgment amount stringency is the leave-one-out average of the difference between judgment amounts and ad damnum amounts for cases ending in an eviction. We also calculate stringency related to granting stays of the eviction order among the cases ending in an eviction order.

Table F.16: Correlation between various judge stringency measures (New York)

	Stringency	Stays	Emergency Assistance
Stringency(Eviction Order)	1	0.0457	−0.058
Stringency(Stays)	0.0457	1	−0.0821
Stringency(Emerg. Assistance)	−0.058	−0.0821	1

Notes: This table reports the correlation between judge's eviction stringency and two other stringency measures in New York City. The first measure is stringency in granting stays and the second is the rate at which tenant's assigned to the courtroom/judge receive "one-shot" emergency assistance from the city in the 30 days after their filing. All stringencies are judge-year leave-one-out averages.

Table F.17: Regression of log judgment amount on eviction order stringency

	Log judgment amount		
	(1)	(2)	(3)
Stringency	-0.154	-0.091	-0.098
	(0.141)	(0.111)	(0.112)
log ad damnum		0.774***	0.743***
		(0.003)	(0.003)
Constant	7.287***	1.846***	2.700***
	(0.094)	(0.078)	(0.088)
Court × Year FEs		✓	✓
All controls			✓
Observations	221,828	221,828	173,094
R^2	0.091	0.627	0.616
Note:	*p<0.1; **p<0.05; ***p<0.01		

Notes: This table shows regression results from regressing log judgment amount on judge eviction stringency for Cook County. * $p < 0.1,^{**}$ $p < 0.05,^{***}$ $p < 0.01$.

Table F.18: First stage robustness

Sample	Coefficient	Standard Errors	P-Value	Observations
Main	0.822	0.026	0.000	453,618
Controlling for other judge chars.	0.834	0.026	0.000	447,184
Alternate first judge construction	0.826	0.025	0.000	439,943
All cases	0.632	0.027	0.000	577,675
Excluding cases never served	0.827	0.024	0.000	408,967

Notes: This table shows the first-stage regression on the full Cook County Sample. The "Main" row shows the main specification, the "Controlling for other judge chars." row additionally controls for leave-one-out judge stringency in judgment amounts, granting stays, and granting continuances. "All cases" does not impose restrictions on how many cases the judge sees per year. "Excluding cases never served" excludes cases where the tenant was never served.

Table F.19: Robustness of IV estimates to controlling for judgment amount

	IV (1)	IV with amount stringency controls (2)	IV for eviction and amount (3)
Earnings	-426.90	-386.9	-209.4
	(325.20)	(318.8)	(457.0)
Employment	-0.016	-0.013	0.012
	(0.027)	(0.025)	(0.028)

Notes: Column 1 shows the main IV estimates for earnings and employment in Cook County. Column (2) additionally controls for judgment amount stringency—which is the leave-one-out estimate of a judge's stringency in judgment amount for joint action cases ending in eviction (constructed as the judgment amount minus the ad damnum amount). Column (3) includes both an indicator for eviction and the judgment amount, instrumented by judge stringency in eviction orders and judge stringency in judgment amount. Cook County results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-110 and CBDRB-FY20-206.

F.6.3 Monotonicity

Below we evaluate if there is any strong evidence against the monotonicity assumption holding in the data. Tables F.20 and F.21 report the first stage coefficient on judge stringency running the first stage on the sample listed in the first column for each table. The first stage coefficient is positive for all sub-samples and is relatively stable across sub-samples. Following Bhuller et al. (2020) and Norris et al. (2019), Table F.22 runs the first stage regression where judge stringency is calculated on one sub-population (for example women) and then runs the first-stage on the complementing sub-population (for example men).

Table F.20: Monotonicity—Cook County

Sample	Coefficient	Standard Errors	P-Value	Observations
Joint Action	0.730	0.027	< 0.001	$359,025$
Single Action	1.160	0.084	< 0.001	$94,593$
Males	0.776	0.033	< 0.001	$195,190$
Females	0.856	0.033	< 0.001	$258,428$
No attorney	0.832	0.028	< 0.001	$436,740$
Attorney	0.512	0.159	0.001	$16,878$
Black	0.884	0.036	< 0.001	$248,276$
Hispanic	0.905	0.081	< 0.001	$55,352$
Larger landlords	0.593	0.035	< 0.001	$299,674$
Smaller landlords	1.144	0.053	< 0.001	$169,145$

Notes: This table reports results from Cook County for the first stage regressions of eviction on judge stringency. For each row, we calculate judge stringency using the analysis sample and run the first stage on the subsample listed. Standard errors are depicted in parentheses and are clustered at the judge-year level. "Larger landlords" are landlords who appear in the court records more than five times, while "Smaller landlords" are landlords who appear in the court records five or fewer times.

Table F.21: Monotonicity—New York City

Sample	Coefficient	Standard Errors	P-Value	Observations
Male	0.883	0.085	< 0.001	42,094
Female	0.807	0.062	< 0.001	108,587
Black	0.793	0.066	< 0.001	89,399
Hispanic	0.875	0.077	< 0.001	68,222
Rent Stabilized	0.743	0.098	< 0.001	50,963

Notes: This table reports results from New York for the first stage regressions of eviction on judge stringency. For each row, we calculate judge stringency using the analysis sample and run the first stage on the subsample listed. Standard errors are depicted in parentheses and are clustered at the judge-year level.

To better explore potential monotonicity violations, we hand-collected data on judge characteristics for Cook County—where we observe which specific judge is assigned to which case. Using information from judge profiles in Sullivan's Judicial Profiles (2017) supplemented with additional online sources, demographic information was compiled on over 150 of the judges who presided the most cases in our sample. Using this data, gender was determined based on their biographies in Sullivan's Judicial Profiles. Sullivan's Judicial Profiles is the only reference guide with profiles for all judges serving in Illinois. For each judge, we determined gender based on the use of feminine or masculine pronouns used in their respective biographies. Based on conversations with the people behind Sullivan's Judicial Profiles, the biographies are created based on surveys they send to the judges, and the gender of the pronouns for each judge's biography is determined based on the judge's self-chosen gender.

Based on conversations with the Cook County law library officials, there are no references that provide consistent race data on judges, nor were there any reference that contained pictures of all judges. Perceived race of judge was coded by research assistants using photos of the judges found online. For each judge, we required at least two different reputable sources that provide an image of the judge and also specifically mention the judge's name in relation to the picture. Two research assistants compiled links to pages containing images of the judges, and then both research assistants independently coded the race of the judge based on the two pictures of the judges. The values are black, white, Hispanic, and Asian.[62] If either research assistant was uncertain of the race based on the picture, race was coded as missing. There was no disagreement on race when pictures were able to be found, and in many cases race was additionally confirmed in the associated text of the selected source. Perceived race was coded for 111 of the judges.[63]

Table F.23 shows the breakdown of judges by race and gender. In particular, we see that sub-sampling judges based on gender and on being white or black provides us with enough data to adequately interact these characteristics with tenant characteristics. Each of these judge

[62]Puerto Rican is coded as Hispanic.

[63]Only two of the judges considered were identified as Asian and are excluded from our analysis due to the small number of judges.

Table F.22: Split-sample monotonicity

Stringency Sample	First Stage Sample	Cook County (1)	New York (2)
Female	Male	0.789***	0.916***
		(0.035)	(0.080)
Male	Female	0.843***	0.638***
		(0.031)	(0.058)
Black	Not Black	0.574***	0.812***
		(0.033)	(0.085)
Not Black	Black	0.658***	0.682***
		(0.030)	(0.058)
Joint Action Case	Single Action Case	1.060***	
		(0.062)	
Single Action Case	Joint Action Case	0.363***	
		(0.018)	
Rent Stabilized	Not Rent Stabilized		0.738***
			(0.094)
Not Rent Stabilized	Rent Stabilized		0.683***
			(0.066)

Notes: For each city, this table reports results for first stage regressions of eviction on judge stringency. For each row, we calculate judge stringency using the subsample listed under "Stringency Sample", and we run the first stage on the subsample listed under "First Stage Sample". For example, the first row depicts a first stage on the sample of males, with our measure of judge stringency calculated based on the sample of females. "Joint action" is an indicator for if the case was a joint action case and is specific to Cook County. "Rent stabilized" is an indicator for if rent is stabilized, and is specific to NY. Standard errors are depicted in parentheses and are clustered at the judge-year level.

subsamples has at least 30 judges and over $100,000$ cases in total. In contrast, the sample only includes 8 Hispanic judges and a combined caseload of less than $20,000$ cases, suggesting that cross interactions between Hispanic judges and tenant characteristics will suffer from a small sample size limitations and therefore are excluded below.

Table F.24 shows the coefficient for stringency from the regression of the case outcome on stringency and various controls, restricted to a number of different subpopulations that now include interactions between tenant and judge characteristics. Restricting our focus to the columns for male, female, white, and black judges, we see that all interactions result in positive judge stringency coefficients (all statistically significant at the 0.01 level), supporting our monotonicity assumption.

Table F.23: Cook County judge characteristics breakdown

Sample	Male	Female	White	Black	Hispanic
Number of Judges	104	56	74	29	6
Number of Total Cases	331,966	123,978	203,982	180,858	17,599
Stringency Diff. (10-90)	0.072	0.076	0.075	0.078	0.033

Notes: Table shows characteristics of the judges most prevalent in the sample for Cook County. "Stringency Diff. (10-90)" reports the percentage point difference between the 10th and 90th percentile of judge stringency for each group.

Table F.24: Cook County monotonicity checks, two-way interactions

Pro Se Teannts	Male Judges	Female Judges	White Judges	Black Judges
All	0.799	0.793	0.716	0.788
Male	0.737	0.823	0.676	0.805
Female	0.845	0.771	0.743	0.777
White	0.543	0.735	0.570	0.617
Black	0.884	0.819	0.773	0.812
Hispanic	0.895	0.954	0.909	0.922

Notes: The table above reports the coefficient on judge stringency by defendant characteristics interacted with characteristics of the judge for cases assigned to the most common judges in the data for Cook County. Sample is restricted to defendants without lawyers. "Black" and "Hispanic" are imputed using each defendant's last name and census tract. Imputation defines a tenant as part of the group if the estimated posterior probability of being of that race is greater than 0.75.

G Signing the bias in difference-in-differences

G.1 Setup

Consider a panel data setting where we observe the outcome of interest $Y \in \mathbb{R}$ and treatment state $E \in \{0, 1\}$ across individuals and time. For simplicity, we will consider wage income as the outcome. Assume that each individual can only be treated once, in time period 0, so we can denote observations by $E_{i,t}$ and $Y_{i,t}$, where the t subscript refers to time relative to treatment. The observed outcomes are generated by potential outcomes $\xi_{i,t}(e), e \in \{0, 1\}$ (i.e., $Y_{i,t} = E_{i,t}\xi_{i,t}(1) + (1 - E_{i,t})\,\xi_{i,t}(0)$).

Suppose we observe an instrument Z which takes values in $z \in \mathcal{Z}$, and assume classic IV and monotonicity assumptions hold. By Vytlacil (2002), this setup is equivalent to the existence of a latent index selection model of the form

$$E_{i,t}(z) = \begin{cases} 0 & \text{when } t < 0, \\ \mathbf{1}\{-\varepsilon_i < v(z)\} & \text{otherwise,} \end{cases}$$

where $v(z)$ is a function and Z is independent of potential outcomes and ε.

Throughout this section we ignore time-invariant observed covariates that we may want to condition the analysis on. We also assume that

$$v(Z) = \gamma Z.$$

Note that this setup is equivalent to a classical selection model that imposes a linear index assumption.[64] To introduce further notation, let

$$Y_{i,t} = \beta_{i,t} E_{i,t} + \nu_{i,t},$$

where $\beta_{i,t} \equiv \xi_{i,t}(1) - \xi_{i,t}(0)$ and $\nu_{i,t} \equiv \xi_{i,t}(0)$. In addition, we write $\beta_{i,t}$ as

$$\beta_{i,t} = \beta_t + \Psi_{i,t},$$

where $\Psi_{i,t}$ captures the idiosyncratic portion of the effect of an eviction and β_t captures the average effect.

In the remainder of this section we discuss potential assumptions on $\nu_{i,t}$ and connect these assumptions to intuition about the processes $\xi_{i,t}(e), e \in \{0, 1\}$, which, in our application, are earnings processes in the evicted and non-evicted states of the world.

[64] In other words, this model is equivalent to assuming treatment is taken if $\gamma Z + \varepsilon_i > 0$, where ε_i is iid and independent of Z, which is equivalent to what we have here with some re-arranging.

G.2 Analysis

The probability limit of the DiD estimator that uses period k as the post-treatment period and period ℓ as the pre-treatment period consists of the following four components:

$$\mathbb{E}[Y_{i,k}|E_{i,0}=1] = \beta_k + \mathbb{E}[\Psi_{i,k}|E_{i,0}=1] + \mathbb{E}[\nu_{i,k}|\varepsilon_i > -\gamma Z_i] \tag{G.1}$$
$$\mathbb{E}[Y_{i,-\ell}|E_{i,0}=1] = \mathbb{E}[\nu_{i,-\ell}|\varepsilon_i > -\gamma Z_i] \tag{G.2}$$
$$\mathbb{E}[Y_{i,k}|E_{i,0}=0] = \mathbb{E}[\nu_{i,k}|\varepsilon_i \leq -\gamma Z_i] \tag{G.3}$$
$$\mathbb{E}[Y_{i,-\ell}|E_{i,0}=0] = \mathbb{E}[\nu_{i,-\ell}|\varepsilon_i \leq -\gamma Z_i]. \tag{G.4}$$

The probability limit is

$$\beta_k + \mathbb{E}[\Psi_{i,k}|E_{i,0}=1] + (\mathbb{E}[\nu_{i,k} - \nu_{i,-\ell}|E_{i,0}=1]) - (\mathbb{E}[\nu_{i,k} - \nu_{i,-\ell}|E_{i,0}=0]),$$

which can be written as

$$\beta_k + \mathbb{E}[\Psi_{i,k}|E_{i,0}=1] + \underbrace{(\mathbb{E}[\nu_{i,k}|E_{i,1}=1] - \mathbb{E}[\nu_{i,k}|E_{i,1}=0])}_{A} - \underbrace{(\mathbb{E}[\nu_{i,-\ell}|E_{i,1}=1] - \mathbb{E}[\nu_{i,-\ell}|E_{i,1}=0])}_{B}.$$

If we assume that positive income shocks are associated with lower probabilities of eviction, then we would expect both A and B to be weakly negative, and the relative magnitudes of these two terms will determine the sign of the bias.

Assuming that the conditional expectation $\mathbb{E}[\nu_{i,t}|\epsilon_i]$ is linear in ε, we can rewrite this equation as

$$\begin{aligned} &\beta_k + \mathbb{E}[\Psi_{i,k}|E_{i,0}=1] \\ +\ &(b(\nu_{i,k},\varepsilon_i) - b(\nu_{i,-\ell},\varepsilon_i))(\mathbb{E}[\varepsilon_i|\varepsilon_i > -\gamma Z_i] - \mathbb{E}[\varepsilon_i|\varepsilon_i \leq -\gamma Z_i]), \end{aligned} \tag{G.5}$$

where $b(\nu_{i,k},\varepsilon_i)$ is the population regression coefficient from $\nu_{i,k} = a + b\varepsilon_i + e_i$ (Heckman and Robb, 1985; Ashenfelter and Card, 1985).[65]

Note that $(\mathbb{E}[\varepsilon_i|\varepsilon_i > -\gamma Z_i] - \mathbb{E}[\varepsilon_i|\varepsilon_i \leq -\gamma Z_i]) > 0$, so the sign of the bias term will depend on $b(\nu_{i,k},\varepsilon_i) - b(\nu_{i,-\ell},\varepsilon_i)$, which in turn depends on the sign of $(cov(\nu_{i,k},\varepsilon_i) - cov(\nu_{i,-\ell},\varepsilon_i))$. The correlation between ε_i and a given earnings shock $\nu_{i,t}$ will depend on how eviction outcomes are determined and the information set of the judge. Here we assume that the contemporaneous earnings innovation $\nu_{i,0}$ affects the eviction decision and that there is full information about any shocks up through period $t=0$, when the eviction decision is made.

[65] This step uses the assumption that $\mathbb{E}[\nu_{i,k}|\epsilon_i > -\gamma Z_i]$ is linear in ε_i, allowing us to rewrite this expression as $\frac{cov(\nu_{i,k},\epsilon_i)}{var(\varepsilon_i)}\mathbb{E}[\varepsilon_i|\varepsilon_i > -\gamma Z_i]$. As discussed in Chabé-Ferret (2015), the assumption of linearity of the conditional expectation is a restriction. This assumption is satisfied for joint normality, as well as for a larger family of elliptical distributions.

For a large number of specifications of the earnings process, we can sign the bias when the pre-period is further from the period of eviction ($t = 0$) than the post period, i.e., when $\ell > k$. Consider a specification where $\nu_{i,t}$ consists of an ARMA process, a fixed effect θ_i that potentially also directly enters the eviction decision, and a random walk—where each of these components is independent of the others and enters additively.

The ARMA process will bias the estimated impact on earnings downward when $\ell > k$ and positive earnings innovations reduce the likelihood of eviction, and the ARMA process satisfies two additional conditions: (1) the process is covariance stationary and (2) the auto-covariance function is positive and weakly decreasing.

Next, let the innovation $\nu_{i.t}$ also contain an independent and additive random walk $\eta_{i,t} = \eta_{i,t-1} + \eta^*_{i,t}$, where $\eta^*_{i,t}$ is the i.i.d. innovation. Assuming that the level of $\eta_{i,0}$ enters negatively into the eviction outcome, the bias of the DiD estimate caused by the random walk will be be negative as long as $var(\eta_{i,0}) > var(\eta_{i,-\ell})$, which must be true for the random walk unless $\ell = 0$.

Finally, the DiD will eliminate the impact of the fixed effect θ_i except for its potential contribution to $\mathbb{E}[\Psi_{i,k}|E_{i,0} = 1]$. Below we consider two specific examples that meet these requirements. One example includes a fixed effect and an AR(1) process, and the other has a fixed effect and a random walk. If we were to consider an example with both a random walk and an AR(1) term, the bias from the two terms will go in the same direction only when $k - \ell < 0$. In particular, if the lagged term ($t = -\ell$) is further from the period in which eviction is determined than the lead period ($t = k$), then we have a negative bias term.

AR(1) earnings process

Using the model above, assume that the earnings process consists of a fixed effect θ_i and an AR(1) error term $\eta_{i,t}$. Similarly, assume that the shock to the eviction outcome depends on the individual fixed effect and the innovation to earnings at time $t = 0$:

$$\begin{aligned} \nu_{i,t} &= \theta_i + \eta_{i,t} \\ \eta_{i,t} &= \rho\eta_{i,t-1} + \eta^*_{i,t} \\ \varepsilon_i &= \alpha_1\theta_i + \alpha_2\eta_{i,0} + \varepsilon^*_i, \end{aligned}$$

where $\eta^*_{i,t}$ and ε^*_i are idiosyncratic shocks. We will assume that larger fixed effects in the earnings regression reduce the probability of eviction ($\alpha_1 < 0$), and that positive earnings innovations lower the probability of eviction ($\alpha_2 < 0$). Under this setting,

$$\begin{aligned} cov(\nu_{i,k}, \varepsilon_i) &= cov(\theta_i + \eta_{i,k}, \alpha_1\theta_i + \alpha_2\eta_{i,0} + \varepsilon^*_i) \\ &= \alpha_1^2 var(\theta_i) + \alpha_2 cov(\eta_{i,k}, \eta_{i,0}) \\ &= \alpha_1^2 var(\theta_i) + \alpha_2\rho^k var(\eta_{i,0}). \end{aligned}$$

Similarly, we have

$$cov(\nu_{i,-\ell}, \varepsilon_i) = \alpha_1^2 var(\theta_i) + \alpha_2 \rho^\ell var(\eta_{i,-\ell}).$$

Under stationarity in the earnings process, $var(\eta_{i,0}) = var(\eta_{i,-\ell})$, so if $k = \ell$, the bias term $(b(\nu_{i,k}, \varepsilon_i) - b(\nu_{i,-\ell}, \varepsilon_i))$ in Equation G.5 will be equal to 0, which is the symmetric difference-in-differences result. Similarly, if $k > \ell$ the bias term will be positive, and if $k < \ell$ the bias term will be negative.[66]

G.2.1 Random walk earnings process

Using the model above, assume that the earnings process consists of a fixed effect θ_i and a random walk error term $\eta_{i,t}$. Similarly, assume that the shock to the eviction outcome depends on the individual fixed effect and the innovation to earnings at time $t = 0$:

$$\begin{aligned}
\nu_{i,t} &= \theta_i + \eta_{i,t} \\
\eta_{i,t} &= \eta_{i,t-1} + \eta^*_{i,t} \\
\varepsilon_i &= \alpha_1 \theta_i + \alpha_2 \eta_{i,0} + \varepsilon^*_i,
\end{aligned}$$

where $\eta^*_{i,t}$ and $\eta^*_{i,t}$ are idiosyncratic shocks. Under this setting,

$$\begin{aligned}
cov(\nu_{i,k}, \varepsilon_i) &= cov(\theta_i + \eta_{i,k}, \alpha_1 \theta_i + \alpha_2 \eta_{i,0} + \varepsilon^*_i) \\
&= \alpha_1^2 var(\theta_i) + \alpha_2 cov(\eta_{i,k}, \eta_{i,0}) \\
&= \alpha_1^2 var(\theta_i) + \alpha_2 var(\eta_{i,0}).
\end{aligned}$$

Similarly,

$$\begin{aligned}
cov(\nu_{i,-\ell}, \varepsilon_i) &= \alpha_1^2 var(\theta_i) + \alpha_2 cov(\eta_{i,-\ell}, \eta_{i,0}) \\
&= \alpha_1^2 var(\theta_i) + \alpha_2 var(\eta_{i,-\ell}).
\end{aligned}$$

Given the assumed random walk, the variance of $\eta_{i,t}$ will be increasing in t. Plugging into the bias term, the sign of the bias depends on the sign of $var(\eta_{i,0}) - var(\eta_{i,-\ell})$, which will be negative unless $\ell = 0$. Thus, the bias will be negative assuming that $\alpha_2 < 0$ or $\ell = 0$, and the bias will increase as ℓ increases.

[66] If we do not assume that $var(\eta_{i,\ell}) = var(\eta_{i,k}) \ \forall \ \ell, k$, but rather assume that $var(\eta_{i,\ell}) < var(\eta_{i,k})$ if $k > \ell$, then even a symmetric difference-in-differences would be downward biased assuming α_2 is negative.