Eviction and Poverty in American Cities *

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July 2022

More than two million U.S. households have an eviction case filed against them each year. Policymakers at the federal, state, and local levels are increasingly pursuing policies to reduce the number of evictions, citing harm to tenants and high public expenditures related to homelessness. We study the consequences of eviction for tenants using newly linked administrative data from two large 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 problem, we use an instrumental variables approach based on cases randomly assigned to judges of varying leniency. We find that an eviction order increases homelessness, and reduces earnings, durable consumption, and access to credit. 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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*Part of this research was conducted as a part of the U.S. Census Bureau's Evidence Building Project Series. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau's Disclosure Review Board and Disclosure Avoidance Officers have reviewed this information product for unauthorized disclosure of confidential information and have approved the disclosure avoidance practices applied to this release. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2476 (CBDRB-FY22-072). This research uses data from the Census Bureau's Longitudinal Employer Household Dynamics Program, which was partially supported by the following National Science Foundation Grants SES-9978093, SES-0339191 and ITR-0427889; National Institute on Aging Grant AG018854; and grants from the Alfred P. Sloan Foundation. This publication was produced from raw data purchased from or provided by the New York State Department of Health (NYSDOH). However, the conclusions derived, and views expressed herein are those of the author(s) and do not reflect the conclusions or views of NYSDOH. NYSDOH, its employees, officers, and agents make no representation, warranty or guarantee as to the accuracy, completeness, currency, or suitability of the information provided here. The New York portion of this research was conducted at the Center for Innovation Through Data Intelligence (CIDI). The views expressed here are not those of CIDI, the Office of Court Administration, the New York City Human Resource Administration, the Federal Reserve Bank of Philadelphia, or the Federal Reserve System. The authors gratefully acknowledge financial support from the National Science Foundation (SES-1757112, SES-1757186, SES-1757187), the Laura and John Arnold Foundation, the Spencer Foundation, the Kreisman Initiative on Housing Law and Policy, the Horowitz Foundation for Social Policy, the Robert Wood Johnson Foundation, the Becker Friedman Institute, and the Tobin Center for Economic Policy. We would like to thank Lawrence Wood and others at the Legal Assistance Foundation in Chicago, Melissa C. Chiu and Kathryn Mcnamara at the U.S. Census Bureau, Lydia Stazen Michael at All Chicago, Carmelo Barbaro, Ruth Coffman, and Emily Metz at UChicago Urban Labs, Eileen Johns and Maryanne Schretzman at CIDI, Joe Altonji, Raj Chetty, Eric Chyn, Kevin Corinth, Ingrid Gould Ellen, Angela Denis Pagliero, Michael Dinerstein, Rashmi Dyal-Chand, Bill Evans, Alex Frankel, Peter Ganong, Pieter Gautier, Matt Gentzkow, Michael Greenstone, Daniel Grossman, Jim Heckman, Tatiana Homonoff, Ali Hortaçsu, Peter Hull, Louis Kaplow, Ezra Karger, Paymon Khorrami, Thibaut Lamadon, Kevin Lang, Jeff Lin, Maarten Lindeboom, Hamish Low, Jens Ludwig, Sarah Miller, Magne Mogstad, Derek Neal, Matt Notowidigdo, Dan O'Flaherty, Ed Olsen, Katherine O'Regan, Francisca Richter, Azeem Shaikh, Beth Shinn, Jeff Smith, Jim Sullivan, Nicole Summers, Chris Taber, Alex Torgovitsky, Bas van der Klaauw, Laura Wherry, and many seminar participants for helpful discussion. Isabel Almazan, Iliana Cabral, Ella Deeken, Deniz Dutz, Katherine Kwok, and Naomi Shimberg provided excellent research assistance. Any errors are our own. This article subsumes and replaces our previous work: Collinson and Reed (2019) and Humphries et al. (2019).

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1 Introduction

More than two million eviction court cases are filed in the United States each year. These cases predominantly involve low-income and minority households. About half of proceedings end in a court order for eviction: a judgment requiring the tenant to vacate the property.[1] According to data collected by the OECD, the U.S. is an outlier in the number of eviction cases per renter household, with a rate 1.5 times higher than the next-highest country (Canada) and at least 3.8 times higher than the remaining ten countries for which data are available (OECD, 2020). In recent years, policymakers at the federal, state, and local levels have introduced assistance programs and legislative changes aimed at reducing the number of evictions, frequently citing harms to tenants and the high costs of homelessness-related public services.[2] Measuring the consequences of an eviction for tenants is crucial for evaluating these reforms and, more broadly, for understanding the role of housing instability as a driver of poverty and inequalities in income, socioeconomic mobility, and health that have been documented in recent literature (Piketty and Saez, 2003; Chetty et al., 2014; Case and Deaton, 2015).

Despite the large number of tenants who interact with housing courts in the U.S. each year and the growing interest from policymakers, the consequences of eviction for households are not well documented or understood. While researchers have argued that eviction is a cause of poverty, homelessness, poor health, and other forms of physical and material hardship (e.g., Desmond, 2012, 2016), quantitative empirical research in this area has been hampered by two main challenges. First, it is difficult to link data on households facing eviction to data on their subsequent outcomes. Second, it is not obvious how to separate the impact of eviction from the impact of correlated sources of distress such as job loss or declining health. This paper overcomes both of these barriers to provide new evidence on the effect of eviction on earnings, employment, residential mobility, interactions with homelessness services, financial distress, and health. We link newly constructed data sets based on housing court records from two large urban areas—New York City, NY, and Cook County, IL (which includes the City of Chicago)—to a broad range of administrative data sets. 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 who

[1] Based on the most complete data set of eviction court cases available, the Princeton Eviction Lab estimates that more than two million cases were filed each year since 2002, and about one million cases ended in an eviction order annually (Desmond et al., 2018a). Since this data set does not have national coverage, these numbers are conservative. An alternative data point can be obtained from the 2017 American Housing Survey, in which about 800,000 renter households reported being threatened with an eviction notice in the past three months, which extrapolates to 3.2 million over the year (U.S. Census Bureau, 2017).

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

systematically vary in their tendency to evict.[3]

We first show that tenants in our linked housing court sample differ substantially from randomly-chosen tenants who live in the same neighborhoods. Compared to these neighbors, tenants we observe in housing court have lower earnings, lower employment, less access to credit, and more debt in collections. In addition, both evicted and non-evicted tenants experience striking drops in earnings, employment, and credit scores and rising hospital visits, unpaid bills, and payday loan inquiries in the two years before the case. These "Ashenfelter dips" are more pronounced for evicted tenants and suggest the presence of unobserved factors that are correlated with both the eviction decision and post-court outcomes, and are likely to introduce bias in estimates based on cross-sectional or difference-in-differences comparisons. For this reason, our main estimates are based on a quasi-experimental IV research design using the random assignment of judges.

Using the IV approach, we find that eviction causes spikes in homelessness and increases in residential mobility. In the first year after case filing, an eviction order increases the probability of observing the tenant at a new address by 8 percentage points (28% of the non-evicted mean) and increases the probability of staying in emergency shelters by 3.4 percentage points (more than 300% of the non-evicted mean). The effects on residential mobility and homelessness persist through the second year after filing. These increases in housing instability do not result in large changes in neighborhood quality: after the court case, evicted tenants live in neighborhoods with similar poverty rates as tenants who are not evicted.

During this period of increased housing instability and homelessness, eviction negatively impacts earnings. Our IV estimates imply that eviction lowers earnings in the year after filing by \$323 per quarter (8% of the non-evicted mean), which is similar to evicted tenants' average drop in quarterly earnings in the year leading up to case filing (\$337). The impact on earnings is larger in the second year after the case, with eviction causing a \$613 (14%) reduction in quarterly earnings. The effects on employment are more modest, with eviction causing a 1.5 percentage point reduction in the fraction of quarters employed in the year after the case, and a 1.8pp reduction two years after the case, neither of which is statistically significant. The labor market effects of eviction are largely concentrated in the two years after filing. We find particularly sharp negative impacts for female and Black tenants, who drive the effects on labor market outcomes, residential mobility, and interactions with homelessness services. This pattern is consistent with ethnographic research that suggests eviction may have a larger impact on women (Desmond, 2012; Desmond et al., 2013; Desmond, 2016) and with research that finds that Black households experience discrimination while searching for housing (Bayer et al., 2017; Christiansen and Timmins, 2019).

[3]Many papers have used the random assignment of judges to study the impact of court orders in other settings, including incarceration (Kling, 2006; Aizer and Doyle, 2015; Mueller-Smith, 2015; Bhuller et al., 2018, 2020; Norris et al., 2021), 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).

Eviction also worsens financial health and credit access beyond the initial period of increased housing instability and homelessness. Using data from linked credit reports, we find that eviction causes reductions in a composite index of financial health of roughly 0.1 s.d. in the first and second years after the case filing, by 0.21 s.d. 3-4 years after filing, and by 0.26 s.d. 5-6 years after filing. The declines are driven by increases in debt and lower credit scores.[4] We find evidence that eviction reduces the likelihood of having an automobile loan or lease, which may be viewed as a proxy for durable goods consumption (Dobkin et al., 2018; Agarwal et al., 2020). The impacts on credit scores of 16.5 points in the second year after the case are similar in magnitude to the effect of removing a bankruptcy flag (Gross et al., 2020; Dobbie et al., 2020).

Finally, we find that eviction increases the number of hospital visits in the year following court filing by 0.19 visits (29%) and increases visits for mental health-related conditions during the same period by 0.05 visits (133%). The timing of these effects coincides with the disruptions to tenants' housing circumstances in the year after filing.

Our analysis has important policy implications. First, we find that eviction causes significant disruptions that are reflected in increases in residential mobility, homelessness, and hospital use; reductions in earnings; and sustained damage to credit records. These costs are key inputs to the evaluation of a range of policies, such as emergency rental assistance, legal aid to tenants facing eviction, and, most directly, making eviction proceedings more lenient toward tenants.[5] Given the large social costs of homelessness (Evans et al., 2019), our finding that a court-ordered eviction increases the likelihood of emergency shelter use suggests a role for policy in the eviction court setting to reduce homelessness. Second, we show that eviction is frequently preceded by adverse events, which may reflect the inadequacy of existing social insurance policies or self-insurance in preventing evictions. Third, we find that the effects of eviction are driven by traditionally vulnerable groups: Black and female tenants. Since these groups also tend to be over-represented in eviction proceedings, policies aimed at averting eviction may especially benefit them.

This paper is related to a sizeable literature in sociology that studies eviction of low-income renters (Desmond, 2012; Desmond et al., 2015; Desmond and Gershenson, 2016; Desmond, 2016; Desmond and Gershenson, 2017). Our work builds on and extends this literature in several ways. First, we show that the research designs used in previous work on evictions may be vulnerable to selection bias. Second, to address this selection bias, we use a quasi-experimental research design to estimate the causal effects of eviction by leveraging the random assignment of judges to eviction cases. Third, we create a novel data set of

[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] Such policy reforms may also impact landlords, which could have consequences for rental housing supply, rent prices, and screening practices. A full welfare analysis of policy counterfactuals is beyond the scope of this paper.

eviction court records linked to administrative data, which helps mitigate the concerns that may arise when using survey data, including selective non-response and misreporting (Meyer et al., 2015). The linked data additionally lets us characterize tenants' housing, labor market, health, and credit circumstances in the lead-up to and aftermath of filing. Finally, we provide a unified analysis across two large U.S. urban areas not previously studied using more than a decade of administrative data, lending support to the external validity of our findings.

We examine the impact of eviction on earnings, homelessness, and financial health, outcomes that have not been studied in prior work. We find that eviction causes increases in homelessness and reduces earnings in the two years after the case filing, and leads to longer-run deterioration in financial health. Prior studies have examined the impact of eviction on loss of employment (Desmond and Gershenson, 2016), mental health (Desmond and Kimbro, 2015), and moves to high-poverty neighborhoods (Desmond and Shollenberger, 2015). Relative to these studies, we find more modest impacts of eviction on employment and no impact on the poverty rate of neighborhoods to which evicted tenants move, using our quasi-experimental research design. Taken together, our results imply that eviction exacerbates the rising economic distress experienced by tenants in the lead-up to a court filing, creating disruption for tenants and spillover costs to society.

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. (2020) 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. (2020) 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.[6] We consider several additional dimensions that eviction is likely to impact, including employment, earnings, homelessness, and hospital use.

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 (2017) find that family size, job loss, neighborhood crime, eviction rates, and network disadvantage are additional risk factors. Kroeger and La Mattina (2020) find that

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

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 U.K. and find that these substantially increased rental 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 describes the data collection and record linkage processes. Section 4 describes our samples, 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, as in most jurisdictions, the housing court process begins with a notice served to the tenant by the landlord, followed by one or more court hearings, and finally a judge's decision on whether to issue an eviction order that requires the tenant to vacate the property.

A landlord must serve the tenant a written notice to begin the eviction court process. The notice typically includes the reason for terminating the lease and the number of days until termination. A landlord may seek an eviction for any alleged violation of the lease terms, and non-payment of rent is the most commonly-stated reason.[7] In both Cook County and New York, the landlord has no discretion over the district that will handle their case, since the district is determined by the address of the property under dispute. As we discuss below, cases in both jurisdictions are randomly assigned to courtrooms, with judges assigned to courtrooms on a fixed rotational basis.

Nearly all eviction cases are handled in a resolution process overseen by a judge.[8] When the landlord and tenant meet in a courtroom, the hearing is typically brief: court observation studies have found that the average eviction hearing lasts only a few minutes (Doran et al., 2003). Tenants are usually unrepresented, while landlords are usually represented by an attorney.[9]

[7] 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 to pay rent. In Cook County and New York, over three quarters of cases involve disputes over non-payment of rent, and studies of housing court in other cities, e.g., Milwaukee (Desmond et al., 2013), have also found that non-payment of rent is the most commonly stated reason for eviction.

[8] In principle, either party may request a jury trial but, in our court records, such requests are made in only 3 percent of Cook County cases and less than 1 percent of New York cases.

[9] In our data, approximately 3 percent of tenants in Cook County and 1 percent of tenants in New York were represented by an attorney, whereas 75 percent of landlords in Cook County and 95 to 99 percent of landlords in New York were represented by an attorney.

In order to proceed with an eviction, the landlord needs a court order that authorizes the enforcement agent, such as a Sheriff or Marshal, to execute the eviction order. In both jurisdictions, we define an eviction as a case ending with an eviction order. This definition is based on whether the last recorded outcome in the case history provides legal authority for the landlord to take possession of the property via an enforcement agent.[10] Appendix C.3 explains in more detail how we construct eviction orders from the housing court data. In cases where the landlord is seeking rental arrears, the judge may include an order to pay rental arrears along with the eviction order, called a money judgment.

The alternative to an eviction order is often a formal agreement between the 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 an initial agreement. Cases can also be discontinued, which happens if the landlord decides not to pursue the case further. Only five percent of non-evictions are dismissals that bar the landlord from bringing another eviction case with the same allegations against the tenant.

An eviction order may or may not be followed by the execution of the order by an enforcement officer such as a Sheriff or Marshal. We refer to the execution of an eviction order as an enforcement, and it typically involves changing the locks and the removal of the tenant's possessions. Whether an eviction order is enforced depends on several factors. For example, the landlord may choose not to file the order with the enforcement agent 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 enforce the eviction order, in which case the landlord may cancel the enforcement of the order.

There are several reasons an eviction order may affect tenants' future outcomes. First, an eviction order legally obligates a tenant to move, either following or in anticipation of the enforcement of the order, and thus 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, in cases where the landlord seeks a money judgment, an eviction order will typically include a money judgement, which can be used by the landlord to obtain an order for garnishment of wages, tax refunds, or other assets. Garnishment requires a separate court process and is rare in practice. See Appendix B for additional institutional

[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.

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

details.

Cook County. Roughly 33,000 eviction cases are filed in Cook County each year. These are handled by the Forcible Entry and Detainer Section of the Circuit Court of Cook County. Roughly 80 percent of Cook County cases are joint action cases, which are cases where the landlord is seeking payment of rental arrears in addition to possession of the property. The remaining 20 percent of cases are single action cases, where the landlord is only seeking possession of the property. The court divides the county into six districts, each with its own courthouse, eviction courtrooms, and eviction case judges. Landlords must file eviction cases in the district in which the property is located. 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.

Eviction cases are randomly assigned to courtrooms within a district 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.

Approximately 65 percent of eviction cases in Cook County end with an eviction order. We estimate the share of non-evicted cases with a formal agreement to be upwards of 39 percent.[12] Around 45 percent of cases without an eviction order in Cook County are discontinued, and roughly 5 percent are dismissed. The Cook County Sheriff's Office executes about 26 percent of cases ending with an eviction order.[13]

New York City. Each year, around 240,000 cases are filed in housing court in New York. The Civil Court of New York City, part of the state Unified Court System, oversees the New York City Housing Court. Housing Court hears cases involving landlord-tenant disputes or housing code violations. Cases are handled by seven courthouses: one for each county (borough) in New York City (Bronx, Kings, New York, Queens, and Richmond) and two smaller, specialized courts in Harlem and Red Hook. The courthouse is determined by location of the filing address. The vast majority of eviction cases heard in housing court are non-payment filings (86 percent), with the remaining being other lease violation disputes known as "holdover" cases (14 percent).

Cases are randomly assigned to courtrooms by the Housing Court Information System (HCIS) computers within the courthouse of the assigned case. Judges rotate through courtrooms for year-long terms on a predetermined rotation system. Cases are assigned to courtrooms rather than judges, and therefore if the judge rotates out of a courtroom during

[12] The electronic court record, 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 non-evicted cases.

[13] Enforcement 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.

an active case, the case will remain in the assigned courtroom. Some types of cases, such as those involving the public housing authority, are not randomly assigned to courtrooms, and we exclude these from the analysis. For details, see Appendix C.2.

In New York, about 35 percent of nonpayment cases end with an eviction order. Among those ending without an eviction order, approximately 64 percent end with a settlement agreement, 29 percent are discontinued, and 5 percent are dismissed. The enforcement of an eviction order is conducted by a City Marshal. In our data, 31 percent of cases ending with an eviction order in New York result in an enforcement of the order conducted by a City Marshal.

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, interactions with the homelessness services system, and credit bureau records. We additionally link the New York court data to records of hospital visits. This section summarizes our data sources, sample construction, data linkage, and main outcomes. We provide additional details in Appendix C.

3.1 Court data

Our linked data sets are based on the near-universes 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 the case-individual, so that each tenant who appears as a defendant in the case will have a separate record.[14] Other key elements we observe in the court records are case type, 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.[15] We also observe other judge decisions throughout the case, such as whether to grant a continuance in the case or a stay of the eviction order. We define an eviction as a case ending with an eviction order.[16]

While the data are similar across our two settings, there are a couple of differences to note. In Cook County, the data include the value of any money judgment awarded and the

[14]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.

[15]In Cook County, the case types are single action and joint action, and in New York, the case types are holdover and nonpayment.

[16]For a subset of years, we also link court records to data held by the Sheriff's office (Cook County) or Marshal's office (New York) so that we know whether an eviction order is enforced. The New York court data also contain information on enforced orders, which we validate with records of enforcement by City Marshals from the Department of Investigations.

name of the judge associated with each action in the court record, but we do not observe either in New York.

3.1.1 Sample restrictions

We impose several restrictions on our court samples. In both locations, we drop eviction cases associated with businesses, cases associated with condominiums, cases with a missing defendant name or address, cases involving more than $100,000 in claimed damages, and cases filed during a week in which only a single judge (courtroom in New York) was hearing cases. These sample restrictions are necessary to focus our analysis on residential eviction cases involving renters where we can link to outcomes and construct the instrument. We also restrict the sample to cases in which the judge (courtroom in New York) presided over a minimum number of cases during the year: 100 in Cook County and 500 in New York. This restriction removes judges/courtrooms that hear substantially fewer cases than is typical in the setting, which removes noise in the instrument.[17]

In New York, some cases are not randomly assigned to courtrooms: cases involving public housing units, cases involving co-ops or condominiums, cases assigned based on zip code through several policy initiatives, cases for family members of active military personnel, and cases involving the District Attorney's office or the New York City Police Department. We can identify these cases directly in the New York courts data and drop them from our sample.

The court sample includes around 414,000 cases for Cook County and 580,000 cases for New York before linking to outcomes data. Appendix Tables C.1 and C.2 describe how sample counts change with these restrictions in Cook County and New York, respectively.

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 study in our analysis. We separately analyze linked records for Cook County and New York because of data security restrictions.[18] 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 wage earnings, and our main employment outcome is an indicator for positive earnings. 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

[17] In Appendix G.1, we show that our first stage is robust to different choices of sample restrictions.

[18] Due to restrictions in the data sharing agreement with the New York courts system, we were unable to bring the New York courts data into the Census Bureau RDC for analysis.

areas we study. Employment and earnings records only cover formal employment and exclude individuals not covered by UI benefits, such as the self-employed.

The UI records for New York are from the New York State Department of Labor and do not include states other than New York. They cover the years 2004 to 2016. The UI records for Cook County are 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 data.[19] The years available vary by state, but all states have data from 1995 to 2014.[20]

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 identifiers by year.[21] We use the MAFARF to build an indicator for the tenant being observed at the filing address in each time period. While the data are rarely missing, some individuals do not have an address listed in certain years. Appendix Figure E.1 plots the proportion of evicted and non-evicted tenants with address information each year, relative to the filing year. Evicted tenants are somewhat less likely to have a reported address, and this difference grows moderately after the eviction case is filed. When studying mobility outcomes below, we additionally report and discuss robustness results based on how these missing observations are handled.

In New York, we combine two sources of address histories: consumer reference data from Infutor Data Solutions and administrative benefits records.[22] Similar to the Cook County

[19]The eleven LEHD "Option A" states are: Arizona, Arkansas, Delaware, Indiana, Iowa, Kansas, Maine, Maryland, Nevada, Oklahoma, and Tennessee.

[20]For Cook County, 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 earnings are missing. In Appendix I we provide additional evidence on how eviction affects migration out of state and migration out of the 13 states for which we observe LEHD earnings. For New York, out-of-state earnings are not observed and therefore if a person moves or works out of state and has no in-state earnings they would be recorded as having zero earnings in the data.

[21]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).

[22]Infutor compiles data from several sources including public and private telephone billing data, deed and

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 the New York 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. Appendix Table I.3 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.

Measuring address-level moves at an annual frequency in the U.S. is challenging, and particularly so for our population of unstably-housed tenants, yet we believe these administrative data sets provide the best measures available.

Using the address data described above, we additionally link to neighborhood poverty rates. In Cook County, we use census tract-level neighborhood poverty rates constructed from restricted-access American Community Surveys (ACS) from 2005 to 2018, based on five-year moving averages. In New York, we use the publicly available census tract five-year estimates from the ACS 2006-2010.

Homelessness. We measure interactions with homelessness services in both settings using local Homeless Management Information System (HMIS) data.[23] 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: an indicator for the individual staying in an emergency shelter, and an indicator for the individual interacting with any homelessness services. In Cook County, homelessness services include emergency shelter use, permanent supportive housing, coordinated assessment of need, rapid rehousing, transitional housing, and street outreach. In New York, this indicator additionally includes applications to shelter,

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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 continue 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.

[23]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.

which cover instances where families are diverted or deemed ineligible.[24]

Financial health. We measure financial health with credit records from Experian, one of the three major credit bureaus in the United States.[25] 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, the linked credit report data are quarterly snapshots from June 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 currently either 30 days or more delinquent or in collections, where the latter are balances that the lender turns 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 has been used as a proxy for durable goods consumption (Dobkin et al., 2018; Agarwal et al., 2020). 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 credit.

As a summary measure, we create an index of financial health based on the credit bureau variables described above and following the approach of Dobbie et al. (2017). Each component of the index is standardized based on the non-evicted mean and standard deviation in the filing year. We then sum the standardized components, with the indicator for no revolving credit and the amount of unpaid bills entering the index negatively, so that all components can be viewed as contributing to financial health. We then re-standardize the index based on the mean and standard deviation of the index for the non-evicted group in the filing year. Lastly, we observe payday loan account inquiries and borrowing for individuals in Cook County, which includes both online and storefront loans. The majority of these loans are originated online. We describe the payday loans data in detail and present the analysis in Appendix C.7.

Health. For New York, we also 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.[26] 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 (non-pregnancy-related) hospital visits, including inpatient or outpatient visits, the total

[24] 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.

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

[26] 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 fulfillment.

number of emergency department visits, and the total number of hospital visits for mental health conditions.[27]

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).[28] 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 for the years 2004 to 2016. The data include individuals receiving Medicaid, the Supplemental Nutrition Assistance Program (SNAP), Temporary Assistance for Needy Families (TANF), or other city-specific cash subsidies. Appendix D.2 describes this process in detail. The data have personal identifiers, including Social Security Number (SSN) and date of birth, that we use to link individuals to the outcomes data. The data also include 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 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 court records to measures of financial health from Experian credit reports. This linkage yields match rates of 61.3 percent in Cook County and 68 percent in New York, which are comparable to match rates in previous studies that link data to records from the major credit bureaus.[29] The linked credit sample consists only of individuals who have a credit record. In low-income neighborhoods, more than 70 percent of all adults have credit records (Brevoort et al., 2015).

Appendix D compares the court record populations in Cook County and New York to the

[27] 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.4 provides the category labels associated with these codes.

[28] 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).

[29] 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. The linked data used to study the Oregon Health Experiment have a match rate of 68.5 percent (Finkelstein et al., 2012).

sub-populations successfully linked to outcomes and also examines court record characteristics predictive of a match. More disadvantaged tenants (those without legal representation or those evicted) are somewhat less likely to be linked to Census records in Cook County and slightly more likely to be linked to benefits data in New York. The pattern is similar for links to Experian data in Cook County yet the opposite for links to Experian data in New York. These patterns will not affect the internal validity of our results since, conditional on linking to outcomes, the baseline characteristics of the case and tenant are not predictive of judge stringency, as we show in Section 5.

Appendix D also studies the relationship between judge stringency and the probability of being linked to outcome data. We find that in three out of the four analysis samples, judge stringency is uncorrelated with the probability that a case is linked to outcomes. The exception is the Cook County linked Census sample, which has an economically small but statistically significant relationship between stringency and the probability of being assigned a PIK (Appendix Table D.1). Moving from the 10th percentile of stringency to the 90th percentile of stringency—a 7 percentage point difference—is associated with only a -0.38 percentage point reduction in the likelihood of having a PIK ($-.054 \times .07$, using the estimate from column 2 in Appendix Table D.1). This correlation likely arises due to the Census linkage process, which may incorporate post-filing information that is impacted by eviction.[30] We emphasize that the correlation between stringency and the probability of having a Census PIK does not threaten the internal validity of our estimates because conditional on having a PIK, judge stringency is unrelated to individual and case characteristics, which we discuss below and show in Table 3. We also show in Appendix Table G.4 that stringency is uncorrelated with lagged values of all our outcomes that are linked using Census PIKs. Lastly, there is no relationship between stringency and being linked to the New York benefits sample, which yields a similar pattern of results to the Cook County linked Census sample, suggesting differences in PIK rates are not driving the effects we document in Section 6.

4 Trends and evidence of selection

This section provides new descriptive facts about the demographics, earnings, employment, housing, health, and financial circumstances of tenants in housing court, based on the linked panel data described in the previous section. We show that, while evictions primarily occur in neighborhoods with high poverty rates, tenants in our linked housing court sample are also negatively selected on pre-court earnings and employment relative to randomly-chosen renters who live in the same neighborhoods. Within housing court, we also find notable differences between evicted and non-evicted tenants. These differences show up in both levels and trends leading up to the moment the case is filed for nearly all outcomes considered, suggesting

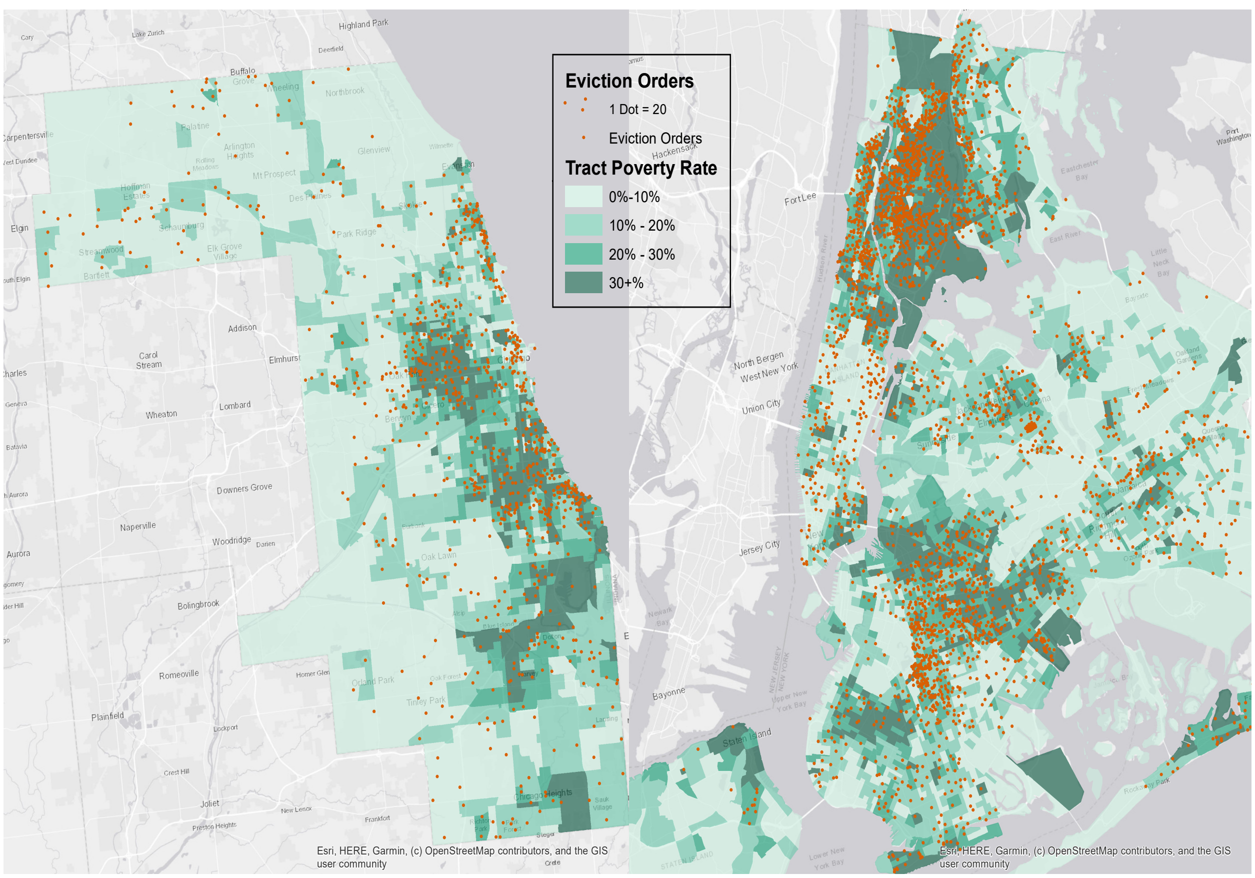
[30] We are unable to impose restrictions on how the Census Bureau assigns PIKs, such as requiring the linkage to use pre-filing information only, as we do in constructing the credit bureau samples and the New York benefits sample.

the presence of unobserved factors that are correlated with both the eviction decision and post-court outcomes. This motivates our IV research design described in Section 5.

4.1 Tenants in housing court

Figure 1 maps the location of evictions in 2010 by census tract for Cook County and New York, together with tract-level poverty rates. While evictions occur throughout both areas, 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 Kimbro (2015), and Desmond and Gershenson (2017), who find that eviction is common in poor communities in Milwaukee. Appendix Figure B.1 shows how eviction filing rates (the number of evictions filed relative to the number of occupied rental units) 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.

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).

While evictions primarily occur in neighborhoods with high poverty rates, tenants in our linked housing court sample are also negatively selected on pre-court earnings and employment relative to randomly-chosen tenants who live in the same neighborhoods. Table 1 shows descriptive statistics for three groups: evicted tenants, non-evicted tenants with a case filed in housing court, and ACS respondents who are renters, weighted so the distribution of their neighborhoods matches the distribution of neighborhoods for tenants in our linked sample. Relative to renters from the same neighborhoods, tenants in the linked sample have lower levels of earnings and employment than renters from the same neighborhoods. *Within* housing court, differences persist, with evicted tenants showing lower levels of earnings and employment than non-evicted tenants. For example, in Cook County, average quarterly earnings in the eight quarters before case filing are \$4,876 for non-evicted tenants and \$3,907 for evicted tenants, and in New York these numbers are \$3,628 and \$3,080, respectively.[31]

Table 1 further shows that, relative to renters from the same neighborhoods, both evicted and non-evicted tenants are more likely to be female (62 vs. 54 percent in Cook County and 71 vs. 54 percent in New York) and more likely to be Black (68 vs. 47 percent in Cook County and 58 vs. 35 percent in New York). Hispanic tenants are under-represented in our linked housing court sample in Cook County (12 vs. 20 percent), but over-represented in NYC (46 vs. 39 percent). By contrast, the demographic characteristics of evicted and non-evicted tenants within housing court are similar.

The bottom panel of Table 1 displays case characteristics. The average ad damnum amount—the judgment amount the landlord is seeking from the court—for evicted tenants is around \$2,000 in Cook County and \$4,600 in New York, both of which are a few hundred dollars more than for non-evicted tenants. 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 somewhat more similar in these respects (53 vs. 54 percent have no prior case and nearly all tenants in NYC are unrepresented at the time of the initial hearing).[32]

[31] The lower earnings levels in New York relative to Cook County reflect that the New York sample is restricted to those with some pre-filing benefits receipt.

[32] In New York, we observe if a tenant is self-represented at the time of the first appearance in court and these summary statistics may understate the level of representation if some tenants pursue representation after their initial hearing.

Table 1: Linked Sample Summary Statistics

	Cook County			New York		
	Evicted	Not Evicted	Renters from same neighbor-hoods	Evicted	Not Evicted	Renters from same neighbor-hoods
	(1)	(2)	(3)	(4)	(5)	(6)
Individual characteristics:						
Age	37.51	37.44	33.98	38.55	40.49	34.98
	(10.35)	(10.22)	(10.26)	(9.50)	(9.22)	(10.51)
Female	0.62	0.62	0.54	0.71	0.74	0.54
	(0.49)	(0.49)	(0.50)	(0.46)	(0.44)	(0.50)
Black	0.69	0.66	0.47	0.58	0.58	0.35
	(0.46)	(0.47)	(0.46)	(0.49)	(0.49)	(0.43)
Hispanic	0.12	0.11	0.20	0.46	0.47	0.39
	(0.32)	(0.31)	(0.42)	(0.50)	(0.50)	(0.47)
Quarterly earnings	3,907	4,876	6,237	3,080	3,628	7,059
	(4,636)	(5,561)	(9,251)	(4,066)	(4,471)	(14,987)
Employment	0.58	0.61	0.72	0.47	0.51	0.70
	(0.46)	(0.47)	(0.43)	(0.42)	(0.44)	(0.45)
Neighborhood poverty rate (5 yr avg)	0.21	0.20	0.20	0.29	0.29	0.24
	(0.13)	(0.13)	(0.09)	(0.12)	(0.12)	(0.10)
Neighborhood median rent (5 yr avg)	762	788	1,045	973	962	1,163
	(195)	(229)	(159)	(199)	(214)	(327)
Case characteristics:						
Ad damnum (1000s)	2.01	1.74		4.60	4.16	
	(2.99)	(3.09)		(27.79)	(31.45)	
No prior case	0.63	0.67		0.54	0.53	
	(0.48)	(0.47)		(0.50)	(0.50)	
Tenant without attorney	0.97	0.94		1.00	0.99	
	(0.16)	(0.23)		(0.07)	(0.10)	
Observations	193,000	108,000	36,559	87,294	70,474	103,614

Notes: The statistics in columns (1), (2), (4), and (5) are for the samples matched to earnings and employment records. Columns (1) and (4) include summary statistics for those in housing court who are evicted. Columns (2) and (5) include summary statistics for those in housing court who are not evicted. For these samples, quarterly earnings is the average quarterly earnings in quarters 1-8 before filing, and employment is the fraction of quarters with positive earnings in quarters 1-8 before filing. Columns (3) and (6) include summary statistics for renters aged 18-55 in the ACS PUMS 2006–2010, weighted to match the distribution of neighborhoods (Public Use Microdata Areas) for tenants who have housing court cases filed against them. For the ACS samples, quarterly earnings is the annual wage income divided by four, and employment 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-FY22-072.

4.2 Trends around court filing

We next plot the trends in our main outcomes for evicted and non-evicted tenants relative to the time the eviction case is filed. Figure 2 shows these trends for earnings, employment, residential mobility, neighborhood poverty, emergency shelter use, and use of homelessness services; Figure 3 shows these trends for financial outcomes; and Figure 4 shows these trends for health outcomes.

The figures are based on the regression

$$Y_{i,t} = \gamma_t + \alpha \times E_i + \sum_{r=S;r\neq O}^{F} \beta_r + \sum_{r=S;r\neq O}^{F} \delta_r \times E_i + \epsilon_{i,t}, \tag{4.1}$$

where r indexes time relative to the eviction filing, E_i is an indicator for the case ending in an eviction order, β_r are coefficients on indicators for time relative to the case filing, and δ_r are coefficients on indicators for relative time interacted with the eviction outcome. The only controls we include are calendar year dummies γ_t. The start and end periods are S and F, respectively, and O is the omitted period. We estimate equation 4.1 separately by location and present equal-weighted averages. Location-specific trends are presented in Appendix E. Figures 2-4 display regression estimates of β_r and $\alpha + \delta_r + \beta_r$, with the non-evicted group mean in the omitted period added to both sets of coefficients so that the magnitudes are easy to interpret. Since we add the mean in the omitted period, the levels in the figure are not sensitive to the choice of omitted period.

The top two panels of Figure 2 depict trends in quarterly earnings and employment—the result of estimating equation 4.1 between 16 quarters prior to filing and 24 quarters after filing. Both evicted and non-evicted groups show signs of declining earnings in the year prior to case filing. This decline is steeper for tenants who are evicted (-\$340) than for those who are not evicted (-\$157). Similarly, the probability of being employed for both evicted and non-evicted tenants declines in the year prior to filing, with the decline for evicted tenants more severe (-1.3pp) than for non-evicted tenants (-0.5pp). Following eviction, employment does not recover to its pre-filing peak over the next six years. There is a slight tapering of employment for the entire sample period after filing, which is not due to aging into retirement since our sample includes individuals between 18 and 55 years old at the time of the eviction filing.

Turning next to residential mobility, Figure 2, panel C shows the probability that we observe a tenant at an address different from the filing address. We study the same time window as for employment and earnings, now at the annual frequency that is imposed by the MAFARF. In the year of filing, 22 percent of tenants are observed at an address different from that recorded in the case. The fact that this estimate is not zero reflects moves in the year of filing as well as noise in the mobility data. The probability of observing a tenant at a new address increases to about 37 percent for the evicted group in the first year after filing and rises to 81 percent six years after filing. This probability rises faster for evicted than for non-evicted tenants, yielding a gap of about 16 percentage points six years after filing.[33] This gap may be an underestimate if evicted individuals are less likely to have updated addresses, which we find some evidence of in Appendix Figure E.1.[34] While evictions are associated with increased residential mobility, panel D shows that there is little change in the average neighborhood poverty rate before or after the case is filed.

One of the most striking results is that the use of homelessness services spikes in the year after filing, particularly for the evicted group (Figure 2, panel E).[35] The relative magnitudes of these increases are sizeable: for the evicted group, the probability of using homelessness services increases from 1.4 percent in the filing year to 7.1 percent in the first year after filing, an increase of approximately 400 percent. The non-evicted group also increases their use of homelessness services over the same period but the increase is smaller, from 1.3 percent to 1.9 percent. Panel F shows that this increase in use of homelessness services is primarily due to increased use of emergency shelters: for evicted tenants, the probability of using an emergency shelter jumps from 1 percent to 6 percent between the year of case filing and the following year.

We next examine trends in financial health, presented in Figure 3. We study trends between eight quarters prior and 20 quarters after the case filing because there are fewer years available in the credit bureau sample in New York. Mirroring the trends in earnings, the financial health index declines in the year prior to filing by roughly 0.067 s.d. for non-evicted tenants and 0.085 s.d. for evicted tenants. Looking at the index's components, credit scores fall, unpaid bills rise, and access to credit decreases in the year prior to filing.[36] These figures reveal that tenants facing eviction are financially distressed prior to court: they have low average credit scores and high levels of indebtedness in the years prior to housing court, and the mean tenant would be considered a subprime borrower. Following the eviction case, tenants have diminished financial health—including elevated indebtedness and diminished credit access—for several years regardless of the outcome of the court case. In the four years following the case, the financial health index does not return to its pre-filing peak for either group. The gap in financial health between evicted and non-evicted tenants also widens in the aftermath of eviction court, increasing from about -0.14 s.d. two years prior to the case

[33] High mobility among non-evicted tenants is consistent with Brummet and Reed (2021). Using linked Census Bureau microdata from the Census 2000 and American Community Surveys 2010–2014, they find 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.

[34] Appendix Figure E.1 shows that in Cook County, evicted tenants are around one to two percentage points less likely to be observed in the years prior to the case, with this gap growing to around 5 percentage points by three years after the filing. A similar check is not possible in New York because the sources of residential addresses only record address changes, and therefore we cannot distinguish between the tenant not moving and the lack of an updated address.

[35] For homelessness services, we study the period between one year prior and three years after filing, as the data are only available from 2014 to 2018 for Cook County.

[36] Appendix C.7 shows trends in payday loan inquiries for the Cook County sample, and shows rising demand for payday loans in the two years prior to filing.

to about -0.18 s.d. two quarters after the case (before tapering slightly over the next two years). While the gap in unpaid bills that arises immediately following the case closes by quarter 4, the gap in access to credit widens in the aftermath of the court case. The difference in the likelihood of having no source of revolving credit is about 3.7 percentage points four quarters before the case, rises to about 5.4 percentage points by one quarter after the case, and remains elevated through quarter 12.

Figure 4 shows trends in total hospital visits, total emergency room visits, and total hospital visits related to mental health in the New York sample. Panel A shows that total hospital visits increase in the two years leading up to the eviction filing and peak during the quarter of filing, 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. Panel B shows that the vast majority of these hospital visits are trips to the emergency room, while panel C shows that the total number of mental health-related hospital visits also increases during the period leading up to housing court. The gap between evicted and non-evicted tenants in hospital visits widens following eviction in all three panels.

4.3 Considerations for empirical design

The analysis up to this point has revealed patterns that are consistent with changes to pre-filing earnings, health, and financial circumstances being correlated with both the case filing and with receiving an eviction order. Evicted tenants have lower earnings, worse credit, and higher rates of hospitalization than non-evicted tenants several years before filing, and they experience sharper drops in earnings, and steeper jumps in unpaid bills and hospital visits in the immediate run-up to filing. This raises concerns about selection on correlated unobservables at both the filing and the eviction stage.

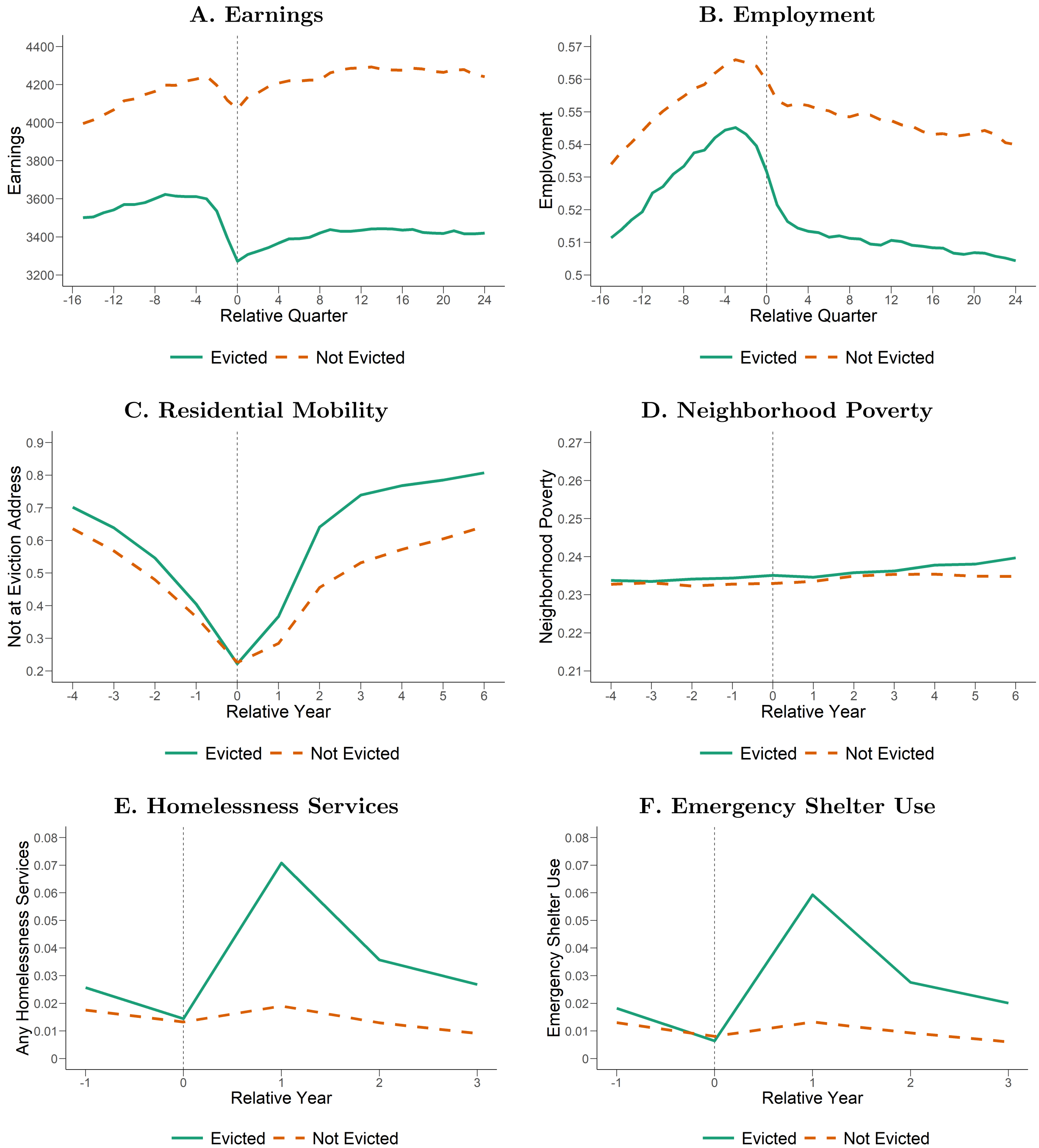
The presence of such correlated unobservables can bias frequently-used methods for identifying the effects of eviction, such as cross-sectional comparisons corrected only for observable characteristics, and difference-in-differences methods. We explore the potential bias of such methods using our data. We first examine what a simple demographic- and location-adjusted cross-sectional comparison of evicted tenants to renters outside of court would yield for the impacts of eviction on earnings. The result appears as the left-most bar in Appendix Figure F.1 and implies that eviction reduces average quarterly earnings by roughly \$1,600 in Cook County and \$1,100 in New York. Moving to a within-court comparison of evicted and not evicted tenants (middle bar), the estimates shrink by approximately one-third to \$1,000 in Cook County and \$600 in New York. This suggests that comparisons of tenants outside of court to those inside will likely overstate the effect of eviction because they will incorrectly attribute selection into court to the eviction itself, echoing similar patterns found in a different court setting by Aizer and Doyle (2015).

While the estimates shown in the second bar remove bias due to selection into court, they do not address bias stemming from the selection on levels or trends within court. Difference-in-differences (DiD) is a natural choice of method for addressing selection on levels. The third bar of Appendix Figure F.1 shows estimates from a DiD specification.[37] Adjusting for differences in levels between evicted and not-evicted in the lead-up to case filing shrinks the estimates further. However, DiD estimates are likely to still be biased due to the differential pre-trends among evicted and non-evicted tenants within court that we see in Figures 2, 3, and 4.

Whether DiD estimates are biased upward, biased downward, or unbiased will depend on properties of the data generating process. As a result, we would need to make assumptions about these properties to know whether the estimates in the third bar are biased, and in which direction. Rather than making such assumptions, we instead rely on our quasi-experimental instrumental variables research design, which we describe in the next section. This design addresses the sources of selection that we document above and allows us to identify a local average treatment effect of eviction.

[37] The DiD estimates reported in Appendix Figure F.1 are from a panel DiD specification with a symmetric base period and outcome window, which is described in more detail in Appendix J. Heckman and Robb (1985) show that under an (arguably strong) stationarity assumption, this symmetric DiD estimator is unbiased.

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

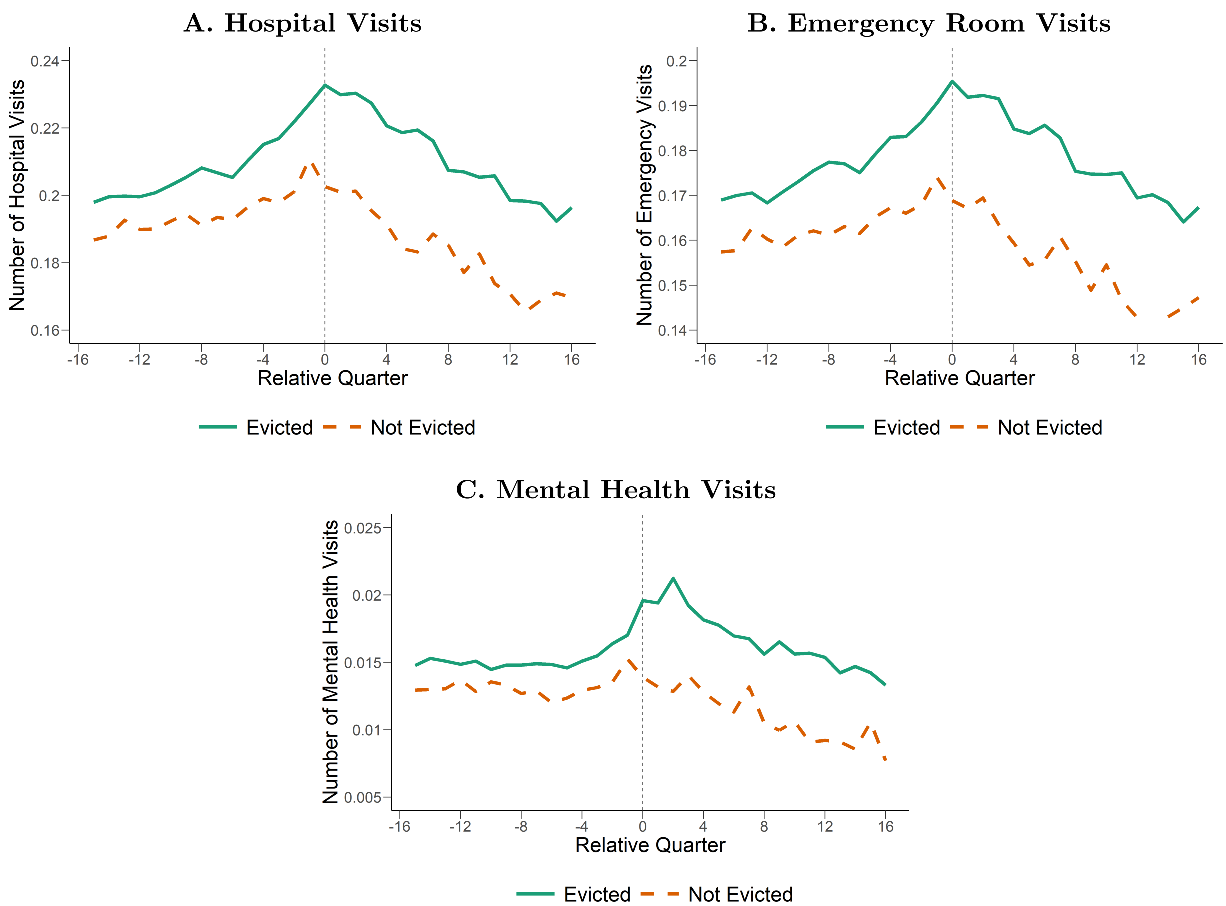


Notes: This figure shows trends in labor market and housing outcomes relative to eviction filing, combined across Cook County and New York. For each location, we estimate equation 4.1 and plot the equal-weighted average for the evicted and non-evicted groups in each time period. The only controls are calendar year dummies. 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. The employment and earnings outcomes are measured at a quarterly frequency, while the housing outcomes are measured at an annual frequency. Appendix E shows these trends by location. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

Figure 3: Financial Health Outcomes Relative to Time of Eviction Filing

Notes: This figure plots trends in credit report outcomes relative to eviction filing, combined across Cook County and New York. For each location, we estimate equation 4.1 and plot the equal-weighted average for the evicted and non-evicted groups in each time period. The only controls are calendar year dummies. 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. All outcomes are measured at a quarterly frequency. Appendix E shows results by location.

Figure 4: Health Outcomes Relative to Time of Eviction Filing (New York)



Notes: This figure shows trends in health outcomes relative to eviction filing in New York. We observe health outcomes in the New York sample only. We estimate equation 4.1 and plot results for the evicted and non-evicted groups in each time period. The only controls are calendar year dummies. 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. All outcomes are measured at a quarterly frequency.

5 Empirical framework

This section describes 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 provide tests of these assumptions. We also describe how we combine estimates across locations.

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 estimate causal effects of eviction. A common approach in court settings is to exploit the random assignment of cases to judges 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:

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

$$Y_i = \beta E_i + X_i' \delta + \nu_i \ , \tag{5.2}$$

where the least squares regression is run separately for each outcome and time period. Here E_i is an indicator for whether case-individual i has an eviction, Y_i is the observed outcome, and X_i is a set of controls for individual and case characteristics. Controls include court-by-year-quarter fixed effects, ad damnum amount, gender, race indicators, census tract poverty rates, census tract rent, a cubic in age at filing date, and indicators for missing controls.[38] Our main OLS and IV specifications include additional controls for lagged values of the dependent variable, which are described in the table notes. 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. In a

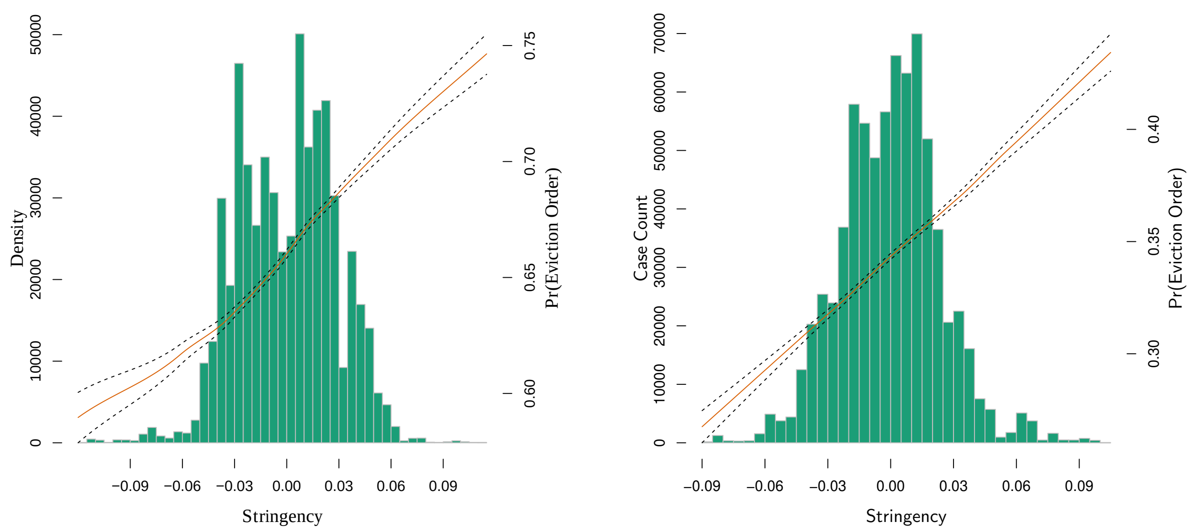
[38] The age, gender, and race controls are constructed using the Census Bureau Numident file and supplemented with the 2010 Decennial Census in Cook County, and using the administrative benefits data in New York. In the credit bureau samples, we omit race controls because of data use restrictions. Similarly, we do not observe gender in the New York credit sample, so we omit the gender control in the New York financial outcomes analysis.

typical year, there are 21 judges in Cook County and 29 courtrooms in New York hearing cases. Over our sample period, we observe 127 unique judges in Cook County. We construct the instrument from an average of 1,600 cases per judge (per year) in Cook County and 3,400 per courtroom (per year) in New York.

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 locations: 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.

Figure 5: Judge Stringency

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



Notes: For each location, this figure shows a histogram of judge stringency, residualized by court-year-quarter, with the number of cases indicated along the left vertical axis. Each panel also depicts fitted values from a local linear first-stage regression of eviction on judge stringency and court-year-quarter fixed effects (solid line, plotted along the right vertical axis). Dotted lines show 95 percent confidence intervals.

5.1.2 Validating the IV design

This section discusses 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.

Relevance. Columns 1 and 3 in Table 2 report first-stage estimates from equation 5.1 for Cook County and New York, respectively. Judge stringency has a large and statistically

Table 2: First Stage

	Cook County		New York	
	(1)	(2)	(3)	(4)
Judge stringency	0.741***	0.740***	0.831***	0.825***
	(0.025)	(0.024)	(0.057)	(0.057)
Controls	No	Yes	No	Yes
Observations	268,000	268,000	150,662	150,662

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This table reports results from the first stage regression of eviction on judge stringency, for Cook County and New York using the linked labor market sample. Columns (1) and (3) include our judge stringency measure with court-year-quarter fixed effects, but without individual controls. Columns (2) and (4) add controls. The additional controls include ad damnum amount, gender, race indicators, census tract poverty rate, census tract rent, a cubic in age at filing date, and indicators for missing controls. Appendix Table G.1 provides additional evidence on the robustness of the first-stage regression. 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 number CBDRB-FY22-072.

significant impact on evictions, with a partial F-statistic for judge stringency of 934 in Cook County and 288 in New York, relieving concerns about weak instruments. Columns (2) and (4) show that the first stage coefficients change very little when we include controls, consistent with judge stringency being uncorrelated with individual and case characteristics. Appendix Table G.2 additionally reports F-statistics for the Black and female subgroups that we also study in Section 6.

Appendix G.1 provides additional robustness checks on the first stage. We show that the first stage is robust to: (i) controlling for other dimensions of judge behavior, (ii) using an alternate approach to measuring the first judge or courtroom in the court records, and (iii) using different sample selection criteria.

Exogeneity. Table 3 shows the result of a standard balance test of random assignment. As we would expect, columns (1) and (3) show that case and tenant characteristics predict receiving an eviction order in both locations. Importantly, columns (2) and (4) show that these characteristics are not predictive of the stringency of the judge randomly assigned to the case. Only one of the seventeen coefficients in columns (2) and (4) is statistically significant and is quantitatively small (a -0.017 s.d. decrease in stringency for joint action cases). In addition, we fail to reject the null hypothesis that the coefficients are jointly equal to zero in both locations, consistent with random assignment. Appendix G.2 provides additional evidence that judge stringency is uncorrelated with lagged values of our key outcomes.

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 order but may also influence other aspects of the case such as the judgment amount (in cases in which the landlord is seeking rental arrears or damages) or whether or not a stay of enforcement is granted (which allows extra time for the tenant to move before an enforcement). The multidimensionality of judge discretion could make it challenging to isolate the impact of the eviction order (Mueller-Smith, 2015; Bhuller et al., 2020).

Exclusion will be violated if judge stringency is correlated with other dimensions of judge discretion that affect tenant outcomes. To assess the plausibility of the exclusion restriction, we first examine whether eviction order stringency is correlated with other dimensions of judge stringency. Appendix Tables G.6 and G.8 report pairwise correlations between eviction order stringency (the instrument) and stringency constructed along alternative dimensions of the case.[39] In each instance, the correlations are weak. Next, in Appendix Table G.7 we re-estimate our first stage with and without these alternative stringency measures and find that including these measures has minimal impact on the first stage coefficient, providing additional support for the plausibility of the exclusion restriction. Additionally, in Cook County—where we can observe judgment amount—we re-estimate the main IV regressions for housing, labor, and financial outcomes in the first year, with an additional control for judgment amount stringency, and find that the main conclusions are unchanged.

Finally, the practical aspects of case proceedings provide additional reassurance that judge discretion in judgment amounts is unlikely to be a threat to our research design. For instance, we find the judgment amount for a case is closely linked to the amount the landlord initially requests in the filing. In Cook County, the correlation between the judgment amount and the ad damnum amount (the amount the landlord requests) is 0.81 . This lends support for the idea that judges' differences along this dimension are likely to be small and unlikely to be driving our results. Taken together, the robustness checks in Appendix G.3 suggest that the multidimensionality of judge discretion is unlikely to be a threat to the exclusion restriction in our settings.

Monotonicity. For the IV estimates to be interpreted as a positively weighted average of local average treatment effects (LATEs), we need monotonicity to be satisfied (Imbens and Angrist, 1994). 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 judge designs if judges are relatively harsh for some types of cases or individuals and relatively lenient for others, or if judges differ in both diagnostic skills and preferences regarding the outcome of the case, as discussed by Chan et al. (2022). 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 G.11 and G.12 show non-negative first-stage estimates for various subsamples in Cook County and New York. As a second test, we calculate judge stringency on one sub-population (for example, women) and then use that stringency measure in the first

[39] As discussed in Appendix G, these dimensions differ across locations due to differences in data availability.

stage for the complementing sub-population (for example, men), as in Bhuller et al. (2020) and Norris et al. (2021). Appendix Table G.13 presents this test and shows non-negative and similar-sized first-stage estimates across specifications. Hence, neither of these tests provide evidence against the monotonicity assumption.

5.2 Combining estimates across locations

Due to restrictions on data sharing, we are unable to pool individual observations from Cook County and New York. We therefore estimate each specification separately by location and then report average point estimates in the tables in Section 6, along with each location-specific estimate. The combined point estimates weight results from the two locations equally, and 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 average effect of eviction for compliers in Cook County and New York.

Table 3: Testing Balance

	Cook County		New York	
	Evicted	Stringency	Evicted	Stringency
	(1)	(2)	(3)	(4)
Age at case	-0.03329***	-0.00012	-0.00403***	-0.00001
	(0.00376)	(0.00020)	(0.00016)	(0.00001)
Female	0.00882	0.00041	-0.04413***	-0.00009
	(0.00644)	(0.00036)	(0.00310)	(0.00011)
Black	0.06297***	0.00012	0.00923***	0.00010
	(0.00628)	(0.00028)	(0.00323)	(0.00018)
White	0.00358	0.00011	-0.01494**	-0.00032
	(0.00582)	(0.00030)	(0.00616)	(0.00027)
Hispanic	0.05957***	0.00045	-0.00743**	0.00001
	(0.00603)	(0.00030)	(0.00368)	(0.00017)
Neighborhood poverty rate (5 yr avg)	0.5540***	0.00208	-0.02487*	-0.00025
	(0.04813)	(0.00221)	(0.01453)	(0.00066)
Ad damnum (in 1000s)	0.00731***	0.00001	0.00001***	-0.00000
	(0.00055)	(0.00002)	(0.00000)	(0.00000)
No prior case	-0.04037***	-0.00013	-0.01228***	-0.00014
	(0.00221)	(0.00013)	(0.00413)	(0.00014)
Joint action	0.01183**	-0.00061**		
	(0.00525)	(0.00025)		
Observations	301,000	268,000	150,662	150,662
Joint F-Statistic	102.3	1.497	224.8	1.007
P-Value	0.000	0.104	0.000	0.443

Notes: *Notes*: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. For each location, the left column presents results from a regression of eviction on case and defendant characteristics, and the right column shows results from a regression of judge stringency on case and defendant characteristics. Neighborhood poverty rate is the 5-year average poverty rate in the defendant's census tract. Ad damnum is the amount the landlord listed as owed by the defendant at the time of filing. Joint action is an indicator for the case type in which the landlord is seeking both an eviction order and a money judgment rather than only an eviction order, and is specific to Cook County. No prior case is an indicator for the defendant having no prior eviction case in our sample. All regressions also include indicators for each right-hand side variable having a missing value, which are not reported in the table. All regressions include court-year-quarter fixed effects. Standard errors are shown in parentheses and are clustered at the judge(courtroom)-year level. Cook County observation counts are rounded in accordance with U.S. Census Bureau disclosure requirements. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

6 Results

This section presents OLS and IV estimates of the effects of eviction on tenants' residential mobility, use of homelessness services, labor market outcomes, financial strain, and hospital use. The estimates show that eviction increases residential mobility and causes spikes in emergency shelter use and hospital visits (particularly for mental health-related conditions) in the year after filing. Housing instability persists in the second year after filing, with eviction triggering increased use of homelessness services. These findings suggest a period of instability of at least two years. During this period, evicted tenants also experience reductions in earnings. In the longer-run, we find that eviction worsens financial health through increased indebtedness and reductions in credit scores.

6.1 Eviction order enforcements, residential mobility, and neighborhood poverty

We first study how eviction impacts a tenant's housing situation, focusing on enforced eviction orders, residential moves, and neighborhood poverty. We report estimates for the full sample, separately by location, and separately for female and Black tenants. We focus on female and Black tenants in our subgroup analysis because these groups are over-represented in housing court in Cook County and New York and because prior research suggests they may face greater adverse consequences of eviction. Qualitative research (Desmond et al., 2013; Desmond, 2016) points to two potential 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 (Sugrue, 2005; Desmond, 2012; U.S. Department of Housing and Urban Development, 2019). Second, landlords may be reluctant to rent to households with children because children may cause nuisances to neighbors or attract inspections by Child Protective Services or the city's health department for lead hazards (Roberts, 2001). 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, 2016).

Order enforcement. To better characterize the treatment, we first consider the extent to which eviction orders are enforced by a Sheriff or Marshal. This experience may cause tenants to move out more quickly or unexpectedly, leaving them unable to secure new housing before they are locked out. So, in addition to potentially increasing the likelihood of moving, an eviction order may change the circumstances under which households move. Table 4 shows that receiving an eviction order substantially increases the probability of experiencing an enforcement within one year, with an IV estimate of 43.5 percentage points and an OLS estimate of 30.1 percentage points. Moves occurring after enforced orders may be more likely to occur under greater stress and exigency, and may potentially result in moves to lower-quality neighborhoods or homelessness. We investigate the effects on neighborhood quality and homelessness below.

Residential mobility. As we showed in Section 4, tenants in housing court have high move rates regardless of the case outcome, with evicted tenants being more mobile both before and after the case. The IV models allow us to estimate how much additional residential mobility is caused by an eviction. Table 4 shows that, for compliers, receiving an eviction order increases the probability of appearing at a new address by 8.2 percentage points one year after filing (an increase of 28 percent relative to a mobility rate of 29.2 percent for the non-evicted group). The OLS estimate is similar though slightly smaller (7.3 percentage points). In Appendix I, we explore alternative approaches to defining moves and find that eviction increases residential mobility under a variety of alternative definitions.[40] The impacts of eviction on residential mobility are similar across locations and subgroups. Columns (4)-(6) show that these effects persist into the second year, and the IV estimate implies that eviction increases the probability of not being at the eviction address by 11.1 percentage points (23 percent).[41] In both time periods, the effects on residential mobility are larger for women.[42]

Neighborhood quality. In the bottom panel of Table 4, we consider the effect of an eviction order on neighborhood quality, as measured by the census tract poverty rate. We find little evidence that eviction causes tenants to move to neighborhoods with higher poverty rates, either in the combined estimates, the location-specific estimates, or the demographic-specific estimates. These estimates are fairly precise, and we can rule out an impact on the neighborhood poverty rate of more than 2.2 percentage points for the combined sample with 95% confidence. Neither our IV nor our OLS specifications point to an increase in the neighborhood poverty rate. Individuals at risk of eviction live in high-poverty neighborhoods prior to filing, which may help explain why eviction does not cause them to move to even higher-poverty neighborhoods on average. Our findings contrast with Desmond and Shollenberger (2015), who find that among recent movers, those who experience a forced move relocate to neighborhoods with 5 percentage points higher poverty rates.[43] Given that tenants who are

[40]This estimate of 8.2 percentage points may in fact be an underestimate. As we show in Appendix Table I.1, evicted tenants are more likely to have a missing address. Appendix Table I.1 provides an alternative specification that defines the outcome as being observed at a new address or not observed at all, which more than doubles the IV estimate.

[41]In Appendix Table H.10, we report estimates from an OLS regression of appearing at a new address on judge stringency. These reduced-form estimates have a causal interpretation even if the exclusion restriction or monotonicity assumption fail to hold. The reduced-form estimates are very similar to the IV estimates, due to the strong relationship between judge stringency and eviction orders documented in Table 2.

[42]Appendix Table H.9 shows that only 8.8 percent of tenants who avoid an eviction receive a new eviction order within one year at the same address, and 13.9 percent receive a new order within two years. This suggests that residential mobility among non-evicted tenants is not driven by follow-up eviction cases at the same address.

[43]An important distinction is that our study population is tenants facing eviction, and we compare evicted to non-evicted tenants. Desmond and Shollenberger (2015) compare forced movers to other recent movers, a comparison group that may include upwardly mobile tenants.

evicted move to observably similar neighborhoods, the effects we consider below on other socioeconomic outcomes likely do not arise due to changes in neighborhood environment, as in studies of housing mobility programs (Chetty et al., 2016) or public housing demolitions (Chyn, 2018).

6.2 Homelessness

Homelessness carries substantial private and social costs (Evans et al., 2019). While eviction has the potential to be a direct cause of homelessness, there is currently no causal evidence on this relationship. The event studies in Section 4 show a striking increase in homelessness after filing for evicted tenants, which suggests a causal link. We investigate this link directly using our IV research design. In addition to their policy relevance, the effects on homelessness are informative as a measure of material hardship and a possible mechanism for the labor market impacts studied in Section 6.3.

Table 5 shows that an eviction order increases the probability of using emergency shelter in the year after filing by 3.4 percentage points in the IV specification and 3.1 percentage points in the OLS specification, which are both large relative to the non-evicted mean of 0.9 percent. We don't find evidence of increased use of emergency shelters after the first year, as seen in column (6) and in the longer-run results presented in Appendix Table H.5. Similarly, the OLS estimates are approximately half as large after the first year. These results suggest that evicted tenants experience difficulty finding alternative housing in the immediate aftermath of the court case and are consistent with economic models of homelessness that emphasize the transitory dynamics of homelessness (O'Flaherty, 2004).

We find similar impacts in the first year when looking at use of any homelessness service for both IV and OLS, though the IV estimate is not statistically significant. While the effects on shelter use are concentrated in the year after filing, the effect on using any homelessness service remains elevated beyond the first year. The IV estimates indicate that evicted tenants are 3.6 percentage points more likely to use homelessness services than tenants who avoid eviction in the second year after filing (an increase of 200 percent relative to the non-evicted group mean of 1.2 percentage points). As with residential mobility, longer-term interactions with homelessness services are driven by effects for female and Black tenants, with an IV estimate for female tenants of 6.8 percentage points (467 percent) and an IV estimate for Black tenants of 5.7 percentage points (307 percent).

The results above indicate that eviction causes a large increase in homelessness both in the first year after a case (through increases in emergency shelter use) and beyond (through elevated use of homelessness services). We view these results as complementary to work on short-term emergency financial assistance and homelessness (Evans et al., 2016), which finds that temporary assistance to at-risk tenants can lead to persistent reductions in homelessness. These results also connect to research emphasizing the socioeconomic consequences of changes to proceedings in eviction court (Greiner et al., 2012). While homelessness remains rare, even for tenants in eviction court, our estimates nevertheless imply substantial additional homelessness caused by evictions. In a given year, across our two locations, we estimate that evictions produce more than 3,600 adults staying in emergency shelter in the year after filing and 2,500 adults using homelessness services the following year.

6.3 Earnings and employment

We now shift attention to estimates of the causal effects of an eviction order on earnings and employment. Table 6 reports estimates for quarters 1-4 and 5-8 after case filing. The first row reports the combined estimates for earnings. The IV estimate shows that eviction decreases average quarterly earnings in quarters 1-4 by $323 (7 percent of the non-evicted mean of $4,300). This effect is similar in magnitude to the earnings drop among evicted tenants in the year prior to filing. The effects of eviction on earnings are larger in the second year after filing, reducing average quarterly earnings by $613 (14 percent of the non-evicted mean). The point estimates are larger for female and Black tenants, although formal tests of equality fail to reject a null hypothesis of equality (see Appendix Table H.1). The estimated effects are also comparable across the two locations.[44] Comparing the IV and OLS estimates, the OLS estimates are systematically smaller, suggesting that impacts may be larger for compliers. As discussed in Section 3, in New York, earnings are not observed when an individual moves out of state, and in Cook County, earnings are not observed outside of the select 13 states for which we have access to LEHD wage income records. Appendix I provides evidence that differential migration is likely not driving our results.[45]

Turning to employment, the IV estimate shows that for marginal tenants, eviction causes a 1.5 percentage point reduction in employment 1-4 quarters after filing, though this estimate is not statistically significant. The OLS estimate is statistically significant and similar in magnitude, suggesting that evicted tenants have employment rates that are 1.3 percentage points lower than non-evicted tenants. The IV point estimates in quarters 5-8 are similar and remain statistically insignificant. In contrast, the subgroup estimates suggest that eviction decreases Black employment by 8.9 percentage points (15 percent of the non-evicted mean), which is statistically significant but somewhat imprecisely estimated. Nevertheless, we can reject a test of equality of effects for Black and non-Black tenants at conventional levels (see Appendix Table H.1).

[44] Since our analysis period coincides with the Great Recession, in Appendix H.1 we study Great Recession years and non-Great Recession years separately and find that the estimates are similar across time periods, although they are somewhat imprecise.

[45] In Appendix I, we show that eviction has a negative and statistically significant impact on moving out of state. We show that selection into moving out of state is unlikely to be driving our earnings estimates for two reasons. First, the estimates are quantitatively small and therefore a selection pattern would have to be implausibly large to drive the earnings estimates, which we show with a simple simulation exercise. Second, the negative impact of eviction on earnings is larger in quarters 5-8 compared to quarters 1-4, while the the out-of-state moves estimates have the opposite pattern—larger in quarters 1-4 and small and insignificant in quarters 5-8—suggesting that if anything, selection is likely attenuating our earnings estimates in the short run.

Appendix Table H.6 shows that the longer-run impacts of eviction on earnings and employment (quarters 9-16 and 17-24 after filing) are for the most part smaller in magnitude, though estimated with somewhat less precision. We can rule out effects larger than a $837 reduction in quarterly earnings in quarters 9-16 after filing with 95 percent confidence.

Our results show that eviction causes reductions in earnings in the first two years after the case, consistent with the disruptive effects of eviction on housing stability described previously. Perhaps the closest prior research on earnings is based on the Milwaukee Area Renters Study and matched comparisons of renters who report experiencing a forced move in the past two years to those who do not. Desmond and Gershenson (2016) report that a forced move increases job loss by 11 to 22 percentage points, depending on the specification and estimation method. Our analysis differs on a number of dimensions. First, our treatment is an eviction order rather than the broader category of forced moves, which includes court-ordered evictions but also informal evictions, landlord foreclosures, and housing condemnations. Second, we study tenants in eviction court rather than tenants considered at risk of eviction. Relative to Desmond and Gershenson (2016), we find more modest effects on the extensive margin of employment. Nevertheless, we find economically meaningful effects on the intensive margin of earnings, which to our knowledge has not been studied. These impacts on earnings and employment are concentrated in the first two years after filing when housing disruptions are also the most pronounced.

6.4 Financial health

We next examine the effects of eviction on financial health and present these results in Table 7. The first row reports estimates of the impact of eviction on our index of overall financial health, and the remaining panels report impacts on each outcome that is used to construct the financial health index.[46]

Eviction worsens tenants' financial health, reducing the financial health index by 0.11 s.d. in quarters 1-4 after filing for IV, which is marginally significant, and 0.10 s.d. for OLS. During this period, we find that eviction reduces the probability of having any auto loan or lease, which may be viewed as a proxy for durable goods consumption (Dobkin et al., 2018; Agarwal et al., 2020), by 6.1 percentage points (36 percent relative to the non-evicted group mean), which is driven entirely by Cook County. The other point estimates during the first year imply reductions in credit access and increasing debt, but none of the estimates are individually significant. In quarters 5-8, the point estimate for effects on the financial health index are even more negative but also less precise and not significant. Eviction reduces credit scores by 16.5 points in this period.[47] By reducing credit scores, eviction could lead to

[46] We do not report results by race or gender as race is not included in the data provided by the credit bureau for either location, and gender is not included in the credit bureau data for New York.

[47] We explore effects on payday loan inquiries and borrowing for Cook County only in Appendix C.7. The IV estimates for the impacts on payday loan inquiries and borrowing are imprecise and do not permit strong takeaways.

increased borrowing costs for tenants and, to the extent landlords use credit scores to screen tenants, hamper tenants' ability to secure new housing.

The negative impacts of eviction on financial health are more pronounced in the longer run. In Appendix Table H.7, we report estimates for effects in quarters 9-16 and 17-24 after filing. Eviction reduces the composite index by 0.21 and 0.26 s.d. in years 3-4 and 5-6, respectively, both of which are statistically significant at the 5% level. In quarters 9-16 after filing, eviction lowers credit scores (IV estimate of -16.8) and increases balances in delinquent accounts (IV estimate of $847).[48] In quarters 17-24, eviction increases the probability of having no open source of revolving credit (IV estimate of 9.3pp, $p < 0.10$) and decreases the likelihood of having an auto loan or lease (IV estimate of 8.3pp, $p < 0.10$). For both balances in delinquent accounts and credit scores, the IV estimate is larger than the OLS estimate, suggesting that compliers are more likely to be on the margin of having access to conventional credit sources.

Taken together, these results suggest that eviction causes further deterioration in tenants' financial circumstances and reduces subsequent access to credit. We find reductions in the financial health index that are comparable to the effect of having a Chapter 13 bankruptcy filing dismissed (Dobbie et al., 2017). Our estimated impacts on credit scores are 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 credit report (Gross et al., 2020; Dobbie et al., 2020). In contrast to the impacts on housing, homelessness, and labor market outcomes documented above, the impacts on financial health are larger in the longer run.

6.5 Hospital visits

We next investigate the effects of eviction on hospital use in New York, where we have access to hospital data. Table 8 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 first and second year following a case.

In the first year after the case, eviction increases 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 in magnitude, although they are not statistically significant. Eviction also increases the number of visits to a hospital for mental health conditions by about 0.05 visits in the first year, a more than 100 percent increase over the non-evicted mean.[49] In the second year after the case, the IV estimates are insignificant

[48]An eviction may affect debt directly if the defendant does not pay the money judgment associated with the eviction case, but in practice this rarely occurs. In this situation, the plaintiff would use the court process to collect the judgment amount, including obtaining a citation to discover assets and a wage garnishment order, and then send any unpaid debt to a collections agency.

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

and less precise. We explore longer-run effects on hospital use in Appendix Table H.8, where results remain statistically insignificant and imprecise. Compared to the IV estimates, OLS estimates tend to be somewhat smaller in the first year and somewhat larger in later years.

Overall, the effects of eviction on hospital use appear concentrated in the period shortly after the case filing. The finding that eviction causes increases in hospital visits is consistent with evidence from Currie and Tekin (2015), who find that foreclosures increase trips to the hospital. 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.[50]

6.6 Comparisons across locations

In this section, we compare estimates for Cook County and New York, document where estimates are similar, and explore potential sources of differences when they diverge. Figure 6 plots estimates for Cook County on the vertical axis and estimates for New York on the horizontal axis. We standardize all estimates by multiplying the regression coefficient by the standard deviation of the eviction indicator and dividing by the standard deviation of the outcome. Across outcomes, the estimates are very similar across locations, with many of the OLS and IV estimates falling close to the 45 degree line, and very similar estimates for employment and earnings across locations. The impacts on financial health outcomes tend to be somewhat larger in Cook County, with statistically significant differences in having an auto loan or lease one year after the case. This may partially be driven by higher rates of car ownership in Cook County.[51] The impacts on residential mobility are somewhat larger for New York, which is consistent with New York's lower vacancy rate, and may be driven by fewer non-evicted tenants choosing to leave in the year or two after the case in New York. Impacts on homelessness outcomes are also somewhat larger for New York, which is again consistent with a tighter housing market and may also stem from New York's more extensive homeless shelter network and right to shelter law.

6.7 What about non-complier cases?

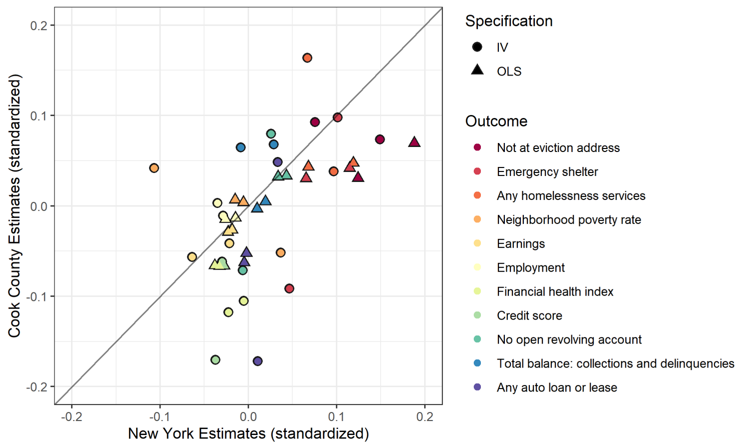
The IV estimates above can be interpreted as the causal effects of eviction for compliers. One might additionally be interested in whether these estimates are representative of effects for the full population of evicted tenants. One possible approach to drawing inference about these effects is difference-in-differences (DiD). However, as discussed in Section 4, there are differential pre-trends between evicted and non-evicted tenants for several outcomes in our settings, raising concern about the parallel trends assumption.[52] Heckman and Robb (1985) and Ashenfelter and Card (1985) show that if shocks to outcomes follow a transitory

[50]See Elejalde-Ruiz (2018) for anecdotal evidence of this. Moore and Rosenheck (2016) also discuss the need of shelter as a potential reason for emergency department visits.

[51]Appendix Table H.2 tests for equality of the IV estimates between the two locations.

[52]For example, see the figures in Section 4.3 and Appendix Figure E.2.

Figure 6: Comparing Estimates Across Locations



Notes: This figure plots standardized estimates for Cook County (y-axis) against standardized estimates for New York (x-axis). All coefficients have been standardized by multiplying by the ratio of the standard deviation of the fraction evicted to the standard deviation of the outcome. Results are for one and two years after the case filing. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072. Circles report IV estimates, while triangles report OLS estimates.

and covariance stationary process, the DiD estimator will be unbiased when the pre- and post-period are chosen symmetrically around the treatment period, even when the parallel trends assumption does not hold.[53] In Appendix J, we further develop the symmetric DiD approach and compare symmetric DiD estimates to IV estimates which, under the appropriate assumptions, allow us to compare the ATT to the IV estimates.

Appendix Tables J.1-J.4 compare the IV estimates to symmetric DiD estimates. The symmetric DiD estimates for housing outcomes are quite similar to the IV estimates, while the effects for residential mobility are somewhat smaller. For labor market and financial health outcomes, DiD estimates have the same sign but also tend to be smaller in magnitude. For health-related outcomes, the DiD and IV estimates both point to sizeable increases in hospital use in the year after filing, but DiD estimates remain positive and statistically significant in the second year. Overall, the DiD estimates consistently show results that are broadly similar to the IV estimates but smaller in magnitude, suggesting that the effects for the average evicted tenant may be smaller than those for the marginal tenant.

[53] See also, Chabé-Ferret (2015), which 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 typically outperforms matching.

Table 4: Impact on Housing Situation

	1 Year After Filing			2 Years After Filing		
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Enforcement:	0.002	0.301***	0.435***	0.002	0.313***	0.422***
	(0.031)	(0.005)	(0.039)	(0.032)	(0.005)	(0.037)
			[329,279]			[307,837]
By Location						
Cook County	0.004	0.270***	0.451***	0.004	0.275***	0.442***
	(0.059)	(0.004)	(0.050)	(0.062)	(0.004)	(0.049)
New York	0.000	0.333***	0.419***	0.000	0.351***	0.401***
	(0.017)	(0.009)	(0.060)	(0.018)	(0.010)	(0.057)
By Group						
Female	0.002	0.290***	0.425***	0.002	0.302***	0.418***
	(0.030)	(0.005)	(0.046)	(0.032)	(0.005)	(0.045)
Black	0.002	0.307***	0.464***	0.002	0.319***	0.436***
	(0.032)	(0.005)	(0.046)	(0.035)	(0.006)	(0.044)
Not at eviction address:	0.293	0.073***	0.082**	0.478	0.129***	0.111**
	(0.318)	(0.003)	(0.036)	(0.348)	(0.003)	(0.053)
			[218,228]			[183,227]
By Location						
Cook County	0.363	0.031***	0.093	0.568	0.070***	0.074
	(0.481)	(0.003)	(0.057)	(0.495)	(0.004)	(0.064)
New York	0.222	0.116***	0.071	0.389	0.188***	0.149*
	(0.415)	(0.006)	(0.045)	(0.487)	(0.004)	(0.084)
By Group						
Female	0.280	0.081***	0.093**	0.461	0.139***	0.136**
	(0.312)	(0.004)	(0.046)	(0.343)	(0.003)	(0.060)
Black	0.272	0.079***	0.066	0.454	0.138***	0.098
	(0.310)	(0.004)	(0.056)	(0.345)	(0.004)	(0.080)
Neighborhood poverty rate:	0.247	-0.000	-0.002	0.246	-0.001	-0.008
	(0.088)	(0.000)	(0.010)	(0.090)	(0.001)	(0.014)
			[173,909]			[127,891]
By Location						
Cook County	0.195	0.001	-0.014	0.196	0.002**	0.011
	(0.130)	(0.001)	(0.018)	(0.133)	(0.001)	(0.021)
New York	0.298	-0.001***	0.009	0.295	-0.004***	-0.027
	(0.120)	(0.000)	(0.007)	(0.123)	(0.001)	(0.020)
By Group						
Female	0.258	-0.000	-0.004	0.256	-0.001	-0.009
	(0.090)	(0.001)	(0.012)	(0.091)	(0.001)	(0.020)
Black	0.267	-0.001	-0.005	0.266	-0.003***	-0.023
	(0.088)	(0.001)	(0.013)	(0.090)	(0.001)	(0.022)

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. 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 (OLS) 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. Results are shown for one year (columns (1)-(3)) and for two years (columns (4)-(6)) after eviction case is filed. Each panel shows results for a given outcome. Below the combined estimates in each panel, we report estimates separately for each location and for the female and Black subsamples. "Enforcement" is defined as an enforcement officer executing the eviction order associated with the case, and is defined cumulatively. "Not at eviction address" is an indicator for being observed living at a different address than the filing address. "Neighborhood poverty rate" is the tract-level poverty rate. The main set of controls included in all model specifications are: ad damnum amount, gender, race, census tract poverty rates, census tract 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. For "Not at eviction address" and "Neighborhood poverty rate" we also control for whether tenants were not at eviction address 1 year and 2 years prior and tenants' neighborhood poverty rate 1 year and 2 years prior to eviction case filing, respectively. Standard errors for regression model coefficients are included in parentheses and are clustered at the judge(courtroom)-year level. Observation counts for the full sample are listed under the standard errors, in brackets. Observation counts for all outcomes and subgroups can be found in Appendix Table H.15. The reduced form results for all outcomes can be found in Appendix Table H.10. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

Table 5: Impact on Use of Homelessness Shelter and Services

	1 Year After Filing			2 Years After Filing		
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Emergency shelter:	0.009	0.031***	0.034**	0.008	0.014***	-0.001
	(0.068)	(0.001)	(0.017)	(0.062)	(0.001)	(0.013)
			[210,840]			[198,898]
By Location						
Cook County	0.007	0.010***	0.023	0.006	0.006***	-0.019
	(0.086)	(0.002)	(0.028)	(0.077)	(0.001)	(0.019)
New York	0.011	0.052***	0.046**	0.009	0.022***	0.016
	(0.105)	(0.002)	(0.019)	(0.097)	(0.001)	(0.017)
By Group						
Female	0.009	0.032***	0.024	0.008	0.016***	0.024
	(0.066)	(0.002)	(0.018)	(0.061)	(0.001)	(0.015)
Black	0.010	0.034***	0.036	0.009	0.015***	0.007
	(0.072)	(0.002)	(0.024)	(0.068)	(0.001)	(0.019)
Any homelessness services:	0.015	0.036***	0.029	0.012	0.019***	0.036**
	(0.086)	(0.002)	(0.023)	(0.076)	(0.001)	(0.015)
			[210,840]			[198,898]
By Location						
Cook County	0.017	0.016***	0.012	0.012	0.013***	0.048**
	(0.128)	(0.002)	(0.042)	(0.110)	(0.002)	(0.023)
New York	0.013	0.056***	0.046**	0.011	0.025***	0.024
	(0.114)	(0.002)	(0.018)	(0.104)	(0.001)	(0.019)
By Group						
Female	0.015	0.038***	0.030	0.012	0.020***	0.068***
	(0.086)	(0.002)	(0.023)	(0.077)	(0.001)	(0.020)
Black	0.017	0.040***	0.049*	0.014	0.020***	0.057***
	(0.092)	(0.002)	(0.029)	(0.083)	(0.001)	(0.022)

Notes: * $p<0.1$,** $p<0.05$,*** $p<0.01$. 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 (OLS) and two-stage least squares (IV) estimates of the impact of eviction on outcomes related to the tenant's homelessness situation. Outcomes are listed on the left of each row. Results are shown for one year (columns (1)-(3)) and two years (columns (4)-(6)) after eviction case is filed. Each panel shows results for a given outcome. Below the combined estimates in each panel, we report estimates separately for each location and for the female and Black subsamples. "Emergency shelter" is an indicator for if the individual was observed staying at an emergency homeless shelter. "Any homelessness services" is an indicator for having any interaction with homelessness services. The main controls for all model specifications are the same as those described in Table 4. In each regression, we also control for whether tenants stayed at an emergency shelter and whether tenants had any interaction with emergency homelessness services 1 year and 2 years prior to eviction case filing. Standard errors for regression model coefficients are included in parentheses and are clustered at the judge(courtroom)-year level. Observation counts for the main combined specifications are reported in brackets below the standard errors in columns (3) and (6). Observation counts for all regressions shown above can be found in Appendix Table H.15. The reduced form results for regressions shown above can be found in Appendix Table H.11. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

Table 6: Impact on Earnings and Employment

	1-4 Quarters After Filing			5-8 Quarters After Filing		
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Earnings:	4,300	-229***	-323*	4,254	-269***	-613**
	(3,809)	(9)	(175)	(3,885)	(13)	(248)
			[374,400]			[336,396]
By Location						
Cook County	4,821	-286***	-445*	4,821	-320***	-627*
	(5,810)	(12)	(249)	(5,956)	(17)	(337)
New York	3,779	-172***	-201	3,687	-218***	-599*
	(4,926)	(14)	(245)	(4,991)	(19)	(363)
By Group						
Female	4,136	-195***	-504***	4,094	-238***	-767***
	(3,545)	(10)	(185)	(3,610)	(14)	(295)
Black	4,319	-199***	-377	4,252	-247***	-931***
	(3,664)	(12)	(234)	(3,718)	(16)	(307)
Employment:	0.565	-0.013***	-0.015	0.549	-0.019***	-0.018
	(0.317)	(0.001)	(0.021)	(0.322)	(0.001)	(0.027)
			[376,400]			[340,396]
By Location						
Cook County	0.623	-0.012***	0.003	0.613	-0.014***	-0.010
	(0.432)	(0.001)	(0.027)	(0.438)	(0.002)	(0.030)
New York	0.507	-0.014***	-0.032	0.485	-0.024***	-0.027
	(0.465)	(0.002)	(0.032)	(0.471)	(0.002)	(0.046)
By Group						
Female	0.585	-0.013***	-0.036	0.568	-0.019***	-0.003
	(0.315)	(0.001)	(0.025)	(0.320)	(0.002)	(0.034)
Black	0.583	-0.011***	-0.059*	0.566	-0.018***	-0.089**
	(0.316)	(0.001)	(0.031)	(0.321)	(0.002)	(0.040)

Notes: $^{*}\ p < 0.1,^{**}\ p < 0.05,^{***}\ p < 0.01$. 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 (OLS), and two-stage least squares (IV) estimates of the impact of eviction on labor outcomes. Outcomes are listed on the left of each row. Results are shown for 1-4 quarters (columns (1)-(3)) and 5-8 quarters (columns (4)-(6)) after the eviction case is filed. Each panel shows results for a given outcome. Below the combined estimates in each panel we report estimates separately for each location and for the female and Black subsamples. "Earnings" are average quarterly wage income from our labor market data described in Section 3. "Employment" is the share of quarters with positive wage income from our labor market data described in Section 3. Controls for all model specifications are the same as those described in Table 4. In each regression, we also control for tenants' earnings and employment in each of the four quarters before filing, as well as averaged values over the eight quarters (2 years) prior to the case filing. Standard errors for regression model coefficients are included in parentheses and are clustered at the judge(courtroom)-year level. Observation counts for the main combined specifications are reported in brackets below the standard errors in columns (3) and (6). Observation counts for all regressions shown above can be found in Appendix Table H.15. The reduced form results for regressions shown above can be found in Appendix Table H.12. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

Table 7: Impact on Financial Health

	1-4 Quarters After Filing			5-8 Quarters After Filing		
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Financial health index:	-0.054	-0.107***	-0.107*	0.008	-0.103***	-0.141
	(0.737)	(0.005)	(0.060)	(0.747)	(0.005)	(0.094)
			[269,814]			[271,230]
By Location						
Cook County	-0.075	-0.127***	-0.202**	-0.027	-0.130***	-0.230
	(0.990)	(0.008)	(0.102)	(1.002)	(0.009)	(0.174)
New York	-0.032	-0.087***	-0.012	0.043	-0.077***	-0.053
	(1.091)	(0.005)	(0.063)	(1.108)	(0.006)	(0.072)
Credit score:	547.59	-8.40***	-7.86	551.84	-7.99***	-16.53**
	(67.11)	(0.38)	(5.18)	(68.61)	(0.41)	(6.67)
By Location						
Cook County	531.94	-9.19***	-8.69	536.62	-9.40***	-24.16**
	(74.04)	(0.55)	(8.29)	(74.56)	(0.59)	(11.15)
New York	563.24	-7.60***	-7.03	567.06	-6.58***	-8.90
	(111.94)	(0.53)	(6.21)	(115.19)	(0.56)	(7.33)
No open revolving account:	0.481	0.032***	-0.039	0.468	0.037***	0.052
	(0.334)	(0.002)	(0.025)	(0.331)	(0.003)	(0.051)
By Location						
Cook County	0.587	0.032***	-0.072*	0.589	0.034***	0.080
	(0.491)	(0.003)	(0.043)	(0.491)	(0.005)	(0.099)
New York	0.375	0.032***	-0.006	0.347	0.041***	0.024
	(0.452)	(0.002)	(0.025)	(0.445)	(0.002)	(0.027)
Total balance: collections and delinquencies:	2,550	153***	310	2,378	44*	548
	(4,099)	(24)	(393)	(3,936)	(25)	(502)
By Location						
Cook County	2,759	54	735	2,516	-34	739
	(5,504)	(36)	(659)	(5,291)	(38)	(930)
New York	2,342	253***	-115	2,240	122***	357
	(6,075)	(32)	(428)	(5,829)	(31)	(377)
Any auto loan or lease:	0.170	-0.021***	-0.061**	0.176	-0.025***	0.031
	(0.264)	(0.001)	(0.030)	(0.269)	(0.002)	(0.036)
By Location						
Cook County	0.197	-0.040***	-0.130**	0.198	-0.047***	0.037
	(0.396)	(0.002)	(0.054)	(0.397)	(0.003)	(0.066)
New York	0.142	-0.002	0.008	0.155	-0.004**	0.025
	(0.349)	(0.002)	(0.027)	(0.362)	(0.002)	(0.026)

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. 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 lagged dependent variable OLS (OLS) and two-stage least squares (IV) estimates of the impact of eviction on outcomes related to the tenant's financial health. Outcomes are listed on the left of each row. Results are shown for 1-4 quarters (columns (1)-(3)) and for 5-8 quarters (columns (4)-(6)) after the eviction case is filed. Each panel shows results for a given outcome. Below the combined estimates in each panel, we report estimates separately for each location. "Financial Health Index" is the average of any observed values of the index during the listed quarters. The index is an equally weighted index of the attributes listed below, and described in Section 3. "Credit Score" is the average of observed Vantage Scores during the listed quarters. "No open revolving account" is the average of indicators for having no open revolving account over the listed quarters. "Total balance: collections and delinquencies" is the average of the balance in delinquent accounts or in collections over the listed quarters. "Any auto loan" is an indicator for whether the individual is observed with an auto loan or lease in any of the listed quarters. 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. We additionally control for lagged values of the relevant outcome for up to two years prior to filing. Standard errors for regression model coefficients are included in parentheses and are clustered at the judge(courtroom)-year level. Observation counts for the main combined specifications are reported in brackets below the standard errors in columns (3) and (6), in the top panel. Observation counts for all regressions shown above can be found in Appendix Table H.15. The reduced form results for regressions shown above can be found in Appendix Table H.13.

Table 8: Impact on Hospital Use

	1-4 Quarters After Filing			5-8 Quarters After Filing		
	$\mathbb{E}[Y\|E=0]$ (1)	OLS (2)	IV (3)	$\mathbb{E}[Y\|E=0]$ (4)	OLS (5)	IV (6)
Number of hospital visits	0.739	0.055***	0.188**	0.632	0.039***	-0.113
	(1.321)	(0.006)	(0.094)	(1.208)	(0.006)	(0.142)
Number of emergency visits	0.588	0.045***	0.106	0.511	0.028***	-0.065
	(1.091)	(0.005)	(0.089)	(1.010)	(0.005)	(0.124)
Number of mental health visits	0.047	0.016***	0.054*	0.045	0.012***	-0.035
	(0.295)	(0.001)	(0.030)	(0.346)	(0.002)	(0.055)
			[179,024]			[154,531]

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This table reports the impacts of eviction on hospital use in New York. The table includes the non-evicted sample means ($\mathbb{E}[Y|E = 0]$), OLS (OLS) estimates and two-stage least squares (IV) estimates of the impact of eviction on hospital use. Outcomes are listed on the left of each row. Results are shown for 1-4 quarters (columns (1)-(3)) and 5-8 quarters (columns (4)-(6)) after the eviction case is filed. "Number of hospital visits" is the total number of non-pregnancy-related hospital visits. "Number of emergency visits" is the total number of non-pregnancy-related emergency room visits, and "Number of mental health visits" is the total number of non-pregnancy-related hospital visits for mental health conditions, where mental health conditions are defined in C.4. Controls for all model specifications are the same as those described in Table 4 and lagged values of the number of hospital visits, and visits by diagnosis type . Standard errors are included in parentheses and are clustered at the courtroom-year level. Observation counts for all outcomes are listed at the bottom of the table, in brackets. The reduced form results for regressions shown above can be found in Appendix Table H.14.

7 Conclusion

Evictions are a widespread phenomenon in the U.S. housing market, affecting more than 2 million households each year who overwhelmingly reside in poor or minority neighborhoods. Growing concern over evictions has spurred governments to pursue policies to reduce their incidence, such as legal aid to tenants facing eviction, emergency rental assistance, and just cause eviction laws, citing substantial costs to tenants and local governments in the fallout from eviction. Despite the large number of evictions and the growing policy interest, the consequences of eviction are not well understood. We explore how eviction impacts tenants in housing court using newly linked administrative data from two large urban areas and a quasi-experimental research design that enables us to isolate the causal effect of eviction.

We document signs of increasing economic distress in the lead-up to case filing across a broad range of measures: falling earnings, decreased attachment to the labor market, rising unpaid bills, and increases in hospital visits. This suggests many eviction cases are precipitated by adverse events. As we show, these patterns are likely to bias both comparisons of evicted tenants to renters outside of court and comparisons of evicted to not-evicted tenants within court, underscoring the value of our IV design that uses the random assignment of judges to estimate the impact of an eviction order for complier cases.

Using our IV design, we find that eviction exacerbates the economic distress experienced by tenants in the lead-up to a court filing. In the two years following a case, eviction increases homelessness, residential mobility, and hospital visits. During this period of disruption, eviction also reduces earnings, with particularly large effects for female and Black tenants. In the longer run, eviction worsens financial health through reduced credit scores and increased indebtedness.

This research speaks to an active policy debate on how, if at all, governments should address evictions. While aspects of the ongoing debates around the desirability of various eviction-related policies remain unsettled, we make significant progress on the key question of whether and how eviction affects tenants' outcomes. Our results suggest that averting an eviction order may yield considerable benefits for tenants, particularly female and Black tenants, two groups that are over-represented in housing court. Beyond the reductions in earnings and worsened credit, the increases in hospital visits and use of homelessness services suggest that eviction impacts physical, mental, and material hardship. The high cost to local governments of providing healthcare and homelessness services (Evans et al., 2019) imply that there are also considerable spillover costs for society from eviction. These costs are important inputs to evaluating eviction-related policies. While our results suggest policies to prevent evictions could be welfare-enhancing, many of these policies may have important general equilibrium effects that are not well understood. We leave it to future work to fully assess the costs and benefits of individual policies to reduce evictions.

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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 orders up to 90 days after the judgment, for considerations such as whether the tenant defaulted on rent due to extraordinary circumstances. Separately,

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.

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 satisfies 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 six 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.

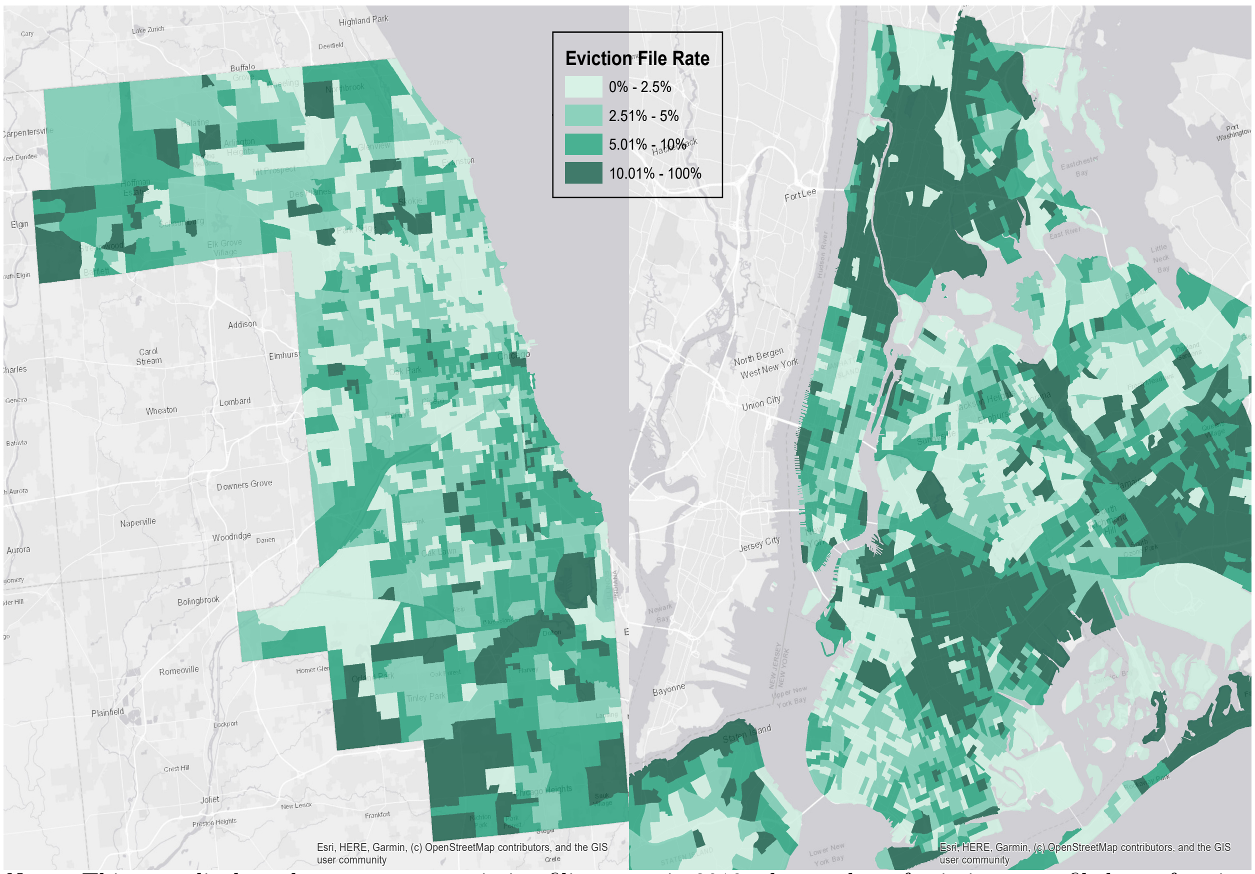
Berkeley, CA In March 2017, the Berkeley City Council passed the Tenant Protection Ordinance, which prohibits landlords from conducting illegal evictions using fraudulent/misleading information or intimidating/coercive conduct. Landlords are also prohibited from exploiting tenants on the basis of their 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 Eviction rates across neighborhoods

Figure B.1 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.1: Neighborhood Eviction Filing Rates



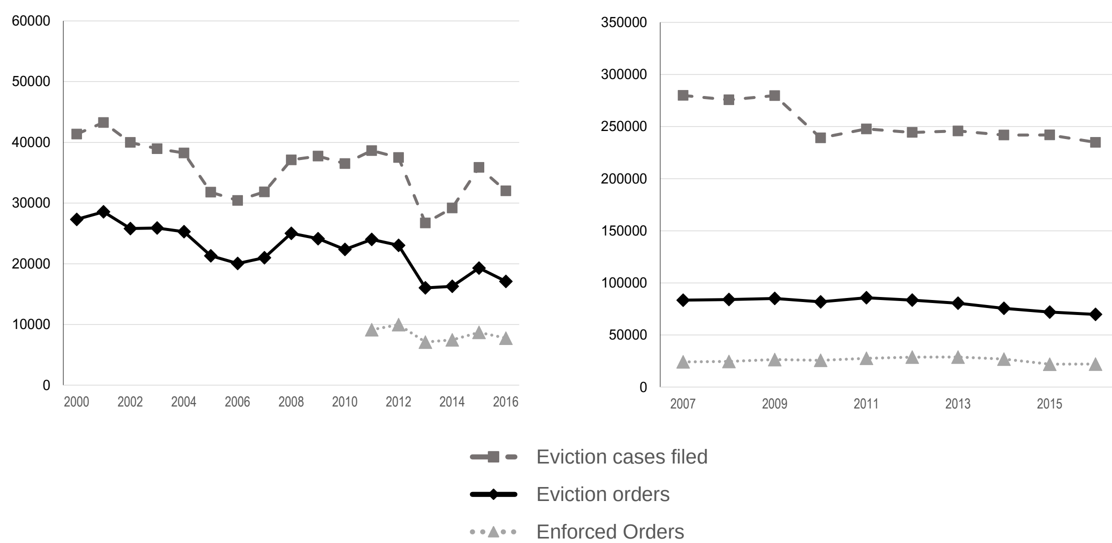
Notes: This map displays the census tract eviction filing rates in 2010: the number of eviction cases filed as a fraction of all rental occupied housing units (from the ACS 2006–2010). The overall eviction filing rate was 4 percent in Cook County and 11 percent in New York.

B.2 Time trends in eviction filings, orders, and enforced orders

Figure B.2 plots the number of eviction cases filed (top line), eviction orders (middle line), and enforced orders (bottom line) in Cook County (left panel) and New York (right panel).

Figure B.2: Time Trends in Eviction Filings, Orders, and Enforced Orders

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



Notes: These figures display time trends in eviction filings, eviction orders, and enforced orders 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 enforced order counts are based on data from the Sheriff's Office (Cook County) and the Marshal's office (New York).

B.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.[54] 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 locations, we capture both a more- and a less-regulated rental housing market and can examine differences in estimates across them (which we do in Section 6.6). 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 not have right-to-shelter, and homeless individuals may be turned away from shelters that are at capacity.[55] 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.

B.4 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).[56]

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

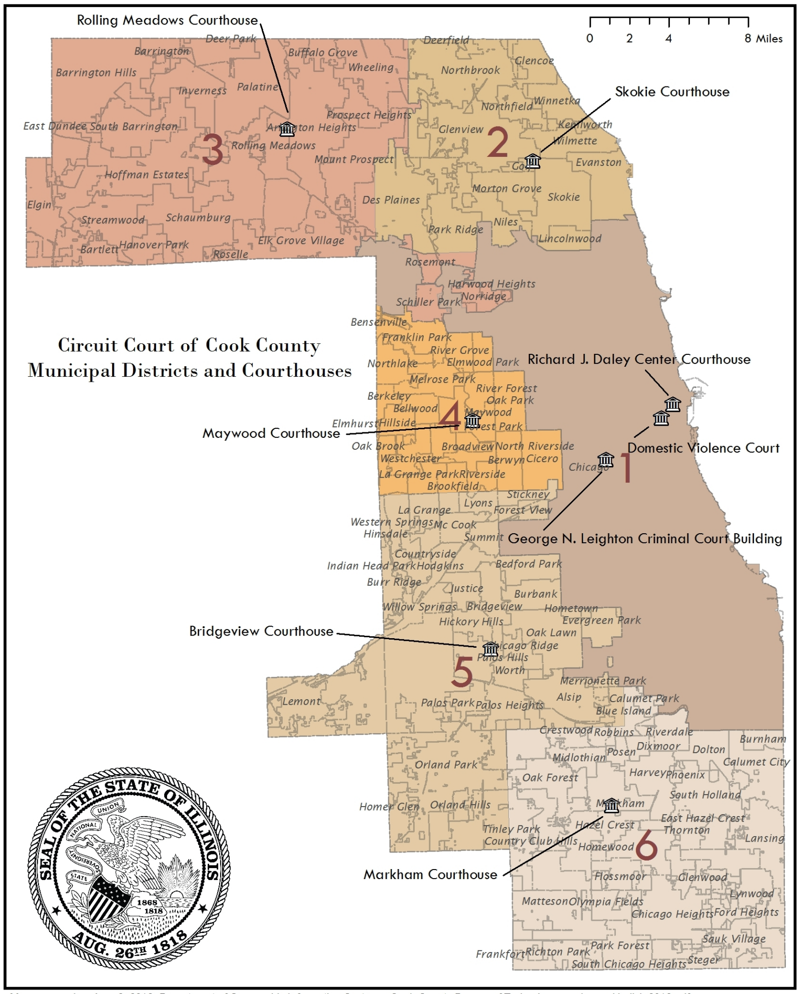
[54] Other cities with some form of rent control or stabilization are Washington, D.C., and several cities in California, Maryland, and New Jersey.

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

[56] 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.

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'.

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.

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.

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 be present in court 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.[57] This allows the landlord and tenant to negotiate certain binding conditions, which, if adhered to, result in the eviction case being dismissed. Typically,

[57]Examples of a settlement forms are available online, but not all settlement forms follow standardized formats, and formats have changed over time.

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 a variety of reasons (e.g., the tenant already left the premises).

On the day of eviction, the landlord (or his/her 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.[58] 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

[58]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 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.5 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 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.[59] 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 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 enforce an eviction order. If the tenant is able to pay the rental arrears owed, then judgment is said to be *satisfied* with the possessory judgment going 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.[60] 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.

[59] 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.

[60] 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 in the full sample, and how this 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. We drop businesses as businesses because we are interested in impacts of eviction on residential tenants. We drop condos as they typically represent the eviction of condo owners, rather than renters. We drop cases with missing names or only ”unknown occupants”, as we are not able to link these cases. We drop cases with more than $100,000 in damages, as there is a very small right tail involving very large damages, which we believe are a combination of entry errors and outlier cases. 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 100 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. These three restrictions help guarantee that we can identify the judge, that the judge sees a sufficient number of cases to accurately estimate stringency, and that there was more than one judge to which the case could have been assigned to. This final row corresponds to our “analysis sample” prior to linking to outcomes.

Table C.1: Sample Construction for Cook County

Restriction	Cases
None	583,874
No businesses or condos	555,167
Non-missing names	546,698
Damages < $100,000	546,193
Non-missing judge	545,447
Judge sees more than 100 cases per year	483,200
Valid Courtrooms	413,976

Notes: This table provides a description of how the sample in Cook County changes as sample restrictions are applied.

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 because we are interested in impacts of eviction on residential tenants. 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, since we cannot construct the instrument for these cases. We require that the courtroom hears at least 500 total cases in a year, which results in a very small number of total cases being dropped. Functionally, this restriction drops courtrooms that are used only sporadically, which may feature non-random set of cases. 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 Staten 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

Notes: This table provides a description of how the sample in New York changes as sample restrictions are applied.

C.3 Court records: construction of case outcomes

In both jurisdictions, we define an eviction as a case *ending* with the judge issuing an eviction order. This definition includes instances when a tenant fails to meet the terms of an initial settlement and the judge ultimately issues an eviction order. It will also include cases where the tenant files an appeal to halt the eviction, but is unsuccessful. We focus on the last recorded outcome on the docket for each case. We classify these terminal actions based on whether it returns the property to landlord, allowing an enforcement agency to execute a warrant. 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*.[61] For each type of 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.

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

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 in 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 may understate the fraction of cases that have an agreement, since it is possible that documents related to an agreement were not included in the microfilms 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 end 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.[62] We also classify 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.[63] Second, cases can end in a verdict for the defendant. In Cook County, 5 percent of cases fall into these two groups.

[62]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.

[63]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, and is often accompanied by an agreement, and is therefore more similar to the stipulation agreement outcome in New York.

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 in Table C.3. In the top panel, we reports the results of regressing indicators of data source availability on an indicator for receiving an eviction order. Receiving an eviction order has small positive impact on the availability of any residential moves data ("Any Move Data"). Households receiving an eviction order are slightly more likely to have address data from benefits records but are slightly less likely to have Infutor data. In both cases, the relationship is small. In the bottom panel we report results from regressing indicators of data source availability on stringency. Stringency is uncorrelated with having any residential move data, and with the particular sources of the move data. This suggests that our primary mobility results from New York are unlikely to be affected by differential data availability. In Appendix I we examine how sensitive the New York residential mobility results are to using alternative definitions of moving depending on the source of moves data, and find that they are qualitatively similar across data sources.

Table C.3: Move Data Source—New York

	Move Data Availability		
	Any Move Data (1)	Any Benefits Data (2)	Any Infutor Data (3)
Eviction Order	0.003*	0.015***	-0.012***
	(0.002)	(0.002)	(0.002)
Stringency	0.017	0.012	0.032
	(0.023)	(0.030)	(0.024)
	[150,662]	[150,662]	[150,662]

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This top panel of the table reports the results of separately regressing measures of move availability on an indicator for receiving an eviction order. The bottom panel reports results from regressing measures of move availability on an stringency. "Any Move Data" takes the value 1 if we have move data from Infutor or benefits data for a given tenant and 0 otherwise. "Any Benefits Data" takes the value 1 if we have benefits data and 0 otherwise. "Any Infutor Data" takes the value 1 if we have data on mobility from Infutor and 0 otherwise.

C.6 CCS codes for mental health

Table C.4 lists the CSS codes used to classify if a hospital visit was mental health related. The first column lists the CSS code and the second column lists the diagnosis category name.

Table C.4: 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 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. 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. 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.

In our analysis we study payday inquiries, which are the borrower's inquiry into getting a loan, and payday borrowing, which are the actual loans taken out. We present results for four outcomes: any inquiry, number of inquiries, any loan, number of loans. The majority of the payday loans in our data originate online. We observe payday loan inquiries for all months between September 2011 and November 2018, and loans for all months between January 2010 and November 2018. We only observe payday loan data for consumers who have a record in our main credit file.

Note that there may be one or many inquiries before a loan is underwritten, or there may be a loan with no associated inquiries, which may occur for roll-over loans or in cases in which a borrower is well known to the lender. From the inquiries file, we keep only inquiries for new credit, which excludes soft inquiries and those due to collections or leases. In Table C.5, 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.

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.

Figure C.1 presents event studies for payday inquiries and loans in Cook County. Panel A shows trends in the probability of making an inquiry into a payday loan, while panel B shows trends in the number of payday inquiries. Tenants in housing court have high levels of payday loan inquiries, even two years prior to filing, when about 3 percent of tenants have an inquiry each quarter. In the two 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 3 percent per quarter to 4.4 percent per quarter, as seen in panel A. 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.

In panel C, the dependent variable is an indicator for having a new payday loan in the corresponding quarter, while in panel D the dependent variable is the number of new payday loans. Both panels show an increase in payday borrowing in the run-up to eviction filing, with a steeper increase for non-evicted tenants. After the eviction filing, while payday borrowing falls for both groups, the non-evicted group has higher long-run levels of payday borrowing compared to the evicted group. The fact that inquiries fall in parallel following eviction court but borrowing remains higher for non-evicted tenants suggests that an eviction may have a negative effect on the probability of having a loan approved. Since the mean loan size is \$1535 and the number of payday loans per person in a given quarter is between 0.3 percent and 0.6 percent, individuals have about \$4-9 dollars of payday borrowing per quarter.

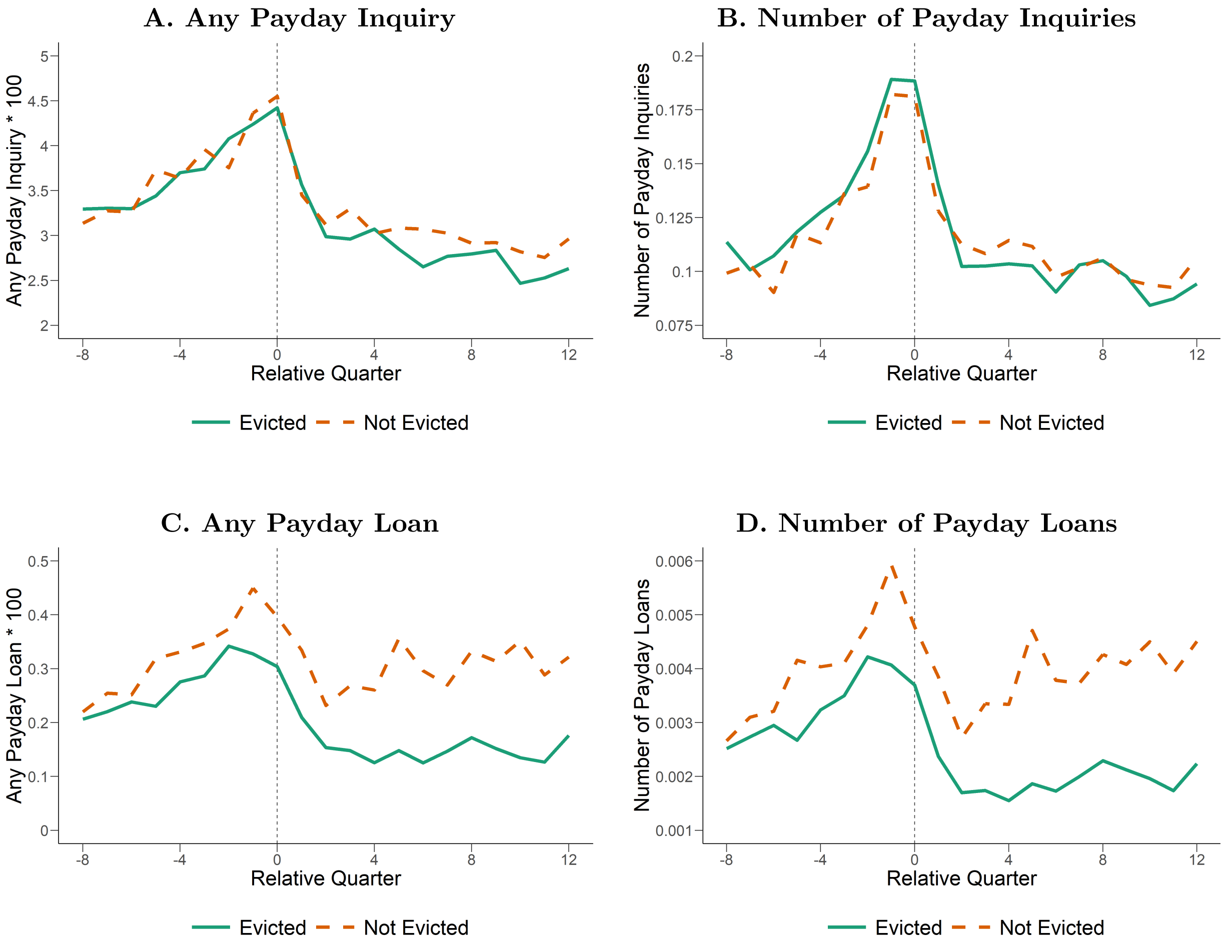
Table C.6 presents OLS and IV estimates of the impact of eviction on payday loan inquiries and borrowing. The results for payday inquiries show that evicted and non-evicted tenants have only minor differences in their likelihood of making a payday inquiry in the first and second years after filing, echoing the findings of the event studies, and the IV estimates are statistically insignificant and somewhat imprecisely estimated. The OLS estimates in the bottom two panels show that payday loan borrowing is lower for evicted tenants in the first and second years after filing, again echoing the event studies. The IV estimates are statistically insignificant, with the exception of payday borrowing by women. For women, an eviction increases the likelihood of having a payday loan by 3.8 percent in the first year (a 165 percent increase relative to the baseline), and this effect is statistically significant at the 5 percent level. This effect is insignificant in the second year, diminishing to 1.8 percent, but this estimate remains economically meaningful.

Table C.5: Payday Loans Data: Summary Statistics

	Random Sample (1)	Eviction Court Sample (2)
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)
Number of Inquiries	687,364	2,469,116
Number of Unique Individuals	78,957	179,574
Accounts:		
Amount	2,154.98	1,535.61
	(3,388.13)	(2,595.41)
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)
Number of Accounts	46,531	126,372
Number of Unique Individuals	17,746	47,626

Notes: This table provides sample means and standard deviations of key variables in the payday loans database, for both the linked Cook County eviction court sample and the linked Cook County random sample. The top panel presents the sample of payday loan inquiries, and the bottom panel 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.

Figure C.1: Event Studies: Payday Inquiries and Loans



Notes: This figure plots trends in payday inquiries and loans relative to eviction filing in Cook County. We estimate equation 4.1 and plot results for the evicted and non-evicted groups in each time period. The only controls are calendar year dummies. 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. All outcomes are measured at a quarterly frequency.

Table C.6: Impact on Payday Inquiries and Loans

	1-4 Quarters After Filing			5-8 Quarters After Filing		
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Any payday inquiry $\times 100$**:**	11.70	-0.04	0.79	11.57	-0.19	0.98
	(32.14)	(0.23)	(3.64)	(31.98)	(0.20)	(3.21)
			[75,608]			[91,571]
By Group						
Female	12.14	0.12	5.92	12.05	-0.23	5.64
	(32.66)	(0.32)	(4.18)	(32.56)	(0.27)	(4.48)
Number of payday inquiries:	0.606	0.014	0.158	0.594	-0.001	0.495
	(3.495)	(0.025)	(0.399)	(3.505)	(0.024)	(0.378)
By Group						
Female	0.636	0.055	0.525	0.637	0.010	0.545
	(3.448)	(0.037)	(0.484)	(3.609)	(0.030)	(0.578)
Any payday loan $\times 100$**:**	1.382	-0.297***	1.221	1.509	-0.344***	-1.073
	(11.676)	(0.074)	(1.166)	(12.193)	(0.062)	(1.505)
			[99,570]			[115,404]
By Group						
Female	1.448	-0.261***	3.837**	1.559	-0.336***	1.805
	(11.947)	(0.093)	(1.624)	(12.388)	(0.089)	(1.667)
Number of payday loans:	0.021	-0.004***	0.010	0.025	-0.006***	-0.016
	(0.223)	(0.001)	(0.023)	(0.263)	(0.001)	(0.032)
By Group						
Female	0.022	-0.004**	0.059**	0.027	-0.007***	0.023
	(0.230)	(0.002)	(0.026)	(0.273)	(0.002)	(0.035)

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This table reports the non-evicted sample means ($\mathbb{E}[Y|E = 0]$), OLS (OLS) estimates and two-stage least squares (IV) estimates of the impact of eviction on payday inquiries and loans, for Cook County only. Outcomes are listed on the left of each row. Results are shown for 1-4 quarters (columns (1)-(3)) and 5-8 quarters (columns (4)-(6)) after the eviction case is filed. Each panel shows results for a given outcome, and results for the female subsample. . Controls for all model specifications are the same as 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. Observation counts for the full specification of "Any payday inquiry" and "Number of payday inquiries" are listed in columns (3) and (6) of the "Any payday inquiry" panel, in brackets under the standard errors, and observation counts for the full specification of "Any payday loan" and "Number of payday loans" are listed in columns (3) and (6) of the "Any payday loan" panel, in brackets under the standard errors. Observation counts for all outcomes and specifications are in Appendix Table H.15.

D Appendix: Linked sample details

D.1 Cook County

Individuals in our Cook County eviction records are assigned unique identifiers (PIKs) by the U.S. Census Bureau, based on names and addresses contained in the court records. We link individuals by their PIKs to earnings, employment, and homelessness outcomes contained in Census data sets. Table D.1 reports estimates from a regression of an indicator for an individual being successfully assigned a PIK on case characteristics. The case characteristics include indicators for race (predicted from census tract and last name), gender (predicted from first name), an indicator for the tenant being without an attorney, and an indicator for the case outcome being an eviction order. We find that cases ending in an eviction and cases in which the tenant does not have an attorney are less likely to be assigned a PIK. Cases with white or Black tenants are more likely to be assigned a PIK, while cases with Hispanic tenants are less likely to be assigned a PIK. The second column of Table D.1 re-estimates the regression but replaces the eviction order right-hand side variable with judge stringency. The column (2) estimates show that moving from the 10th to the 90th percentile in stringency (a difference of 0.07) is associated with a decrease in the likelihood of being assigned a PIK by 0.38 percentage points (-0.054×0.07), which is a 0.73 percent decrease relative to the overall PIK rate of 61 percent. This estimate shows that being assigned a strict judge is associated with a small decrease in the likelihood of being linked to Census data sets. This correlation likely arises due to the Census linkage process, which may incorporate post-filing information that is impacted by eviction. We emphasize that this result does not threaten the internal validity of our analysis, conditional on having a PIK, we have balance in individual and case characteristics, as shown in Table 3, and explore exogeneity further in G.2.

Table D.2 provides estimates from the analogous regression for the credit bureau sample. To construct the credit bureau analysis sample, Experian assigned a unique identifier to each individual in the court records based on names and addresses. Table D.2 shows the estimates of an indicator for being successfully assigned an Experian identifier on the same set of case characteristics excluding race variables, to comply with our data use agreement with Experian. The second column shows that the stringency of the assigned judge does not predict a case being linked.

D.2 New York

We link individuals in our New York court records from 2007-2016 to public assistance records covering Medicaid, Food Stamps, and cash assistance, which we then link to earnings, employment, residential mobility, homelessness, and hospital visit outcomes. We limit our sample to records that link to a benefits case prior to the housing court filing.

We first describe the procedure used to link housing court records to administrative benefits files. Our housing court records contain only first name, last name, and address.

The administrative benefits data include every address that an individual has provided for the years covered in our benefits data. 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 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 know 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.

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

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:

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

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.[64]

We next analyze the characteristics that are predictive of a match in New York. Table D.3 reproduces Table D.1 for the New York benefits sample. 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. Column (2) shows that stringency is uncorrelated with matching to the benefits data.

The New York housing court records were linked to Experian records using tenant name and address.[65] Table D.4 reproduces Table D.2 for the New York credit bureau sample. Cases with a tenant predicted to be female are more likely to be linked to an Experian record, while cases ending with an eviction order are slightly less likely to be linked. Judge stringency is uncorrelated with the likelihood of being linked to Experian data.

In Table D.5, we report additional results for the relationship between stringency and linkages to available data in New York. The first two columns repeat regressions of an indicator for linking to the benefits data on stringency, without controls (column 1) and with controls (column 2). The controls here are characteristics that can be observed in the court records: ad damnum amount, neighborhood characteristics, whether the tenant had legal representation and whether the tenant had a prior case. Stringency is uncorrelated with linkages to the benefits data and this relationship is unchanged with the inclusion of controls. We also investigate whether stringency is related to having a valid SSN within the linked sample. Having a valid SSN is necessary for being included in the linkage for labor market outcomes. Columns (3) and (4) use the linked-benefits sample and explore the relationship between missing a valid SSN, and the stringency instrument (with and without controls, respectively). Within the benefits data, stringency is uncorrelated with missing a valid SSN.

Table D.6 compares the linked benefits sample to the overall housing court population in New York City to better understand the nature of selection into the benefits sample. The characteristics of the 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 and household sizes for the linked sample appear smaller.

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

[65] This linkage included additional housing court records from 2017, which were not available at the time of the benefits linkage.

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

	Has PIK	
	(1)	(2)
Intercept	0.5514***	0.5632***
	(0.00903)	(0.01984)
Evicted	-0.05073***	
	(0.00239)	
Judge stringency		-0.05394**
		(0.02658)
Joint action	0.02719***	0.02618***
	(0.00315)	(0.00322)
Female (predicted)	0.06049***	0.06097***
	(0.00171)	(0.00171)
White (predicted)	0.1284***	0.1248***
	(0.00671)	(0.00676)
Black (predicted)	0.1232***	0.1173***
	(0.00623)	(0.00621)
Hispanic (predicted)	-0.03052***	-0.0366***
	(0.0066)	(0.00668)
Tenant without attorney	-0.05457***	-0.06266***
	(0.00433)	(0.00432)
Ad damnum (1000s)	-0.04006***	-0.04249***
	(0.00314)	(0.00317)
Neighborhood poverty rate	-0.07974***	-0.0808***
	(0.00924)	(0.00929)
Neighborhood median rent	-0.03832***	-0.03267***
	(0.0062)	(0.00626)
Number of Observations	457,000	457,000

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This table shows results in column (1) from the regression of an indicator for if the individual was linked to Census records in Cook County 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). The regressions also include controls for court and year, and indicators for missing covariates (not reported). Standard errors are in parentheses. 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 number CBDRB-FY22-072.

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

	(1)	(2)
Evicted	-0.019**	
	(0.002)	
Judge stringency		-0.011
		(0.031)
Female	0.187**	0.187**
	(0.003)	(0.003)
Joint action	0.069**	0.068**
	(0.003)	(0.003)
Tenant without attorney	-0.008	-0.010*
	(0.005)	(0.004)
Ad damnum (1000s)	-0.004**	-0.004**
	(0.000)	(0.000)
Neighborhood poverty rate	-0.020*	-0.022**
	(0.008)	(0.008)
Neighborhood median rent	0.006	0.007
	(0.006)	(0.006)
Number of observations	444,565	444,565

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This table shows results in column (1) from the regression of an indicator for if the individual was linked to Experian records in Cook County regressed on covariates and eviction order, and column (2) shows the results from a regression on covariates and the stringency instrument. The regressions also include controls for court and year, and indicators for missing covariates (not reported). Standard errors are in parentheses and are clustered at the judge-year level.

Table D.3: Linked to Benefits—New York

	Matched to Benefits	
	(1)	(2)
Evicted	0.057***	
	(0.002)	
Judge 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)
Ad damnum (1000s)	-0.000	0.000
	(0.000)	(0.000)
Tenant without attorney	0.043***	0.022***
	(0.007)	(0.007)
Neighborhood poverty rate	0.200***	0.149***
	(0.008)	(0.008)
Neighborhood median rent	-0.001	0.076***
	(0.005)	(0.004)
Observations	577,823	577,823

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. 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). The regressions also include controls for court and year of filing, and indicators for missing covariates (not reported). Standard errors are in parentheses, and are clustered at the courtroom/judge-year level.

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

	Matched to Experian (1)	(2)
Evicted	-0.025***	
	(0.002)	
Judge stringency		-0.006
		(0.029)
Female (predicted)	0.198***	0.199***
	(0.003)	(0.003)
Ad damnum (1000s)	0.000	0.000
	(0.000)	(0.000)
Tenant without attorney	0.032***	0.029**
	(0.009)	(0.009)
Neighborhood poverty rate	-0.077***	-0.078***
	(0.014)	(0.014)
Neighborhood median rent	-0.055***	-0.056***
	(0.006)	(0.006)
Observations	278,875	278,875

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This table shows results in column (1) from the regression of an indicator for if the individual was linked to Experian 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. The regressions also include controls for court and year of filing, and indicators for missing covariates (not reported). Standard errors are in parentheses, and are clustered at the courtroom/judge-year level.

Table D.5: Matching—New York

	Matched to Benefits		Missing SSN	
	(1)	(2)	(3)	(4)
Judge stringency	-0.0131	-0.0112	0.0072	0.0083
	(0.0180)	(0.0175)	(0.0143)	(0.0139)
Controls	No	Yes	No	Yes
Observations	577,851	577,823	181,887	181,840

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This table reports the relationship between our instrument and indicators of data matching success in the New York City sample. In Columns (1) and (2), 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, with and without controls. In Columns (3) and (4), we regress an indicator for missing a valid social security number on our instrument. All specifications include court-by-time of filing fixed effects. Standard errors are in parentheses and are clustered at the courtroom-year level.

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

Sample Comparison

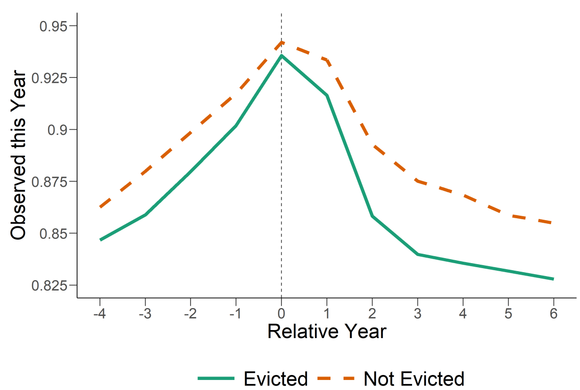
Variable	Matched Sample (2007-2016)	Housing Court Survey (2016)
	(1)	(2)
Female	0.70	0.66
Male	0.30	0.34
Age (Mean)	44.3	44.1
Age Distribution:		
19-24	0.05	0.03
25-34	0.24	0.22
35-44	0.25	0.29
45-54	0.25	0.25
55-64	0.14	0.14
65+	0.07	0.06
Has Children	0.47	0.51
Household Size:		
1	0.39	0.26
2	0.22	0.24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

E Appendix: Outcome trends around court filing: by city and additional results

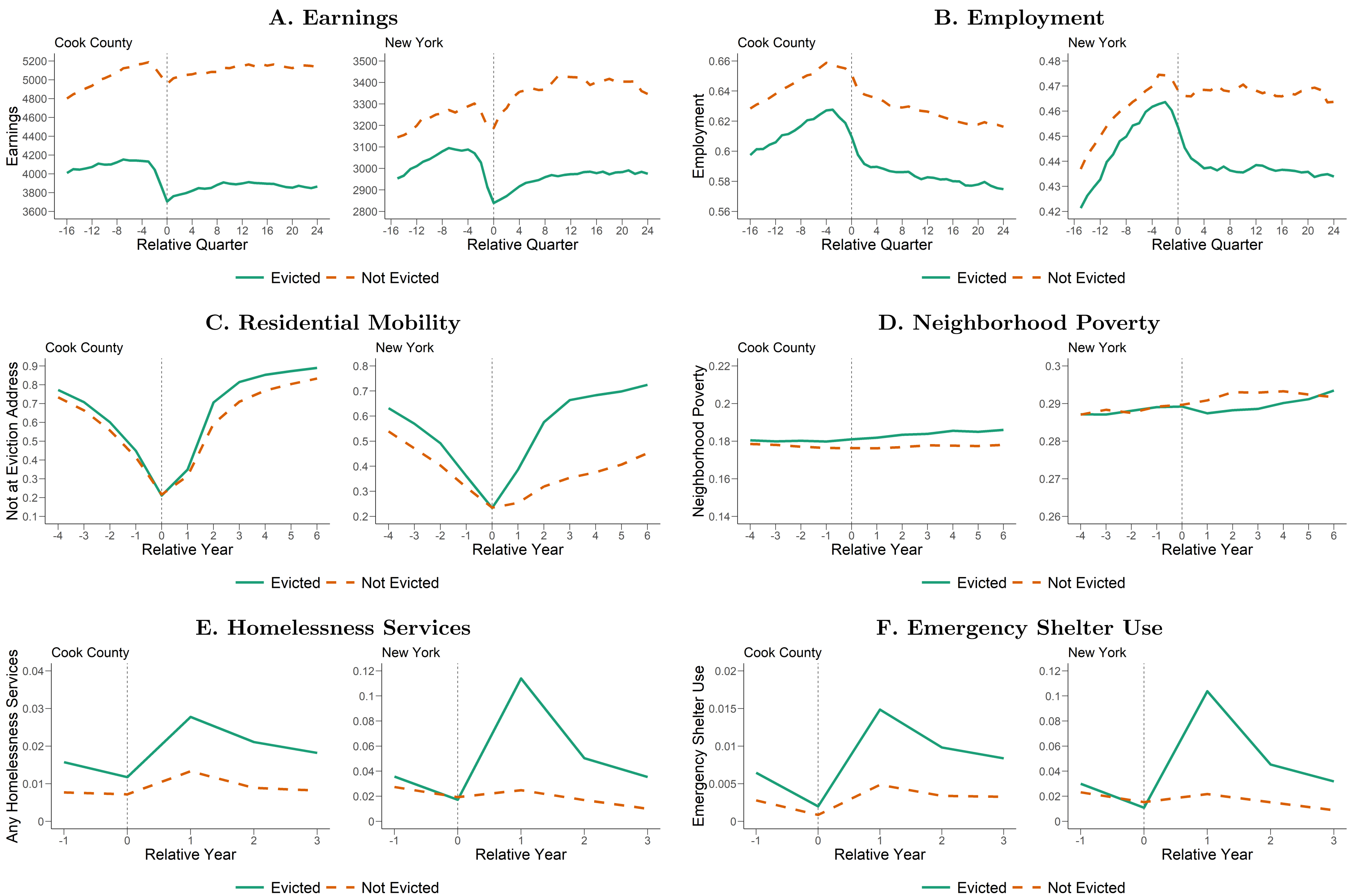
This section provides additional descriptive trends similar to those in Section 4 in the main draft. Figure E.1 shows trends for if the individual was reported at any address in the MAFARF data used in Cook County. Prior to eviction, evicted tenants are somewhat less likely to be observed than their non-evicted counterparts. This gap grows after the eviction case is filed. Figures E.2 and E.3 reproduce Tables 2 and 3 from the paper separately by city.

Figure E.1: Observed at address this year (Cook County)



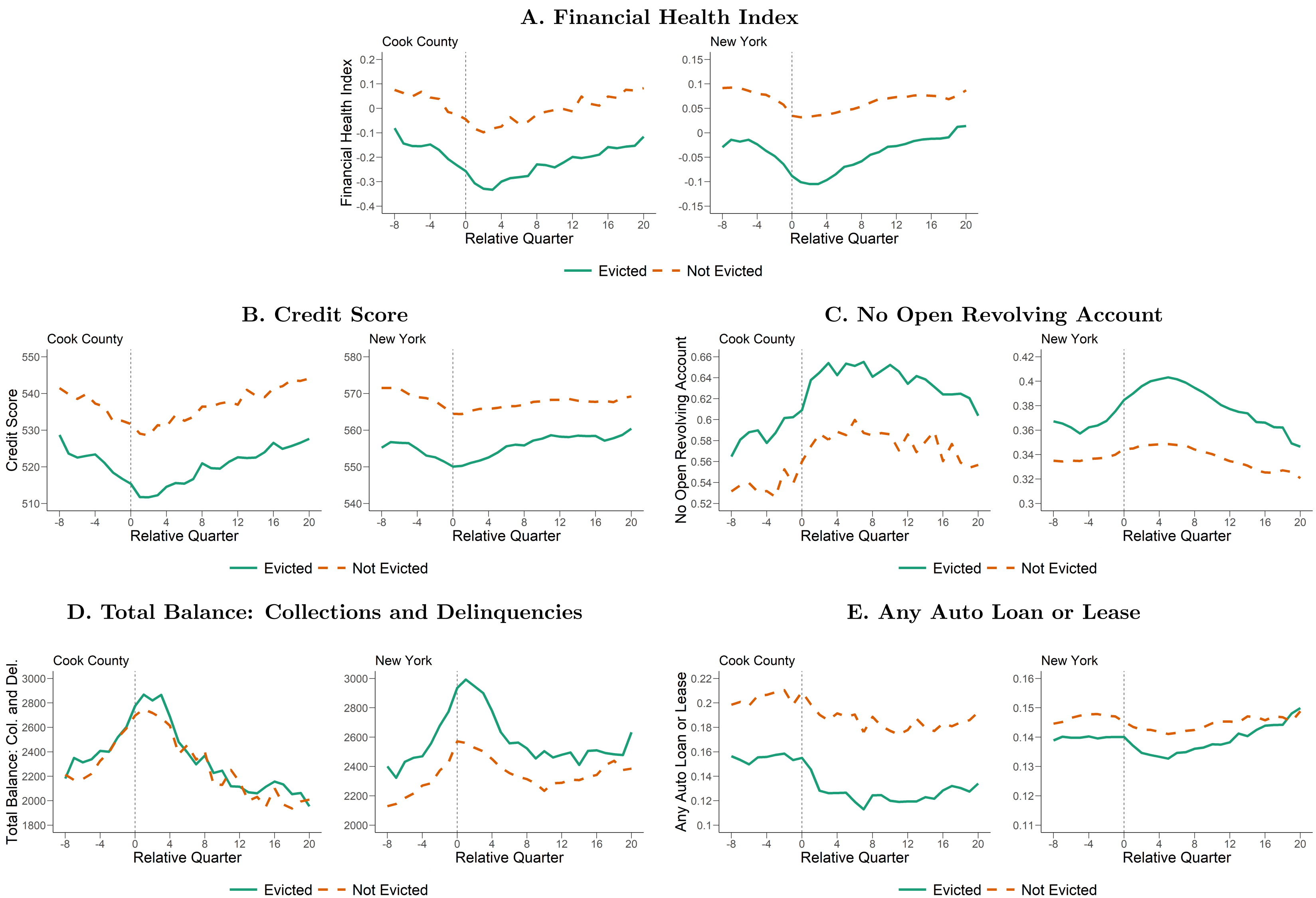
Notes: This figure plots trends on if a person was observed in the MAFARF data relative to eviction filing for each location, estimated using equation 4.1.The outcome is an indicator equal to 1 if an individual is recorded at an address in the MAFARF that year, and 0 otherwise, measured at an annual frequency. We plot results for the evicted and non-evicted groups in each time period. The only controls are calendar year dummies. 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 number CBDRB-FY22-072.

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



Notes: This figure plots trends in labor market and housing outcomes relative to eviction filing for each location, estimated using equation 4.1. The only controls are calendar year dummies. 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. The employment and earnings outcomes are measured at a quarterly frequency, while the housing outcomes are measured at an annual frequency. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

Figure E.3: Financial Health Outcomes Relative to Time of Eviction Filing



Notes: This figure plots trends in credit bureau outcomes relative to eviction filing for each location, estimated using equation 4.1. The only controls are calendar year dummies. 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. All outcomes are measured at a quarterly frequency.

F 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.[66] We show in Section 4 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 may 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 F.1 illustrates the extent of selection into eviction court and selection into the eviction case outcome within court with earnings as the outcome (Figure F.2 repeats the exercise for employment). We begin with our linked court sample and append a random sample of comparison tenants.[67] Focusing first on selection into court, the 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, adjusting for age and demographics. Evicted tenants earn over $1,600 less per quarter in Cook County and approximately $1000 less per quarter in New York. 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 F.1 are remarkably similar across cities.

If we were to interpret the leftmost bar in the top panel of Figure F.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 second bar, this difference shrinks by roughly one third, 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.

As we document in Section 4, there is also selection into who receives an eviction order

[66] See, for example, Burgard et al. (2012), Desmond and Shollenberger (2015), Desmond and Kimbro (2015), Desmond et al. (2015), Desmond and Gershenson (2017).

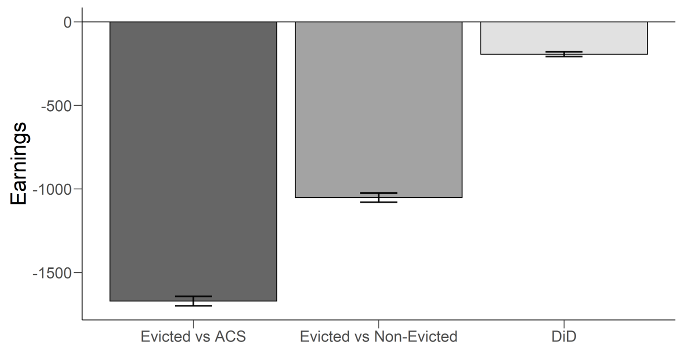
[67] In Cook County, this is a sample of renters in the ACS linked to quarterly earnings and employment records. In New York, it is a representative sample of adults receiving public assistance linked to quarterly earnings and employment records. In New York we do not observe whether individuals are 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 census tracts in Cook County and ZIP codes in New York.

within housing court. Evicted tenants have lower levels of earning and employment than not-evicted tenants in court the period before filing. We can adjust for differences in levels with a DiD estimator. The right-most bar adds estimates from a symmetric DiD, which is described in detail in J and is estimated using equation J.4. The difference between evicted tenants and not-evicted tenants in court shrinks further when we adjust for pre-existing differences in levels to -\$194 in New York and -\$157 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.

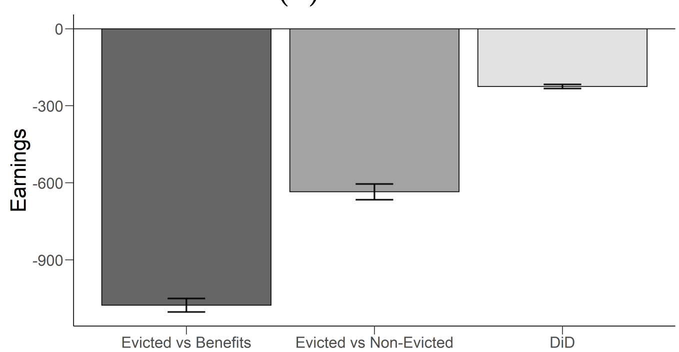
The extent of selection into eviction court reveals the importance of having an appropriate comparison group for evicted tenants; comparing evicted tenants to tenants from the same neighborhood is likely to overstate the impact of an eviction. The extent of selection within court into the eviction case 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.

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

(a) Cook County



(b) New York

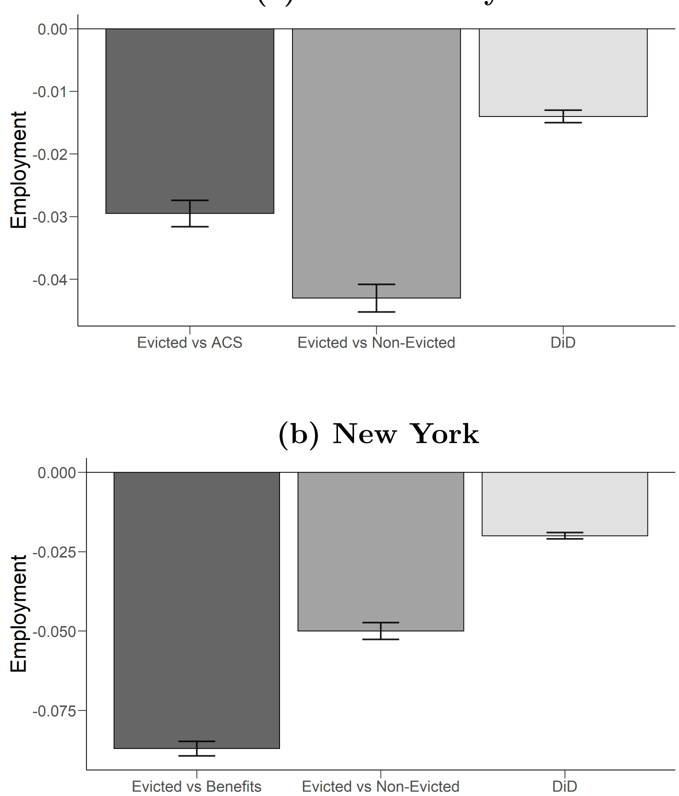


Notes: This figure shows evidence of earnings-based selection into housing court and eviction for Cook County and New York for quarters 1-8 after eviction case filing. Each bar represents the coefficient from a regression of earnings on a dummy for being evicted in housing court where the reference group is the second group listed under the bar. Earnings is defined as average quarterly 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" (or "Evicted vs Benefits" for New York) compares evicted individuals to an ACS (Benefits) weighted sample; "Evicted vs Non-Evicted" compares evicted individuals to non-evicted individuals in housing court; and "DiD" shows the results from our DiD specification. The regressions used to produce the estimates in the left-most bar and center bar include age, race, and gender controls. The DiD specification includes individual fixed effects and time relative to filing dummies and is described in equation . Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

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

(a) Cook County

(b) New York



Notes: This figure 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 Appendix Figure F.1 for details about specifications and definitions. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

Table F.1: Summary Statistics: Financial Health Outcomes

	Cook County			New York	
	Evicted (1)	Not Evicted (2)	Random Sample (3)	Evicted (4)	Not Evicted (5)
Person Characteristics					
Age at filing	36.536	36.647	37.049		
	(9.300)	(9.178)	(10.236)		
Female	0.653	0.649	0.489		
	(0.476)	(0.477)	(0.500)		
Outcomes: Quarters 1-8 Before Filing					
Financial health index	-0.174	0.022	0.683	-0.112	0.002
	(0.887)	(0.981)	(1.166)	(1.006)	(1.005)
Credit score	521	536	605	548	564
	(65)	(75)	(108)	(115)	(112)
No open revolving account	0.591	0.542	0.368	0.395	0.358
	(0.487)	(0.494)	(0.479)	(0.449)	(0.439)
Total balance: collections and delinquencies	2,340	2,301	1,458.082	2,448	2,234
	(4,953)	(5,014)	(4,420)	(6,136)	(5,842)
Any auto loan or lease	0.150	0.199	0.199	0.118	0.122
	(0.352)	(0.395)	(0.396)	(0.300)	(0.305)
Observations	105,666	57,254	250,881	48,319	108,543

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, and for New York. (We do not have a random sample for New York.) For the random sample, we randomly assign a placebo eviction date to compare baseline financial outcomes relative to the eviction sample. To permit comparability, the financial health variable in the random sample is standardized based on the eviction court sample's non-evicted mean and standard deviation in the filing year. 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.

G Appendix: Robustness of the IV empirical design

This section provides additional robustness of the IV empirical design, providing evidence behind the relevance, exogeneity, exclusion, and monotonicity assumptions.

G.1 First stage

Table G.1 shows that the strong first stage is robust to several different instrument constructions for each location. We show that the first stage is insensitive to controlling for other dimensions of judge behavior. Moreover, the first stage remains strong using an alternate approach to measuring the first judge/courtroom in the court records, excluding cases in which the tenant was never served, or constructing the instrument without imposing any restrictions on how many cases the judge sees per year.

Table G.2 reports the partial F-statistic from the first stage regressions of the eviction indicator on judge stringency with the full set of controls described in Table 2. Column (1) shows F-statistics for Cook County and Column (2) shows F statistics for New York. The first row reports the F-statistic for the full analysis sample. The second row reports the F-statistic when restricting the analysis to the female sample, and the final row reports the F-statistic for when restricting to the Black sample.

Table G.3 reports the coefficient on judge stringency from the first stage in the linked Experian samples for Cook County and New York.

Table G.1: First Stage Robustness

Sample	Coefficient	Standard Error	P-Value	Observations
Cook County				
Main	0.822	0.026	0.000	453,618
Controlling for other judge stringency dimensions	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
New York				
Main	0.850	0.018	0.000	577,850
Alternate courtroom construction	0.785	0.019	0.000	577,820
Controlling for other stringencies	0.866	0.021	0.000	577,850
All cases	0.954	0.008	0.000	899,599

Notes: The top panel 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. “Alternative first judge construction” assigns the first judge associated with the case after the tenant was served, rather than the judge implied by the random assignment of court room and time. “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. The bottom panel reports the first stage using the main specification (“Main”), using the last courtroom with a hearing rather than first courtroom assigned (“Alternate courtroom construction”), controlling for stay stringency and emergency assistance stringencies (“Controlling for other stringencies”), and using all cases including public housing cases and others not randomly assigned (“All cases”). Across both panels we use the full set of cases from each location rather than those linked to outcomes.

Table G.2: F-Statistics from First Stage Regressions

	Cook County (1)	New York (2)
F-Statistic - Full Sample	934	288
F-Statistic - Female Sample	563	233
F-Statistic - Black Sample	482	179

Notes: This table reports the partial F-statistics on the instrument from the first stage regression run on the full sample (shown in first row), on the female subsample (in second row) and on the Black subsample (in third row). Column (1) shows the F-statistics for Cook County, and column (2) shows the F-statistics for New York. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

Table G.3: Experian First Stage

	Cook County		New York	
	(1)	(2)	(3)	(4)
Judge stringency	0.723***	0.721***	0.898***	0.890***
	(0.043)	(0.043)	(0.025)	(0.026)
Controls	No	Yes	No	Yes
Observations	168,555	168,555	156,862	156,862

Notes: $^{*}\ p < 0.1,^{**}\ p < 0.05,^{***}\ p < 0.01$. This table reports results for the first stage regression of eviction on judge stringency for the Cook County and New York credit bureau samples. Columns (1) and (3) show the regression including only judge stringency. Columns (2) and (4) add controls listed in Table 7. All columns include court-year fixed effects. Standard errors are included in parentheses and are clustered at the judge(courtroom)-year level.

G.2 Exogeneity

As discussed in Section 5.1.2, the validity of our instrument depends on an assumption of exogeneity. Since both court systems randomly assign cases to judges/courtrooms (see Section 2) this assumptions seems plausible in our settings. In Table 3 we show that our judge stringency measure is uncorrelated with a variety of baseline characteristics of tenants in the samples.

Due to differences in data availability, our analytical samples tend to differ slightly by outcome. Therefore, we provide additional evidence that stringency is exogenous within each of the relevant outcome samples. The top panel of Table G.4 reports the coefficients on judge stringency from separate regressions of lagged values of each of our key outcomes on judge stringency. The bottom panel repeats this for baseline demographic and case characteristics. Across both locations, judge stringency appears uncorrelated with baseline outcomes, demographics, and case characteristics.

Similarly, in Table G.5 we estimate a series of "placebo" IV regressions. Each row reports the results of a separate regression where the dependent variable is a lagged value of an outcome, we instrument for eviction order with judge stringency, and we include controls from before the lagged outcome to mirror our IV specification used in Tables 4-6. In the third column of Table G.5, we produce a combined estimate from the two locations. In each case, we find that eviction is uncorrelated with the lagged outcomes when we instrument for it with judge stringency. Taken together, these results suggest that exogeneity is unlikely to be violated in our settings.

Table G.4: Testing the Relationship Between Stringency and Lagged Outcomes, and Between Stringency and Demographic Characteristics

	Cook		New York	
	$E[Y\|E=0]$ (1)	RF (2)	$E[Y\|E=0]$ (3)	RF (4)
Lagged Outcomes				
Earnings (1-8 quarters before filing)	4,978.0	-125.2	3,557.3	-109.9
	(5,549.0)	(319.5)	(4,427.3)	(412.1)
		[255,000]		[144,429]
Earnings (9-12 quarters before filing)	4,771.0	-61.5	3,477.7	-226.9
	(5,652.0)	(361.1)	(4,381.4)	(415.6)
		[249,000]		[144,429]
Emergency Shelter (1 year before filing)	0.0023	-0.0047	0.0050	0.0054
	(0.0475)	(0.0071)	(0.0706)	(0.0068)
		[19,000]		[181,887]
Any homelessness services (1 year before filing)	0.0104	0.0029	0.0073	-0.0046
	(0.1010)	(0.0201)	(0.0852)	(0.0095)
		[19,000]		[181,887]
Not at eviction address (1-2 years before filing)	0.6618	-0.0224	0.2571	-0.0086
	(0.4731)	(0.0327)	(0.4370)	(0.0514)
		[204,000]		[125,296]
Neighborhood poverty rate (1-2 years before filing)	0.1976	-0.0205*	0.2944	-0.0028
	(0.1247)	(0.0116)	(0.1192)	(0.0111)
		[114,000]		[124,362]
Demographic Characteristics				
Female	0.6223	0.0134	0.7260	-0.0371
	(0.4849)	(0.0300)	(0.4460)	(0.0280)
		[255,000]		[181,887]
Black	0.6649	-0.0099	0.5865	0.0408
	(0.4723)	(0.0365)	(0.4925)	(0.0431)
		[255,000]		[181,887]
Hispanic	-0.0078	0.0518	0.4551	0.0118
	(0.4475)	(0.0321)	(0.4980)	(0.0402)
		[255,000]		[181,887]
Ad damnum (1000s)	1.6860	-0.1356	3.6692	-0.2029
	(3.0380)	(0.2448)	(2.8324)	(0.2551)
		[255,000]		[181,887]
Joint action	0.7981	-0.1018**		
	(0.4014)	(0.0454)		
		[255,000]		
No prior	0.1741	-0.0290	0.5276	-0.0115
	(0.8619)	(0.0249)	(0.4992)	(0.0338)
		[255,000]		[181,887]

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This table shows results from the regressions of lagged values of key outcomes on judge stringency in the top panel, and from regressions of demographic characteristics on judge stringency in the bottom panel. Columns (1) and (2) report the non-evicted sample means ($\mathbb{E}[Y|E=0]$) and reduced form (RF) results for Cook County, and columns (3) and (4) report these estimates for New York. Outcomes are listed on the left of each row. Standard errors are in parentheses, and are clustered at the judge(courtroom)-year level. Observation counts are shown in brackets, below standard errors. 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 number CBDRB-FY22-072.

Table G.5: Placebo Tests: Estimating the Impact of Eviction on Pre-Filing Outcomes

	Cook County (1)	New York (2)	Combined (3)
Housing:			
Not at eviction address	0.007	-0.027	-0.010
	(0.055)	(0.049)	(0.037)
	[110,000]	[97,004]	[207,004]
Emergency shelter (1 year before filing)	-0.004	0.006	0.001
	(0.010)	(0.008)	(0.006)
	[19,000]	[181,840]	[200,840]
Any homelessness services (1 year before filing)	0.020	-0.006	0.007
	(0.031)	(0.011)	(0.017)
	[19,000]	[181,840]	[200,840]
Labor Market:			
Earnings	-35	59	12
	(221)	(242)	(164)
	[255,000]	[150,662]	[405,662]
Employment	-0.031	0.032	0.000
	(0.024)	(0.029)	(0.018)
	[255,000]	[150,662]	[405,662]

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table shows IV regressions for outcomes 1-8 quarters prior to eviction case filing, with the exception of emergency shelter and any homelessness services, which are 1-4 quarters prior to evictions case filing. These outcomes serve as placebos, since the instrument should not affect outcomes prior to the filing of the case. Standard errors are in parentheses, and are clustered at the judge(courtroom)-year level. Controls for all model specifications are the same as those described in Table 4. The earnings (1-8 quarters before filing) regression also controls for earnings 3 and 4 years before filing; the earnings (9-12 quarters before filing) regression also controls for earnings 5 and 6 years before filing; emergency shelter and any homelessness services regressions also control for emergency shelter and any homelessness services 2 and 3 years before filing; the not at eviction address and neighborhood poverty rate regressions also control for not at eviction address and neighborhood poverty rate 3 and 4 years before filing. Observation counts are shown in brackets, below standard errors. 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 number CBDRB-FY22-072.

G.3 Exclusion

We next evaluate whether there is evidence against the exclusion restriction. Wherever possible, we provide evidence from both Cook County and New York. However, since not all aspects of judge behavior are recorded in both settings, we occasionally rely on data from one location only. In Tables G.6 and G.8, we report correlations between our instrument (judge stringency in the eviction order) and other dimensions of stringency in judge behavior. Across both locations, all stringency measures are calculated as judge-year leave-one-out averages. For Cook County, we construct judge stringency in granting continuances, stringency in judgment amount, and stringency in granting stays. For New York, we construct stringency in granting stays and stringency in tenant receipt of emergency rental assistance in the 30 days after filing. The latter stringency measure aims to capture the idea that judges may differ in their propensity to encourage tenants to seek out emergency rental assistance, using data on receipt of emergency cash assistance. Across both locations, the eviction order stringency instrument is, at most, weakly correlated with other dimensions of judge behavior, lending confidence to the exclusion restriction holding in our settings.

Next, we examine how inclusion of alternative judge stringencies impacts the first stage relationship between eviction order and eviction order stringency. Table G.7 reports results the coefficient on eviction order stringency from a first stage with no other measures (columns 1 and 3), and then with the other stringencies included (columns 2 and 4). The first stage coefficients are largely unchanged when including other stringencies, consistent with the low correlation we find in Tables G.6 and G.8.

Given the relative salience of money judgments, we further explore the role of judge differences in these judgments in Cook County, where we are able to observe the money judgments included with an eviction order. First, we regress judgment amounts on eviction order stringency, shown in Table G.9. The estimates show that while judgment amount is strongly correlated with the ad damnum amount (the amount the landlord is seeking), it does not have a strong correlation with our main eviction order stringency measure. As we note in Section 5, judgment amounts are very closely linked to the amount landlord requests (correlation of 0.8), implying that judge differences in setting money judgment amounts are unlikely to drive case outcomes. Nevertheless, we re-estimate our main housing and labor market outcomes for the Cook County in quarters 1-4 after filing including additional controls for judgment amount stringency (column 2) in Table G.10. The estimates lose some precision in column (2), but the point estimates are broadly aligned with the main estimates.

Taking all of these results together, we do not find evidence that the exclusion restriction is violated in our context.

Table G.6: 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 G.7: First Stage with Alternative Stringency

	Cook County		**New York**	
	(1)	(2)	(3)	(4)
Judge stringency	0.740***	0.671***	0.825***	0.822***
	(0.024)	(0.043)	(0.057)	(0.058)
Alternative stringency		0.003		0.009
		(0.003)		(0.027)
Observations	268,000	215,000	150,662	150,662

Notes: This table reports results from the first stage regression of eviction on judge stringency, for Cook County and New York. Columns (1) and (3) include our main judge stringency measure and court-year fixed effects and controls. Columns (2) and (4) add a control for an additional dimension of judge stringency. In Cook County, this control is judgment amount stringency, while in New York, this control is judge stringency in granting stays of eviction orders, which allows tenants additional time before the city can perform the lockout. Judgment amount stringency is constructed using joint action cases that end in an eviction order (the only cases with a potential money judgment) and only for judges who see at least 100 of these cases, which leads to fewer observations in column (2). Judgment amount stringency is measured as the leave-one-out mean difference between the judgment amount and the ad damnum amount for each judge in each year. In New York, stay stringency is the leave-one-out mean likelihood of the judge in granting a stay of the eviction order. Standard errors are clustered at the judge(courtroom)-year level and are reported in parentheses. 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 number CBDRB-FY22-072.

Table G.8: 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 (emergency 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 G.9: Regression of log judgment amount on eviction order stringency

	Log judgment amount		
	(1)	(2)	(3)
Eviction 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

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table shows estimates of a regression of log judgment amount on judge stringency in granting eviction orders for Cook County, and is restricted to joint action cases with positive ad damnum amounts.

Table G.10: Robustness of IV estimates to controlling for amount stringency (Cook County)

	IV	IV with Amount Stringency Control
	(1)	(2)
Housing:		
Not at eviction address	0.093	0.038
	(0.057)	(0.066)
	[114,000]	[90,500]
Emergency shelter	0.023	0.033
	(0.028)	(0.037)
	[29,000]	[24,000]
Any homelessness services	0.012	0.054
	(0.042)	(0.061)
	[29,000]	[24,000]
Labor Market:		
Earnings	-445*	-421
	(249)	(323)
	[230,000]	[184,000]
Employment	0.003	0.017
	(0.027)	(0.033)
	[232,000]	[186,000]
Financial Health:		
Financial health index	-0.202**	-0.331***
	(0.102)	(0.108)
	[83,335]	[67,318]
Credit score	-8.69	-11.54
	(8.29)	(9.51)
	[91,394]	[73,931]
No open revolving account	-0.072*	-0.017
	(0.043)	(0.052)
	[92,028]	[74,397]
Total balance: collections and delinquencies	735	927
	(659)	(831)
	[83,537]	[67,467]
Any auto loan or lease	-0.130**	-0.172***
	(0.054)	(0.064)
	[92,028]	[74,397]

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. Column (1) shows the main IV estimates for labor market and housing outcomes in Cook County for quarters 1-4 after filing. 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). Results are only for Cook County, as judgment amount is not observed in New York. Standard errors for regression model coefficients are included in parentheses and are clustered at the judge-year level. Observation counts for each outcome and specification are shown in brackets under the standard errors. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

G.4 Monotonicity

We next provide evidence that the monotonicity assumption holds in our setting. Tables G.11 and G.12 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. (2021), Table G.13 runs the first stage regression where judge stringency is calculated on one sub-population (e.g., women) and then runs the first-stage on the complementary sub-population (e.g., men).

To better test potential monotonicity violations, we hand-collected data on judge characteristics in Cook County, where we observe the judge name on the case. Using information from Sullivan's Judicial Profiles (2017) supplemented with online sources, we compiled demographic information on over 150 judges who presided over most of the cases in our sample. For each judge, we determined gender based on the pronouns used in their respective biographies. Based on conversations with the people behind Sullivan's Judicial Profiles, the biographies are created from surveys of judges, and the pronouns are based on judges' responses.

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 references 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.[68] 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 available, 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.[69]

Table G.14 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 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 small sample sizes and are therefore excluded below.

Table G.15 shows the coefficient for stringency from the regression of the case outcome on stringency and various controls, restricted to a number of different sub-populations that include interactions between tenant and judge characteristics. Restricting our focus to the

[68] Puerto Rican is coded as Hispanic.

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

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 the monotonicity assumption.

Table G.11: 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 shown in the second column and are clustered at the judge(courtroom)-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 G.12: 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 courtroom-year level.

Table G.13: Split-Sample Monotonicity Checks

Stringency Sample	First Stage Sample	Cook County (1)	New York (2)
Female	Male	0.840***	0.916***
		(0.030)	(0.080)
Male	Female	0.785***	0.638***
		(0.034)	(0.058)
Black	Not Black	0.646***	0.812***
		(0.030)	(0.085)
Not Black	Black	0.571***	0.682***
		(0.033)	(0.058)
Joint Action Case	Single Action Case	0.363***	
		(0.018)	
Single Action Case	Joint Action Case	1.080***	
		(0.062)	
Rent Stabilized	Not Rent Stabilized		0.738***
			(0.094)
Not Rent Stabilized	Rent Stabilized		0.683***
			(0.066)
Small Landlord	Not Small Landlord	1.030***	
		(0.044)	
Not Small Landlord	Small Landlord	0.384***	
		(0.021)	

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. 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. "Small landlord" is defined as landlords with fewer than five cases appearing in the court sample. Standard errors are depicted in parentheses and are clustered at the judge(courtroom)-year level.

Table G.14: 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 difference (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 difference (10-90)" reports the percentage point difference between the 10th and 90th percentile of judge stringency for each group.

Table G.15: Cook County Monotonicity Checks, Two-Way Interactions

Pro Se Tenants	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. Sample is restricted to "pro se" tenants that have no formal legal representation.

H Appendix: Additional IV results

H.1 Heterogeneity on observables

This subsection presents an analysis of heterogeneity in the main estimates. We study heterogeneity by race, gender, and time period (Great Recession and non-Great Recession years).

In the paper, we report our main estimates for female and Black tenants. Table H.1 reproduces these estimates, and additionally includes the estimates for male and non-Black tenants. Table H.1 also provides tests of equality of the estimates for these subgroups. For most outcomes and time periods, we fail to reject a test of equality of differences, with a couple of notable exceptions. First, the impact of eviction on use of homelessness services in year 2 is significantly larger for women than men, and larger for Black tenants than non-Black tenants. This impact is seen in increased emergency shelter use by female tenants. Second, the negative impact on earnings is larger for women, and this pattern is seen across years 1 and 2, although it is not statistically significant at conventional levels. The negative impact on employment is larger for Black tenants than non-Black tenants, and this effect is statistically significant.

In the paper, we also report our main estimates for Cook County and New York separately. Table H.2 reproduces these estimates and provides tests of equality of the estimates across location. We fail to reject the null of equality for all outcomes in year 2, and all but two outcomes in year one. In year one, the difference in "any auto loan or lease" is statistically significant at the 5 percent level. This may partially be explained by different rates of car ownership in Cook County and New York though, given the point estimates are quite different, this likely could not account for the full difference. The difference between the financial health index is statistically significant at the 10 percent level. Given that "any auto loan or lease" is one of the four inputs into the financial health index, it may be driving both results.

Turning to heterogeneity by time period, we note that our main estimates cover a period that includes the Great Recession. An important question is whether the impact of eviction varies with business cycle conditions. We might expect the impact of eviction on earnings to be more negative during a recession if the negative impact of eviction is compounded by weaker labor market conditions. On the other hand, if demand for housing is lower, landlords may view potential tenants with an eviction record less negatively, and tenants may find housing more quickly, mitigating the negative impact of eviction.[70]

To explore heterogeneity with respect to business cycle conditions, we estimate the impact of eviction on earnings separately for Great Recession years (2008-2012) and non-Great Recession years in the sample. We provide estimates for quarters 1-4 and 5-8 after filing for earnings and financial health. The earnings estimates, presented in Table H.3, appear more negative in non-Great Recession years. The location-specific estimates, however, reveal

[70] For example, the literature on job displacement has found a more negative impact of job loss on future earnings during a recession (Davis and von Wachter, 2011).

a lack of precision after splitting the sample in Great Recession and non-Great Recessions years, making it difficult to draw strong conclusions. Importantly, it doesn't appear as if the negative labor market effects that we find are specific to the Great Recession period, if anything, they appear more pronounced in non-Great Recession years.

Turning to the financial health results, Table H.4 shows that eviction may more strongly impact financial health for Cook County in quarters 1-4 in non-Great Recession years compared to Great Recession years.[71] However, looking at the components of the index, and comparing estimates in quarters 1-4 to those in quarters 5-8, the results are mixed and lack precision. Overall, we conclude that, if anything, the impacts of eviction on labor market and financial outcomes appear stronger in non-Great Recession years, which is consistent with the scarring impact of an eviction order being more muted during downturns. The analysis also reveals that the main impacts we report in the paper are not driven entirely by Great Recession years, which is reassuring for the broader conclusions of the paper.

[71]The NYC linked credit data file only covers filings starting in 2014, so it was not possible to investigate what the impacts would've been in the Great Recession

Table H.1: Test of Equality Between Demographic Subgroups - IV Estimates

	1-4 Quarters After Filing			**5-8 Quarters After Filing**		
	Female (1)	Male (2)	p-value (3)	Female (4)	Male (5)	p-value (6)
By Gender						
Earnings:	-504***	151	0.104	-767***	-284	0.411
	(185)	(432)		(295)	(584)	
	[246,907]	[126,974]		[220,846]	[115,044]	
Employment:	-0.036	0.034	0.125	-0.003	-0.069	0.295
	(0.025)	(0.041)		(0.034)	(0.058)	
	[247,907]	[127,974]		[223,846]	[117,044]	
Not at eviction address:	0.093**	0.041	0.527	0.136**	0.031	0.342
	(0.046)	(0.071)		(0.060)	(0.101)	
	[160,045]	[80,238]		[129,598]	[67,080]	
Emergency shelter:	0.024	0.053**	0.369	0.024	-0.050	0.015
	(0.018)	(0.026)		(0.015)	(0.031)	
	[147,200]	[63,629]		[137,616]	[61,249]	
Any homelessness services:	0.030	0.031	0.981	0.068***	-0.029	0.008
	(0.023)	(0.036)		(0.020)	(0.031)	
	[147,200]	[63,629]		[137,616]	[61,249]	
	Black (1)	Not Black (2)	p-value (3)	Black (4)	Not Black (5)	p-value (6)
By Race						
Earnings:	-377	-277	0.805	-931***	-405	0.325
	(234)	(335)		(307)	(446)	
	[245,625]	[128,758]		[221,695]	[114,189]	
Employment:	-0.059*	0.043	0.039	-0.089**	0.075	0.014
	(0.031)	(0.034)		(0.040)	(0.051)	
	[246,625]	[129,758]		[224,695]	[116,189]	
Not at eviction address:	0.066	0.064	0.982	0.098	0.126	0.825
	(0.056)	(0.068)		(0.080)	(0.091)	
	[151,612]	[88,671]		[125,692]	[70,987]	
Emergency shelter:	0.036	0.030	0.866	0.007	-0.014	0.438
	(0.024)	(0.025)		(0.019)	(0.017)	
	[125,464]	[85,261]		[119,108]	[79,754]	
Any homelessness services:	0.049*	-0.005	0.225	0.057***	0.002	0.077
	(0.029)	(0.033)		(0.022)	(0.019)	
	[125,464]	[85,261]		[119,108]	[79,754]	

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This table reports race and gender specific estimates for the main labor market and housing outcomes and tests if male and female coefficients are equal (top panel) and if Black and not Black coefficients are equal (bottom panel). Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

Table H.2: Test of Equality Between Locations - IV Estimates

	1-4 Quarters After Filing			**5-8 Quarters After Filing**		
	Cook (1)	New York (2)	p-value (3)	Cook (4)	New York (5)	p-value (6)
Labor market outcomes						
Earnings:	-445*	-201	0.509	-627*	-599*	0.957
	(249)	(245)		(337)	(363)	
	[230,000]	[144,400]		[215,000]	[121,396]	
Employment:	0.003	-0.032	0.411	-0.010	-0.027	0.748
	(0.027)	(0.032)		(0.030)	(0.046)	
	[232,000]	[144,400]		[219,000]	[121,396]	
Housing outcomes						
Not at eviction address:	0.093	0.071	0.765	0.074	0.149*	0.475
	(0.057)	(0.045)		(0.064)	(0.084)	
	[114,000]	[104,228]		[114,000]	[69,227]	
Emergency shelter:	0.023	0.046**	0.638	-0.019	0.016	0.330
	(0.028)	(0.019)		(0.019)	(0.017)	
	[29,000]	[181,840]		[38,500]	[160,398]	
Any homelessness services:	0.012	0.046**	0.480	0.048**	0.024	0.552
	(0.042)	(0.018)		(0.023)	(0.019)	
	[29,000]	[181,840]		[38,500]	[160,398]	
Financial health outcomes						
Financial health index:	-0.202**	-0.011	0.083	-0.230	-0.047	0.227
	(0.102)	(0.058)		(0.174)	(0.065)	
	[83,335]	[186,479]		[83,781]	[187,449]	
Credit score:	-8.69	-7.03	0.876	-24.16**	-8.90	0.243
	(8.29)	(6.21)		(11.15)	(7.33)	
	[91,394]	[186,479]		[91,184]	[187,449]	
No open revolving account:	-0.072*	-0.006	0.157	0.080	0.024	0.480
	(0.043)	(0.025)		(0.099)	(0.027)	
	[92,028]	[186,479]		[91,762]	[187,449]	
Total balance: collections and delinquencies:	735	-115	0.274	739	357	0.649
	(659)	(428)		(930)	(377)	
	[83,537]	[186,479]		[83,998]	[187,449]	
Any auto loan or lease:	-0.130**	0.008	0.011	0.037	0.025	0.840
	(0.054)	(0.027)		(0.066)	(0.026)	
	[92,028]	[186,479]		[91,762]	[187,449]	

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This table reports location specific estimates for the main labor market, housing and financial health outcomes and tests if Cook County and New York coefficients are equal. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

Table H.3: Great Recession Robustness: Earnings

	1-4 Quarters After Filing						**5-8 Quarters After Filing**					
	Non-Great Recession			Great Recession			Non-Great Recession			Great Recession		
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Earnings:	4,530	-233***	-439**	3,789	-218***	3	4,514	-285***	-688**	3,825	-238***	-551
	(3,822)	(13)	(202)	(3,724)	(15)	(357)	(3,917)	(19)	(310)	(3,812)	(17)	(414)
			[232,864]			[141,036]			[197,860]			[139,036]
By Location												
Cook County	4,950	-308***	-502*	4,587	-248***	-352	4,959	-351***	-572	4,587	-271***	-820
	(5,715)	(15)	(289)	(5,972)	(23)	(480)	(5,869)	(21)	(389)	(6,093)	(27)	(650)
			[148,000]			[81,500]			[136,000]			[79,500]
New York	4,109	-158***	-375	2,991	-187***	358	4,070	-219***	-805*	3,062	-205***	-282
	(5,076)	(20)	(283)	(4,452)	(19)	(527)	(5,189)	(31)	(482)	(4,584)	(21)	(512)
			[84,864]			[59,536]			[61,860]			[59,536]

Notes: The table above presents OLS and IV regressions studying the impact of eviction on several key short-run outcomes, separately by time period. We divide the data into a Great Recession time period (2008-2012) and all other sample years.

Table H.4: Great Recession Robustness in Cook County: Financial Health

	1-4 Quarters After Filing						5-8 Quarters After Filing					
	Non-Great Recession			Great Recession			Non-Great Recession			Great Recession		
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Financial health index:	-0.032	-0.147***	-0.362***	-0.102	-0.115***	-0.060	0.016	-0.162***	-0.086	-0.077	-0.092***	-0.495
	(0.984)	(0.009)	(0.118)	(0.992)	(0.011)	(0.154)	(1.019)	(0.010)	(0.206)	(0.979)	(0.012)	(0.361)
			[30,633]			[52,702]			[45,385]			[38,396]
Credit Score:	530.51	-9.62***	-1.35	532.80	-8.92***	-14.73	535.65	-10.55***	-16.15	537.73	-8.05***	-42.86*
	(73.64)	(0.71)	(10.72)	(74.28)	(0.77)	(11.91)	(75.79)	(0.75)	(12.82)	(73.12)	(0.83)	(24.90)
			[33,067]			[58,327]			[48,988]			[42,196]
No open revolving account:	0.556	0.047***	-0.031	0.606	0.024***	-0.107*	0.552	0.054***	-0.021	0.632	0.010**	0.306*
	(0.497)	(0.004)	(0.063)	(0.486)	(0.004)	(0.057)	(0.497)	(0.005)	(0.125)	(0.479)	(0.005)	(0.185)
			[33,312]			[58,716]			[49,305]			[42,457]
Total balance: collections and delinquencies:	2,746	53	895	2,767	54	667	2,593	-81	423	2,427	20	1,281
	(5,572)	(64)	(903)	(5,462)	(44)	(936)	(5,424)	(52)	(1,071)	(5,131)	(53)	(1,821)
			[30,711]			[52,826]			[45,478]			[38,520]
Any auto loan or lease:	0.216	-0.044***	-0.293***	0.186	-0.037***	0.016	0.219	-0.057***	0.073	0.173	-0.036***	-0.046
	(0.412)	(0.004)	(0.080)	(0.387)	(0.003)	(0.062)	(0.413)	(0.004)	(0.072)	(0.375)	(0.004)	(0.143)
			[33,312]			[58,716]			[49,305]			[42,457]

Notes: The table above presents OLS and IV regressions studying the impact of eviction on several key short-run outcomes, separately by time period. We divide the data into a Great Recession time period (2008-2012) and all other sample years.

H.2 The impacts of eviction 3-4 and 5-6 years after filing

This subsection reports the long-run effects of eviction on housing, labor market, and financial outcomes. We report estimates for housing outcomes in years 3-4 and 5-6 after filing, and the equivalent time periods for labor market and financial outcomes (quarters 9-16 and 17-24 after filing).

Table H.5 reports the long-run estimates for housing outcomes. The key takeaway is that the impact of eviction on moving residences diminishes in the long run and becomes statistically insignificant. This result is consistent with the interpretation that in the long run, the causal effect of the eviction order for marginal tenants is muted by the non-evicted group becoming more likely to move. The long-run estimates for homelessness are too imprecise to yield strong conclusions.

Table H.6 reports the long-run estimates for labor market outcomes. The IV estimates are diminished in magnitude relative to the short-run estimates, although again we note the imprecision and difficulty of drawing strong conclusions about the long-run effects from this analysis.

An interesting exception to this pattern is the analysis of financial outcomes, presented in Table H.7. In the 9-16 quarters after filing, the financial health index is -.206 standard deviations lower than the non-evicted group in the filing year, and this estimate becomes more negative in the long run, -.248 standard deviations lower. Both estimates are significant at the 5 percent level. These results are driven by a larger impact on credit score and unpaid bills in the 9-16 quarters after filing, and a negative impact on access to credit in the 17-24 quarters after filing, as seen in the impact on having no open source of revolving credit and on having an auto loan.

Taken as a whole, the long-run estimates show that while the housing and labor market effects appear to diminish in the long run, the negative impact on financial health and access to credit are longer lasting. These results show that an eviction has a negative impact on individuals' financial health that lasts years after the case concludes.

Table H.5: Housing Outcomes: Estimates 3-4 and 5-6 Years after Filing

	3-4 Years After Filing			**5-6 Years After Filing**		
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Not at eviction address:	0.666	0.123***	0.047	0.762	0.095***	0.084
	(0.325)	(0.003)	(0.059)	(0.298)	(0.004)	(0.063)
			[162,540]			[134,428]
By Location						
Cook County	0.773	0.045***	-0.022	0.820	0.025***	0.018
	(0.419)	(0.003)	(0.056)	(0.384)	(0.003)	(0.054)
New York	0.560	0.201***	0.116	0.704	0.165***	0.149
	(0.496)	(0.006)	(0.104)	(0.456)	(0.006)	(0.114)
By Group						
Female	0.656	0.131***	0.026	0.754	0.103***	0.096
	(0.323)	(0.004)	(0.061)	(0.299)	(0.004)	(0.069)
Black	0.656	0.133***	0.009	0.760	0.102***	0.073
	(0.325)	(0.004)	(0.071)	(0.298)	(0.004)	(0.075)
Neighborhood poverty rate:	0.243	-0.000	-0.015	0.241	0.001	0.006
	(0.090)	(0.001)	(0.018)	(0.092)	(0.001)	(0.022)
			[129,327]			[105,182]
By Location						
Cook County	0.195	0.003***	0.025	0.194	0.004***	0.033*
	(0.133)	(0.001)	(0.021)	(0.135)	(0.001)	(0.020)
New York	0.291	-0.004***	-0.054*	0.288	-0.001	-0.020
	(0.123)	(0.001)	(0.030)	(0.124)	(0.002)	(0.040)
By Group						
Female	0.254	-0.001	-0.024	0.251	0.001	-0.012
	(0.091)	(0.001)	(0.025)	(0.092)	(0.001)	(0.035)
Black	0.263	-0.003**	-0.024	0.260	-0.001	-0.045
	(0.089)	(0.001)	(0.030)	(0.090)	(0.001)	(0.039)
Emergency shelter:	0.012	0.016***	-0.012	0.014	0.012***	0.028
	(0.078)	(0.001)	(0.023)	(0.083)	(0.001)	(0.025)
			[169,500]			[122,925]
By Location						
Cook County	0.009	0.010***	0.002	0.008	0.006***	0.012
	(0.095)	(0.001)	(0.027)	(0.087)	(0.001)	(0.018)
New York	0.016	0.023***	-0.025	0.020	0.019***	0.044
	(0.125)	(0.001)	(0.038)	(0.141)	(0.002)	(0.047)
By Group						
Female	0.012	0.017***	0.032	0.013	0.014***	0.025
	(0.077)	(0.001)	(0.027)	(0.080)	(0.001)	(0.030)
Black	0.015	0.018***	-0.055*	0.018	0.013***	0.019
	(0.085)	(0.001)	(0.030)	(0.093)	(0.001)	(0.034)
Any homelessness services:	0.020	0.020***	-0.022	0.020	0.015***	0.036
	(0.100)	(0.001)	(0.028)	(0.100)	(0.001)	(0.030)
			[169,500]			[122,925]
By Location						
Cook County	0.023	0.014***	-0.016	0.018	0.011***	0.039
	(0.148)	(0.001)	(0.037)	(0.132)	(0.002)	(0.030)
New York	0.018	0.025***	-0.028	0.023	0.020***	0.033
	(0.133)	(0.001)	(0.041)	(0.150)	(0.002)	(0.051)
By Group						
Female	0.020	0.021***	0.029	0.020	0.017***	0.037
	(0.100)	(0.001)	(0.031)	(0.100)	(0.001)	(0.037)
Black	0.025	0.022***	-0.065*	0.026	0.017***	0.038
	(0.109)	(0.001)	(0.036)	(0.112)	(0.002)	(0.040)

Notes: $^{*}\ p < 0.1,^{**}\ p < 0.05,^{***}\ p < 0.01$. This table extends results from Tables 4 and 5 for the periods 3-4 years after filing and 5-6 years after filing. Standard errors are clustered at the judge(courtroom)-year level, and shown in parentheses. Observation counts for the main combined specifications are reported below the standard errors in columns (3) and (6). Observation counts for all long-run OLS and IV regressions can be found in Appendix Table H.16. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

Table H.6: Labor Market Outcomes: Estimates 9-16 and 17-24 Quarters after Filing

	9-16 Quarters After Filing			17-24 Quarters After Filing		
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Earnings:	4,168	-290***	-217	4,114	-355***	-237
	(3,887)	(14)	(316)	(3,975)	(21)	(513)
			[298,552]			[227,845]
By Location						
Cook County	4,877	-344***	-138	4,933	-424***	-691
	(6,019)	(19)	(330)	(6,216)	(27)	(521)
New York	3,460	-235***	-296	3,294	-286***	217
	(4,919)	(20)	(538)	(4,955)	(32)	(885)
By Group						
Female	4,005	-267***	-253	3,927	-302***	-252
	(3,605)	(16)	(372)	(3,666)	(24)	(469)
Black	4,129	-275***	-438	4,053	-332***	-268
	(3,693)	(17)	(465)	(3,767)	(26)	(615)
Employment:	0.527	-0.019***	-0.019	0.508	-0.020***	-0.008
	(0.312)	(0.001)	(0.032)	(0.314)	(0.002)	(0.051)
			[303,552]			[232,845]
By Location						
Cook County	0.607	-0.013***	0.034	0.599	-0.013***	-0.008
	(0.423)	(0.002)	(0.028)	(0.430)	(0.002)	(0.037)
New York	0.447	-0.024***	-0.072	0.416	-0.027***	-0.008
	(0.458)	(0.002)	(0.057)	(0.457)	(0.003)	(0.095)
By Group						
Female	0.546	-0.019***	-0.006	0.526	-0.020***	0.001
	(0.310)	(0.002)	(0.040)	(0.313)	(0.002)	(0.056)
Black	0.543	-0.020***	-0.069	0.522	-0.020***	-0.056
	(0.311)	(0.002)	(0.047)	(0.313)	(0.002)	(0.061)

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This table extends results from Table 6 for the periods 9-16 quarters after filing and 17-24 quarters after filing. Standard errors are clustered at the judge(courtroom)-year level, and shown in parentheses. Observation counts for the main combined specifications are reported below the standard errors in columns (3) and (6). Observation counts for all long-run OLS and IV regressions can be found in Appendix Table H.16. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

Table H.7: Financial Health Outcomes: Estimates 9-16 and 17-24 Quarters after Filing

	9-16 Quarters After Filing			17-24 Quarters After Filing		
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Financial health index:	0.077	-0.103***	-0.214***	0.180	-0.106***	-0.262**
	(0.749)	(0.004)	(0.066)	(0.751)	(0.005)	(0.124)
			[306,223]			[197,230]
By Location						
Cook County	0.045	-0.139***	-0.275***	0.166	-0.145***	-0.212*
	(0.997)	(0.006)	(0.102)	(1.031)	(0.006)	(0.117)
New York	0.108	-0.068***	-0.154*	0.195	-0.067***	-0.313
	(1.117)	(0.006)	(0.083)	(1.092)	(0.008)	(0.219)
Credit score:	557.59	-8.01***	-16.78***	564.60	-9.50***	-14.30
	(67.14)	(0.35)	(6.29)	(73.66)	(0.42)	(12.98)
By Location						
Cook County	543.01	-8.84***	-18.01**	552.06	-9.58***	-5.90
	(75.70)	(0.40)	(8.42)	(79.55)	(0.43)	(9.18)
New York	572.17	-7.18***	-15.55*	577.14	-9.41***	-22.70
	(110.90)	(0.57)	(9.34)	(124.00)	(0.73)	(24.28)
No open revolving account:	0.453	0.037***	0.045	0.430	0.036***	0.093*
	(0.322)	(0.002)	(0.034)	(0.334)	(0.003)	(0.056)
By Location						
Cook County	0.579	0.041***	0.038	0.541	0.043***	0.060
	(0.489)	(0.003)	(0.054)	(0.496)	(0.003)	(0.075)
New York	0.327	0.034***	0.053	0.318	0.029***	0.126
	(0.421)	(0.002)	(0.041)	(0.447)	(0.004)	(0.082)
Total balance: collections and delinquencies:	2,212	50**	847**	2,113	44	730
	(3,584)	(21)	(335)	(3,870)	(27)	(730)
By Location						
Cook County	2,199	30	1,234**	2,064	34	-56
	(4,956)	(29)	(535)	(4,890)	(34)	(697)
New York	2,226	70**	460	2,162	54	1,516
	(5,179)	(31)	(404)	(6,000)	(42)	(1,283)
Any auto loan or lease:	0.201	-0.026***	-0.029	0.202	-0.024***	-0.083*
	(0.282)	(0.001)	(0.028)	(0.283)	(0.002)	(0.045)
By Location						
Cook County	0.193	-0.048***	-0.050	0.212	-0.046***	-0.177***
	(0.391)	(0.002)	(0.046)	(0.407)	(0.002)	(0.058)
New York	0.208	-0.004*	-0.009	0.191	-0.002	0.011
	(0.406)	(0.002)	(0.034)	(0.393)	(0.003)	(0.068)

Notes: $^{*}\ p < 0.1,^{**}\ p < 0.05,^{***}\ p < 0.01$. This table extends results from Table 7 for the periods 9-16 quarters after filing and 17-24 quarters after filing. Standard errors are clustered at the judge(courtroom)-year level, and shown in parentheses. Observation counts for the main combined specifications are reported below the standard errors in columns (3) and (6) in the top panel. Observation counts for all long-run OLS and IV regressions can be found in Appendix Table H.16.

Table H.8: Hospital Outcomes: Estimates 9-16 Quarters after Filing

	9-16 Quarters After Filing		
	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)
Number of hospital visits	1.330	0.116***	-0.162
	(3.670)	(0.028)	(0.881)
Number of emergency visits	1.071	0.087***	-0.440
	(3.238)	(0.024)	(0.753)
Number of mental health visits	0.094	0.039***	0.057
	(0.982)	(0.009)	(0.278)
			[82,719]

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This table extends results from Table 8 for the period 9-16 quarters after filing. Standard errors are clustered at the judge(courtroom)-year level, and shown in parentheses. Observation counts for all regressions are reported below the standard errors in column (6).

H.3 Additional outcomes

Table H.9 reports IV and OLS results on the impact of eviction on future eviction at the same address. This outcome allows us to better characterize what happens in the future for tenants in court who avoid an eviction order. The table reports estimates for new eviction cases at the same address within one year, and within two years. The main takeaway is that being evicted reduces the likelihood of a future eviction at the same address by 9.5pp in year 1, and by 11.9pp in years 1 or 2. It is unsurprising that eviction decreases the likelihood of eviction at the same address, since tenants are legally obligated to move after eviction. What is notable is that the non-evicted mean is 8.8pp in the year 1 and 13.9pp within 2 years. Hence, while non-evicted tenants face some probability of a future eviction at the same address, the probability is modest.

Table H.9: Impact of Eviction on Future Eviction

	1 Year After Filing			**2 Years After Filing**		
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Future eviction:	0.088	-0.047***	-0.095***	0.139	-0.072***	-0.119***
	(0.200)	(0.001)	(0.015)	(0.244)	(0.001)	(0.021)
			[911,564]			[859,314]
By Location						
Cook County	0.095	-0.048***	-0.141***	0.124	-0.062***	-0.184***
	(0.294)	(0.001)	(0.018)	(0.329)	(0.002)	(0.019)
New York	0.080	-0.046***	-0.048**	0.154	-0.082***	-0.054
	(0.271)	(0.001)	(0.024)	(0.361)	(0.002)	(0.037)
By Group						
Female	0.089	-0.047***	-0.092***	0.142	-0.074***	-0.113***
	(0.202)	(0.001)	(0.019)	(0.247)	(0.002)	(0.025)
Black	0.097	-0.054***	-0.103***	0.153	-0.084***	-0.126***
	(0.209)	(0.001)	(0.022)	(0.254)	(0.002)	(0.034)

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. 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 and two-stage least squares (IV) estimates of the impact of eviction on future eviction at the same address. Below the combined estimates, we report estimates separately for each location and for the female and Black subsamples. "Future eviction" is an indicator for if the individual has a future eviction at the same address. The outcome is defined cumulatively so that for columns (4)-(6) the outcome is an indicator for a future eviction in either the first or second year after filing. Each regression includes the main controls listed in Table 4. Standard errors are in parentheses and are clustered at the judge(courtroom)-year level. Observations for the main combined specifications are reported in brackets below the standard errors in columns (3) and (6). Other observation counts for all regressions shown above can be found in Appendix Table H.15.

H.4 Reduced Form Estimates

This subsection provides reduced-form estimates for the IV results presented in Tables 4-8.

Table H.10: Reduced Form for Table 4: Impacts of Eviction on Housing Situation

	1 Year After Filing		2 Years After Filing	
	$\mathbb{E}[Y\|E=0]$ (1)	RF (2)	$\mathbb{E}[Y\|E=0]$ (3)	RF (4)
Enforcement:	0.002	0.332***	0.002	0.324***
	(0.031)	(0.028)	(0.032)	(0.028)
		[329,279]		[307,837]
By Location				
Cook County	0.004	0.357***	0.004	0.350***
	(0.059)	(0.041)	(0.062)	(0.040)
New York	0.000	0.307***	0.000	0.297***
	(0.017)	(0.039)	(0.018)	(0.038)
By Group				
Female	0.002	0.333***	0.002	0.329***
	(0.030)	(0.038)	(0.032)	(0.038)
Black	0.002	0.378***	0.002	0.357***
	(0.032)	(0.036)	(0.035)	(0.036)
Not at eviction address:	0.293	0.063**	0.478	0.083**
	(0.318)	(0.027)	(0.348)	(0.040)
		[218,228]		[183,227]
By Location				
Cook County	0.363	0.065*	0.568	0.052
	(0.481)	(0.039)	(0.495)	(0.045)
New York	0.222	0.060	0.389	0.115*
	(0.415)	(0.038)	(0.487)	(0.065)
By Group				
Female	0.280	0.070**	0.461	0.100**
	(0.312)	(0.034)	(0.343)	(0.045)
Black	0.272	0.047	0.454	0.068
	(0.310)	(0.040)	(0.345)	(0.056)
Neighborhood poverty rate:	0.247	-0.001	0.246	-0.007
	(0.088)	(0.007)	(0.090)	(0.011)
		[173,909]		[127,891]
By Location				
Cook County	0.195	-0.010	0.196	0.008
	(0.130)	(0.013)	(0.133)	(0.015)
New York	0.298	0.007	0.295	-0.023
	(0.120)	(0.006)	(0.123)	(0.016)
By Group				
Female	0.258	-0.003	0.256	-0.007
	(0.090)	(0.009)	(0.091)	(0.014)
Black	0.267	-0.004	0.266	-0.020
	(0.088)	(0.010)	(0.090)	(0.016)

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This table reports equally-weighted averages of Cook County and New York non-evicted sample means ($\mathbb{E}[Y|E=0]$), and reduced form (RF) estimates of the impact of eviction on outcomes related to the tenant's housing situation. Outcomes are listed on the left of each row. Results are shown for 1-4 quarters (columns (1)-(3)) and for 5-8 quarters (columns (4)-(6)) after the eviction case is filed. Each panel shows results for a given outcome. Below the combined estimates in each panel, we report estimates separately for each location and for the female and Black subsamples. These RF estimates correspond to the IV estimates from Table 4, and controls are the same as those for the IV estimates in Table 4. Standard errors for regression model coefficients are included in parentheses and are clustered at the judge(courtroom)-year level. Observations for the main combined specifications are reported in brackets below the standard errors in columns (3) and (6). Observation counts for all RF regressions can be found in Appendix Table H.15. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

Table H.11: Reduced Form for Table 5: Impacts of Eviction on Homelessness Services

	1 Year After Filing		**2 Years After Filing**	
	$\mathbb{E}[Y\|E=0]$ (1)	RF (2)	$\mathbb{E}[Y\|E=0]$ (3)	RF (4)
Emergency shelter:	0.009	0.027**	0.008	-0.000
	(0.068)	(0.013)	(0.062)	(0.009)
		[210,840]		[198,898]
By Location				
Cook County	0.007	0.016	0.006	-0.013
	(0.086)	(0.020)	(0.077)	(0.013)
New York	0.011	0.038**	0.009	0.012
	(0.105)	(0.016)	(0.097)	(0.013)
By Group				
Female	0.009	0.019	0.008	0.018
	(0.066)	(0.013)	(0.061)	(0.011)
Black	0.010	0.026	0.009	0.005
	(0.072)	(0.017)	(0.068)	(0.013)
Any homelessness services:	0.015	0.024	0.012	0.026**
	(0.086)	(0.017)	(0.076)	(0.011)
		[210,840]		[198,898]
By Location				
Cook County	0.017	0.009	0.012	0.033**
	(0.128)	(0.030)	(0.110)	(0.016)
New York	0.013	0.038**	0.011	0.019
	(0.114)	(0.015)	(0.104)	(0.014)
By Group				
Female	0.015	0.023	0.012	0.049***
	(0.086)	(0.016)	(0.077)	(0.014)
Black	0.017	0.035*	0.014	0.038**
	(0.092)	(0.021)	(0.083)	(0.015)

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This table reports equally-weighted averages of Cook County and New York non-evicted sample means ($\mathbb{E}[Y|E = 0]$), and reduced form (RF) estimates of the impact of eviction on outcomes related to the tenant's homelessness situation. Outcomes are listed on the left of each row. Results are shown for 1-4 quarters (columns (1)-(3)) and for 5-8 quarters (columns (4)-(6)) after the eviction case is filed. Each panel shows results for a given outcome. Below the combined estimates in each panel, we report estimates separately for each location and for the female and Black subsamples. These RF estimates correspond to the IV estimates from Table 5, and controls are the same as those for the IV estimates in Table 5. Standard errors for regression model coefficients are included in parentheses and are clustered at the judge(courtroom)-year level. Observations for the main combined specifications are reported in brackets below the standard errors in columns (3) and (6). Observation counts for all RF regressions can be found in Appendix Table H.15. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

Table H.12: Reduced Form for Table 6: Impacts of Eviction on Labor Outcomes

	1-4 Quarters After Filing		5-8 Quarters After Filing	
	$\mathbb{E}[Y\|E=0]$ (1)	RF (2)	$\mathbb{E}[Y\|E=0]$ (3)	RF (4)
Earnings:	4,300	-251*	4,254	-453**
	(3,809)	(139)	(3,885)	(186)
		[374,400]		[336,396]
By Location				
Cook County	4,821	-331*	4,821	-470*
	(5,810)	(185)	(5,956)	(254)
New York	3,779	-171	3,687	-436
	(4,926)	(208)	(4,991)	(273)
By Group				
Female	4,136	-408***	4,094	-573**
	(3,545)	(146)	(3,610)	(223)
Black	4,319	-302	4,252	-680***
	(3,664)	(185)	(3,718)	(234)
Employment:	0.565	-0.013	0.549	-0.013
	(0.317)	(0.017)	(0.322)	(0.020)
		[376,400]		[340,396]
By Location				
Cook County	0.623	0.002	0.613	-0.007
	(0.432)	(0.020)	(0.438)	(0.022)
New York	0.507	-0.028	0.485	-0.019
	(0.465)	(0.027)	(0.471)	(0.034)
By Group				
Female	0.585	-0.030	0.568	-0.001
	(0.315)	(0.020)	(0.320)	(0.026)
Black	0.583	-0.048**	0.566	-0.065**
	(0.316)	(0.024)	(0.321)	(0.029)

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This table reports equally-weighted averages of Cook County and New York non-evicted sample means ($\mathbb{E}[Y|E = 0]$), and reduced form (RF) estimates of the impact of eviction on the tenant's labor outcomes. Outcomes are listed on the left of each row. Results are shown for 1-4 quarters (columns (1)-(3)) and for 5-8 quarters (columns (4)-(6)) after the eviction case is filed. Each panel shows results for a given outcome. Below the combined estimates in each panel, we report estimates separately for each location and for the female and Black subsamples. These RF estimates correspond to the IV estimates from Table 6, and controls are the same as those for the IV estimates in Table 6. Standard errors for regression model coefficients are included in parentheses and are clustered at the judge(courtroom)-year level. Observations for the main combined specifications are reported in brackets below the standard errors in columns (3) and (6). Observation counts for all RF regressions can be found in Appendix Table H.15. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

Table H.13: Reduced Form for Table 7: Impacts of Eviction on Financial Health

	1-4 Quarters After Filing		5-8 Quarters After Filing	
	$\mathbb{E}[Y\|E=0]$	RF	$\mathbb{E}[Y\|E=0]$	RF
	(1)	(2)	(3)	(4)
Financial health index:	-0.054	-0.086*	0.008	-0.092
	(0.737)	(0.050)	(0.747)	(0.062)
		[269,814]		[271,230]
By Location				
Cook County	-0.075	-0.162*	-0.027	-0.137
	(0.990)	(0.083)	(1.002)	(0.108)
New York	-0.032	-0.011	0.043	-0.046
	(1.091)	(0.056)	(1.108)	(0.063)
Credit score:	547.59	-6.41	551.84	-11.36**
	(67.11)	(4.23)	(68.61)	(4.75)
By Location				
Cook County	531.94	-6.64	536.62	-14.90**
	(74.04)	(6.43)	(74.56)	(7.01)
New York	563.24	-6.18	567.06	-7.82
	(111.94)	(5.48)	(115.19)	(6.41)
No open revolving account:	0.481	-0.030	0.468	0.035
	(0.334)	(0.020)	(0.331)	(0.032)
By Location				
Cook County	0.587	-0.056*	0.589	0.048
	(0.491)	(0.033)	(0.491)	(0.059)
New York	0.375	-0.005	0.347	0.021
	(0.452)	(0.022)	(0.445)	(0.024)
Total balance: collections and delinquencies:	2,550	246	2,378	382
	(4,099)	(327)	(3,936)	(333)
By Location				
Cook County	2,759	593	2,516	450
	(5,504)	(535)	(5,291)	(577)
New York	2,342	-101	2,240	313
	(6,075)	(375)	(5,829)	(332)
Any auto loan or lease:	0.170	-0.047*	0.176	0.022
	(0.264)	(0.024)	(0.269)	(0.023)
By Location				
Cook County	0.197	-0.101**	0.198	0.022
	(0.396)	(0.042)	(0.397)	(0.040)
New York	0.142	0.007	0.155	0.022
	(0.349)	(0.024)	(0.362)	(0.023)

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This table reports equally-weighted averages of Cook County and New York non-evicted sample means ($\mathbb{E}[Y|E=0]$), and reduced form (RF) estimates of the impact of eviction on outcomes related to the tenant's financial health. Outcomes are listed on the left of each row. Results are shown for 1-4 quarters (columns (1)-(3)) and for 5-8 quarters (columns (4)-(6)) after the eviction case is filed. Each panel shows results for a given outcome. Below the combined estimates in each panel, we report estimates separately for each location and for the female and Black subsamples. These RF estimates correspond to the IV estimates from Table 7, and controls are the same as those for the IV estimates in Table 7. Standard errors for regression model coefficients are included in parentheses and are clustered at the judge(courtroom)-year level. Observations for the main combined specifications are reported in brackets below the standard errors in columns (3) and (6), in the top panel. Observation counts for all RF regressions can be found in Appendix Table H.15.

Table H.14: Reduced Form for Table 8: Impacts of Eviction on Hospital Use

	1-4 Quarters After Filing		**5-8 Quarters After Filing**	
	$\mathbb{E}[Y\|E=0]$	RF	$\mathbb{E}[Y\|E=0]$	RF
	(1)	(2)	(3)	(4)
Number of hospital visits	0.739	0.161**	0.632	-0.079
	(1.321)	(0.072)	(1.208)	(0.105)
Number of emergency visits	0.588	0.090	0.511	-0.049
	(1.091)	(0.070)	(1.010)	(0.090)
Number of mental health visits	0.047	0.049**	0.045	-0.026
	(0.295)	(0.023)	(0.346)	(0.039)
		[179,024]		[154,531]

Notes: $^{*}\ p < 0.1,^{**}\ p < 0.05,^{***}\ p < 0.01$. This table reports the the impacts of eviction on hospital use in New York. The table includes the non-evicted sample means ($\mathbb{E}[Y|E = 0]$), and reduced form (RF) estimates of the impact of eviction on the tenant's hospital use in New York only. Outcomes are listed on the left of each row. Results are shown for 1-4 quarters (columns (1)-(3)) and for 5-8 quarters (columns (4)-(6)) after the eviction case is filed. 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 are clustered at the judge(courtroom)-year level. Observation counts for all outcomes are listed at the bottom of the table, in brackets. These RF estimates correspond to the IV estimates from Table 8.

H.5 Number of observations

This subsection provides the number of observations for each regression result from th OLS, IV, and RF specifications. Note that Cook County's observations are rounded following the requirements of the U.S. Census Bureau.

Table H.15: Observation Counts for RF, IV and OLS Regressions - Estimates 1-2 Years (1-8 Quarters) after Filing

	1-4 Quarters (1 Year) After Filing					**5-8 Quarters (2 Years) After Filing**				
	All	Cook	NY	Female	Black	All	Cook	NY	Female	Black
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Housing outcomes:										
Enforcement:	329,279	147,439	181,840	203,943	182,063	307,837	147,439	160,398	188,359	169,207
Not at eviction address:	218,228	114,000	104,228	160,045	151,612	183,227	114,000	69,227	129,598	125,692
Neighborhood poverty rate:	173,909	73,500	100,409	108,860	100,524	127,891	72,000	55,891	82,107	78,088
Emergency shelter:	210,840	29,000	181,840	147,200	125,464	198,898	38,500	160,398	137,616	119,108
Any homelessness services:	210,840	29,000	181,840	147,200	125,464	198,898	38,500	160,398	137,616	119,108
Future eviction:	911,564	551,603	359,961	518,736	443,077	859,314	525,430	333,884	489,601	418,395
Labor market outcomes:										
Earnings:	374,400	230,000	144,400	246,907	245,625	336,396	215,000	121,396	220,846	221,695
Employment:	376,400	232,000	144,400	247,907	246,625	340,396	219,000	121,396	223,846	224,695
Financial health outcomes:										
Financial health index:	269,814	83,335	186,479			271,230	83,781	187,449		
Credit score:	277,873	91,394	186,479			278,633	91,184	187,449		
No open revolving account:	278,507	92,028	186,479			279,211	91,762	187,449		
Total balance: collections and delinquencies:	270,016	83,537	186,479			271,447	83,998	187,449		
Any auto loan or lease:	278,507	92,028	186,479			279,211	91,762	187,449		
Robustness-check outcomes:										
Not at eviction address or unobserved:		114,000		71,000*	77,500*		114,000		71,000*	77,500*
Moved out of state:		103,000		65,000*	70,500*		97,000		61,500*	66,500*
Earnings out of 13 States:		232,000		144,000*	161,000*		219,000		137,000*	153,000*
Payday outcomes:										
Any payday inquiry ($\times$100):		75,608		44,994*			91,571		54,579*	
Number of payday inquiries:		75,608		44,994*			91,571		54,579*	
Any payday loan ($\times$100):		99,570		59,259*			115,404		68,553*	
Number of payday loans:		99,570		59,259*			115,404		68,553*	

Notes: This table shows observation counts for all outcomes and samples used in the OLS, IV and RF specifications for 1-4 quarters (columns (1)-(5)) and 5-8 quarters (columns (6)-(10)) after eviction case filing. Observation counts for female and Black subgroups are counts across both Cook County and New York, unless marked with an asterisk, which indicates they are only for Cook County. Observation counts for Cook County are rounded in accordance with Census Bureau disclosure requirements for housing, labor market and robustness-check outcomes. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

Table H.16: Observation Counts for RF, IV and OLS Regressions - Estimates 3-6 Years (9-24 Quarters) after Filing

	9-16 Quarters (3-4 Years) After Filing					17-24 Quarters (5-6 Years) After Filing				
	All (1)	Cook (2)	NY (3)	Female (4)	Black (5)	All (6)	Cook (7)	NY (8)	Female (9)	Black (10)
Housing outcomes:										
Not at eviction address:	162,540	110,000	52,540	112,102	110,786	134,428	101,000	33,428	90,423	91,197
Neighborhood poverty rate:	129,327	86,500	42,827	82,906	81,913	105,182	80,500	24,682	68,162	69,440
Emergency shelter:	169,500	40,500	129,000	116,327	101,553	122,925	53,500	69,425	82,051	75,314
Any homelessness services:	169,500	40,500	129,000	116,327	101,553	122,925	53,500	69,425	82,051	75,314
Labor market outcomes:										
Earnings:	298,552	201,000	97,552	195,414	198,063	227,845	161,000	66,845	148,576	153,018
Employment:	303,552	206,000	97,552	197,414	200,063	232,845	166,000	66,845	150,576	156,018
Financial distress outcomes:										
Financial health index:	306,223	134,984	171,239			197,230	113,385	83,845		
Credit score:	317,515	146,276	171,239			207,559	123,714	83,845		
No open revolving account:	318,486	147,250	171,236			208,740	124,898	83,842		
Total balance: collections and delinquencies:	306,658	135,419	171,239			197,759	113,914	83,845		
Any auto loan or lease:	318,489	147,250	171,239			208,743	124,898	83,845		

Notes: This table shows observation counts for all outcomes and samples used in the OLS, IV and RF specifications for 9-16 quarters (columns (1)-(5)) and 17-24 quarters (columns (6)-(10)) after eviction case filing. Observation counts for female and Black subgroups are counts across both Cook County and New York. Observation counts for Cook County are rounded in accordance with Census Bureau disclosure requirements. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

I Appendix: Additional outcomes: attrition and out-of-state moves

This section studies the impact of eviction on several robustness-related outcomes not presented in the paper, including attrition and out-of-state moves. In Cook County, where we observe out-of-state moves and where we can construct a flag for earnings outside the states for which we can observe quarterly wage income, we study these outcomes directly. We also use these estimates to conduct a simple simulation to examine the sensitivity of our main earnings estimates to potential selection patterns. In New York, where our analysis uses non-Census data, we study the sensitivity of our analysis to the data source for measuring moves. We also conduct an analysis of attrition in New York.

I.1 Cook County

In Cook County, we first examine the sensitivity of our main estimate of the impact of eviction on moving addresses to how we define a move. Using the MAFARF data, we construct an alternative, more-inclusive definition of moving from the address associated with the eviction case. This alternative measure takes a value of 1 if either the person is observed at a new address or the person has a missing address. The impact of eviction on this outcome, shown in Table I.1, is 0.181, which is about twice as large as the impact on not being at the eviction address, which is 0.093 in Cook County, as shown in Table 4. This result suggests that eviction increases the likelihood of having a missing address, and suggests that the main analysis does not fully capture the extent an eviction order impacts the likelihood of moving addresses.[72]

We turn next to out-of-state moves. If judge stringency impacts the likelihood of moving out of state, our IV estimates may be biased. We investigate this possibility empirically using the Cook County sample, which allows us to study whether eviction has an impact on out-of-state moves. To the extent that eviction impacts tenants' likelihood of moving out of state similarly across locations, these estimates will also inform the sign and potential magnitude of a potential bias in New York. The middle panel of Table I.1 shows the impact of eviction on moving out of state using the Cook County sample. We find that eviction decreases the likelihood of moving out of state by 3.9 percentage points, and the estimate is statistically significant at the 10 percent level. This result may seem surprising, since eviction increases the likelihood of moving overall. Nevertheless, this result is consistent with eviction making it more difficult for tenants to relocate at a far distance. Eviction may, for example, increase a tenant's dependence on family for housing. This effect diminishes to a statistically insignificant 0.7 percentage points in the 5-8 quarters after filing.

[72] While address data in New York has national coverage using data from Infutor, it is not possible to perform the analogous exercise because Infutor data does not distinguish between the person not moving and the address not being updated. Hence, conditional on matching to any Infutor record, missing addresses are not observed.

Recall that in Cook County we only observe earnings in Illinois and the 12 LEHD Option "A" states. To understand the extent of attrition out of these 13 states, we construct an indicator for having any positive income across the 50 states (using the LEHD U.S. Indicators File) and zero earnings within the 13 states that we observe, and report the impact of eviction on this variable in the bottom panel of Table I.1. The results show that eviction decreases the likelihood of having any earnings outside the LEHD states by 4.1 percentage points in the 1-4 quarters after case filing, which is significant at the 5 percent level. This result is consistent with our analysis of out-of-state moves. The magnitude diminishes to 1.9 percentage points in the 5-8 quarters, and is statistically insignificant. Moving out of state is unlikely to be driving our earnings estimates, for two reasons. First, non-evicted tenants are more likely to move out of state and hence be recorded as having zero earnings. This pattern would tend to attenuate our estimates if these non-evicted movers have higher earnings than non-evicted stayers. Second, the negative impact of eviction on earnings is larger in quarters 5-8 compared to quarters 1-4, as seen in Table 6, while the the out-of-state earnings estimates have the opposite pattern. This comparison suggests that, if anything, selection is likely attenuating our earnings estimates in the short run.

We conduct an additional simulation exercise to examine the extent that selection into moving out of state may impact our earnings results. The exercise shows that even implausibly large selection patterns would have a relatively muted impact on our main earnings estimates. This exercise is based on Bharadwaj et al. (2013) and is conducted entirely outside the Census RDC using simulated data. The idea of this exercise to use the estimates from our empirical analysis (which includes only LEHD states, "non-movers") and to impute stronger and stronger selection patterns for the non-LEHD states ("movers"), and then to re-estimate an OLS regression of earnings on eviction using the simulated sample that includes both movers and non-movers.

We use Cook County estimates from Tables 1 and 6, since we observe out-of-state moves in Cook County. We generate a simulated sample of 301,000 with 193,000 evicted (following Table 1). We simulate earnings to have a mean of approximately \$4821 and a standard deviation of \$5810 (following Table 6, column 1) and assume the negative impact of eviction is \$445 (following Table 6, column 3). For simplicity, we assume that none of the evicted group move, and 3.9 percent of the non-evicted group move (from Table I.1). While it is not obvious that out-of-state movers would be negatively selected—and, in fact, it seems more plausible that they would be positively selected—we assume negative selection here because this type of selection would attenuate our earnings estimates, while positive selection would imply our estimates underestimate the true earnings impact of eviction. Table I.2 shows the estimation of an OLS regression with the simulated data, under different assumptions about the extent of selection. Column (1) assumes no selection into moving, and delivers an estimate of \$-455. Column (2) assumes non-evicted movers have \$100 less quarterly earnings (relative to non-evicted stayers), while columns (3), (4), and (5) assume they have \$200, \$500, and \$1000 less quarterly earnings, respectively. Note that the earnings estimate is attenuated in each successive column, which is by construction of the simulated data. Even in column (5), which has the strongest selection pattern—with non-evicted movers having \$1000 (or roughly 20 percent) lower earnings than non-evicted stayers—the estimate is attenuated by only about \$70 relative to the baseline. Our conclusion is that selective migration is unlikely to be driving our earnings estimates, and this conclusion is intuitive, given that overall migration rates are low.

I.2 New York

While the sources of residential address data for NYC do not allow us to distinguish "unobserved" addresses from not-moving (as in the Cook County sample), we can evaluate the sensitivity of the New York residential mobility results to the sources of moves data. Table I.3 reports our IV estimates of the effects of eviction orders on an indicator for not being at the eviction address in the second year after filing under different constructions of "not at eviction address" based on different data sources.[73] Column (1) repeats the estimate from Table 4 which uses our baseline definition: having a new address in either benefits or Infutor data. Columns (2) and (3) evaluate how sensitive the move results are when defining moves from the filing address as: (2) a new address in the benefits data (but not Infutor), and (3) a new address in Infutor but not the benefits data. Column (4) examines moves for individuals using only the benefits data. Column (5) reports the estimate for individuals using only the Infutor data. Tenants in housing court are much more likely to appear in the benefits data than in the Infutor alone 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, the estimates using only Infutor data 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.

Next, we attempt to quantify attrition—to the extent possible—in the NYC sample. A challenge in the NYC data sources is that we cannot readily distinguish non-employment (for labor market outcomes), no hospitalizations (for health outcomes), or no benefits records (for residential mobility outcomes) from a move out of New York state. Nevertheless, it is useful to examine whether stringency is related to appearing in the NYC outcomes data. Permanent attrition out of a covered jurisdiction should result in a pattern of all zeros for appearances in an administrative outcome measure (Grogger, 2012). As in Chyn (2018), we examine whether our treatment is followed by a run of all zeros in our outcome data. In Table I.4, we regress an indicator for any appearance in the different NY-state specific data sources on our instrument (with and without controls) during the post-filing period. It is important to note that this exercise could conflate treatment effects with attrition. That said, the results suggest that judge stringency is not strongly related to appearance (or non-appearance) in any of the NY-specific outcome data sources in the post-filing period.

Stringency is uncorrelated with having all zeros for earnings in: the entire post-period ("Any Post Earnings"), all quarters at least 4 quarters after filing ("Any Post Earnings Q4+"),

[73] We focus on residential mobility results in the second year because this is when effects appear to materialize in NYC.

and all quarters at least eight quarters after filing ("Any Post Earnings Q8+"). Stringency is also uncorrelated with having having any benefits data after filing which would allow to observe a new address ("Any Post Benefit"). Finally, stringency is uncorrelated with having any appearance in the state-wide hospitalization data ("Any Post Hospital"), and uncorrelated with appearing in any linked data set after filing ("Any Post Record").

Table I.1: Additional Cook County outcomes: 1-4 and 5-8 Quarters After Filing

	1-4 Quarters After Filing			**5-8 Quarters After Filing**		
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Not at eviction address or unobserved:	0.445	0.051***	0.181***	0.687	0.108***	0.090
	(0.497)	(0.003)	(0.060)	(0.464)	(0.004)	(0.056)
			[114,000]			[114,000]
Cook County, By Group						
Female	0.440	0.057***	0.143**	0.683	0.118***	0.096
	(0.496)	(0.004)	(0.070)	(0.465)	(0.005)	(0.063)
Black	0.429	0.055***	0.184**	0.674	0.114***	0.051
	(0.495)	(0.004)	(0.073)	(0.469)	(0.005)	(0.068)
Moved out of state:	0.022	0.002*	-0.039*	0.048	0.008***	0.007
	(0.147)	(0.001)	(0.022)	(0.213)	(0.002)	(0.035)
			[103,000]			[97,000]
Cook County, By Group						
Female	0.019	0.002	-0.032	0.042	0.009***	0.021
	(0.137)	(0.001)	(0.025)	(0.201)	(0.002)	(0.040)
Black	0.014	0.003***	-0.018	0.036	0.011***	-0.007
	(0.119)	(0.001)	(0.023)	(0.187)	(0.002)	(0.035)
Earnings out of 13 states:	0.036	0.003***	-0.041**	0.051	0.003***	-0.019
	(0.186)	(0.001)	(0.017)	(0.219)	(0.001)	(0.020)
			[232,000]			[219,000]
Cook County, By Group						
Female	0.029	0.004***	-0.027	0.043	0.003***	0.014
	(0.169)	(0.001)	(0.017)	(0.202)	(0.001)	(0.021)
Black	0.027	0.004***	-0.032*	0.038	0.004***	-0.017
	(0.161)	(0.001)	(0.019)	(0.191)	(0.001)	(0.022)

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This table shows IV and OLS results one year and two years after filing for three additional outcomes. "Not at eviction address or unobserved" is an indicator which is one if an individual is not observed in the MAFARF address data or is observed at an address other than the address associated with the eviction case. "Moved out of state" is an indicator for observing an individual at an address outside of the state of Illinois. "Earnings out of 13 states" is an indicator for if the individual had wage income reported outside of the 13 states in which we have LEHD quarterly earnings data. Observations for the main combined specifications are reported below the standard errors in columns (3) and (6). Observation counts for all outcomes and subgroups are in Appendix Table H.15. Results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

Table I.2: Simulation: Examining the Sensitivity of Earnings Estimates to Selection

	Baseline	\$100	\$200	\$500	\$1000
	(1)	(2)	(3)	(4)	(5)
Quarterly earnings	-455.84***	-451.94***	-444.16***	-424.69***	-385.76***
	(22.11)	(22.11)	(22.11)	(22.12)	(22.14)
Number of observations	301,000	301,000	301,000	301,000	301,000
R^2	0.001	0.001	0.001	0.001	0.001
Mean of dependent variable	4654	4651	4646	4634	4609
S.d. of dependent variable	5819	5819	5819	5820	5825

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This table presents a simple simulation to examine the sensitivity of the earnings estimates to out-of-state moves. The simulated data is based on 301,000 observations with 193,000 evicted and an approximate mean and standard deviation of earnings of \$4821 and \$5810, respectively, and a negative impact of eviction of approximately \$445. The simulation assumes zero migration for evicted tenants and a 3.9 percent migration rate for non-evicted tenants. In the baseline (column 1) there is no selective migration. Columns 2-5 assume increasingly severe negative selection for non-evicted movers. In column 2, the non-evicted movers are simulated to have \$100 lower earnings than non-evicted stayers, while in columns 3, 4, and 5 the extent of selection is simulated to be \$200, \$500, and \$1,000, respectively. The estimate reported is a simple OLS regression of quarterly earnings on eviction, and includes both movers and non-movers to examine the sensitivity of the estimate to increasingly strong selection patterns.

Table I.3: New York Residential Mobility Results by Alternative Move Definitions (Q5-Q8)

	Move in Either (1)	Move in Benefits (2)	Move in Infutor (3)	Using Only Benefits Data (4)	Using Only Infutor Data (5)
Evicted	0.149 *	0.187 **	0.152 *	0.196 **	0.143
	(0.084)	(0.093)	(0.085)	(0.096)	(0.279)
Observations	69227	69227	69227	54844	14350

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. This table reports the IV estimates on "Not at eviction address" in the second year after filing by different definitions of address changes. "Move in Either" is our base definition used in Table 4 and takes the value 1 if the individual is observed at a new address other than their filing 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 (for individuals who appear in both), 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 (for individuals who appear in both), and 0 otherwise. "Using Only Benefits Data" is estimated using the subset of individuals who we observe in the post period in the benefits data, and take the value 1 if the individual appears at an address that is not the filing address, 0 otherwise. "Using Only Infutor Data" is estimated using the subset of individuals who we observe in the post period in the Infutor data, and take the value 1 if the individual appears at an address that is not the filing address.

Table I.4: Attrition—New York

	Any Post Earnings		Any Post Earnings Q4+		Any Post Earnings Q8+		Any Post Benefits		Any Post Hospitalization		Any Post Record	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Judge stringency	0.0182	-0.0065	0.0337	0.0029	-0.0187	-0.0233	0.0072	0.0115	-0.0331	-0.0141	0.0183	0.0168
	(0.0383)	(0.0328)	(0.0440)	(0.0388)	(0.0638)	(0.0501)	(0.0351)	(0.0298)	(0.0374)	(0.0357)	(0.0239)	(0.0230)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	144,429	144,400	121,413	121,396	102,770	102,755	150,698	150,662	150,698	150,662	150,698	150,662

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. In Columns (1) and (2), we regress an indicator for having all zeros for labor records in quarters 1–39 post-filing on our instrument. In Columns (3) and (4), we regress an indicator for having all zeros for earnings record from the 8–39 quarters after filing on our instrument. In Columns (5) and (6), we regress an indicator for having any benefits record after filing on our instrument. In Columns (7) and (8), we regress an indicator for any hospital visit after filing on our instrument. In Columns (9) and (10), we regress an indicator for having any earnings, hospital visit, or benefits record post-filing on our instrument. All specifications include court-by-time of filing fixed effects. The specifications in columns (2), (4), (6), (8), (10) and (12) include the controls listed in Table 4. 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.

J Appendix: Symmetric difference-in-differences

This section briefly sets up notation and then discusses what difference-in-differences (DiD) recovers under different settings. We then briefly develop the symmetric DiD estimator from Heckman and Robb (1985). The final subsection reports symmetric DiD estimates.

J.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.[74] We further assume that the observed outcome can be written as the linear equation:

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

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.

[74] 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.

In the next subsection, we discuss potential assumptions on $\nu_{i,t}$ and the implications for what DiD would then estimate.

J.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:

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

And the probability limit is

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

which can be written as

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

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. This also highlights why the choice of pre-period will affect DiD estimates when there are differential pre-trends.

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

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

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).[75]

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

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

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.

Heckman and Robb (1985) and Ashenfelter and Card (1985) show that, when the earnings process is covariance stationary, then DiD estimates which are taken symmetrically around treatment will be unbiased, even when parallel trends does not hold. One simple example of this would be if earnings follow an AR(1) process. In such a case, evicted individuals may have larger negative shocks in the run-up to the eviction case, which would result in non-parallel trends in absence of treatment, yet symmetric DiD will still provide an unbiased estimate. The AR(1) case is worked out below as a simple example. See Chabé-Ferret (2015) for a more recent and rigorous consideration of symmetric DiD.

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 J.5 will be equal to 0, which is the symmetric DiD result.

J.3 Difference-in-differences estimates

In panel settings, DiD is commonly used to estimate the ATT using a panel regression of the form:

$$Y_{i,t} = \lambda_i + \mu_\tau + \alpha_t + \sum_{k \neq -\ell} \gamma_k^{-\ell} E_i \times \mathbf{1}\{k = t\} + \nu_{i,t}, \tag{J.6}$$

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

For equation J.6 to recover the ATT, a common assumption is that trends would be parallel between treatment and control in the absence of treatment. In our setting, this assumption is unlikely to hold given the patterns of differential pre-trends in outcomes shown in Section 4.3 and Figure E.2, panel A.[76]

The regression equation for the symmetric DiD is:

$$\begin{aligned} Y_{i,t} = \gamma_t + \alpha_i + \alpha \times E_{i,t} + \sum_{r=S;r\notin -R}^{-1} \beta_r + \sum_{r=S;r\notin -R}^{-1} \delta_r \times E_{i,t} + \sum_{r=0;r\notin R}^{F} \beta_r + \sum_{r=0;r\notin R}^{F} \delta_r \times E_{i,t} \\ + \beta_R \times I\{r \in R\} + \delta_R \times E_{i,t} \times I\{r \in R\} + \epsilon_{i,t} \end{aligned} \tag{J.7}$$

where R is the outcome window (e.g., for quarters 1-8, $R = [1, 8]$, and $-R = [-8, -1]$). We omit $-R$ for symmetry with the outcome window, and we report the estimate for δ_R, which is the symmetric DiD estimate. The regression includes individual fixed effects, α_i, and γ_t, which are calendar quarter fixed effects, and the standard errors are clustered at the individual level.

Tables J.1 and J.2 compare DiD and IV estimates for housing and labor market outcomes respectively. For the "Not at eviction address" outcome, DiD estimates are smaller than the IV estimates, with the combined estimate in year 1 of 0.045, compared to the IV estimate of 0.082. The combined estimates for emergency shelter and any homelessness services are similar for DiD and IV. For earnings and employment, the DiD estimates are consistently smaller in magnitude than the IV estimates. For example, the combined impact on earnings is -131 from symmetric DiD, but -323 for IV.

Table J.3 compares DiD estimates to IV estimates for credit bureau outcomes, while Table J.4 compares DiD estimates to IV estimates for hospitalization outcomes. For credit bureau and hospital utilization outcomes, the DiD estimates are consistently smaller than the IV estimates.

Overall, the results suggest that the ATT may be smaller in magnitude than the estimates

[76] See Appendix J, which further discusses the potential bias from violation of the parallel trends assumption.

for compliers identified for by the instrumental variables strategy for many outcomes. This comparison requires relatively strong assumptions in order to interpret the symmetric DiD estimates as unbiased estimates of the ATT.

Table J.1: Comparison of IV and DiD estimates - Housing Outcomes

	1 Year After Filing			2 Years After Filing		
	$\mathbb{E}[Y\|E=0]$	IV	DiD	$\mathbb{E}[Y\|E=0]$	IV	DiD
	(1)	(2)	(3)	(4)	(5)	(6)
Not at eviction address:	0.293	0.082**	0.047***	0.478	0.111**	0.104***
	(0.318)	(0.036)	(0.003)	(0.348)	(0.053)	(0.003)
		[218,228]	[2,110,008]		[183,227]	[2,110,008]
By Location						
Cook County	0.363	0.093	0.004	0.568	0.074	0.063***
	(0.481)	(0.057)	(0.004)	(0.495)	(0.064)	(0.004)
New York	0.222	0.071	0.090***	0.389	0.149*	0.145***
	(0.415)	(0.045)	(0.004)	(0.487)	(0.084)	(0.005)
By Group						
Female	0.280	0.093**	0.052***	0.461	0.136**	0.109***
	(0.312)	(0.046)	(0.003)	(0.343)	(0.060)	(0.004)
Black	0.272	0.066	0.041***	0.454	0.098	0.103***
	(0.310)	(0.056)	(0.004)	(0.345)	(0.080)	(0.004)
Emergency shelter:	0.009	0.034**	0.037***			
	(0.068)	(0.017)	(0.002)			
		[210,840]	[1,766,585]			
By Location						
Cook County	0.007	0.023	0.006***			
	(0.086)	(0.028)	(0.002)			
New York	0.011	0.046**	0.068***			
	(0.105)	(0.019)	(0.003)			
By Group						
Female	0.009	0.024	0.040***			
	(0.066)	(0.018)	(0.002)			
Black	0.010	0.036				
	(0.072)	(0.024)				
Any homelessness services:	0.015	0.029	0.039***			
	(0.086)	(0.023)	(0.002)			
		[210,840]	[1,766,585]			
By Location						
Cook County	0.017	0.012	0.005*			
	(0.128)	(0.042)	(0.003)			
New York	0.013	0.046**	0.073***			
	(0.114)	(0.018)	(0.003)			
By Group						
Female	0.015	0.030	0.042***			
	(0.086)	(0.023)	(0.002)			
Black	0.017	0.049*	0.044***			
	(0.092)	(0.029)	(0.002)			

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. 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 DiD estimates of the impact of eviction on the tenant's housing situation. Outcomes are listed on the left of each row. Results are shown for 1-4 quarters (columns (1)-(3)) and for 5-8 quarters (columns (4)-(6)) after eviction case is filed. DiD estimates for the bottom two panels in column (6) and the Black subsample in "emergency shelter" panel are suppressed due to small cell size. Each panel shows results for a given outcome. Below the combined estimates in each panel, we report estimates separately for each location and for the female and Black subsamples. The IV estimates are identical to those in Tables 4 and 5. The DiD regressions include individual fixed effects and time relative to filing dummies and is described in equation . Standard errors are clustered at judge-year level and are shown in parentheses. The number of person-years appearing in the full sample of the DiD specifications are listed in brackets under standard errors in columns (3) and (6). Table H.15 reports observation counts for all IV regressions, and Table J.5 reports observation counts for all DiD regressions. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

Table J.2: Comparison of IV and DiD estimates - Labor Outcomes

	1-4 Quarters After Filing			5-8 Quarters After Filing		
	$\mathbb{E}[Y\|E=0]$	IV	DiD	$\mathbb{E}[Y\|E=0]$	IV	DiD
	(1)	(2)	(3)	(4)	(5)	(6)
Earnings:	4,300	-323*	-175***	4,254	-613**	-241***
	(3,809)	(175)	(9)	(3,885)	(248)	(11)
		[374,400]	[15,127,852]		[336,396]	[15,127,852]
By Location						
Cook County	4,821	-445*	-157***	4,821	-627*	-227***
	(5,810)	(249)	(14)	(5,956)	(337)	(19)
New York	3,779	-201	-194***	3,687	-599*	-255***
	(4,926)	(245)	(11)	(4,991)	(363)	(12)
By Group						
Female	4,136	-504***	-149***	4,094	-767***	-196***
	(3,545)	(185)	(10)	(3,610)	(295)	(12)
Black	4,319	-377	-163***	4,252	-931***	-221***
	(3,664)	(234)	(10)	(3,718)	(307)	(13)
Employment:	0.565	-0.015	-0.015***	0.549	-0.018	-0.019***
	(0.317)	(0.021)	(0.001)	(0.322)	(0.027)	(0.001)
		[376,400]	[15,457,852]		[340,396]	[15,457,852]
By Location						
Cook County	0.623	0.003	-0.014***	0.613	-0.010	-0.015***
	(0.432)	(0.027)	(0.001)	(0.438)	(0.030)	(0.002)
New York	0.507	-0.032	-0.016***	0.485	-0.027	-0.023***
	(0.465)	(0.032)	(0.001)	(0.471)	(0.046)	(0.001)
By Group						
Female	0.585	-0.036	-0.014***	0.568	-0.003	-0.015***
	(0.315)	(0.025)	(0.001)	(0.320)	(0.034)	(0.001)
Black	0.583	-0.059*	-0.014***	0.566	-0.089**	-0.019***
	(0.316)	(0.031)	(0.001)	(0.321)	(0.040)	(0.001)

Notes: $^{*}\ p < 0.1,^{**}\ p < 0.05,^{***}\ p < 0.01$. 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 DiD estimates of the impact of eviction on tenant's labor outcomes. Outcomes are listed on the left of each row. Results are shown for 1-4 quarters (columns (1)-(3)) and for 5-8 quarters (columns (4)-(6)) after eviction case is filed. Each panel shows results for a given outcome. Below the combined estimates in each panel, we report estimates separately for each location and for the female and Black subsamples. The IV estimates are identical to those in Tables 6. The DiD regressions include individual fixed effects and time relative to filing dummies and is described in equation . Standard errors are clustered at judge-year level and are shown in parentheses. The number of person-quarters appearing in the full sample of the DiD specifications are listed in brackets under standard errors in columns (3) and (6). Table H.15 reports observation counts for all IV regressions, and Table J.5 reports observation counts for all DiD regressions. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

Table J.3: DiD: Impacts of Eviction on Financial Health

	1-4 Quarters After Filing			5-8 Quarters After Filing		
	$\mathbb{E}[Y\|E=0]$	IV	DiD	$\mathbb{E}[Y\|E=0]$	IV	DiD
	(1)	(2)	(3)	(4)	(5)	(6)
Financial health index:	-0.054	-0.107*	-0.031***	0.008	-0.141	-0.037***
	(0.737)	(0.060)	(0.004)	(0.747)	(0.094)	(0.005)
		[269,814]	[3,790,079]		[271,230]	[3,790,079]
By Location						
Cook County	-0.075	-0.202**	-0.034***	-0.027	-0.230	-0.035***
	(0.990)	(0.102)	(0.007)	(1.002)	(0.174)	(0.007)
New York	-0.032	-0.012	-0.029***	0.043	-0.053	-0.040***
	(1.091)	(0.063)	(0.005)	(1.108)	(0.072)	(0.007)
Credit score:	547.59	-7.86	-1.18***	551.84	-16.53**	-1.36***
	(67.11)	(5.18)	(0.38)	(68.61)	(6.67)	(0.49)
By Location						
Cook County	531.94	-8.69	-2.22***	536.62	-24.16**	-1.73***
	(74.04)	(8.29)	(0.48)	(74.56)	(11.15)	(0.51)
New York	563.24	-7.03	-0.14	567.06	-8.90	-1.00
	(111.94)	(6.21)	(0.60)	(115.19)	(7.33)	(0.84)
No open revolving account:	0.481	-0.039	0.015***	0.468	0.052	0.023***
	(0.334)	(0.025)	(0.002)	(0.331)	(0.051)	(0.003)
By Location						
Cook County	0.587	-0.072*	0.008**	0.589	0.080	0.016***
	(0.491)	(0.043)	(0.003)	(0.491)	(0.099)	(0.004)
New York	0.375	-0.006	0.021***	0.347	0.024	0.031***
	(0.452)	(0.025)	(0.002)	(0.445)	(0.027)	(0.004)
Total balance: collections and delinquencies:	2,550	310	98***	2,378	548	38
	(4,099)	(393)	(34)	(3,936)	(502)	(40)
By Location						
Cook County	2,759	735	70	2,516	739	-30
	(5,504)	(659)	(49)	(5,291)	(930)	(49)
New York	2,342	-115	127***	2,240	357	105*
	(6,075)	(428)	(46)	(5,829)	(377)	(63)
Any auto loan or lease:	0.170	-0.061**	-0.007***	0.176	0.031	-0.009***
	(0.264)	(0.030)	(0.002)	(0.269)	(0.036)	(0.002)
By Location						
Cook County	0.197	-0.130**	-0.012***	0.198	0.037	-0.016***
	(0.396)	(0.054)	(0.003)	(0.397)	(0.066)	(0.003)
New York	0.142	0.008	-0.001	0.155	0.025	-0.003
	(0.349)	(0.027)	(0.001)	(0.362)	(0.026)	(0.003)

Notes: * $p < 0.1$,** $p < 0.05$,*** $p < 0.01$. 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 DiD estimates of the impact of eviction on tenant's financial distress outcomes. Outcomes are listed on the left of each row. Results are shown for 1-4 quarters (columns (1)-(3)) and for 5-8 quarters (columns (4)-(6)) after eviction case is filed. Each panel shows results for a given outcome. Below the combined estimates in each panel, we report estimates separately for each location and for the female and Black subsamples. The IV estimates are identical to those in Tables 7. The DiD regressions include individual fixed effects and time relative to filing dummies and is described in equation . Standard errors are clustered at judge-year level and are shown in parentheses. The number of person-quarters appearing in the full sample of the DiD specifications are listed in brackets under standard errors in columns (3) and (6). Table H.15 reports observation counts for all IV regressions, and Table J.5 reports observation counts for all DiD regressions.

Table J.4: DiD: Impacts of Eviction on Hospital Utilization

	1-4 Quarters After Filing			5-8 Quarters After Filing		
	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	DiD (3)	$\mathbb{E}[Y\|E=0]$ (4)	IV (5)	DiD (6)
Number of hospital visits	0.739	0.188**	0.062***	0.632	-0.113	0.058***
	(1.321)	(0.094)	(0.006)	(1.208)	(0.142)	(0.006)
Number of emergency visits	0.588	0.106	0.057***	0.511	-0.065	0.054***
	(1.091)	(0.089)	(0.006)	(1.010)	(0.124)	(0.005)
Number of mental health visits	0.047	0.054*	0.018***	0.045	-0.035	0.014***
	(0.295)	(0.030)	(0.002)	(0.346)	(0.055)	(0.002)
		[179,024]	[1,516,330]		[154,531]	[1,516,330]

Notes: This table reports non-evicted sample means ($\mathbb{E}[Y|E=0]$), as well as equally-weighted averages of location-specific two-stage least squares (IV) and DiD estimates of the impact of eviction on tenant's health outcomes in New York only. Outcomes are listed on the left of each row. Results are shown for 1-4 quarters (columns (1)-(3)) and for 5-8 quarters (columns (4)-(6)) after eviction case is filed. Each panel shows results for a given outcome. Below the combined estimates in each panel, we report estimates separately for each location and for the female and Black subsamples. The IV estimates are identical to those in Tables 8.The DiD regressions include individual fixed effects and time relative to filing dummies and is described in equation . Standard errors are clustered at judge-year level and are shown in parentheses. The observation count for the IV specification is shown in brackets under standard errors in columns (2) and (5), and the number of person-years appearing in the full sample of the DiD specifications are listed in brackets under standard errors in columns (3) and (6).

Table J.5: Observation Counts for DiD Regressions

	1-4 Quarters					5-8 Quarters				
	All (1)	Cook (2)	NY (3)	Female (4)	Black (5)	All (6)	Cook (7)	NY (8)	Female (9)	Black (10)
Housing outcomes:										
Not at eviction address:	2,110,008	1,282,000	828,008	1,381,474	1,278,004	2,110,008	1,282,000	828,008	1,381,474	1,278,004
Emergency shelter:	1,766,585	222,000	1,544,585	1,226,834	1,034,574					
Any homelessness services:	1,766,585	222,000	1,544,585	1,226,834	1,034,574					
Labor market outcomes:										
Earnings:	15,127,852	10,690,000	4,437,852	9,704,278	9,564,675	15,127,852	10,690,000	4,437,852	9,704,278	9,564,675
Employment:	15,457,852	11,020,000	4,437,852	9,880,278	9,737,675	15,457,852	11,020,000	4,437,852	9,880,278	9,737,675
Financial health outcomes:										
Financial health index:	3,790,079	999,753	2,790,326			3,790,079	999,753	2,790,326		
Credit score:	3,892,074	1,101,734	2,790,340			3,892,074	1,101,734	2,790,340		
No open revolving account:	3,968,233	1,114,314	2,853,919			3,968,233	1,114,314	2,853,919		
Total balance: collections and delinquencies:	3,793,010	1,002,677	2,790,333			3,793,010	1,002,677	2,790,333		
Any auto loan or lease:	3,904,646	1,114,314	2,790,332			3,904,646	1,114,314	2,790,332		

Notes: This table shows the observation counts for all outcomes and samples used in the DiD specifications, for 1-4 quarters (columns (1)-(5)) and 5-8 quarters (columns (6)-(10)) after eviction case filing. Observation counts are in person-years for housing outcomes, and in person-quarters for labor market and financial health outcomes. Observation counts for female and Black subgroups are counts across both Cook County and New York. Observation counts for Cook County are rounded in accordance with Census Bureau disclosure requirements for housing and labor market outcomes. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.