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EVICTION AND POVERTY IN AMERICAN CITIES

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ABSTRACT

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 Cook County (which includes Chicago) and New York City. 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.

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A data appendix is available at http://www.nber.org/data-appendix/w30382

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

[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 pre-pandemic (pre-2020) 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. Since the onset of the pandemic, there has been an unprecedented amount of policy activity around evictions, including but not limited to: moratoria on eviction filing and enforcement, and substantial expansions of federal emergency rental assistance for renters at risk of eviction. See Benfer et al. (2022) and Reina et al. (2021) for recent surveys of pandemic-era eviction policies.

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 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

[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).

with research that finds that Black households experience discrimination while searching for housing (Bayer et al., 2017; Christiansen and Timmins, 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

[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 the supply of rental housing, rents, and screening practices.

the random assignment of judges to eviction cases. Third, we create a novel data set of 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

[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.

network disadvantage are additional risk factors. Kroeger and La Mattina (2020) find that criminal nuisance ordinances substantially increase eviction filing rates and eviction rates. Finally, Fetzer et al. (2020) study the effect of cuts to rental subsidies in the 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

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

landlords in New York were represented by an attorney.

[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.

a separate court process and is rare in practice. See Appendix B for additional institutional 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

[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.

courtrooms rather than judges, and therefore if the judge rotates out of a courtroom during 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]

[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.

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 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

[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.

other dollar amounts are expressed in 2016 USD using the CPI-U for the two metropolitan areas we study. Employment and earnings records only cover formal employment and exclude individuals not covered by UI benefits, such as the self-employed.

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

[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).

Infutor Data Solutions and administrative benefits records.[22] Similar to the Cook County data, we define a tenant as not at their eviction address if we observe them at a different address than the one listed on the court filing according to either the benefits data or the Infutor data in the relevant outcome window. A concern with 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,

[22] Infutor compiles data from several sources including public and private telephone billing data, deed and property information, customer information from utility companies, subscription services, and other sources. The data have been used to track housing instability among low-income tenants but may miss households with more limited paper trails (Phillips, 2020). The benefits records contain address histories for households as long as they 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

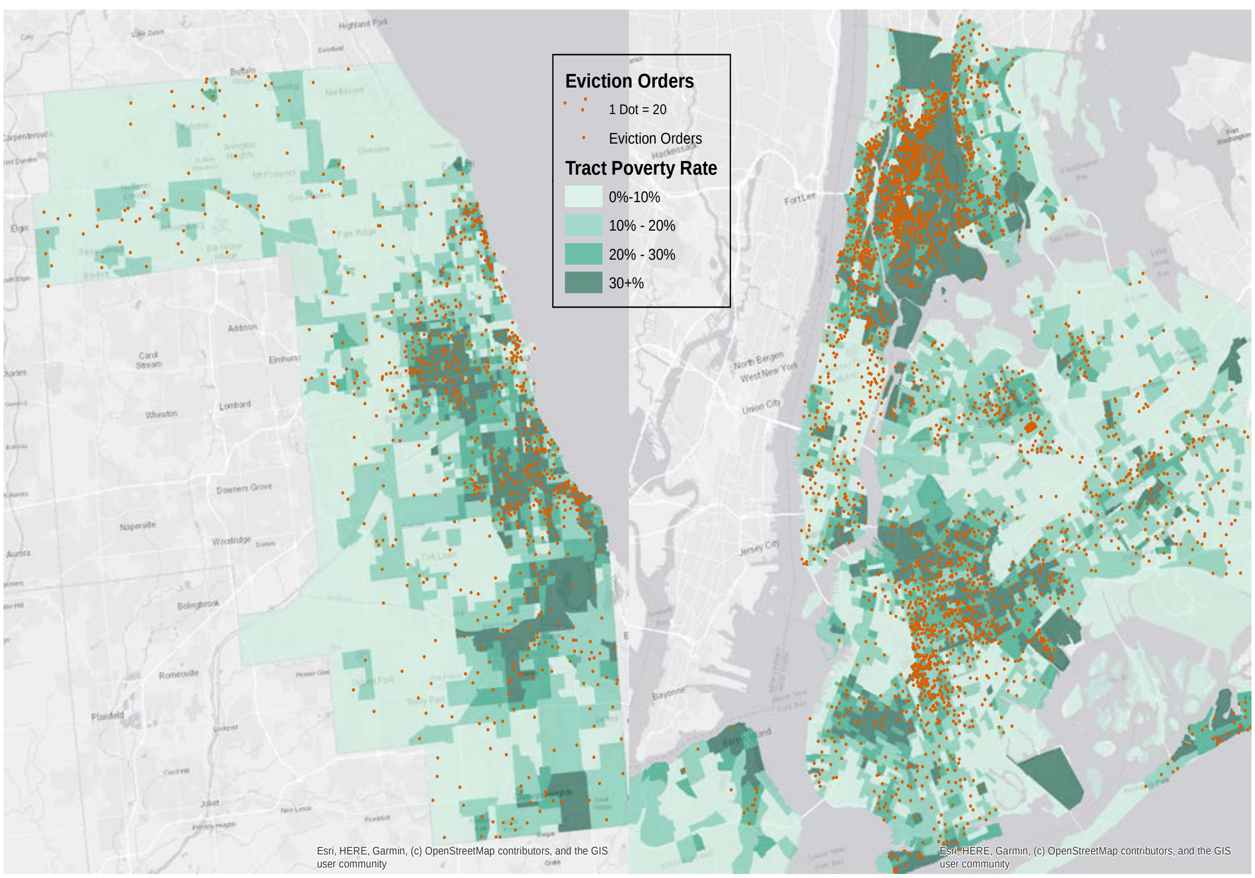
[30] We are unable to impose restrictions on how the Census Bureau assigns PIKs, such as requiring the linkage to use pre-filing information only, as we do in constructing the credit bureau samples and the New York benefits sample.

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.

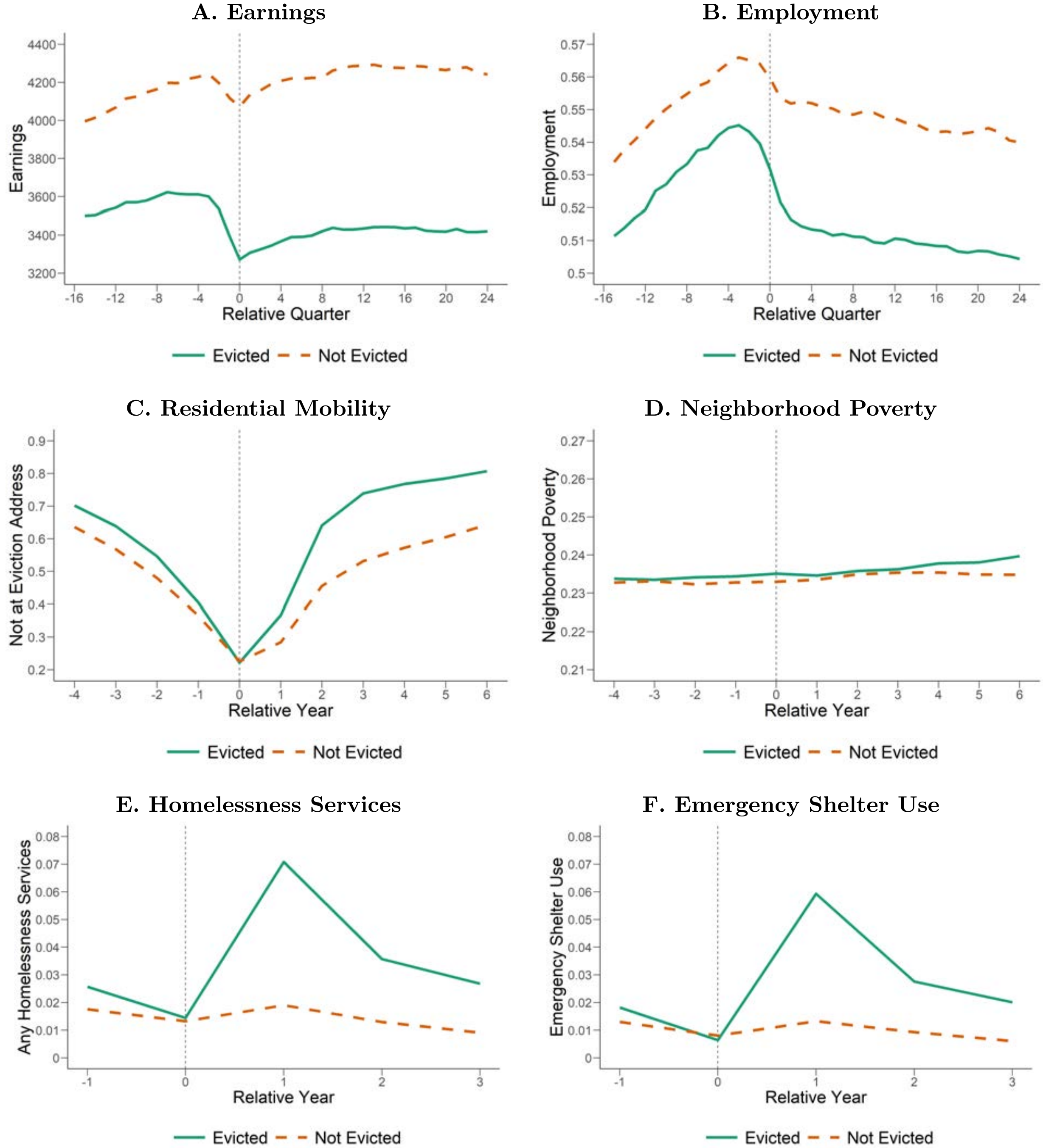
The presence of such correlated unobservables can bias frequently-used methods for identifying the effects of eviction, such as cross-sectional comparisons corrected only for observable characteristics, and difference-in-differences methods. We explore the potential bias of such methods using our data. 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

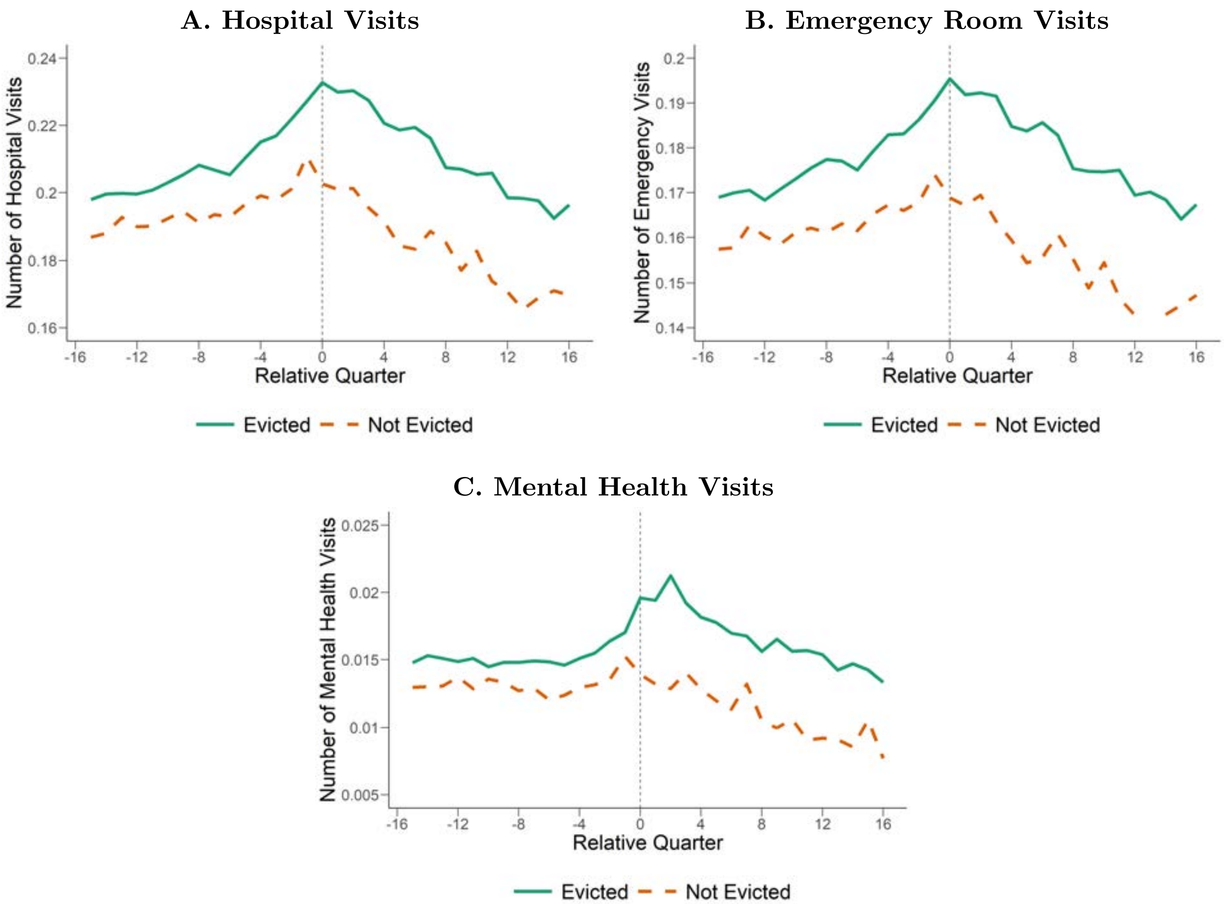


Notes: This figure shows trends in labor market and housing outcomes relative to eviction filing, combined across Cook County and New York. For each location, we estimate equation 4.1 and plot the equal-weighted average for the evicted and non-evicted groups in each time period. The only controls are 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

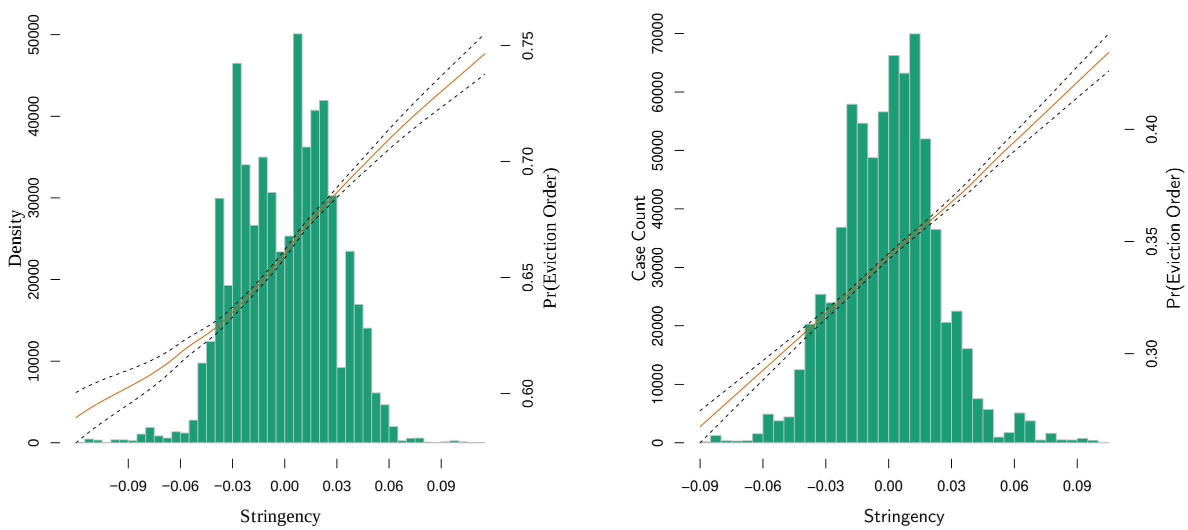
[38]The age, gender, and race controls are constructed using the Census Bureau Numident file and supplemented with the 2010 Decennial Census in Cook County, and using the administrative benefits data in New York. In the credit bureau samples, we omit race controls because of data use restrictions. Similarly, 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] After year 2, the impact of eviction on moving residences diminishes and becomes statistically insignificant (see Appendix H.2), although these effects are estimated with less precision. This result is consistent with the interpretation that after year 2, the causal effect of the eviction order on moving residences is muted by the non-evicted group becoming more likely to move.

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-

[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.

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 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

[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.

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

[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.

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).

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

[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.

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 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

[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.

[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.

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 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?

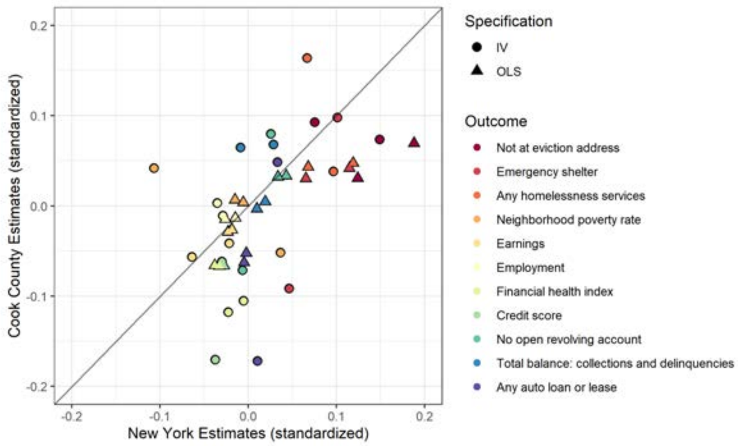
The IV estimates above can be interpreted as the causal effects of eviction for compliers. One might additionally be interested in whether these estimates are representative of effects for the full population of evicted tenants. One possible approach to drawing inference about

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

[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.

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.

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 and covariance stationary process—relatively strong assumptions in our setting—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

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

[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.

evicted tenant may be smaller than those for the marginal tenant.

Table 4: Impact on Housing Situation

	1 Year After Filing			2 Years After Filing		
	$\mathbb{E}[Y\|E=0]$ (1)	OLS (2)	IV (3)	$\mathbb{E}[Y\|E=0]$ (4)	OLS (5)	IV (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 debate over eviction-related policies, such as the extent of general equilibrium effects, remain unsettled, we make significant progress on the key question of whether and how eviction affects tenants. Our results suggest that averting an eviction order may yield considerable benefits for tenants. 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.

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