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DOES EVICTION CAUSE POVERTY?
QUASI-EXPERIMENTAL EVIDENCE FROM COOK COUNTY, IL

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Does Eviction Cause Poverty? Quasi-Experimental Evidence from Cook County, IL
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ABSTRACT

Each year, more than two million U.S. households have an eviction case filed against them. Many cities have recently implemented policies aimed at reducing the number of evictions, motivated by research showing strong associations between being evicted and subsequent adverse economic outcomes. Yet it is difficult to determine to what extent those associations represent causal relationships, because eviction itself is likely to be a consequence of adverse life events. This paper addresses that challenge and offers new causal evidence on how eviction affects financial distress, residential mobility, and neighborhood quality. We collect the near-universe of Cook County court records over a period of seventeen years, and link these records to credit bureau and payday loans data. Using this data, we characterize the trajectory of financial strain in the run-up and aftermath of eviction court for both evicted and non-evicted households, finding high levels and striking increases in financial strain in the years before an eviction case is filed. Guided by this descriptive evidence, we employ two approaches to draw causal inference on the effect of eviction. The first takes advantage of the panel data through a difference-in-differences design. The second is an instrumental variables strategy, relying on the fact that court cases are randomly assigned to judges of varying leniency. We find that eviction negatively impacts credit access and durable consumption for several years. However, the effects are small relative to the financial strain experienced by both evicted and non-evicted tenants in the run-up to an eviction filing.

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... and no one knew whether the house was mine or yours because there was no disagreement between me and you. But now I am being subjected to violence by your very own Ptolema, who sent me word to this effect: "Give up the house. Otherwise your household furnishings will be put out."

- Letter from the third century A.D. by an Egyptian tenant to his landlord (Frier, 1980).

1 Introduction

Each year, more than two million U.S. households have an eviction case filed against them (Desmond et al., 2018a). Many of these households live in poor urban communities where eviction is a frequent occurrence; in some census tracts, more than 10 percent of renter households are summoned to eviction court annually. Many cities have introduced policy measures to reduce the number of evictions,¹ motivated by a growing body of research showing that being evicted is associated with subsequent adverse economic outcomes for low-income households.² Yet it is difficult to know the extent to which such associations represent a causal relationship because eviction itself is likely to be a consequence of adverse life events, such as job loss, negative financial shocks, or deteriorating health. Quasi-experimental research capable of establishing such a causal relationship has thus far not been available to inform this policy debate.

This paper provides new evidence on the consequences of eviction for financial distress, residential mobility, and neighborhood quality. We collect the near-universe of Cook County court records over a period of seventeen years, and link these records to credit bureau and payday loans data. We first characterize trends in financial strain in the run-up and aftermath of eviction court for both evicted and non-evicted households, documenting high levels and striking increases in financial strain in the years before an eviction case is filed. This pattern is reminiscent of an ‘Ashenfelter dip,’ which has been shown to drive erroneous conclusions about causal impacts in other settings. Guided by this descriptive evidence, we employ two approaches to draw causal inference on the effect of eviction. The first takes advantage of the panel data through a difference-in-differences (DiD) design. The second is an instrumental variables strategy (IV), relying on the fact that court cases are randomly assigned to judges of varying leniency.³ Our findings lead to two broad conclusions.

First, we find that eviction negatively impacts credit access, credit scores, and durable consumption for several years, and increases debt in collections. However, when we consider the

¹See Appendix A for a review of recently proposed or passed reforms.

²See, e.g., Crane and Warnes (2000); Desmond (2012); Desmond and Kimbro (2015); Desmond et al. (2015); Desmond and Gershenson (2016a), and Desmond (2016).

³Our analysis focuses on the causal effect of a court-ordered eviction, also referred to as an order for possession. A court-ordered eviction is well defined by the legal system and recorded in data sets derived from court records. However, it is important to note that this is a more narrowly-drawn concept than an “involuntary move,” and distinct from a tenant being illegally coerced to move by his landlord, which is sometimes referred to as an “informal eviction.” Both concepts appear in the sociology literature and are occasionally used interchangeably with the term “eviction.” We depart from that practice in using the term “eviction” solely to denote an eviction order issued by the court.

magnitude of these effects in the context of the financial strain experienced by both evicted and non-evicted tenants in the years preceding an eviction case, the effects are small. For example, the IV estimate for total debt in collections 13 to 36 months after the case is \$209 and statistically insignificant, yet both groups have, on average, approximately \$3,000 dollars in collection four years prior to the case, and both groups experience increases of \$1,000 to \$1,200 dollars between two years before and two years after the case. In addition, we do not find evidence of a causal impact on residential mobility or neighborhood poverty. The IV and DiD estimates are, for the most part, similar in magnitude, but the IV estimates are less precise.

Second, bias due to selection on levels and trends, if ignored, leads to the erroneous conclusion that eviction has large impacts on financial distress. Using an additional panel of credit records for a random sample of Cook County residents, we replicate the empirical strategy used in existing studies, which compare evicted tenants to tenants not in eviction court (controlling for observable characteristics). The results from this analysis imply large effects of eviction. In contrast, when we limit the sample to tenants in eviction court, OLS regressions comparing evicted tenants to non-evicted tenants produce much smaller estimates. For example, the sample restriction reduces the estimated effect on credit score by more than 75 percent. Our results suggest that, while we find evidence that eviction exacerbates financial strain, the financial strain faced by those in eviction court is largely pre-existing, and not a consequence of being evicted *per se*.

Our research speaks to an active policy debate on how, if at all, local governments should address the high frequency of evictions. Given the prevalence of evictions, it is important to know how tenants might be affected by policies that intervene in eviction court, such as policies that make proceedings more lenient toward them. Our results speak directly to this question. While we find small causal effects on financial health and larger effects on access to credit, the results are much more moderate than the existing work on evictions. Moreover, both evicted and non-evicted households face increasing financial distress more than two years before the eviction court case is filed. This suggests that interventions targeting eviction court are likely to be ineffective at alleviating the financial distress of evicted tenants. The results also point to the possibility that intervening earlier may be necessary to avoid the financial strain faced by evicted tenants.

There is little research by economists on evictions. Our paper contributes to the broader economics literature on the consequences of housing policy for the economic mobility of low-income households. Several studies of housing vouchers and the Moving to Opportunity program have found small benefits of moving to a better neighborhood for adults, and larger effects for children (Kling et al., 2007; Gennetian et al., 2012; Chetty et al., 2016; Chyn, 2018; Van Dijk, 2019). Evans et al. (2016) show that emergency financial assistance is a cost-effective tool for reducing homelessness. We contribute to this literature by studying the dynamics of financial strain surrounding an eviction filing, by implementing a quasi-experimental research design to

identify the causal effect of an eviction,⁴ and by documenting the prevalence of eviction in a major urban area. Moreover, we employ a set of outcome variables that has been under-used for studying financial strain in the context of housing policy. Our credit panel allows us to follow individuals across neighborhoods, not only within Cook County, but throughout the US, which is uncommon in studies of the urban poor.⁵ We also observe a tenant’s interaction with subprime lenders. This data allows us to observe demand for high interest loans, which are common among poor households (Skiba and Tobacman, 2015; Bhutta et al., 2015).

This paper also contributes to a growing body of work in sociology and public health. Recent studies find that eviction has a negative association with the physical and mental health of tenants (Burgard et al., 2012; Desmond and Kimbro, 2015; Sandel et al., 2018), and a positive association with depression, stress, material hardship (Desmond and Kimbro, 2015), suicide (Fowler et al., 2015; Rojas and Stenberg, 2015), job loss (Desmond and Gershenson, 2016a), and homelessness (Crane and Warnes, 2000; Phinney et al., 2007). Desmond and Bell (2015) provide an overview of this literature. Due to the limited availability of administrative data on evictions, the evidence is largely based on ethnographic research and short-term surveys of households at risk of eviction, including the Milwaukee Area Renters Study. We contribute to this literature by assembling a large-scale administrative data set of eviction cases in Cook County that is linked to a panel of credit reports, including payday loan account openings and inquiries. Our study presents some of the first evidence on the effects of eviction that addresses the endogeneity from selection and correlated unobservables. One closely related study is Collinson and Reed (2019), an independent and contemporaneous working paper that studies public assistance recipients who appear in New York City’s eviction court, and also uses a randomized case assignment design to examine the impact of eviction on tenants’ income, mental health, future public assistance receipt, and homelessness.⁶ In addition, Desmond et al. (2018a) have recently assembled and made publicly available area-level data on the number of eviction court cases. Both of these research efforts are complementary to our paper and make important advances in data collection on evictions.

The remainder of the paper is organized as follows. Section 2 provides institutional details relevant for understanding evictions in Cook County. Section 3 describes the data collection and

⁴Several recent studies in other settings rely on random assignment of cases to judges for identification, including Kling (2006); Berube and Green (2007); Green and Winik (2010); Dahl et al. (2014); Maestas et al. (2013); Dobbie and Song (2015); Aizer and Doyle (2015); Bhuller et al. (2019); Mueller-Smith (2015); Dobbie et al. (2018); Hyman (2018).

⁵Several studies use credit bureau data to measure financial strain, including work on health insurance (Mazumder and Miller, 2016; Dobkin et al., 2018) and bankruptcy (Dobbie et al., 2017).

⁶There are methodological differences between Collinson and Reed (2019) and this paper worth highlighting. First, because Collinson and Reed (2019) do not observe the identity of judges, their instrument is based on courtroom leniency rather than judge leniency, which is problematic because judges rotate across courtrooms. Second, the treatment in Collinson and Reed (2019) is the execution of an eviction order by the City Marshal, rather than the court eviction order. In Appendix B, we explain how this approach may lead to concerns related to identification, measurement, and interpretation of the identified parameter. Despite these differences, Collinson and Reed (2019) find a similar pattern of moderate causal effects and we view their results as complementary to those presented here.

linkage process, and provides a description of the population of tenants in the baseline sample. Section 4 explores selection into eviction court and documents that this is an important source of selection bias. Section 5 provides new descriptive evidence on the evolution of financial strain and residential mobility experienced by evicted and non-evicted tenants in the run-up to and aftermath of court filing. Section 6 formalizes the quasi-experimental research design and tests the key underlying assumptions. Section 7 presents the main findings of the causal impact of eviction and a discussion of the mechanisms. Section 8 concludes.

2 Evictions in Cook County

To interpret the causal effects that we estimate in this paper, and to assess the external validity of our findings, it is necessary to understand the institutional environment in which evictions take place in Cook County. Below, we provide aggregate empirical facts based on our newly assembled data, describe the eviction court process, and discuss the representativeness of Cook County for other urban areas.

2.1 Scope and spatial incidence

Thirty to forty thousand evictions cases are filed every year in Cook County. Figure 1 shows the number of eviction cases filed and the number of evictions ordered by a judge from 2000 to 2016, the period covered in our analysis. There is a slight downward trend in both the number of evictions and the number of cases filed, but evictions have been a relatively stable feature of the rental housing market over this period. As a benchmark for the scope and cyclicity of evictions, Figure 1 also shows the number of foreclosure filings between 2005 and 2016. The number of eviction filings exceeds the number of foreclosure filings, except during the financial crisis.

Evictions are concentrated in low-income neighborhoods. Figure 2 presents a map of the first municipal district of Cook County, which includes the City of Chicago, and depicts the number of evictions relative to the total number of occupied rental units in each census tract, for the year 2010. While evictions occur across all of Cook County, they are concentrated in Chicago’s poorer south and southwest side neighborhoods. We find that more than 44 percent of evictions occur in census tracts with more than 20 percent of residents living below the poverty line, and more than 22 percent of evictions occur in census tracts with more than 30 percent of residents living below the poverty line. This finding is consistent with Desmond (2012), Desmond and Shollenberger (2015), and Desmond and Gershenson (2016b) who find that eviction is a common occurrence in poor communities in Milwaukee. In the City of Chicago, the census tracts with the highest concentration of evictions have more than 10 percent of occupied rental units with at least one eviction per year, which is four times the city-wide eviction rate of 2.5 percent.

2.2 The eviction process

This section summarizes the legal process surrounding eviction.⁷ To begin an eviction, the landlord must serve the tenant a written notice, which includes the reason for termination of the lease, and the requisite number of days until the lease will terminate. The notice must list all tenants whose names are on the lease, and it will typically refer to tenants who are not on the lease as “any and all unknown occupants.” The period before termination varies depending on the reason for discontinuation of the lease; for instance, nonpayment of rent has a 5-day notice period, using the property for the furtherance of a criminal offense has a 5-day notice period, and breaking a rule in the lease such as a prohibition of pets has a 10-day notice period. The data does not include the reason for eviction; however, studies of eviction court in other cities have found nonpayment of rent the most common reason for eviction (Desmond et al., 2013).⁸ The landlord has discretion over whether and when to serve a termination notice. Chicago has no official policy specifying the number of days a tenant may be late on the rent before the landlord is allowed to serve a notice.

If the number of days in the written notice elapses without resolution, the landlord has a right to take legal action and file an eviction case with the clerk in the Circuit Court of Cook County. The case filing is the starting point from which we observe evictions in our study. The landlord must file the case in the district where the property is located.⁹ When filing, the landlord must decide whether to file a *single action* case, in which he seeks only possession of the property, or a *joint action* case, in which he seeks both possession and a money judgment. At the time of filing, the landlord or his attorney can select a return date for the first hearing, which must be at least 14 days from the date the case is filed. In our data, the earliest available date is almost always selected.

At the time of filing, the case is randomly assigned to a courtroom on the selected date by a computer algorithm, in accordance with the Circuit Court of Cook County official policy (General Order 97-5, effective June 2, 1997). The landlord is notified of the court room number and time slot to which the case has been randomly assigned, but is not provided with the name of the judge who will preside over the hearing. Our data include the initial judge assignment, which we use to construct our instrument.

Cases may end up being ruled on by a different judge than the one initially assigned, for one of three reasons. First, if the defendant has not been successfully served a court summons by

⁷Appendix C contains further details. The relevant legislation can be found in the Municipal Code of Chicago Residential Landlords and Tenants Ordinance (RLTO) and the Illinois Compiled Statutes (ILCS). Particularly relevant are the Forcible Entry and Detainer Act (735 ILCS 5/9) and the Civil Practice Act (735 ILCS 5/2).

⁸Following the notice, if the tenant pays the full amount of rent due, the landlord must accept the payment and loses the right to evict the tenant. In the City of Chicago, and in many other jurisdictions, accepting partial payment will cause the landlord to lose the right to evict the tenant based on the filed notice. Such instances will not appear in our data since the conflict is resolved prior to court filing.

⁹See Appendix C.3 for a map of the six court districts in Cook County.

the date scheduled for the first hearing, a new attempt must be made to serve the tenant, and the first hearing is rescheduled.¹⁰ Rescheduling the first hearing can sometimes lead to the case being assigned to a different judge; for example, if the currently assigned judge is transferring out of eviction court or is on leave. Second, either party has a right to request assignment of a new judge once. The case is then randomly assigned to another judge.¹¹ Third, either party can request a trial by jury, which will result in the assignment of a jury trial judge, but such cases are rare; they comprise only 3 percent of total case volume. Since requests for a new judge may be endogenous to the initial judge assignment, we use the initial judge assignment to construct our leniency measure.¹²

Hearings in eviction court are typically concluded quickly. Court observation studies have found that the average eviction hearing is completed in less than 2 minutes in Cook County (Doran et al., 2003). The judge’s main decision is whether to grant the landlord an eviction order. For joint action cases, in which the landlord is additionally seeking a money judgment, the judge also decides the amount, if any, to award the landlord. If the judge decides against an eviction order, the case is dismissed, and no money judgment is awarded. We discuss dismissals in greater detail below.

The vast majority of tenants (97%) are not represented by an attorney. This stands in contrast to the high proportion of represented landlords (75%). Figure 3 provides an overview of possible trajectories an eviction case can take through the court system, as well as a breakdown of the fraction of cases that follows each path in our data. About two out of three cases result in an order for eviction.

If an eviction order is granted, the landlord has to subsequently file the order with the Sheriff’s Office and pay a nonrefundable \$60.50 administrative fee. A sheriff’s deputy will then execute the eviction order, which involves changing the locks and removing any possessions. The sheriff does not execute all orders for possession, because the landlord may neglect to file the order or to pay the sheriff, or because occupants voluntarily leave after the judge’s order. According to data from the Cook County Sheriff’s Office from 2011 to 2015, only 50.6 percent of the cases in our data with an eviction order are filed with the Sheriff’s Office, and 51.8 percent of those are executed. For executed evictions, the median time between when the judge issues the eviction order and when the eviction takes place is 71 days.¹³ Tenants do not know the exact date on

¹⁰If the tenant cannot be served after multiple attempts, the judge can allow the case to proceed without the defendant, though in that circumstance no money judgment can be awarded. In about 10 percent of cases, the case is dismissed prior to the tenant being successfully served, which may occur if the tenant has moved out or if the tenant and landlord reach an agreement.

¹¹In the court dockets, 5.9% of cases involve a judge transfer.

¹²The method for deriving this information from the case histories is detailed in Appendix D.2.

¹³Although enforcement of the order may occur as soon as 24 hours after the order has been filed with the Sheriff’s Office, the length of time it takes to complete the eviction is determined by the capacity constraints of the Sheriff’s Office and by a rule that prohibits enforcement of eviction if the temperature is below 15 degrees Fahrenheit. Also, no evictions are executed between Christmas and New Year’s Day.

which the eviction will be executed. The Cook County Sheriff’s Office publishes their planned schedule only 3 days in advance.

2.3 How representative is Cook County?

Cook County’s eviction court procedures are similar to many other eviction courts across the United States. The process from filing an eviction case to the execution of an order typically takes several months, which is similar to the duration in other county courts. Other notable commonalities are the high proportion of unrepresented tenants, the public accessibility of court records, and the swiftness with which eviction hearings are concluded (Hartman and Robinson, 2003; Greiner et al., 2012; Desmond, 2012; Krent et al., 2016).

The regulatory environment of the rental housing market in Cook County is also broadly similar to that of most medium to large American cities, with a handful of exceptions. Cook County has no rent-controlled housing, which can affect landlords’ incentives to evict as well as tenants’ ability to find new housing if evicted (Diamond et al., 2019). The absence of rent control is common in American cities, but notable exceptions are Washington, D.C., and some cities in California, Maryland, New Jersey and New York. Like most cities, Cook County does not have a comprehensive social safety net aimed at preventing housing instability or homelessness. For example, there is no “right to shelter” which would guarantee recently evicted individuals access to a homeless shelter.¹⁴ Finally, in Cook County it is legal for a landlord to decline to extend a lease beyond its stated termination date without citing a specific reason. Such “no cause” evictions are allowed in most U.S. cities.¹⁵ Taken together, these facts suggest that Cook County’s institutional environment is similar to most American cities, though potentially less representative of some large cities with more heavily regulated rental markets.

3 Data collection and linkage

The analysis in this paper relies on extensive data collection efforts that have brought together detailed court case histories spanning a period of seventeen years. These records have been linked to longitudinal data on defendants’ credit reports. Below, we outline the structure of these data sets, linkage procedures, and sample selection criteria. Further details on data cleaning and linkage are available in Appendix D.2.

¹⁴ As of this writing, only Massachusetts, New York City, and Washington D.C. have a right to shelter.

¹⁵ Cities that prohibit or place restrictions on “no cause” evictions include Seattle, San Francisco, Berkeley, San Diego, New York City, New Jersey, and New Hampshire.

3.1 Court records

Our data set on court histories includes the near-universe of eviction cases filed in Cook County in the years 2000-2016. We assembled this data through the collection of public records, supplemented with proprietary data sourced from Record Information Services (RIS), a private company that compiles court records in Illinois. The resulting case-level data set includes the defendant’s name and address, which are used to link the court records to other data sets. The data includes all events entered into the electronic case file, beginning with the filing by the landlord or his attorney. The electronic case files include information on the case type (single or joint action), filing date, judge’s name, plaintiff’s name, defendant’s name, attorneys’ names if any, the amount of money claimed by the plaintiff in a joint action case (ad damnum amount), and details of each hearing, motion, and decision in the case. Importantly, the court records include eviction orders and money judgments issued by the judge. We supplement these data with records from the Sheriff’s Office on court summons and the timing of forcible evictions from 2010-2016, which were obtained by FOIA request. Using tenant addresses, we also append neighborhood characteristics using data from the American Community Survey.

Sample restrictions on court records. Our sample of court cases includes all cases filed between January 1, 2000 and December 31, 2016. We exclude from our analysis cases that were not concluded by the end of this period. The full sample of cases includes 772,846 named individuals in 583,871 cases.¹⁶ As laid out in Appendix Table D.1, we first drop eviction cases associated with businesses, cases associated with condominiums, cases for which the defendant’s name is not provided, and cases involving more than \$100,000 in claimed damages. After these restrictions, we are left with 707,213 named individuals across 546,190 cases, which represent our main sample of court records. We use this sample to construct leniency measures and to link credit bureau data. For our IV analysis, we further restrict the sample to cases in which the judge presided over at least 10 cases that year, and to cases in which at least two judges presided over eviction cases in that week and district. This sample of cases includes 599,366 named individuals in 466,548 cases, and includes 250 judges who presided over 10 cases in at least one year. The average judge in our sample presided over more than 700 cases per year.

Conceptualizing treatment and counterfactual. We define a case as ending in eviction if the judge issues an eviction order. In the court records, this is entered as an “order for possession.” This definition of an eviction is similar to the one used by Desmond et al. (2018b), who compile the most complete national database of eviction filings and executions to date, based on court records.¹⁷

¹⁶There are more named individuals than cases because cases commonly list multiple individuals.

¹⁷An alternative definition of treatment is the execution of the eviction order by the Sheriff’s Office – i.e., the combination of a judge’s order and the Sheriff’s deputy execution of that order. However, this definition introduces

Cases that do not end in eviction are dismissed, and there are multiple types of dismissals. Dismissals “with prejudice” bar the landlord from bringing another eviction case with the same allegations against the tenant. Dismissals “without prejudice” permit the landlord to present the same case in court again. Dismissals may also be recorded as “dismissed by stipulation or agreement,” which reflect a settlement in which the tenant may agree to move out or the parties agree on a payment plan.¹⁸

There are several reasons an eviction may generate costs for the tenant. Landlords may screen tenants based on prior evictions, making it harder for tenants to secure a future rental contract; other potential creditors may do the same. An eviction is also recorded as a civil judgment on the tenant’s credit report if it is a joint action case and it ends in a money judgment. An eviction also requires the tenant to move and incur the costs associated with relocating and reorienting the household’s work and schooling arrangements. A dismissal increases the likelihood of the tenant remaining in their current residence, though this is not guaranteed.

3.2 Credit bureau records

Our primary measures of financial strain come from credit files held by Experian, one of the three largest credit bureaus in the United States.¹⁹ The credit report data are biennial snapshots from March, 2005, to March, 2017, and an additional period, September, 2010. In addition to the linked sample, we also have a panel of a 10 percent random sample of individuals living in Cook County in March, 2011, which is linked to the other observation periods. An advantage of the credit bureau data is that it allows us to follow individuals across eviction cases. We are also able to follow individuals across neighborhoods, not only within Cook County, but throughout the US, which is uncommon in studies of the urban poor.²⁰ Another feature of the data is that we are able to observe a tenant’s interaction with subprime lenders, which we believe is a first in the housing instability literature.

We analyze the following sets of financial outcomes: overall financial health, unpaid bills, durable consumption, and access to credit. We also study demand for high-interest loans using subprime borrowing data, described below. Finally, we study mobility and neighborhood poverty. These outcomes are described in detail in Appendix D and are briefly described here. All dollar amounts are expressed in 2016 dollars using the CPI-U for the Chicago metro area.

We measure overall financial health using VantageScore 3.0, which is on a scale of 300-850;

challenges related to identification, interpretation, and measurement. We discuss these challenges in Appendix B.

¹⁸In order to characterize the fraction of dismissals that result in the tenant moving out, we hand-collected additional court microfilm documents for a random sample of court cases ending in dismissal, which we describe in Appendix D.3. We find that, for cases with available information, approximately half of dismissals involve the tenant agreeing to pay the landlord some amount, and slightly more than half of dismissals involve the tenant agreeing to move.

¹⁹Avery et al. (2003) provide a detailed description of this data.

²⁰The random sample does not include the time-varying ZIP code of residence, so we are unable to compare move rates in the random sample to those in our linked sample.

scores under 600 are considered subprime by the credit bureau. We measure unpaid bills as the total balance in collections. Collections represent unpaid debt, such as credit card balances, which the original lender decides to turn over to a collections agency following a period of delinquency (typically at least 30 days).²¹ Our proxy for durable goods consumption is any positive balance on an auto loan or lease, following an approach taken by [Dobkin et al. \(2018\)](#) and [Dobbie et al. \(2018\)](#). 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.²²

The credit bureau data are also linked to the largest available database of subprime borrowing behavior. We measure inquiries into and openings of high-interest single payment microloans. These account inquiries and openings include those originating from either online or storefront subprime lenders. We observe subprime loan inquiries for all months between September 2011 and November 2018, and subprime account openings for all months between January 2010 and November 2018. Note that we only observe subprime borrowing activity for consumers who have a record in our main credit file. Approximately 66.8 percent of the matched credit sample have a payday loan account inquiry at some point in the sample period, and 17.28 percent have an account opening.²³ These data are described in more detail in Appendix [D.1](#).

The credit bureau has supplementary information including ZIP code of residence and demographic information including gender and year of birth.²⁴ We measure moves as a change in ZIP code, and neighborhood poverty as the ZCTA-level poverty rate. There are 215 ZIP codes in Cook County, and hence the ZIP code represents a relatively fine unit of geography.²⁵

3.3 Data linkage

The court sample was linked by Experian to credit report data by searching name and address identifiers against its master file that includes a name and address history. The overall match rate is 61.3 percent, which is slightly lower but comparable to match rates of studies that use

²¹Collections remain on the credit report for up to 7 years from the date the debt first became delinquent and was not brought current; after 7 years it is automatically removed from the report.

²²A revolving account includes any account in which the individual can carry a balance and is not required to pay the entire amount at the end of the month.

²³The high-interest loans we study are single payment microloans that originate from traditional storefront or online subprime lenders. This data is from Clarity, a credit reporting agency that maintains a subprime database of over 62 million unique consumers. We refer to these loans as “payday loans,” even though not all of these microloans are tied to the individual’s paycheck.

²⁴Evictions are not directly observable on a credit report, but they may be included in the aggregated category of civil judgments if the decision includes a money judgment. In those cases, the credit bureau reports the presence of a money judgment and the judgment amount awarded by the court.

²⁵From our discussions with data experts at Experian, addresses are recorded through the reporting and inquiry process, and the ZIP code is not necessarily the most recent address reported, but is the *modal* address of recently reported ZIP codes. See [Lee and van der Klaauw \(2010\)](#) for a detailed description of the FRBNY consumer credit panel, which is a similar data set to the one used here, and [Molloy and Shan \(2010\)](#), which uses it to track residential moves surrounding foreclosure.

a similar strategy to link administrative data sets.²⁶ In our analysis, we restrict the sample to those individuals who are matched to a credit report *prior* to the eviction filing date, so that the match is not endogenous to the eviction order.

The matched sample are those who are “credit visible,” meaning they have a credit record. The Consumer Financial Protection Bureau reports that in low-income neighborhoods, slightly more than 70 percent of adults have a credit record (Brevoort et al., 2015). We expect this number to be higher in our population since, in Chicago, having a utility bill alone is sufficient to generate a credit record, and individuals with their name on a lease are likely to have had a utility bill.²⁷

To better understand our matched sample and how it relates to the overall population of tenants in eviction court, we explore the characteristics of credit record matches in Appendix E. Appendix Table E.2 shows that evicted tenants are 1.9 percentage points less likely to be matched to a credit record, and tenants without legal representation are 1.5 percentage points less likely to be matched, while tenants in richer neighborhoods are only slightly more likely to be matched: a \$1000 increase in median household income at the ZIP code is associated with a 0.1 percentage point increase in the match rate.

There is minimal attrition in the matched credit panel; an individual who appears in one period has over 99 percent likelihood of appearing in each future period. We explore attrition in Appendix Table E.1, which shows that attrition is unrelated to stringency. We regress an indicator for appearing in a subsequent filing year on judge stringency for each pair of years in the sample, and cannot reject a null effect of stringency on appearing in a future credit bureau record for any pair of years.

3.4 Summary statistics

Table 1 presents summary statistics for the sample used in the IV analysis (columns 1 and 2), and compares this sample to our 10 percent random sample from Cook County, which we reweight to match the ZIP code distribution of eviction cases (column 3). Comparing the eviction court sample to the random sample, the eviction court sample is far more likely to be female (64 percent compared to 50 percent) and is younger than the random sample (40 years old, on average, compared to 46 in the random sample). Individuals who have an eviction case are more likely

²⁶For example, Dobkin et al. (2018), using additional identifiers unavailable to us here (SSNs), are able to match 72 percent of their Medicaid sample, and the Oregon Health Experiment has a match rate of 68.5 percent.

²⁷Up until the summer of 2016, People’s Gas, the main natural gas provider in Chicago, provided “full-file” reporting to credit bureaus, allowing individuals who would otherwise have no credit score due to thin or no credit history to have a credit score, potentially helping many build credit (Turner et al., 2008, 2012). As pointed out by PERC (2006), Illinois was one of the few states with any utility company making full-file reports, which suggests that a much larger portion of our sample may have credit scores and credit histories compared to cities in other states.

to be black than a randomly selected individual from their neighborhood.²⁸ Within the sample of defendants in eviction court, evicted tenants are more likely to be black than non-evicted tenants, although we note that our race measure is imprecise because we use a probabilistic imputation based on name and Census tract, with a 75 percent probability threshold.

Tenants in eviction court have substantially more debt relative to individuals in the random sample from the same neighborhood: the average collections balance for tenants in eviction court, averaged over the 13-36 month period after filing, is over \$3,000 compared to about \$1,200 for the random sample. As we will show in the event studies presented in Section 5, most of this debt is accumulated before the eviction filing. Similarly, the use of payday loans is much higher among tenants in eviction court compared to the random sample: payday inquiries among tenants in eviction court are nearly double that of the random sample, and payday accounts opened are substantially higher as well (38 percent higher for the evicted group, 93 percent higher for the non-evicted group).

Table 1 also reports the fraction of our eviction sample with a subsequent eviction case, either at any address, or at a different address than the reference case. Tenants who appear in eviction court are likely to appear in a future eviction court case: a tenant who is evicted has a 16.8 percent chance of appearing in eviction court within 36 months, at a different address, compared to 17.4 for the non-evicted tenants.

4 Selection into eviction court

The empirical approach that has been available to researchers studying eviction thus far is a comparison of evicted tenants to observationally similar renters, using survey data. This prior literature uses multivariate regression (Burgard et al., 2012; Desmond and Shollenberger, 2015) or matching methods (Desmond and Kimbro, 2015; Desmond et al., 2015; Desmond and Gershenson, 2016a), which rely on the assumption that, conditional on observables, eviction is effectively random. This is a strong assumption, because evicted individuals and individuals not facing an eviction case are likely to differ in unobservable aspects, which are likely to impact future outcomes. The extent to which these research designs are able to isolate causal relationships therefore depends on strong, largely untestable assumptions. In this section, we document the extent of selection into eviction court, and we show that the effect attributed to eviction is much smaller when the comparison group is non-evicted tenants.

For our first analysis, we append the 10 percent random sample from Cook County to our court sample, restricting the random sample to those over age 21 without a mortgage, and randomly assigning a placebo filing month. We also reweight the regression sample so that the random sample of individuals matches our eviction court sample in their distribution across ZIP

²⁸Note that the neighborhood characteristics of the eviction court sample are similar to those of the random sample by construction, because of the reweighting.

codes.

Comparing the eviction court sample to the random sample, we can replicate the finding from the existing literature that the effect of eviction is large and negative. The first bar of Figure 4 shows a nearly 100 point difference in average credit scores between evicted individuals and a representative sample from the neighborhood. For context, the credit score is on a scale of 300-850, and 300-579 is considered “very poor.” The second bar adds controls for age, gender, and year, which does little to close this gap. These results document large differences in levels between evicted individuals and similar individuals from the neighborhood, and this difference remains large after controlling for observables. One limitation of this exercise compared to the literature is that the administrative data contains substantially fewer controls than the survey data used in past papers.²⁹

The third bar of Figure 4 compares evicted and non-evicted tenants within the eviction court sample; the average difference in credit score is reduced to less than 20 points, providing strong evidence that there is substantial selection into eviction court. This difference changes little when controlling for observable covariates. Finally, the fifth bar additionally controls for lagged credit score prior to the eviction case. Controlling for lagged credit score further reduces the gap, demonstrating that there is selection into the eviction decision even once we restrict our comparison to those in eviction court.³⁰

These results suggest that there are two important sources of selection that must be addressed when attempting to quantify the causal impact of eviction: selection into appearing in eviction court, and selection into the court case ending in eviction. To deal with the first source of selection, we use court records that allow us to compare evicted individuals to individuals in eviction court who were not evicted. To deal with the second source of selection, we employ an IV strategy, which we describe in more detail in Section 6. For the remainder of the paper, we use the eviction court sample. We now turn to the dynamics of financial strain surrounding the eviction filing.

5 Trends in financial strain and residential mobility

This section presents an event study analysis comparing defendants who are evicted to defendants whose cases are dismissed. Studying the dynamics into and out of eviction court, we document substantial increases in financial strain for both evicted and non-evicted defendants beginning two years prior to the case, and we show no substantial relative increase in financial strain for the evicted individuals after the case.

²⁹For example, [Desmond and Bell \(2015\)](#) control for income, race, highest level of education, gender, marital status, children, age, past criminal record, past job loss, past relationship dissolution, and housing assistance receipt.

³⁰Appendix Figure G.1 depicts the time path of selection into eviction court for all main analysis variables. This figure shows selection by financial strain, as measured by levels of indebtedness and demand for payday loans, for both evicted and non-evicted tenants, relative to the random sample.

We use the following regression, where r indexes the month relative to the eviction case filing:

$$y_{it} = \gamma_t + \delta \times E_i + \sum_{r=S}^F \beta_r + \sum_{r=S}^F \delta_r \times E_i + \epsilon_{it}. \quad (5.1)$$

In the above equation, E_i represents an indicator for the case outcome being eviction, β_r represents coefficients on indicators for month relative to the case filing month, and δ_r are the coefficients on indicators for relative month interacted with the eviction outcome. For this analysis $S = -41$, $F = 72$, and the omitted month is $S = -42$. The only controls included are calendar year dummies (γ_t). Figure 6 plots the β_r , depicted as open circles, as well as these coefficients added to $\delta + \delta_r$, depicted as closed circles. For both sets of coefficients we add in the non-evicted group mean in $S = -42$ so that the magnitudes are easy to interpret.³¹

Overlaid on these nonparametric event studies, we depict a parametric specification of Equation 5.1, where the right hand side variables include a cubic polynomial in relative month prior to eviction filing ($r < 0$), a cubic polynomial in relative month for the months following eviction filing ($r \geq 0$), and these two cubic polynomials interacted with the eviction case outcome. Again, we add in the baseline mean for ease of interpretation, and the only controls are calendar year dummies. We require the polynomials on either side of the eviction filing to connect at $r = 0$, a choice motivated by the nonparametric event studies, which do not suggest a discrete jump at the time of filing.

Figure 6 reports results of the event study for several sets of outcomes, while Appendix Table 2 reports DiD estimates, based on the parametric specification, at different time horizons relative to $r = -12$. As shown in the top left panel of Figure 6, tenants who appear in eviction court have very poor credit in the run up to eviction and those whose cases end in eviction have worse baseline credit scores than the non-evicted group. Both groups experience declining credit scores, by about 12 credit points, in the 24 months prior to the filing date. Remarkably, the two groups' credit scores remain broadly parallel throughout the sample period, suggesting that an eviction does not have an additional scarring effect on credit scores for the evicted group. However, it takes 4-5 years for the two groups to return to their pre-filing peak.

The top right panel of Figure 6 shows the event study for total balances in collections. Prior to the eviction case being filed, both evicted and non-evicted groups have over \$2,750 of debt in collections, and both groups see roughly parallel increases in balances in collections. After eviction court, both groups experience a steep rise in their balances in collections – the evicted group by approximately \$1,000, the non-evicted group by \$750. While the gap in collections balances between the groups widens, the gap is small compared to their average pre-filing balances. The difference is also small compared to the overall large increase in collections observed for

³¹There is a tradeoff between including a longer time series and introducing composition effects in the coefficients. Appendix Table G.1 shows robustness to several alternative specifications that include restricting to a balanced panel, adding individual fixed effects, and restricting the sample to individuals' first cases.

both groups. In the five years after the case filing, the average collections balance never returns to the pre-filing average for either group.³²

An eviction may be mechanically related to collections debt if the defendant does not pay the money judgment associated with the eviction case. In this situation, the plaintiff can use the court process to collect the money, including obtaining a citation to discover assets, wage garnishment, and using a collections agency. In Appendix Figure G.3, we explore this possibility by presenting the collections event study separately for joint action and single action cases. This figure shows a broadly similar evolution of collections debt regardless of whether the plaintiff seeks a money judgment, suggesting the observed effect is not mechanical.

The bottom two panels of Figure 6 depict the results for having an auto loan or lease and for having no revolving line of credit. The auto loan variable exhibits flat or slightly increasing trends in the run-up to eviction court, followed by a drop after filing, along with a widening gap between evicted and non-evicted tenants. This suggests a decrease in expenditures or consumption of durable goods. Non-evicted tenants also exhibit this pattern of decreased consumption following the filing, which shows that both groups of tenants are experiencing strain that coincides with the timing of the case.³³ The bottom right panel of Figure 6 shows that both groups of tenants have limited access to credit in the run-up to eviction court. Prior to court, about 55 percent of the non-evicted group and 60 percent of the evicted group have no source of revolving credit such as a credit card. This rate notably increases for both groups following the court filing and the gap between evicted and non-evicted widens, suggesting that an eviction has an effect on credit access.

We now turn to payday loans. The data includes single payment microloan inquiries and account openings. The inquiries and account openings include both online and storefront loans, and provide insight into the demand for cash advances among tenants in eviction court. The left panel of Figure 7 shows the event study for inquiries into payday loans, depicting a dramatic increase in the 3 years leading up to eviction filing, from about 1 percent per month to about 1.6 percent per month, which is followed by an immediate decline for both groups after the filing date.

Payday account openings also exhibit a striking increase in the run-up to eviction court. After the eviction filing, while payday account openings fall for both groups, the non-evicted group has higher long-run levels of payday loan openings relative to the evicted group. The fact that inquiries fall in parallel following eviction court but openings remain higher for non-evicted

³²Appendix Figure G.2 further disaggregates the collections event study into the four largest collections categories, revealing that utilities, retail, and medical debt categories all increase \$100 to \$150, on average, in the 24 months prior to filing, and continue to increase by another \$50 to \$100 in the 24 months after.

³³These descriptive results are not sensitive to how we define a court-ordered eviction. In Appendix Figure G.7 we show event studies separately by whether a dismissal is “with prejudice,” meaning the case is dismissed and the landlord may not bring the case again with the same allegations. These event studies disaggregated by case outcome display the same broad descriptive patterns.

tenants suggests that an eviction may have a negative effect on the probability of having a loan approved.

The event studies provide several important takeaways. First, regardless of the case outcome, households in eviction court show signs of financial strain two to three years prior to having a case filed against them – with credit scores falling, collections rising, and increased inquiries into payday loans. Second, even though this analysis restricts the comparison to tenants in eviction court, we still find that evicted tenants are negatively selected on prior financial outcomes, with lower credit scores and higher total balances in collections four years before the filing of the cases. Finally, the event studies do not support the hypothesis that an eviction generates large and lasting financial strain, but instead suggest that eviction may exacerbate the initial decline and slow the recovery.

Appendix Figures G.3-G.5 reproduce the regressions behind Figure 6 in order to contrast these descriptive patterns for several subgroups: (i) joint versus single action cases, (ii) individuals without prior eviction cases versus individuals with prior cases, (iii) multi-headed versus single-headed households (as determined by the number of names listed as defendants in the case), (iv) subprime versus prime credit score two credit periods prior to filing. This sample division is based on a credit score threshold of 600. These analyses of subgroups look similar to the main effects with a few nuanced takeaways.

First, the joint action cases reveal more significant impacts of an eviction on credit access than single action cases. For example, there is a widening gap between evicted and non-evicted tenants in having a source of revolving credit in the aftermath of the eviction filing date, which is present for joint action cases but not single action cases. In addition, the impact of eviction on having an auto loan is much larger for joint action cases than single action cases. These results are consistent with a civil judgment affecting individuals' ability to borrow and to finance durable goods purchases. Second, in the run-up to eviction court, those without prior eviction cases have both higher levels and steeper growth in their demand for payday loans compared to those with prior cases. Third, in contrast to multi-headed households, single-headed households have a steeper rise in their lack of access to revolving credit following eviction court. Single-headed households also appear to be harder hit in terms of durable goods consumption.

Lastly, we compare defendants with subprime credit scores and defendant with non-subprime credit scores 25-48 months before the case. Defendants with non-subprime credit scores have a precipitous drop of 100 points in their credit score in the 24 months prior to filing. This decline suggests an acute source of distress, such as a drop in income or an increase in expenditures. Following eviction, these non-subprime borrowers experience a large – 200 percent – increase in unpaid bills and a doubling in the probability of not having a credit card.

DiD Estimates

In Figure 6, evicted and non-evicted defendants exhibit similar pre-trends, suggesting that the event studies can be used to provide causal estimates of the impact of an eviction using a difference-in-differences design. Appendix Table 2 reports DiD estimates of the effect of eviction, comparing the difference between evicted and non-evicted tenants at 12, 36, and 60 month outcome horizons relative to the difference at $r = -12$. These estimates are based on the parametric specification presented in the event studies depicted in Figure 6.

The DiD estimates reported in Table 2 provide magnitudes for what is observed in the event study figures, and Table G.1 explores the sensitivity of these estimates to including individual fixed effects, restricting the sample to a balanced panel, and restricting to those with no prior cases. Under the assumption of common trends, these tables confirm that an eviction has almost no effect on credit score and has a statistically significant 126 to 262 dollar effect on collections balances at the 36-month horizon. An eviction has a larger impact on access to credit, reflected in the decline in the probability of an auto loan of 1.1 to 1.5 percentage points at the 36-month horizon, and an increase in the probability of having no source of revolving credit by 1.4 to 3.7 percentage points at the 36-month horizon. The negative effect on having a revolving line of credit or a car loan persists at the 60-month horizon. Overall, these results provide initial evidence that the impact of an eviction on financial strain is small, but may have a meaningful impact on having a credit card or auto loan.

Analysis of residential moves

For the event study analysis of residential moves, we modify Equation 5.1 so that r is now measured in years relative to the filing year, rather than months relative to the filing month. We drop the September 2010 period, so that the sample years are March 2005-2017 biennially, which allows us to interpret residential ZIP code moves over constant, 24-month intervals. For this analysis, we include only individuals who are observed 4 or more years prior to eviction filing; hence individuals are observed in years $r = -5, -3, \dots, 5, 7$ or $r = -4, -2, \dots, 6, 8$.³⁴ We define a ZIP code move as a change in the individual's 5-digit ZIP code relative to the ZIP code 24 months prior. We choose the baseline (omitted) years $r = -5, -4$. Thus, the coefficient on β_r in Equation 5.1 is the probability of moving from year $r - 2$ to year r for the non-evicted group. In the figures, as before, we add the non-evicted group mean in the base period to the coefficients for ease of interpretation.

The left panel of Figure 8 presents the estimates of β_r and $\delta + \delta_r + \beta_r$, along with the 95 percent confidence intervals. There are several patterns worth pointing out. First, 2-year ZIP code move rates are high, even several years prior to eviction filing. The non-evicted group has a move probability of nearly 45 percent over the period from $r = -5$ to $r = -3$, while the evicted

³⁴We find qualitatively similar results if we restrict to individuals observed at least 3 years prior to filing.

group has around a 46 percent move probability over the same period. For comparison, the percent of renters that move within a 2-year span in Cook County is approximately 24 percent, according to our estimates based on the American Community Survey. Hence, tenants in eviction court are a highly mobile population, and this is true long before the eviction case filing.

Second, the point estimate spikes at $r = 1$ and peaks at $r = 2$, reflecting much higher ZIP code move rates in the 1-2 years following eviction filing. In particular, the 2-year ZIP code move rate jumps about 7.5 percentage points between the period from $r = -2$ to $r = 0$ and from $r = -1$ to $r = 1$. The evicted group has a higher move probability over the entire period represented in Figure 8, and these estimates are statistically significant at the 5 percent significance level. The gap in move rates between evicted and non-evicted is widest over the period from $r = 0$ to $r = 2$ – a gap of around 3.5 percentage points, which is about 8 percent of the non-evicted group mean.

These estimates reflect ZIP code-level moves, and may mask significant differences in move probabilities within ZIP codes. Appendix J provides an alternative analysis using linked address histories from InfoUSA. This alternative data source allows us to consider unit-level moves, but suffers from high levels of missingness and lower match rates. In the InfoUSA data, we find that more than 20 percent of observed moves in the matched sample are within ZIP code, suggesting we underestimate overall mobility for both group.

Overall, the results show both evicted and non-evicted groups have relatively high move probabilities in both the run-up and the aftermath of eviction court, and it takes 5 years to return to the pre-filing level. The figure also suggests that the difference in move rates of those with an eviction order compared to those with a dismissal is small relative to the overall high degree of housing instability for both groups.

The right panel of Figure 8 re-estimates the event study but with a measure of neighborhood quality as the outcome: the neighborhood (ZCTA-level) poverty rate. The main takeaways from this panel are that households from higher poverty neighborhoods are more likely to be evicted, and the gap between evicted and non-evicted widens slightly following eviction. Yet the overall trend is downward for both groups after three years, indicating that over time both groups are able to relocate to neighborhoods with lower poverty rates. Note that the only included controls are calendar year fixed effects; hence the trend downward may reflect improvements over the lifecycle.

We report DiD estimates of neighborhood moves and neighborhood quality in Table 3 and robustness results in Appendix Table G.2. The DiD regressions are the same regressions as those underlying Figure 8, with estimated differences at several time horizons relative to the difference in the year prior to eviction filing. The estimates reflect what we visually report in Figure 8, that the effect of an eviction on neighborhood moves is small. There is a small effect on the tenant’s neighborhood poverty rate at one year after filing relative to the year prior to filing, increasing the poverty rate by 0.20 off a base of 17.43, but this effect disappears three to five years after filing.

6 Instrumental variable analysis

In this section, we present an IV strategy to address the bias resulting from selection and simultaneity. We discuss how the assumptions that underlie the model are supported by the institutional environment for court-ordered evictions, and provide empirical evidence in support of these assumptions.

6.1 Empirical framework

Our empirical strategy exploits the random assignment of judges to eviction cases. Let E_i be an indicator equal to 1 if the judge orders an eviction for household i , $Z_{j(i)}$ the stringency measure of judge j assigned to i 's case, and Y_i the outcome of interest. To estimate the local average treatment effect (LATE), we use two stage least squares (2SLS) with first and second stage equations:

$$\begin{aligned} E_i &= \gamma Z_{j(i)} + X_i' \delta + \nu_i \\ Y_i &= \beta E_i + X_i' \theta + \eta_i \end{aligned}$$

Here X_i is a set of controls that includes district-year fixed effects and household characteristics.

For judge leniency to be a valid instrument, several assumptions need to be satisfied. First, the instrumental relevance condition needs to hold, which means that judge stringency must be a relevant predictor of the case outcome. Second, we need exogeneity of the instrument, i.e., $Z_{j(i)}$ and η_i are independent after conditioning on controls X_i . This assumption implies that judge leniency affects outcome Y_i only through the eviction order decision E_i .

If the effect of eviction is heterogeneous across individuals, we also require monotonicity in order to interpret the estimate as a local average treatment effect: i.e., those who are evicted would also be evicted by a stricter judge, and those who are not evicted would also not be evicted by a more lenient judge.

Under these assumptions, the analysis will recover the local average treatment effect (LATE) of an eviction: the effect of an eviction for tenants who could have received a different ruling had their case been assigned to a different judge (Imbens and Angrist, 1994). The effects of an eviction for this group of tenants is policy relevant, since changes in policy are likely to affect marginal cases in which the judge's discretion makes a difference in the case outcome. In addition, many recent policy proposals explicitly target the eviction court setting.

6.2 Measuring judge stringency

We estimate judge stringency by computing the yearly leave-out mean eviction rate for the initial judge assignment, and then residualizing it by district-year fixed effects. We use a residualized stringency measure to account for differences in case types across districts and changes in

regulation over time. Residualized stringency is constructed using all cases that meet the sample restrictions laid out in Section 3.3, and not just the linked sample.

The histogram in Figure 5 plots the distribution of judge stringency across cases. The figure shows that there is a substantial amount of variation in our judge stringency measure. In particular, there is a 7 percentage point difference between the 10th percentile and 90th percentile of judge leniency. Appendix Section H.2 provides additional robustness checks, showing that the first stage does not change notably when we use an alternative procedure for assigning judges to cases, when we control for additional judge characteristics, or when we adopt a different threshold for the minimum number of cases a judge must see in a given year for the case to be included in the sample.³⁵

6.3 Validating the empirical design

This section provides evidence in support of the required assumptions for our research design to identify and interpret the LATE. In particular, we validate the random assignment of cases, and the relevance, exclusion, and monotonicity assumptions underlying the IV strategy.

Random assignment. As discussed above, court cases are randomly assigned a room and hearing time when the court case is filed. Here, we provide additional empirical evidence that random assignment holds in practice. Table 4 shows that case characteristics and defendant characteristics predict the eviction outcome but do not predict the residual stringency of the judge assigned to the case. The first column presents a regression of the eviction outcome on case and defendant characteristics, and shows that all of the observed covariates are statistically significant predictors of an eviction outcome. The second column presents results from a regression of judge stringency on the same covariates, and shows that all covariates have small and statistically insignificant effects on the stringency of the judge assigned to the case. The landlord’s total number of cases in the sample is not a significant predictor of judge stringency, lending support to the idea that even experienced landlords are unable to select a favorable judge.

Relevance. Next we show that our IV regression has a strong first stage. The black line in Figure 5 shows the result of a local linear regression of an eviction order on judge stringency, while the histogram shows the underlying distribution of judge stringency in our data. The figure shows that there is a strong relationship between judge stringency and the case outcome. Appendix Table H.3 shows results for the corresponding linear regression. Stringency has a large and statistically significant impact on eviction, with a p -value of less than 0.0001. The F-statistic for the full first stage is 86.7 and the partial F-statistic on judge stringency is 1158.9, suggesting that the stringency instrument passes standard rule-of-thumb tests for weak instruments.

³⁵Appendix Section H.1 additionally shows that the IV estimates do not change substantially with alternative constructions of judge stringency, or when using judge fixed effects as instruments.

Appendix H.2 provides additional robustness checks on the first stage. In particular, Appendix Table H.4 shows that the first stage is not sensitive to the sample selection criteria, nor is it sensitive to controls for other potential dimensions of judge stringency (e.g., residual judge stringency in case length, granting continuances, judgment amount in joint action cases, and granting stays). In addition, the first stage is largely unchanged when using an alternative judge stringency measure based on the first judge observed in the case history rather than the judge assigned at filing. Lastly, we estimate the first stage using a split sample, using stringency constructed from single action cases to instrument for eviction in joint action cases, and the converse. Across all of these robustness checks, we find that the coefficient on residual stringency remains positive, similar to the main specification in magnitude, and is statistically significant with small standard errors. These checks provide evidence that our first stage is robust to additional controls, different sample-selection criteria, different construction of judge stringency, and split-sample estimation of stringency.

Exclusion. In addition to requiring random assignment, our estimation strategy requires exclusion to hold, i.e., that judge stringency only affects tenant outcomes through the eviction order. As discussed above, judges make two key decisions: the eviction order and, in joint action cases, the judgment amount. Multi-dimensional sentencing makes it more challenging to isolate the impact of the component of interest, in this case, the eviction (see, e.g., [Mueller-Smith \(2015\)](#) and [Bhuller et al. \(2019\)](#)). Aside from the two main areas of judge discretion, judges may also influence other minor aspects of the case. For instance, judges may grant a continuance to give the defendant additional time to find legal assistance. Judges may also grant a stay of the eviction order, which provides evicted tenants more time to find new housing arrangements before the sheriff is permitted to carry out the order.

We construct additional stringency measures with respect to the judgment amount, the propensity to grant a continuance, and the propensity to grant a stay of the order. Similar to the stringency measure for eviction orders, these stringency measures are calculated as leave-out means by judge-year and are residualized by district-year fixed effects.³⁶ Exclusion will be violated if eviction stringency is correlated with the other three measures of stringency, the other measures affect outcomes, and they are not directly controlled for in the analysis. Appendix Table H.5 evaluates the correlation between the four residual measures of judge stringency. The correlations between the various residual stringency measures are small. The largest correlation is between eviction stringency and judgment amount stringency, which have a correlation of 0.098. Column 1 of Appendix Table H.6 shows the first stage regression of eviction on judge eviction stringency controlling for the three other measures of judge stringency. While judgment amount stringency and continuance stringency are weakly statistically significant predictors of

³⁶Stringency for granting stays is calculated using only cases ending in an eviction order. Judgment amount stringency is defined as the judgment amount minus the ad damnum amount and is only calculated for joint action cases ending in an eviction order.

eviction, the coefficient on eviction stringency is largely unchanged from our main specification. In the second column, we regress eviction stringency on the other three stringency measures and find that none are statistically significant predictors.³⁷

As we will show in Section 7, the IV results are largely unchanged when either controlling for other dimensions of judge stringency in the first and second stage, or including judgment amount as a second endogenous variable and instrumenting with judgment amount stringency.

Monotonicity. For the IV estimates to be interpreted as local average treatment effects, we need the monotonicity assumption to hold. In our context, monotonicity requires that any defendant who is not evicted would also not be evicted by a more lenient judge and, conversely, that any defendant who is evicted would also be evicted by a more stringent judge. One test of the monotonicity assumption is that the first stage estimates should be non-negative for any subsample, e.g., by race or neighborhood income quartile. The data allow for detailed subsamples, including interactions between judge characteristics and individual characteristics.

Appendix Table H.10 presents the coefficients from a regression of eviction on residual stringency and the controls used in Appendix Table H.3, but restricted to several different subpopulations. If judge stringency is negatively related to eviction for any subpopulation, it would provide evidence that monotonicity of judge leniency does not hold. The first row shows the coefficient from the main sample, while the remaining rows show the coefficient by case type, gender, attorney status, race, and landlord size. Across all subsamples, the coefficient on stringency keeps the same sign and does not vary widely, providing evidence that monotonicity is not violated.

In Section H.4 of the Appendix, we provide additional tests of monotonicity by conditioning on both judge characteristics and defendant characteristics for a subset of judges who see the most cases in our data. For this exercise, we hand-collected additional demographic and background information for more than 150 judges. As shown in Appendix Table H.12, the coefficient is positive for all but three of these two-way interactions, which provides additional evidence that monotonicity is satisfied in this setting.³⁸

³⁷Appendix Table H.7 additionally shows results from the regression of judgment amount on eviction stringency in cases in which there is an eviction and a money judgment. Regardless of which controls are included, eviction stringency is statistically insignificant, further suggesting that a judge’s eviction stringency only affects the amount owed through the eviction ruling.

³⁸The three instances in which the coefficient is not positive, the coefficient is statistically insignificant. All three of these cases involve Hispanic judges, of which there are only 8, resulting in a substantially smaller sample. See Section H.4 of the Appendix for more details.

7 Results: IV analysis

In the regressions that follow, the dependent variable is averaged for each individual over two periods: the 13-36 month period following the eviction filing month, and the 37-60 month period following the filing month. Our results are not sensitive to alternative choices of short and long horizons.

Financial strain

Table 5 presents the main evidence on the effects of eviction on tenants’ financial strain for both the short run (panel I) and the long run (panel II). The OLS estimates presented in columns 1-3 reflect cross-sectional differences between evicted and non-evicted tenants, conditional on covariates.³⁹

Following the court case, tenants who are evicted are more distressed than those who are not evicted. The first row of column 1 shows a gap of 16.7 credit score points (or about 0.2 of a standard deviation). With additional controls in column 2, the credit score difference is cut almost in half, and the results on collections balances and having an auto loan exhibit a similar pattern. Column 3 reweights the OLS regression so that the regression sample matches the distribution of compliers based on observable characteristics.⁴⁰ Column 4 presents the reduced-form regression of the outcome on residualized stringency, and column 5 presents the main IV specification, in which we instrument for eviction using residualized stringency. The IV specification shows a modest negative effect of eviction on credit score over both the 13-36 month period (14.2 points) and the 37-60 month period (15.5 points). Both groups on average remain in the subprime category of creditworthiness following eviction court.

We next examine the effect of eviction on access to credit, as measured by the tenant having no open revolving account. The majority of tenants – 55 percent of the non-evicted group and 61 percent of the evicted group – have no open revolving line of credit in the 13-36 months following eviction. Our IV estimates show that an eviction increases the probability of having no open revolving account: 8.7 percentage points over the 13-36 month horizon, although statistically insignificant, and 14.7 percentage points over the 37-60 month horizon, and significant at the 5 percent level. This effect is large, representing a 27 percent increase from the baseline non-evicted group mean, and suggests one channel through which an eviction impacts tenants is in reducing their access to credit.

³⁹All columns include controls for case type, ad damnum amount, gender, race, a cubic in age at filing date, dummies for missing covariates, and district-year fixed effects. Columns 2-5 control for additional pre-filing measures of financial strain; these are the individual means, over their pre-filing observations of credit score, collections debt, and an indicator for having an auto loan.

⁴⁰Following Bhuller et al. (2019) and Dobbie et al. (2018), we predict the probability of eviction using our baseline controls and divide the sample into 8 subgroups based on their predicted probability, where D_g is an indicator for belonging to subgroup g . We then compute $Pr\{D_g = 1|Complier\}/Pr\{D_g = 1\}$, which are the weights used in column 3. See Appendix H.7 for more details.

Turning next to collections balances, the OLS results show evicted tenants have approximately 664 dollars more in collections debt 13-36 months after eviction, conditional on controls. The reduced form regression on stringency and the IV estimate, presented in columns 4 and 5, are \$134 and \$209, respectively, statistically insignificant and small relative to the non-evicted mean collections balances of \$3,054. The standard error on the IV estimate is fairly large, however; the 95 percent confidence interval is $[-736, 1155]$, meaning we cannot rule out a causal impact of eviction on collections debt of up to \$1155 with 95 percent confidence, which is roughly a 0.26 standard deviation increase. Nevertheless, the IV point estimate aligns with the event study depictions, and lend support to the finding of modest effects on unpaid bills.

We also study the effect of eviction on durable goods consumption, which we proxy by using an indicator for having an auto loan or lease. The IV estimate shows that an eviction causes a decline in the probability of having an open auto loan or lease by 6.0 percentage points over the 13-36 month period. This result is large in magnitude relative to the non-evicted mean of 20 percent and lends support to one of the key takeaways of the event studies, that durable consumption declines as a result of an eviction.

We next examine payday loan inquiries and openings. The OLS results in columns 1-3 reveal that evicted tenants are approximately 1.01 percentage points less likely to have a payday inquiry over the 13-36 months following the eviction filing, relative to a non-evicted group mean of 14.5 percent. This result may reflect that evicted tenants have lower demand for high interest loans following eviction, e.g., because of moving into lower-rent units, or because of changes on the supply side, due to lenders declining to lend to those with an eviction. The IV estimate for payday inquiries is statistically insignificant but the confidence interval is somewhat large; over the 13-36 month period, the confidence interval is $[-1.68, 13.15]$, meaning we can rule out an effect size of .37 of a standard deviation increase or larger with 95 percent confidence.

The OLS estimates on payday account openings show that evicted tenants are about .56 to .64 percentage points less likely to open a payday account in the 13-36 months following eviction court, from a baseline non-evicted mean of 2.34 percent (about a 25 percent decrease). Compared to the OLS estimate, the IV estimate has the opposite sign and is statistically significant at the 10 percent level, implying that an eviction causes an increase in short term high-interest borrowing of 2.92 percentage points (a 125 percent increase). The contrast with the OLS estimate highlights the importance of a causal design in the eviction setting. One plausible interpretation of this finding is that an eviction causes tenants to be cut off from traditional sources of credit (seen in the increase in the probability of having no revolving source of credit) and seek payday loans as an alternative. In the longer run, i.e., over the 37-60 month horizon, an eviction has a -11.69 percentage point effect on the probability of an inquiry, statistically significant at the 1 percent level. The effect on the probability of an account opening is statistically insignificant, although the large confidence interval prevents us from drawing strong conclusions.

Appendix Tables [H.14](#) and [H.15](#) consider heterogeneity along several key dimensions: (i) joint

action versus single action, (ii) multi-headed households versus single-headed households, (iii) those without prior cases versus those with a prior case, and (iv) those living in above median poverty neighborhoods versus those living in below median poverty neighborhoods. Somewhat surprisingly, the negative effect of eviction on credit score is stronger among single action cases compared to joint action cases; the IV estimate is -30.53 credit points relative to -7.37. This result is unexpected because in single action cases, unlike joint action cases, the eviction does not appear as a money judgment on the credit file and thus would not mechanically affect the credit score. The effects on durable goods consumption are larger for joint action cases, however, which suggests greater strain on households from losing a joint action case. This may be caused by the obligation to pay the judgment amount, or because lenders who would underwrite an auto loan can observe the money judgment on tenants' credit files. The negative consumption effects are also larger for those with a prior case, for single-headed households, and for those in higher poverty neighborhoods.

It is important to consider whether the effect sizes we measure depend on our definition of treatment. The control group in the main specification includes all dismissals. A particularly strong form of dismissal is a dismissal with prejudice, which prevents the landlord from bringing the same case with the same allegations against the tenant in the future. As a robustness check, we perform our analysis after redefining the treatment as a dismissal with prejudice, relative to all other case outcomes. The instrument is also redefined, so that residualized stringency is based on dismissals with prejudice. Appendix Table H.16 shows the re-estimated results, with the estimates changing sign due to the redefined treatment. The OLS estimates are similar in magnitude, while the IV estimates are more imprecise than our main results, likely because dismissals with prejudice are uncommon.

As a final robustness exercise, we explore the sensitivity of the IV results to the exclusion restriction. Recall that the IV regression requires an assumption that judge stringency affects the financial strain outcomes only through the order for possession. Since judges may also decide on the money judgment, there is a potential for the effect to run through the judgment amount as well. Hence, following the approach of Bhuller et al. (2019), we construct a second stringency measure based only on judgment amount, where dismissals receive a value of 0. We perform two exercises: first, we control for this second stringency measure in both first and second stages of our main IV regressions; second, we allow for two endogenous regressors (eviction order and judgment amount) and instrument for these two measures using the two stringency measures.

Appendix Table H.8, panel A, depicts the first stage with and without the second stringency measure, showing that the relationship between our main stringency measure and the eviction order is largely unchanged by including the second stringency measure as a control. Panel B.I shows the reduced form regression of our three financial strain outcomes on the eviction stringency, and then again with the second stringency measure added in. The results appear to show the main effects running through our main eviction stringency measure rather than the

judgment amount stringency. Panel B.II controls for the judgment stringency in the first and second stage, and Panel B.III shows the two instrument, two endogenous regressors approach. Both panels show our main IV estimates to be unaffected when we include the second stringency measure, based on the judgment amount. The analysis of auto loans (columns 5 and 6) clearly shows that the eviction order itself, rather than the judgment amount, is the driver of reduced consumption.⁴¹

Residential moves

Table 6 presents the evidence for the effect of eviction on residential moves, neighborhood quality, and future eviction case filings. The sample is identical to the financial strain sample. For each individual the case filing ZIP code is the reference ZIP code. We examine the effect of eviction on having a different ZIP code at 13-36 months after filing and at 37-60 months. If the individual moves between filing and 13-36 months and moves back by the 37-60 month horizon, we code this as a move by 37-60 months. We also examine the effect on any future eviction filing from 1 to 36 months after the case.

Similar to the event studies in Section 5, the OLS analysis shows that evicted tenants have a higher probability of moving to another ZIP code: 13-36 months after the case filing, they are 4.2 percentage points more likely to move ZIP codes (columns 1-3) compared to the non-evicted group, for which 57.8 percent moved ZIP codes. The LATE point estimate implies that an eviction increases the probability of moving ZIP codes by 8.4 percentage points, but it is statistically indistinguishable from 0, hence we cannot rule out that an eviction has no effect on moving to a different neighborhood. The 95-percent confidence interval is quite large and we cannot rule out that eviction increases the move rate by 21.9 percentage points or decreases the move rate by 5.1 percentage points. For the 37-60 month horizon, the OLS results are very similar while the IV results are small, negative, and statistically insignificant, with a short run effect of -1.6 percentage points off a base of 73.2 percent for the control group. Nevertheless, the point estimates for both the short and long run are aligned with the event study, which shows high move rates for both evicted and non-evicted tenants surrounding the eviction, and relatively small differences between evicted and non-evicted tenants over time.

The OLS estimates also show that evicted tenants live in higher poverty neighborhoods relative to non-evicted tenants, and this is true both at 13-36 months and 37-60 months after filing. These OLS results line up with [Desmond and Shollenberger \(2015\)](#), who study survey data collected in Milwaukee. They find that renters who experienced a forced move (including eviction) relocate to poorer neighborhoods, as compared to those who move voluntarily. However,

⁴¹The IV results provide an estimate of the effect of eviction for those whose cases could have had different outcomes if assigned a different judge. Appendix Section I provides estimates of the marginal treatment effect of eviction for outcomes related to financial strain. Following [Brinch et al. \(2017\)](#), we find that the effects of eviction are somewhat larger for those with unobservables that make them more likely to be evicted, though the overall heterogeneity across latent resistance to treatment is limited.

the IV estimate of the effect of an eviction on tenants’ neighborhood poverty rate 13-36 months after filing is -2.03 and statistically significant at the 10 percent level. The point estimate at 37-60 months is similar in magnitude but no longer statistically significant.

Using the credit bureau data, we can follow individuals across eviction cases, despite many of the defendants changing names due to marriage, divorce, or having a first name that exhibits slight variations across cases (e.g., “Jim Smith” versus “James Smith”). The last two rows of each panel of Table 6 examines whether being evicted has a causal effect on a future eviction filing. Being evicted has a large negative causal effect on having an eviction filing in the next 3 years from any address (-16.3 percentage points off a base of 29.2). This result is driven by the fact that non-evicted tenants are more likely to face an eviction case at the same address in the future. The last row of each panel shows the effect of eviction on the probability of a future eviction filing from a different address, and shows no statistically significant effect and a near-zero point estimate. Taken together, these results provide some evidence against the idea that eviction contributes to a “slipperiness” of tenants’ housing situation through an increase in future filings.⁴²

As a robustness check to the ZIP code level move results, Appendix J provides an alternative analysis using InfoUSA address histories. This alternative data allows us to consider unit-level moves, but suffers from high levels of missingness and a lower match rate. Using this data, the OLS results show that an eviction is associated with a 2 percentage point increase in the probability of being observed at a new address, an 8 percentage point reduction in the probability of being observed at the same address, and a 6 percentage point increase in having no updated information on address of residence in the two years after the case is filed. The IV estimates with this data are imprecise with no statistically significant effects.

Using the InfoUSA data, we document two additional important facts. First, 27 percent of observed moves for the matched sample are within ZIP code, suggesting that the analysis above using moves across ZIP code are an underestimate of unit-level mobility. Second, the matched sample is 37 percent less likely to have an updated address in any given year compared to a random sample from Cook County, highlighting the difficulty of estimating the mobility of individuals at risk of eviction.

8 Conclusion

Using seventeen years of linked Cook County court records, this paper uses DiD and IV designs to study the causal effect of eviction on financial distress, residential mobility, and neighborhood quality. The paper draws two broad conclusions.

First, we find that eviction negatively impacts credit access and durable consumption for

⁴²Note that if eviction makes it harder for tenants to get a new lease, tenants may be less likely to be the signatory on a new lease, which may put downward pressure on the probability of a future eviction for the evicted group.

several years. However, when we consider the magnitude of these effects in the context of the financial strain experienced by both evicted and non-evicted tenants in the years preceding an eviction case, the effects are small. In addition, we do not find evidence of a causal impact on debt in collections, residential mobility, or neighborhood poverty, and find a small impact on credit score.

Second, bias due to selection on levels and trends, if ignored, leads to the erroneous conclusion that eviction has large impacts on financial distress. Using an additional panel of credit records for a random sample of Cook County residents, we replicate analyses from existing studies, showing that comparisons of evicted tenants to tenants not in eviction court (controlling for observable characteristics) imply large effects attributed to eviction. In contrast, when we limit the sample to tenants in eviction court, OLS regressions comparing evicted tenants to non-evicted tenants produce much smaller estimates.

This paper provides estimates of how an eviction affects financial distress and residential mobility, but several important questions remain for future research. First, this analysis does not address how policies aimed at reducing the number of evictions, such as making court proceedings more tenant-friendly, may affect the equilibrium in the rental market. For example, landlords may be less willing to rent to low income tenants. Second, we do not provide estimates of the welfare impacts of an eviction on tenants, but rather the effects for a specific subset of observable outcomes. Finally, we cannot directly speak to the effectiveness of policies targeting populations at risk of eviction, such as emergency relief funds, or assistance programs for recently evicted tenants.

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Table 1: Summary statistics: IV sample

	Evicted		Not Evicted		Random sample	
<i>Person characteristics</i>						
Age at case	39.990	(12.659)	40.251	(12.726)	45.713	(17.116)
Female	0.641	(0.480)	0.638	(0.481)	0.501	(0.500)
Black	0.517	(0.500)	0.480	(0.500)		
<i>Case Characteristics</i>						
Eviction order	1.000	(0.000)	0.000	(0.000)		
Ad Damnum Amount (1000s)	2.629	(3.996)	2.333	(4.151)		
Joint Action	0.842	(0.365)	0.805	(0.396)		
Tenant Pro Se	0.970	(0.170)	0.945	(0.228)		
Landlord Pro Se	0.247	(0.431)	0.239	(0.426)		
<i>Neighborhood Characteristics</i>						
Median household inc. (1000s)	47.622	(17.901)	50.028	(19.171)	48.162	(18.012)
Poverty Rate	18.418	(9.782)	17.447	(9.788)	18.176	(9.762)
Median Rent	965.304	(174.219)	987.655	(197.148)	971.870	(182.106)
Pct. White	0.357	(0.291)	0.389	(0.294)	0.365	(0.292)
Pct. Black	0.485	(0.376)	0.448	(0.371)	0.476	(0.374)
<i>Subsequent Outcomes (13-36 mo.)</i>						
Credit Score	528.307	(67.987)	546.962	(80.024)	620.700	(106.213)
No Open Revolving Account	0.625	(0.480)	0.553	(0.493)	0.409	(0.489)
Total bal. collections	3,789.295	(4,780.772)	3,053.894	(4,391.669)	1,164.431	(2,886.026)
Any Auto Loan or Lease	0.140	(0.343)	0.199	(0.396)	0.125	(0.329)
Any Payday Inquiry \times 100	13.609	(34.289)	14.536	(35.247)	7.441	(26.244)
Any Payday Account \times 100	1.676	(12.836)	2.343	(15.126)	1.212	(10.940)
<i>Subsequent Eviction Cases</i>						
Any Eviction Case (36 mo.)	0.205	(0.404)	0.289	(0.453)		
Eviction Case at Dif. Address (36 mo.)	0.168	(0.374)	0.174	(0.379)		

Notes: The table above presents means and standard deviations (in parentheses) of key variables in our linked credit bureau sample used in the IV analysis. The random sample is restricted to those over age 21 with no open mortgage trade, and has been reweighted to match the distribution of individuals across neighborhoods in the eviction sample. We randomly assign a placebo eviction date to the random sample to compare financial outcomes. “Tenant Pro Se” is an indicator for the tenant having no formal legal representation. “Landlord Pro Se” is an indicator for the landlord having no formal legal representation. See notes in the text for the sample restrictions to the eviction court sample. Race is imputed using last name and Census tract (Imai and Khanna, 2016; Khanna et al., 2017), but is unavailable for the random sample.

Table 2: DiD estimates: credit bureau outcomes

	Credit score	Total Collections	Any Auto Loan	No Revolving Credit
	(1)	(2)	(3)	(4)
12-Month Effect	-2.828*** (0.369)	191.218*** (22.971)	-0.012*** (0.002)	0.014*** (0.002)
36-Month Effect	-2.175*** (0.386)	157.990*** (27.426)	-0.013*** (0.002)	0.015*** (0.003)
60-Month Effect	-2.133*** (0.451)	-0.982 (31.355)	-0.003 (0.003)	0.009*** (0.003)
Baseline non-evict mean	552.23	2,674.78	0.18	0.49
Number of individuals	251,036	252,718	254,578	254,578
Number of observations	1,302,930	1,310,057	1,320,322	1,320,322

Notes: The table above presents DiD estimates of the polynomials in Figure 6, at different time horizons relative to $r = -12$. The regression is $y_{it} = \gamma_t + \beta_1 r + \beta_2 r^2 + \beta_3 r^3 + \beta_4 r\{r > 0\} + \beta_5 r^2\{r > 0\} + \beta_6 r^3\{r > 0\} + \delta_0 E + \delta_1 E \times r + \delta_2 E \times r^2 + \delta_3 E \times r^3 + \delta_4 E \times r\{r > 0\} + \delta_5 E \times r^2\{r > 0\} + \delta_6 E \times r^3\{r > 0\} + \epsilon_{it}$. The table includes standard errors of the DiD estimates, which are clustered at the individual level.

Table 3: DiD estimates: moves and neighborhood quality

	Move Zipcode in Past 24 months	Neighborhood Poverty Rate
	(1)	(2)
12-Month Effect	0.004 (0.005)	0.202*** (0.066)
36-Month Effect	0.003 (0.006)	0.127 (0.085)
60-Month Effect	0.000 (0.006)	0.090 (0.103)
Baseline non-evict mean	0.43	17.43
Number of individuals	115,023	115,022
Number of observations	770,472	790,598

Notes: This table presents DiD estimates of the regressions underlying Figure 8, at different time horizons relative to the year prior to eviction filing. The table includes standard errors clustered at the individual level.

Table 4: Random assignment of judges

	(1) Evicted	(2) Judge Stringency
Landlord number of cases	-0.00262*** (0.00045)	0.00003 (0.00002)
Joint Action	0.04533*** (0.00700)	-0.00011 (0.00031)
Ad Damnum Amount (1000s)	0.00364*** (0.00037)	0.00002 (0.00002)
Age at case	-0.00850*** (0.00147)	0.00005 (0.00009)
Age ² /1000	0.16616*** (0.03059)	-0.00076 (0.00174)
Age ³ /1000	-0.00102*** (0.00020)	0.00000 (0.00001)
Female	-0.00485** (0.00212)	-0.00004 (0.00013)
Black	0.02680*** (0.00227)	-0.00003 (0.00011)
Missing female	-0.01549*** (0.00527)	-0.00012 (0.00033)
Missing age	-0.18362*** (0.02413)	0.00064 (0.00138)
Number of observations	232,834	232,834
Joint F-Test Stat.	41.843	0.468
p-value	0.000	0.879

Notes: The left column shows results for a regression of eviction status on case and defendant characteristics. The right column shows results for a regression of residual stringency on case and defendant characteristics. “Landlord number of cases” is the number of eviction cases in which the landlord served as plaintiff in the sample. “Joint Action” is an indicator for if the case was a joint action case seeking an eviction order and a money judgment rather than a single action case seeking only a money judgment. “Ad Damnum Amount” is the amount the landlord listed as owed by the defendant at the time of filing. The “Black” indicator is based on a Bayesian prediction of race based on last name and census tract. Both columns use the linked IV sample and include district-year fixed effects.

Table 5: The effect of eviction on financial strain

	Non-evicted mean	OLS: Evicted			RF: Stringency	IV: Evicted
		(1)	(2)	(3)	(4)	(5)
I. Financial Strain: 13-36 mon.						
Credit Score	546.964 (80.025)	-16.706*** (0.426)	-8.821*** (0.378)	-8.715*** (0.369)	-9.067* (4.822)	-14.151* (7.459)
No Open Revolving Account	0.553 (0.493)	0.064*** (0.003)	0.038*** (0.003)	0.037*** (0.003)	0.056 (0.035)	0.087 (0.054)
Total bal. collections	3,054.056 (4,391.713)	664.343*** (24.453)	444.512*** (22.989)	442.562*** (23.345)	133.910 (308.343)	209.391 (482.336)
Any Auto Loan or Lease	0.199 (0.396)	-0.060*** (0.002)	-0.040*** (0.002)	-0.040*** (0.002)	-0.039* (0.022)	-0.060* (0.035)
Any Payday Inquiry \times 100	14.513 (35.224)	-1.009*** (0.204)	-1.246*** (0.202)	-1.253*** (0.188)	3.669 (2.421)	5.738 (3.784)
Any Payday Account \times 100	2.344 (15.129)	-0.641*** (0.074)	-0.605*** (0.072)	-0.562*** (0.069)	1.890* (0.976)	2.915* (1.523)
II. Financial Strain: 37-60 mon.						
Credit Score	555.935 (81.929)	-15.898*** (0.455)	-7.772*** (0.410)	-7.773*** (0.402)	-9.923* (5.172)	-15.460* (8.105)
No Open Revolving Account	0.541 (0.494)	0.056*** (0.003)	0.034*** (0.002)	0.034*** (0.002)	0.094** (0.038)	0.147** (0.060)
Total bal. collections	2,965.622 (4,436.617)	534.060*** (27.958)	381.941*** (26.124)	374.248*** (25.832)	-126.018 (365.032)	-196.011 (566.201)
Any Auto Loan or Lease	0.199 (0.395)	-0.054*** (0.002)	-0.040*** (0.002)	-0.039*** (0.002)	-0.056* (0.030)	-0.087* (0.047)
Any Payday Inquiry \times 100	13.176 (33.824)	-0.425** (0.171)	-0.739*** (0.170)	-0.762*** (0.157)	-7.399*** (2.650)	-11.687*** (4.178)
Any Payday Account \times 100	2.374 (15.225)	-0.475*** (0.068)	-0.436*** (0.067)	-0.419*** (0.063)	-0.223 (0.971)	-0.343 (1.493)
Additional controls			Yes	Yes	Yes	Yes
Complier re-weighted				Yes		

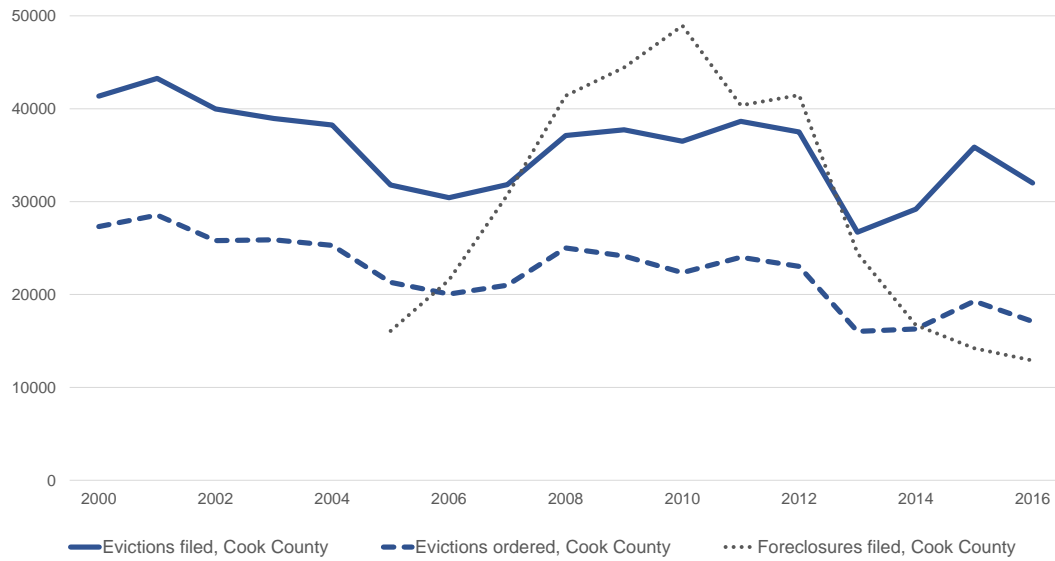
Notes: The table above reports OLS and two-stage least squares results of the impact of eviction on measures of financial strain at 13-36 months after filing (panel I) and 37-60 months after filing (panel II). Columns 1-3 present OLS estimates of the financial strain outcome on an indicator for eviction. Column 4 presents an OLS regression on stringency, which is the judge-year leave-out mean eviction rate after residualizing of district-year fixed effects. Column 5 presents the two-stage least squares regressions, instrumenting for eviction with stringency. The dependent variable is listed in each row. All specifications control for district-year fixed effects, case type, ad damnum amount, a cubic in age at case, and indicators for the tenant being female, black, and for missing covariates. Columns with additional controls include pre-filing averages for credit score, collections balance, and auto loan or lease, and indicators for these pre-filing covariates being missing. Robust standard errors are clustered at the judge-year level.

Table 6: The effect of eviction on moves and neighborhood quality

	Non-evicted mean	OLS: Evicted			RF: Stringency	IV: Evicted
		(1)	(2)	(3)	(4)	(5)
I. Outcomes 13-36 months after filing						
Move Zipcode	0.578 (0.494)	0.042*** (0.002)	0.043*** (0.002)	0.043*** (0.002)	0.052 (0.043)	0.084 (0.069)
Neighborhood poverty rate ($\times 100$)	16.958 (10.077)	0.821*** (0.054)	0.421*** (0.054)	0.434*** (0.054)	-1.252* (0.656)	-2.027* (1.062)
II. Outcomes 37-60 months after filing						
Move Zipcode	0.732 (0.443)	0.043*** (0.002)	0.040*** (0.002)	0.039*** (0.002)	-0.010 (0.036)	-0.016 (0.058)
Neighborhood poverty rate ($\times 100$)	16.674 (10.119)	0.866*** (0.068)	0.469*** (0.068)	0.472*** (0.068)	-1.052 (0.760)	-1.683 (1.213)
III. Future Eviction Cases						
Any Eviction Case (36 mo.)	0.292 (0.455)	-0.090*** (0.003)	-0.099*** (0.003)	-0.100*** (0.003)	-0.102*** (0.035)	-0.163*** (0.055)
Eviction Case at Dif. Address (36 mo.)	0.175 (0.380)	-0.008*** (0.003)	-0.017*** (0.003)	-0.017*** (0.003)	0.004 (0.029)	0.007 (0.046)
Additional controls			Yes	Yes	Yes	Yes
Complier re-weighted				Yes		

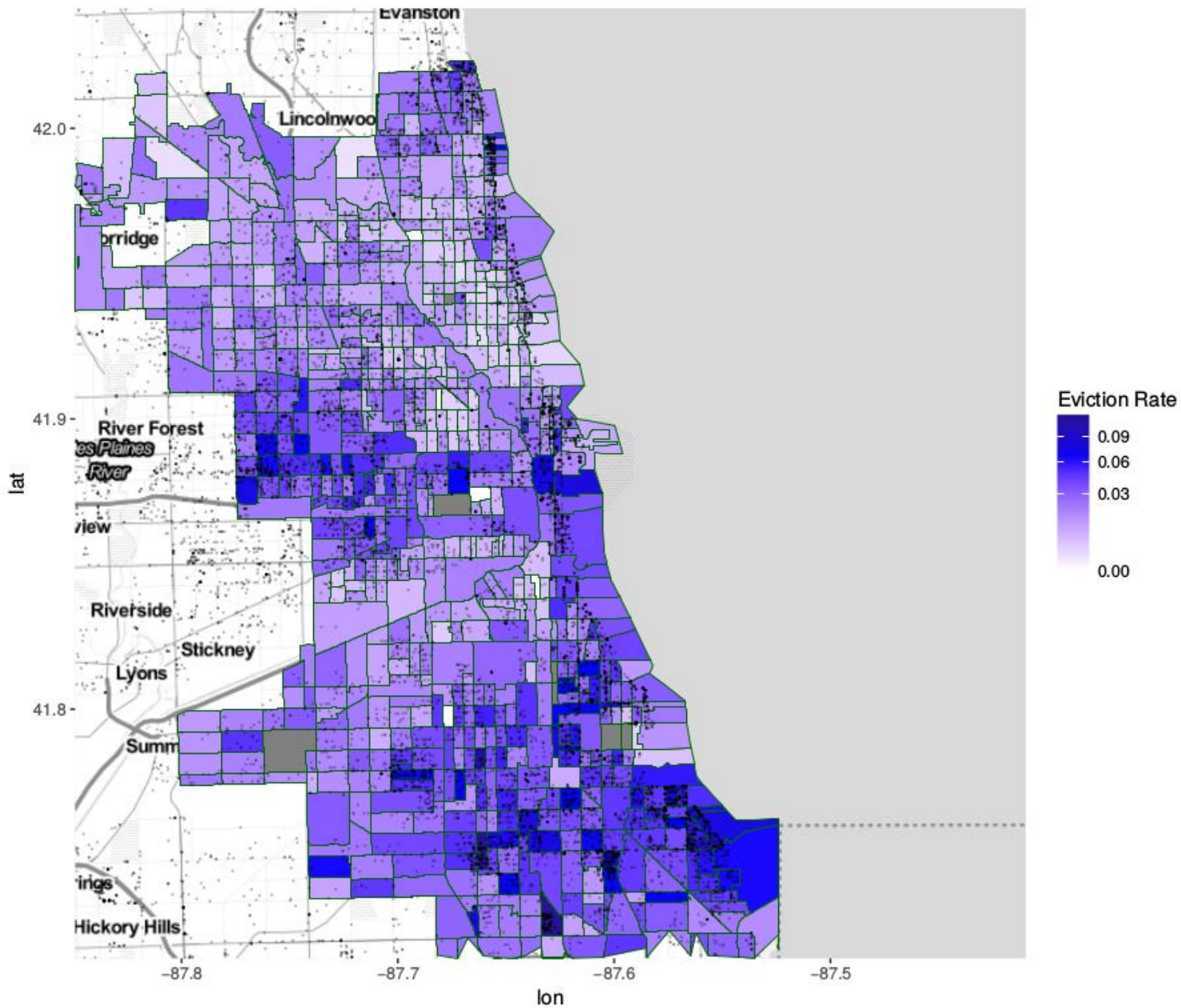
Notes: The table above presents OLS and two-stage least squares regressions of the effect of eviction on the probability of moving ZIP codes between the filing month and the outcome period. It also estimates the effect of eviction on the ZCTA-level poverty rate and on the probability of a future eviction filing. See Table 5 for details on the regression specification of each column and the controls. Robust standard errors are clustered at the judge-year level.

Figure 1: Evictions in Cook County, 2000-2016



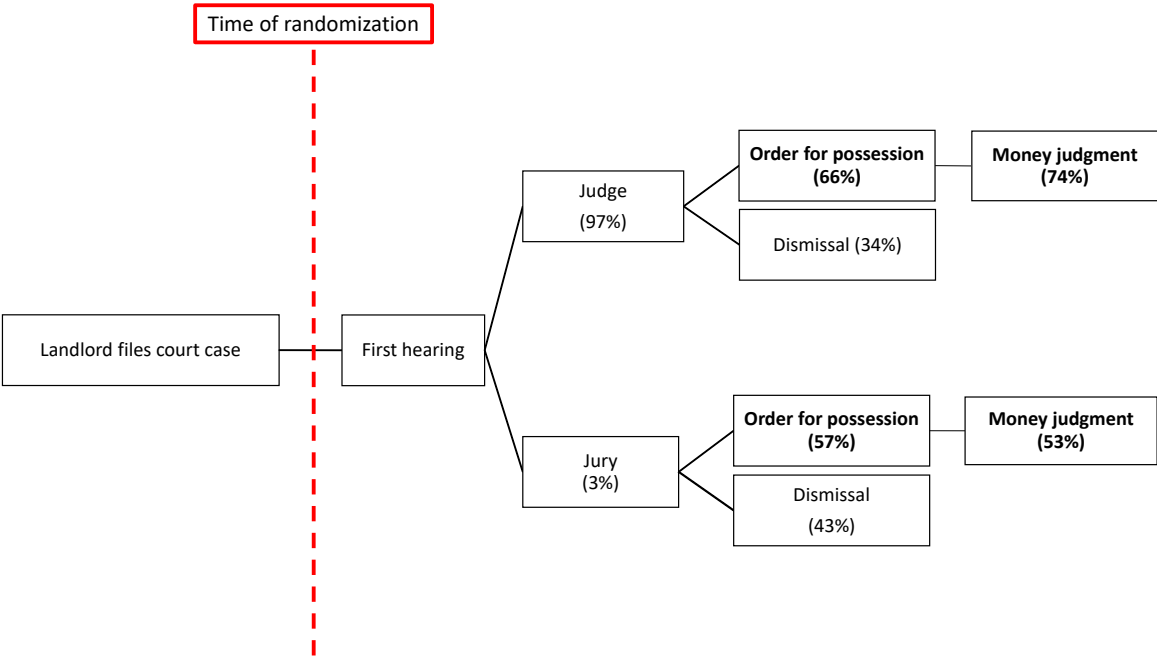
Notes: This figure depicts yearly counts of evictions filed and ordered in Cook County, IL. For comparison, it also depicts the number of foreclosure filings in Cook County, IL. Data on foreclosures is obtained from the data portal maintained by the Institute for Housing Studies at DePaul University ([IHS, 2018](#)).

Figure 2: Eviction rate in 2010



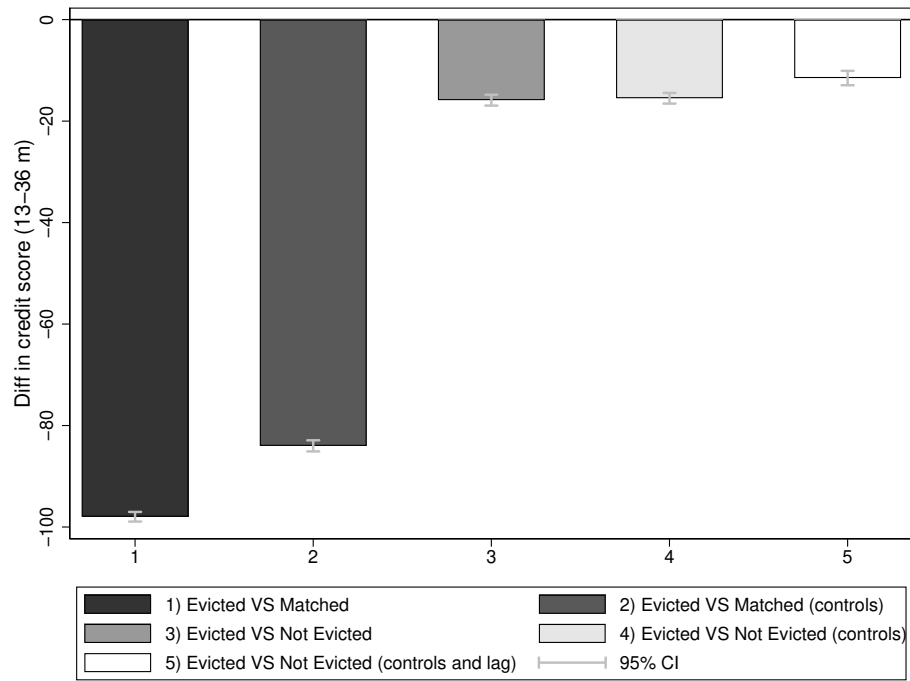
Notes: This figure depicts the locations of properties for which the court ordered an eviction in Chicago (indicated as dots), along with the rate of evictions by census tract (indicated by the shaded regions). The rate is defined as the number of evictions divided by the number of occupied rental units in the census tract (based on 2006-2010 American Community Survey 5-Year Data). Estimates exclude evictions of businesses and other non-residential evictions. In 2010, the eviction rate from occupied rental units was approximately 2.46 percent. This estimate uses occupied rental units as the denominator, which may omit houses that could be, or were previously, rented. Using *all* housing units in the census tract, we find an eviction rate of 0.93 percent, which provides a conservative estimate. There is substantial heterogeneity across tracts, with 9 tracts having eviction rates above 10 percent.

Figure 3: The eviction court process in Cook County



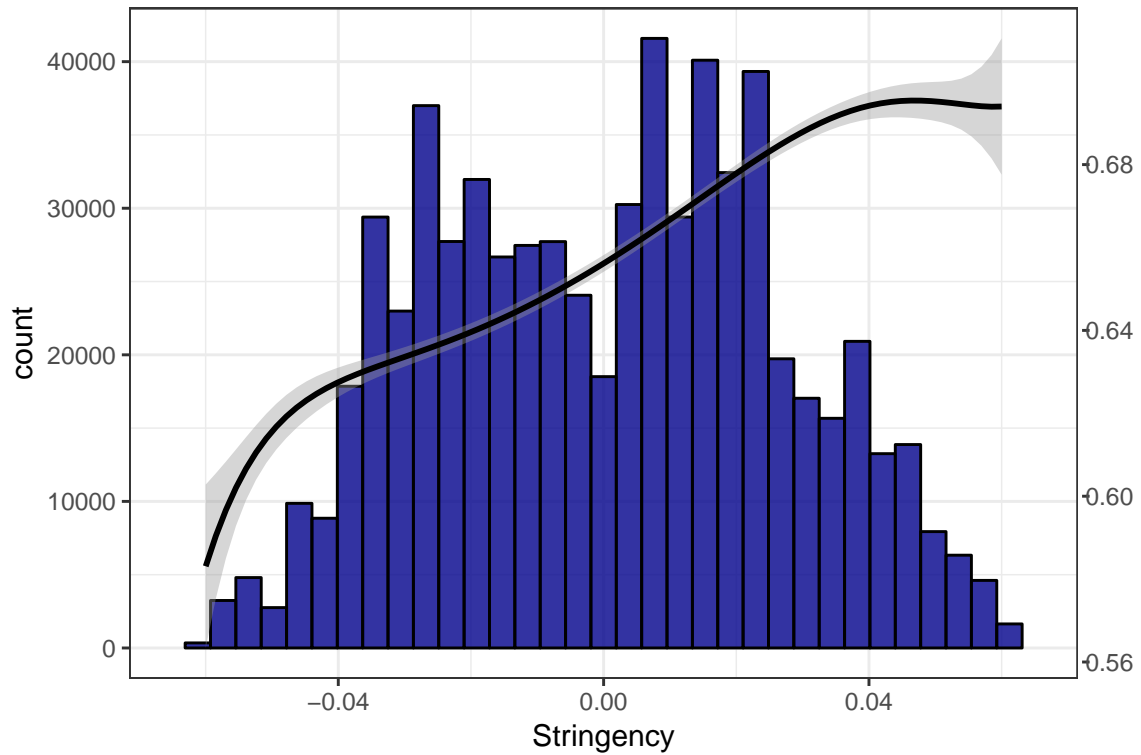
Notes: This figure depicts the possible paths eviction cases can take through the court system. Percentages are calculated for our baseline analysis sample.

Figure 4: Selection into eviction court



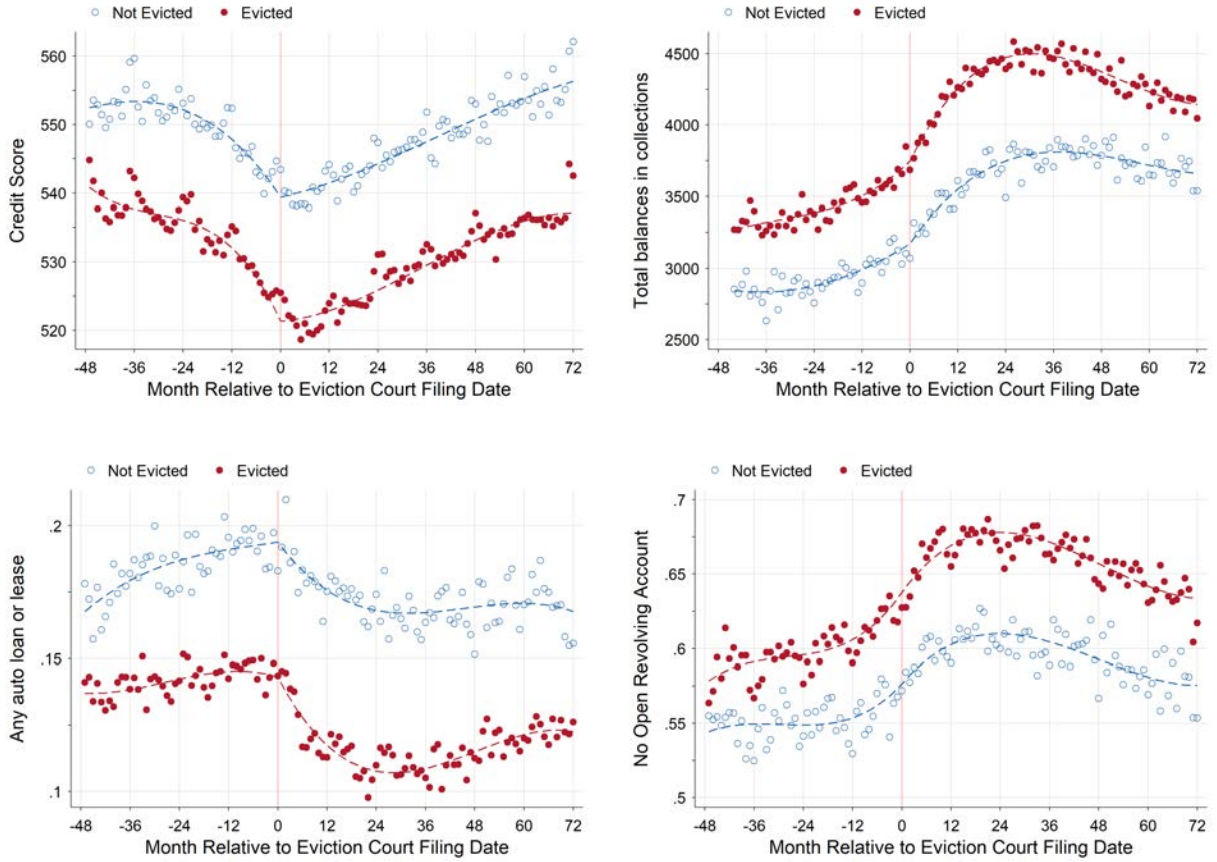
Notes: Column 1 plots the difference in credit score at 13-36 months after filing, for the court sample versus the random sample (for which the filing date is assigned at random). Column 2 is reproduces column 1 with demographic controls (age, gender, and year). Column 3 plots the difference in credit score at 13-36 months after filing, for evicted versus non-evicted in the court sample. Column 4 reproduces column 3 with demographic controls. Column 5 reproduces column 4 with an additional control for individual mean credit score over the pre-filing period. See Appendix Figure F.1 for a similar analysis of other outcomes.

Figure 5: Judge stringency



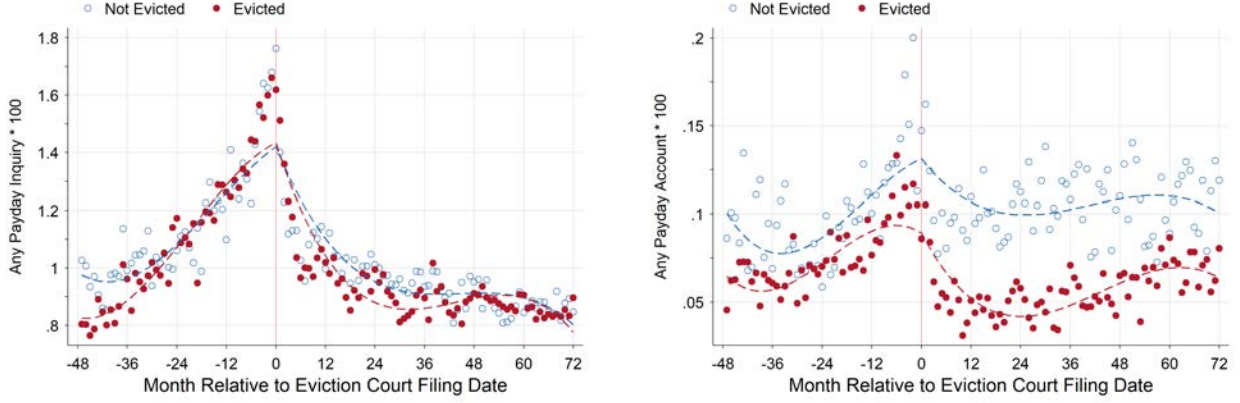
Notes: The figure above graphically depicts the first stage of the main estimation equation, showing how the probability of eviction is affected by judge stringency. The histogram shows the density of year-specific judge stringency for judges who see at least 10 cases per year, and is plotted along the left y-axis. The solid line plots estimates of the first stage regression with eviction as the dependent variable, a local linear polynomial in judge stringency, and district-year fixed effects. The plotted values are fitted values of eviction rate at the value of judge stringency indicated on the x-axis and probability of eviction plotted along the right y-axis. Shaded area shows the 95 percent confidence intervals.

Figure 6: Evolution of financial strain relative to the eviction filing month



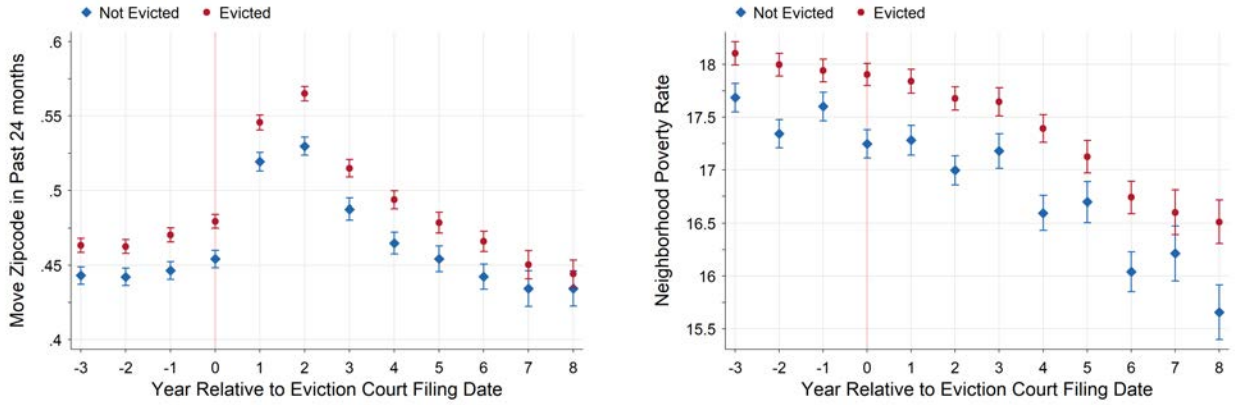
Notes: The figure plots estimates of $\{\beta_r\}$ and $\{\delta + \delta_r + \beta_r\}$ from the regression: $y_{it} = \gamma_t + \delta \times E_i + \sum_{r=S}^F \beta_r + \sum_{r=S}^F \delta_r \times E_i + \epsilon_{it}$. The omitted month is -48. Overlaid is a parametric specification where the right hand side variables include a cubic in relative month in the months leading up to eviction filing ($r < 0$), a cubic in relative month for the months following eviction filing ($r \geq 0$), and these two cubics interacted with eviction case outcome.

Figure 7: Payday loans



Notes: The left panel shows the probability of an individual making a loan inquiry in a given month. The right panel shows the probability of an individual successfully opening a new loan in a given month. The figure is constructed as in Figure 6.

Figure 8: Residential moves relative to the filing year



Notes: The figure depicts results from the regression: $y_{it} = \gamma_t + \delta \times E_i + \sum_{r=S}^F \beta_r + \sum_{r=S}^F \delta_r \times E_i + \epsilon_{it}$, where r is measured in *years* relative to the eviction filing year, and where y_{it} is an indicator for having moved in the past 24 months. The omitted years are -5 and -4. In addition to the sample criteria of Figure 6, we require the individual be observed in the credit bureau sample 3 years prior to the eviction case and in all subsequent sample credit bureau sample years, with non-missing 5 digit ZIP codes in each year. We drop the 2010 sample year for ease of interpretation. Estimates are presented with 95 percent confidence intervals.