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

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July 21, 2019

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.

Keywords: evictions, financial distress, poverty
JEL codes: J01, H00, R38, I30

*First version: March 2018. The authors gratefully acknowledge financial support from the National Science Foundation (SES-1757112, SES-1757186, SES-1757187), the Laura and John Arnold Foundation, the Spencer Foundation, and the Kreisman Initiative on Housing Law and Policy. We would like to thank Lawrence Wood and others at the Legal Assistance Foundation in Chicago, Melissa C. Chiu at the U.S. Census Bureau, Leah Gjertson and Robert Goerge at Chapin Hall, Lydia Stazen Michael at All Chicago, Carmelo Barbaro, Ruth Coffman and Emily Kristine Metz at UChicago Urban Labs, Joe Altonji, Eric Chyn, Angela Denis Pagliero, Michael Dinerstein, Bill Evans, Alex Frankel, Pieter Gautier, Matt Gentzkow, Michael Greenstone, Jim Heckman, Ali Hortaçsu, Peter Hull, Louis Kaplow, Ezra Karger, Paymon Khorrami, Thibaut Lamadon, Jeff Lin, Maarten Lindeboom, Hamish Low, Sarah Miller, Magne Mogstad, Derek Neal, Matt Notowidigdo, Ed Olsen, Azeem Shaikh, Beth Shinn, Jeff Smith, Jim Sullivan, Chris Taber, Alex Torgovitsky, Bas van der Klaauw, Laura Wherry, and seminar participants at the Summer 2017 University of Chicago Crossing Disciplinary Boundaries workshop, the 2017 Fall APPAM conference, the University of Chicago, SOLE, the Philadelphia Fed, the 2018 NBER Summer Institute, the University of Oslo, ALEA, the Institute for Research on Poverty Summer Workshop, Nebraska Law, and the University of Alicante for helpful discussion. Iliana Cabral, Ella Deeken, Deniz Dutz, and Katherine Kwok provided excellent research assistance. Any errors are our own.

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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

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

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

[3]Our 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, Rojas and Stenberg), 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

[4]Several 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).

[5]Several 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).

[6]There 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 cyclicality 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

[7] Appendix 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).

[8] Following 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.

[9] See 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

[10] If 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.

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

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

[13] Although 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.

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[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]

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

[17]An 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.

[18] In 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.

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

[20] The 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

[21] Collections 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 is it automatically removed from the report.

[22] A 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.

[23] The 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.

[24] Evictions 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.

[25] From 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

[26] For 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.

[27] Up 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

[28]Note 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.

[29] For 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.

[30] Appendix 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:

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

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

[31] There 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

[32] Appendix 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.

[33] These 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 $r = -5, -3, \ldots, 5, 7$ or $r = -4, -2, \ldots, 6, 8$.[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

[34] We 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:

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

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 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.

[35] Appendix 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

[36] Stringency 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]

[37] Appendix 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.

[38] The 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.

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[39] All 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.

[40] Following 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,

[41]The 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

[42]Note 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	
Person characteristics						
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)		
Case Characteristics						
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)		
Neighborhood Characteristics						
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)
Subsequent Outcomes (13-36 mo.)						
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)
Subsequent Eviction Cases						
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***	191.218***	-0.012***	0.014***
	(0.369)	(22.971)	(0.002)	(0.002)
36-Month Effect	-2.175***	157.990***	-0.013***	0.015***
	(0.386)	(27.426)	(0.002)	(0.003)
60-Month Effect	-2.133***	-0.982	-0.003	0.009***
	(0.451)	(31.355)	(0.003)	(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	0.202***
	(0.005)	(0.066)
36-Month Effect	0.003	0.127
	(0.006)	(0.085)
60-Month Effect	0.000	0.090
	(0.006)	(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***	0.00003
	(0.00045)	(0.00002)
Joint Action	0.04533***	-0.00011
	(0.00700)	(0.00031)
Ad Damnum Amount (1000s)	0.00364***	0.00002
	(0.00037)	(0.00002)
Age at case	-0.00850***	0.00005
	(0.00147)	(0.00009)
$Age^2/1000$	0.16616***	-0.00076
	(0.03059)	(0.00174)
$Age^3/1000$	-0.00102***	0.00000
	(0.00020)	(0.00001)
Female	-0.00485**	-0.00004
	(0.00212)	(0.00013)
Black	0.02680***	-0.00003
	(0.00227)	(0.00011)
Missing female	-0.01549***	-0.00012
	(0.00527)	(0.00033)
Missing age	-0.18362***	0.00064
	(0.02413)	(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	-16.706***	-8.821***	-8.715***	-9.067*	-14.151*
	(80.025)	(0.426)	(0.378)	(0.369)	(4.822)	(7.459)
No Open Revolving Account	0.553	0.064***	0.038***	0.037***	0.056	0.087
	(0.493)	(0.003)	(0.003)	(0.003)	(0.035)	(0.054)
Total bal. collections	3,054.056	664.343***	444.512***	442.562***	133.910	209.391
	(4,391.713)	(24.453)	(22.989)	(23.345)	(308.343)	(482.336)
Any Auto Loan or Lease	0.199	-0.060***	-0.040***	-0.040***	-0.039*	-0.060*
	(0.396)	(0.002)	(0.002)	(0.002)	(0.022)	(0.035)
Any Payday Inquiry × 100	14.513	-1.009***	-1.246***	-1.253***	3.669	5.738
	(35.224)	(0.204)	(0.202)	(0.188)	(2.421)	(3.784)
Any Payday Account × 100	2.344	-0.641***	-0.605***	-0.562***	1.890*	2.915*
	(15.129)	(0.074)	(0.072)	(0.069)	(0.976)	(1.523)
II. *Financial Strain: 37-60 mon.*						
Credit Score	555.935	-15.898***	-7.772***	-7.773***	-9.923*	-15.460*
	(81.929)	(0.455)	(0.410)	(0.402)	(5.172)	(8.105)
No Open Revolving Account	0.541	0.056***	0.034***	0.034***	0.094**	0.147**
	(0.494)	(0.003)	(0.002)	(0.002)	(0.038)	(0.060)
Total bal. collections	2,965.622	534.060***	381.941***	374.248***	-126.018	-196.011
	(4,436.617)	(27.958)	(26.124)	(25.832)	(365.032)	(566.201)
Any Auto Loan or Lease	0.199	-0.054***	-0.040***	-0.039***	-0.056*	-0.087*
	(0.395)	(0.002)	(0.002)	(0.002)	(0.030)	(0.047)
Any Payday Inquiry × 100	13.176	-0.425**	-0.739***	-0.762***	-7.399***	-11.687***
	(33.824)	(0.171)	(0.170)	(0.157)	(2.650)	(4.178)
Any Payday Account × 100	2.374	-0.475***	-0.436***	-0.419***	-0.223	-0.343
	(15.225)	(0.068)	(0.067)	(0.063)	(0.971)	(1.493)
Additional controls			Yes	Yes	Yes	Yes
Complier re-weighted				Yes		

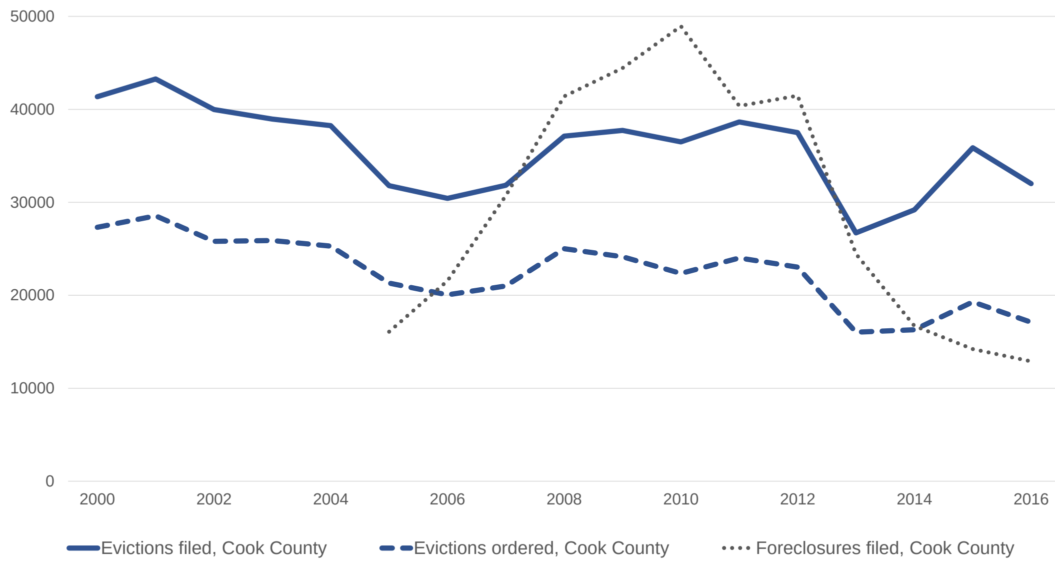
Notes: The table above reports OLS and two-stage least squares results of the impact of eviction on measures of financial strain at 13-36 months after filing (panel I) and 37-60 months after filing (panel II). Columns 1-3 present OLS estimates of the financial strain outcome on an indicator for eviction. 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	0.042***	0.043***	0.043***	0.052	0.084
	(0.494)	(0.002)	(0.002)	(0.002)	(0.043)	(0.069)
Neighborhood poverty rate ($\times$100)	16.958	0.821***	0.421***	0.434***	-1.252*	-2.027*
	(10.077)	(0.054)	(0.054)	(0.054)	(0.656)	(1.062)
II. *Outcomes 37-60 months after filing*						
Move Zipcode	0.732	0.043***	0.040***	0.039***	-0.010	-0.016
	(0.443)	(0.002)	(0.002)	(0.002)	(0.036)	(0.058)
Neighborhood poverty rate ($\times$100)	16.674	0.866***	0.469***	0.472***	-1.052	-1.683
	(10.119)	(0.068)	(0.068)	(0.068)	(0.760)	(1.213)
III. *Future Eviction Cases*						
Any Eviction Case (36 mo.)	0.292	-0.090***	-0.099***	-0.100***	-0.102***	-0.163***
	(0.455)	(0.003)	(0.003)	(0.003)	(0.035)	(0.055)
Eviction Case at Dif. Address (36 mo.)	0.175	-0.008***	-0.017***	-0.017***	0.004	0.007
	(0.380)	(0.003)	(0.003)	(0.003)	(0.029)	(0.046)
Additional controls			Yes	Yes	Yes	Yes
Complier re-weighted				Yes		

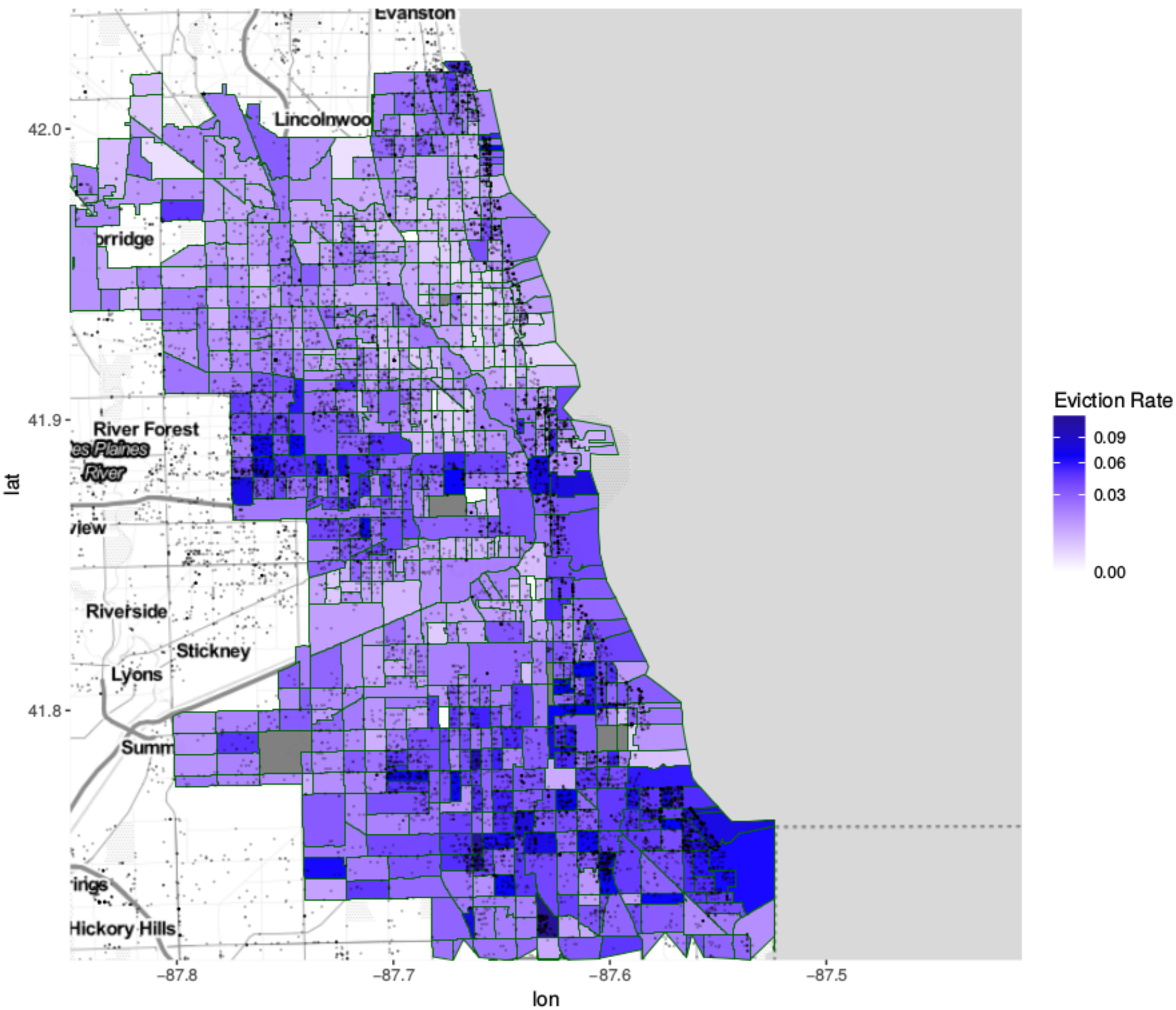
Notes: The table above presents OLS and two-stage least squares regressions of the effect of eviction on the probability of moving ZIP codes between the filing month and the outcome period. It also estimates the effect of eviction on the ZCTA-level poverty rate and on the probability of a future eviction 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



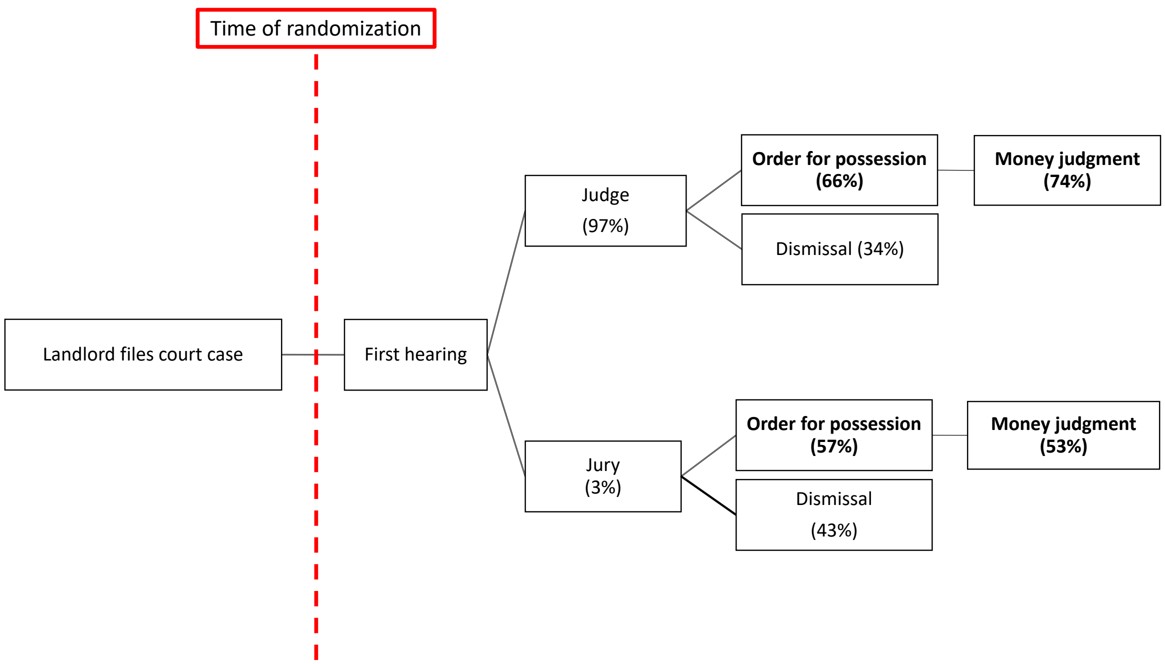
Notes: This figure depicts yearly counts of evictions filed and ordered in Cook County, IL. For comparison, it also depicts the number of foreclosure filings in Cook County, IL. Data on foreclosures is obtained from the data portal maintained by the Institute for Housing Studies at DePaul University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

Figure 2: Eviction rate in 2010



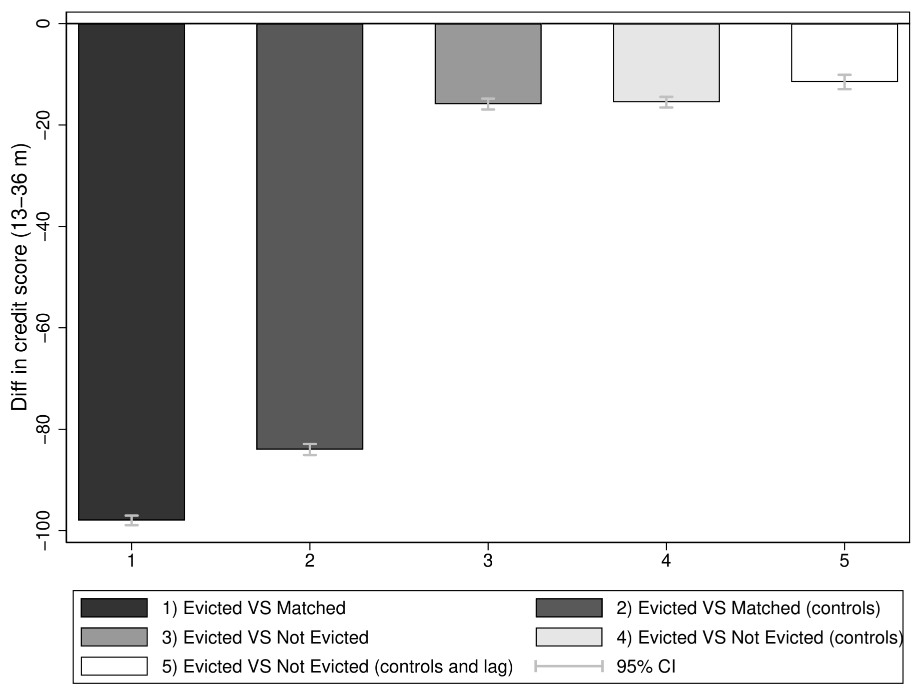
Notes: This figure depicts the locations of properties for which the court ordered an eviction in Chicago (indicated as dots), along with the rate of evictions by census tract (indicated by the shaded regions). The rate is defined as the number of evictions divided by the number of occupied rental units 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



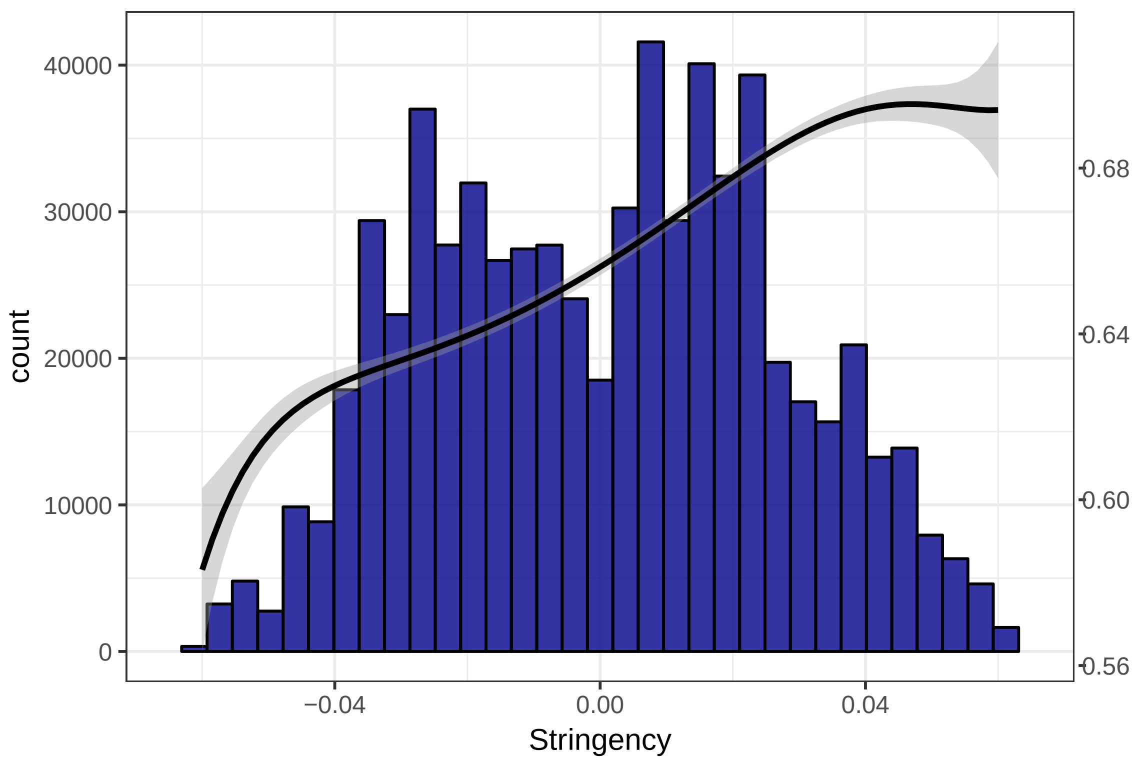
Notes: This figure depicts the possible paths eviction cases can take through the court system. Percentages are calculated for our baseline analysis sample.

Figure 4: Selection into eviction court



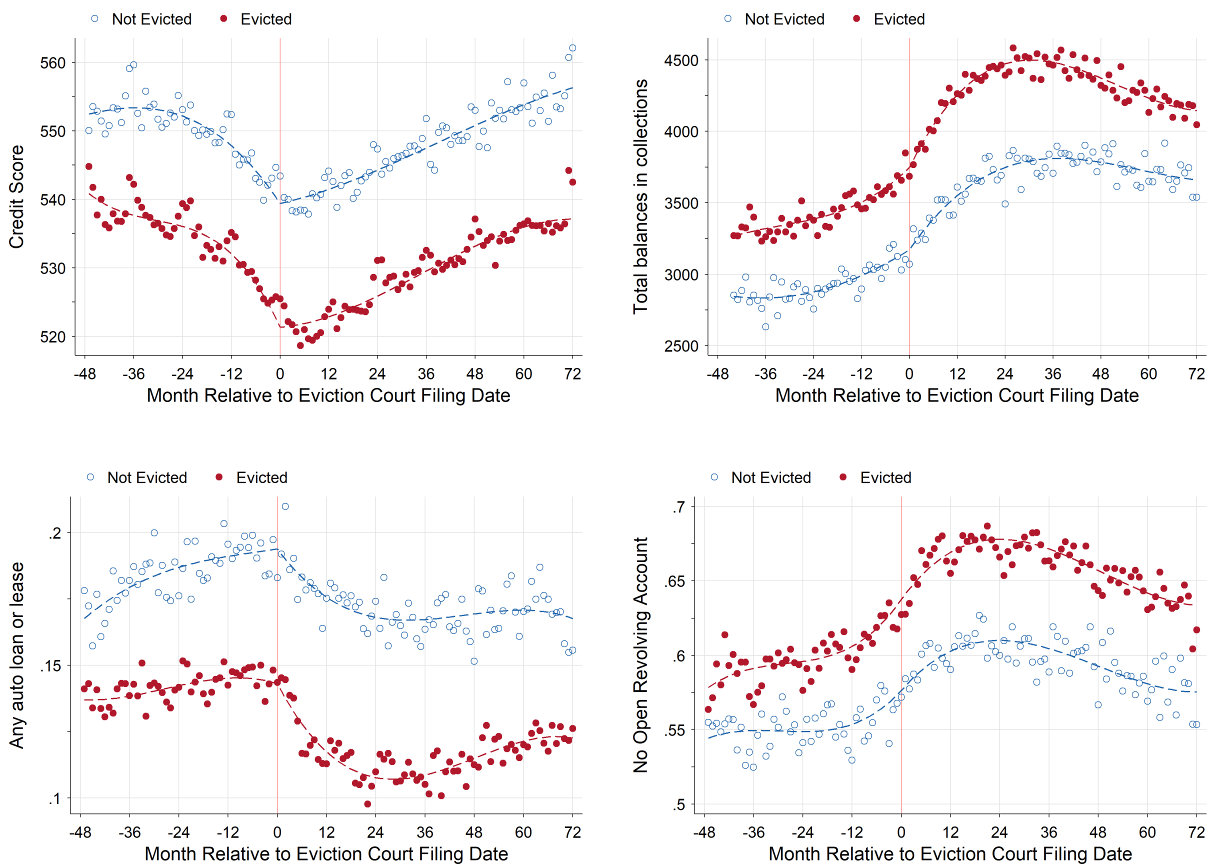
Notes: Column 1 plots the difference in credit score at 13-36 months after filing, for the court sample versus the random sample (for which the filing date is assigned at random). Column 2 is reproduces column 1 with demographic controls (age, gender, and year). Column 3 plots the difference in credit 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



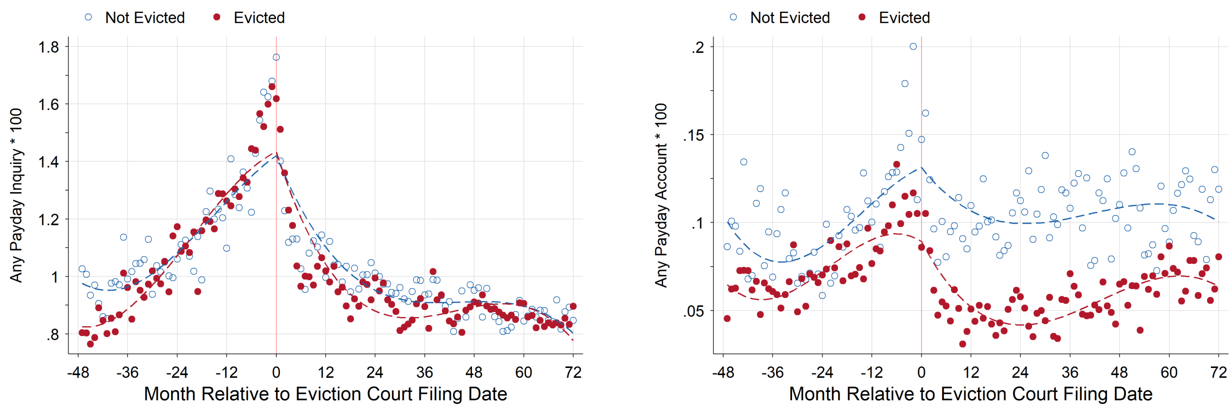
Notes: The figure above graphically depicts the first stage of the main estimation equation, showing how the probability of eviction is affected by judge stringency. The histogram shows the density of year-specific judge stringency for judges who see at least 10 cases per year, and is plotted along the 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



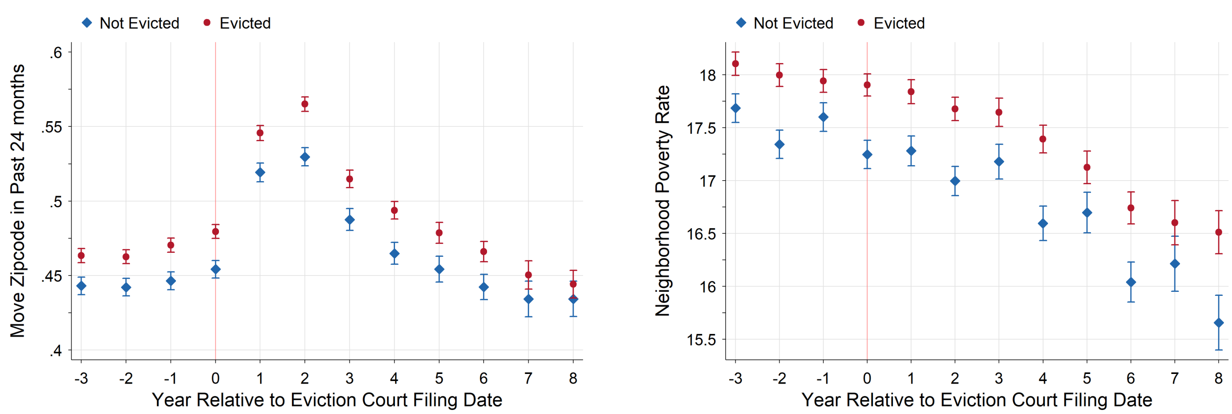
Notes: The figure plots estimates of $\{\beta_r\}$ and $\{\delta + \delta_r + \beta_r\}$ from the regression: $y_{it} = \gamma_t + \delta \times E_i + \sum_{r=S}^{F} \beta_r + \sum_{r=S}^{F} \delta_r \times E_i + \epsilon_{it}$. The omitted month is -48. Overlaid is a parametric specification where the 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.

APPENDIX

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

July 21st, 2019

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

This section details some of most recent reforms at city, county, and state level that alter policies surrounding eviction, rent, and other aspects of tenant-landlord relations. Table A.1 provides a shortened summary of the detailed legislations, with the most recent changes listed first.

2019

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

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

Table A.1: Recent Changes to Eviction Policy

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

Notes: This table shows summary of proposed and implemented changes to eviction policy.

Washington On May 9, 2019, Governor Jay Inslee signed HB 5600, a bill aimed to protect tenants facing eviction. Once the bill is implemented on July 28, 2019, landlords will be required to provide tenants with a 14-day, instead of a 3-day notice when they default on rent payment. The notice must be written in plain language and include information on legal aid resources and court interpreter services. The bill also mandates that a tenant's right to possession of his unit is conditional only on rent and not other monetary amounts (e.g., cost incurred by late payments, attorney fees, etc). Importantly, under HB 5600, judges will be given discretion to stay eviction orders up to 90 days after the judgment, for considerations such as whether the tenant defaulted on rent due to extraordinary circumstances. Separately, the governor signed HB 1440, which will also be implemented on July 28, 2019. This bill will require landlords to provide a 60-day, rather than 30-day notice if they plan to increase the rental rate.

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

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

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

Philadelphia, PA On January 22, 2019, Mayor Jim Kenney signed Bill 170854, which went into effect on April 22, 2019. The new law requires there to be a "good cause" to evict a tenant if the residential lease is less than a year. A few "good cause" reasons include: if the renter has not paid rent, has not followed the terms of the lease, or if there has been property damage. Additionally, even if the landlord has "good cause," he or she must notify the tenant at least 30 days before the eviction date. Finally, the tenants then have the right to contest the "good cause" by filing a complaint with the Fair Housing Commission.

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

2018

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

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

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

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

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

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

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

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

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

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

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

2017

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

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

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

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

B Appendix: Alternative constructions of treatment and instrument

Our analysis uses the judge's eviction order as the treatment variable. An alternative definition of treatment could have been 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's execution of that order. However, that approach introduces challenges related to identification, interpretation, and measurement, which we show in this section.

Interpretation. Suppose we define treatment as the execution of the eviction order. The counterfactual to this treatment includes the judge's dismissal, the landlord failing to pay the fee to the Sheriff's Office, and the tenant leaving the property voluntarily after the judge issues the eviction order. As such, it is unclear which of these margins is relevant for the definition of the LATE parameter identified by IV analysis. Moreover, since only the first of these margins corresponds to a feasible policy change (marginally changing judge leniency), the identified parameter under this definition of treatment does not correspond to a feasible policy change, hence it lacks policy relevance. By contrast, if we define treatment to be the judge's eviction order, the counterfactual is dismissal, and the identified parameter under that definition of treatment corresponds to the policy change of marginally changing judge leniency.

Measurement. Measuring whether an eviction was executed can only be done based on data that is self-reported by the executing Sheriff's Office deputy, with potential incentives to misreport, lacks validation, and has scope for human error. By contrast, eviction orders are entered into the electronic court system, are publicly available, and can be contested by either party if incorrectly recorded.

Identification. The discussion of interpretation above assumes that the conditions for identifying the LATE parameter are met, both in the case where treatment is defined as the eviction order, and in the case where treatment is defined as the combination of the order and its execution. However, under the latter definition of treatment, much stronger assumptions are needed to identify the LATE parameter. This can be seen by constructing a simple model of the two-part process of eviction. For an eviction to be executed, the plaintiff must first receive an order for eviction. Second, the plaintiff must file the eviction order with the Sheriff's Office, pay the required fee, and wait for the sheriff to execute the eviction order, which typically takes one to three months. At any point prior to the Sheriff's Office executing the eviction, the tenant may choose to leave the unit, negotiate, or pay the plaintiff to persuade them to call off the execution of the eviction.

Consider a simple model of eviction where an agent facing a case goes through a two-part process: first, the court issues an eviction order or not, and second, conditional on receiving an eviction order, the sheriff executes the eviction or not. Let $D_1 \in \{0, 1\}$ be in indicator for receiving an eviction order and let $D_2 \in \{0, 1\}$ be an indicator for having the eviction order carried out by the Sheriff. Assume $Z_1 \in \{0, 1\}$ is an instrument for D_1 where $Z_1 = 1$ for judges that are more stringent on eviction orders. Similarly assume $Z_2 \in \{0, 1\}$ is an instrument for D_2, such as the judges' propensity to grant a stay or provide advice to the tenant regarding the execution of the eviction order, where $Z_2 = 1$ for judges that are less likely to grant a stay or advice that reduces the need for the sheriff to execute the eviction order.

Let the agent's outcome be given by

$$Y = Y_0 + (Y_1 - Y_0)D_1 + (Y_2 - Y_1)D_2,$$

where Y_0 is the outcome if there is no eviction order, Y_1 is the outcome if there is an eviction order, but it is not executed, and Y_2 is the outcome if there is an eviction order and it is executed.

For simplicity, we assume there are no observable covariates and the eviction order outcome (D_1) and the eviction execution outcome (D_2) are given by:

$$\begin{aligned} D_1 &= \mathit{1}\{\alpha_0 + \alpha_1 Z_1 - \epsilon_1\} \\ D_2 &= \mathit{1}\{\beta_0 + \beta_1 Z_2 - \epsilon_2\}. \end{aligned}$$

We make no assumptions regarding the dependence between ϵ_1 and ϵ_2, but assume Z_1 and Z_2 are independent. We discuss the case where we relax the assumption of independence between Z_1 and Z_2 below.

Given this setup, we can define the set of always takers (A), compliers (C), and never takers

(N) for each decision. Let T_1 be an indicator for an agent's type for the eviction court ruling:

$$T_1 = \begin{cases} A, & \text{if } \alpha_0 > \epsilon_1 \\ C, & \text{if } \alpha_0 + \alpha_1 > \epsilon_1 > \beta_0 \\ N, & \text{if } \alpha_0 + \alpha_1 < \epsilon_1. \end{cases}$$

We can similarly define $T_2 \in \{A, C, N\}$ as the agent's type for the sheriff enforcing the eviction order. Note that we define this type regardless of the realization of D_1.[43]

Case 1: Treatment defined as D_1

Assuming the stylized model above is the true data generating process, consider the case where, rather than using both instruments, we use only D_1 and Z_1. In this setting, the standard Wald estimator is given by

$$\begin{aligned} Wald(Z_1) = E[Y_1 - Y_0 \mid T_1 = C, T_2 = N] \times \frac{q_1}{Q} \\ + E[Y_1 - Y_0 \mid T_1 = C, T_2 = C] \times \frac{q_2}{Q} \\ + E[Y_2 - Y_0 \mid T_1 = C, T_2 = A] \times \frac{q_3}{Q} \\ + E[Y_2 - Y_0 \mid T_1 = C, T_2 = C] \times \frac{q_4}{Q}, \end{aligned}$$

where

$$\begin{aligned} q_1 &= Pr(T_1 = C, T_2 = N) \\ &= Pr(T_1 = C) \times Pr(T_2 = N | T_1 = C) \\ q_2 &= Pr(T_1 = C) \times Pr(T_2 = C \mid T_1 = C) \times Pr(Z_2 = 0) \\ q_3 &= Pr(T_1 = C) \times Pr(T_2 = A \mid T_1 = C) \\ q_4 &= Pr(T_1 = C) \times Pr(T_2 = C \mid T_1 = C) \times Pr(Z_2 = 1) \end{aligned}$$

and $Q = q_1 + q_2 + q_3 + q_4$.

This result relies on the assumption that that Z_1 and Z_2 are independent. For the case when Z_1 and Z_2 are not independent Z_2 must be directly controlled for, otherwise Case 1 will violate the exclusion assumptions.

In Case 1, the treatment effect is a weighted combination of two effects: (1) the average effect of an eviction ordered and execution for a specific subset of first-event compliers, and (2) the average effect of an eviction ordered but no execution for a specific subset of first-event compliers. The relative weights for these two groups will depend on individuals' types in the in the second

[43] I.e., the type of an individual at at $T = 2$ is defined, even if they are a never taker at $T = 1$.

event (T_2) and the distribution of Z_2.

Case 2: Treatment defined as $D_1 \wedge D_2$

Now alternatively consider the case where, in our empirical analysis, we define a single binary treatment that is 1 if the eviction order is executed and is 0 otherwise. Mirroring treatment, similarly construct the judge instrument as the combined effect of both order and execution stringency:

$$\tilde{D} = f(D_1, D_2) = D_1 \times D_2 \text{ and } \tilde{Z} = g(Z_1, Z_2) = Z_1 \times Z_2.$$

Let the outcome be given by $\tilde{Y} \in \{Y_{\tilde{0}}, Y_{\tilde{1}}\}$ where $\tilde{Y} = Y_{\tilde{0}} + \tilde{D}(Y_{\tilde{1}} - Y_{\tilde{0}})$. Finally, define the variable indicating if the individual is a never-taker, complier, or always-taker in this setting as: $\tilde{T} = \{\tilde{A}, \tilde{N}, \tilde{C}\}$.

Violation of exclusion: With this setup, the two potential outcomes are $Y_{\tilde{0}}$ and $Y_{\tilde{1}}$, but these outcomes mask underlying heterogeneity. Specifically, $Y_{\tilde{0}} \in \{Y_0, Y_1\}$, and $Y_{\tilde{1}} = Y_2$. As such, exclusion restriction requires an additional assumption about outcomes Y_0 and Y_1. Intuitively, this is because the overall individual types (ie $\tilde{A}, \tilde{N}, \tilde{C}$ in the sense of how you respond to a change of $\tilde{Z}$ from 0 to 1) have different outcomes conditional on $\tilde{Z}$, which violates the exclusion restriction. More formally, for the exclusion restriction to hold, the expected value of a potential outcome for any subgroup should not depend on the value of the instrument. For example, for the exclusion restriction to hold, i.e.

$$E[Y^{\tilde{d}} | \tilde{Z} = 0, \tilde{T} = k] = E[Y^{\tilde{d}} | \tilde{Z} = 1, \tilde{T} = k],$$

where $k \in \{\tilde{A}, \tilde{N}, \tilde{C}\}$.

If the exclusion restriction holds, then

$$E[Y^{\tilde{0}} | \tilde{Z} = g(0,0) = 0, \tilde{T} = \tilde{N}] = E[Y^{\tilde{0}} | \tilde{Z} = g(1,1) = 1, \tilde{T} = \tilde{N}], \tag{B.1}$$

yet consider an agent for whom $T_1 = C_1$ and $T_2 = N_2$ (and thus $\tilde{T} = \tilde{N}$). On the left-hand side of Equation B.1 the outcomes is $Y^{\tilde{0}} = Y^0$, but on the right-hand side, it's $Y^{\tilde{0}} = Y^1$, so unless $Y_1 = Y_0$, the exclusion restriction is violated. The assumption that $Y_1 = Y_0$ is strong since, once an eviction order is made, it becomes part of public record, hence the tenant will be subject to any the stigma of having the eviction on public record. Assuming $Y_0 \neq Y_1$, the Wald estimator is not a weighted sum of LATEs and the weights will sum to a value greater than 1.

In addition, case 2 also results in unconfoundedness being violated for the decision, even when $Y_0 = Y_1$. To build intuition, consider the subpopulation (A_1, C_2). Note that $\tilde{Z} = g(1,0) = 0 \implies \tilde{D} = 0$ but $\tilde{Z} = g(0,1) = 1 \implies \tilde{D} = 1$, and finally $\tilde{Z} = g(1,1) = 1 \implies \tilde{D} = 1$, thus an an agent who is (A_1, C_2) will be neither an always taker nor a complier, but it will depend on which realization of Z_1 and Z_2 resulted in $\tilde{Z} = 0$. An intuitive way to think about this assumption in our setting is "An individual i can only be part of one of the subpopulations, i.e., $\tilde{T}_i \in \{\tilde{A}, \tilde{N}, \tilde{C}\}$."

Extending case 2 and violation of monotonicity: Note, that above we constructed $\tilde{Z} = Z_1 \times Z_2$, but it would also be possible to define an alternative multi-valued instrument Z_m where:

$$Z_m = \begin{cases} z_0, & \text{if } Z_1 = 0 \text{ and } Z_2 = 0 \\ z_1, & \text{if } Z_1 = 1 \text{ and } Z_2 = 0 \\ z_2, & \text{if } Z_1 = 0 \text{ and } Z_2 = 1 \\ z_3, & \text{if } Z_1 = 1 \text{ and } Z_2 = 1. \end{cases}$$

In this setup, the Z_m are not inherently ordered, but an ordered instrument can be constructed by assigning each value of Z_m to the overall population probability a case assigned to a given judge type results in an eviction order and its execution. This is what is regularly done in practice with the assignment of leave-out judge stringency. Doing this, we know that z_0 will have the lowest probability of an eviction order and execution and z_3 will have the highest, but the ordering of z_1 and z_2 will depend on the data. Assume without loss of generality that the probability of eviction order and execution when $Z_m = z_1$ is lower than when $Z_m = z_2$. Define the ordered instrument $Z_c = (p_0, p_1, p_2, p_3)$, where each value of p_j is the associated probability of an eviction order and execution when $Z_m = z_j$.

Assume both Z_1 and Z_2 are both valid instruments, they are not perfectly correlated, and that all pairs of $T_1 \cup T_2$ exist in the data. In this setting, the continuous instrument Z_c will violate the monotonicity assumption by construction. For example, consider an individual with $\{T1, T2\} = \{C, A\}$, then $\tilde{D}(p_1) = 1$ and $\tilde{D}(p_2) = 0$, but for an individual with $\{T1, T1\} = \{A, C\}$, $\tilde{D}(p_1) = 0$ and $\tilde{D}(p_2) = 1$. Thus, not all defendants are more likely to have an eviction ordered and executed when assigned p_2 rather than p_1.

C Appendix: Institutional details

C.1 Source materials

This appendix contains a detailed description of the institutional context surrounding residential eviction procedures in Cook County, IL. We draw on the following sources:

- the relevant legislative codes;[44]

[44]Specifically, the Illinois Compiled Statutes (ILCS) and the Municipal Code of Chicago Residential Landlords and

- observation of court hearings by the authors in the years 2016-2018;
- newspaper articles and observation studies performed in Cook County eviction court for earlier years;
- discussions with landlords, attorneys for plaintiffs and defendants, judges, and staff of the Cook County Circuit Court;
- landlord guidebooks specific to Cook County;
- the Chicago Eviction Court Bench Book, by Lawrence Wood (2001);
- publicly available eviction case histories;
- records from the Sheriff's Office on summons and evictions obtained by FOIA request.

The observations in this appendix do not represent the opinions of employees of the Cook County Circuit Court and the Sheriff's Office, nor the opinions of any of the other experts consulted by the authors.

C.2 Estimates of the number of eviction cases nationwide

Although data on the annual number of eviction filings in the U.S. are not collected or reported by statistical agencies, available estimates for 2016 are between 2.1 and 2.7 million orders, shown in Table C.1. The only nationwide estimate for the number of eviction orders in 2016 that we were able to find is 841,516. The numbers in the first column of Table C.1 are based on Desmond et al. (2018a), and they are lower bounds: we compiled them by aggregating state-level data for 2016. The underlying data set covers many, but not all counties in the U.S.. Importantly, New York County and Queens County are not included. We were not able to find an estimate of the number of evictions that are executed by a sheriff, marshall, or constable.

Table C.1: Estimates of total number of evictions annually.

	Desmond et al. (2018a) (estimates are lower bounds for 2016)	American Housing Survey (estimates for 2012)	Marr (2016) (based on RIS data, estimates for 2015)
Estimated # evictions	841,516		
Estimated # filings	2,146,830		2.7m
Estimated # notices		704,000	

Notes: In the first row, eviction refers to a court judgment in which the tenant is ordered to move out – analogous to our definition of eviction in this paper. In the second row, the number of eviction filings includes multiple cases filed at the same address in the given year. Finally in the third row, notices refers to eviction notices served to tenants prior to the court proceedings.

Tenants Ordinance (RLTO).

C.3 Tenant-landlord law in Cook County

Cook County court districts

The Forcible Entry and Detainer Section of the Circuit Court of Cook County handles eviction cases. The court divides the county into six districts. Each district has its own court house with evictions courtrooms, and its own set of judges who handle eviction cases. Landlords must file eviction cases in the district in which the property is located. The vast majority of cases in our data are from the first court district, which handles cases relating to properties located in the City of Chicago. Figure C.1 presents a map of the court districts. Our data set spans all six districts. In the paper and the remainder of this document, we regularly refer to the Forcible Entry and Detainer Section of the Circuit Court of Cook County simply as 'Cook County eviction court'.

Relevant codes of legislation for Cook County

The relevant legislation is recorded in two sources. RLTO – the Municipal Code of Chicago Residential Landlords and Tenants Ordinance, and ILCS – the Illinois Compiled Statutes. RLTO applies only to Chicago (i.e., first district), while ILCS applies to Cook County and thus also Chicago. RLTO trumps ILCS in Chicago, but only when it is more strict (towards landlords). For our data period, the most relevant legislations are the Forcible Entry and Detainer Act (735 ILCS 5/9) and the Civil Practice Act (735 ILCS 5/2).[45]

The legal framework for the rental housing market in Cook County

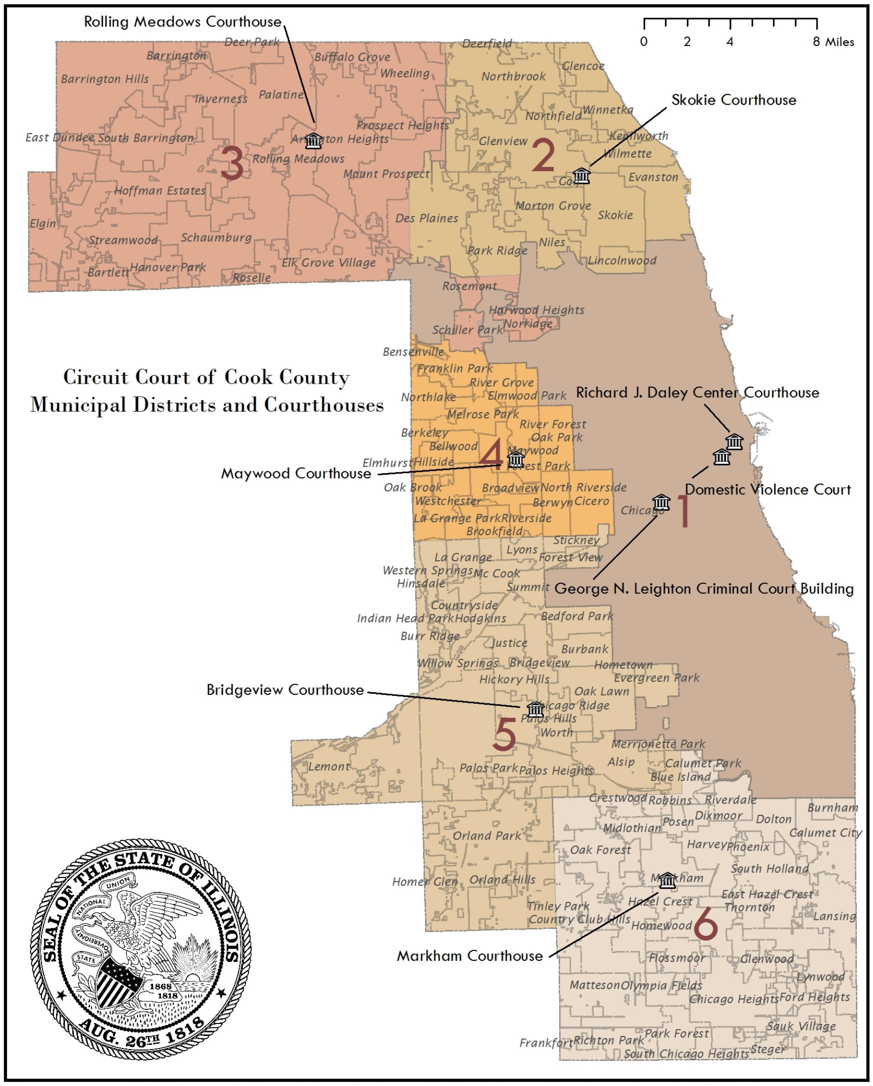
Listed below are points on the historical timeline that are relevant for understanding the evolution of the IL/Cook County legal framework for rental housing markets. These changes occurred before the start of our data period, and are provided here to place the current policy discussion in historical context. Of these legislative and judicial changes, the 1963 and 1972 entries are arguably the most important for understanding how Illinois law has evolved to become more "tenant-friendly" over time.

1874 Illinois Forcible Entry and Detainer Act (735 ILCS 5/9) passed, stating, *"no matters not germane to the distinctive purpose of the [eviction court] proceedings shall be introduced by joinder, counterclaim, or otherwise"*. Until 1972, judges interpreted this as implying the proof of payment of rent is the only defense a tenant could mount against an eviction case related to nonpayment of rent.

1963 Retaliatory Eviction Act (756 ILCS 720/1) enacted, which forbade a landlord from terminating a tenant's lease in response to the tenant complaining to authorities about

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

Figure C.1: Administrative districts of the Cook County Circuit Court



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

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

building or health code violations.

1972 IL Supreme Court ruled, in *Jack Spring, Inc v. Little*, that there is an implied warranty of habitability for leased residential premises. As such, the landlord's failure to keep the unit up to standard is considered a breach of the lease. And complaints regarding conditions of the premise, such as the landlord neglecting to pay for utilities or to repair faulty conditions, are valid arguments in eviction court. This increases the scope of possible defenses for tenants. Prior to this decision, tenants could only dispute the claim of nonpayment of rent, provide proof of retaliatory motives, or bring up technicalities (like failure to serve summons or mistakes in the landlord's paperwork) as a reasonable defense.

1978 Lawyers' Committee for Better Housing (LCBH) and Legal Assistance Foundation Chicago (LAFC) released a report after monitoring eviction court cases, highlighting the limited amount of time available to make judgments. The report prompted a response by the presiding judge and expansion of the number of time slots available to handle eviction cases.

1986 Passage of the Chicago Residential Landlords and Tenants Ordinance (RLTO), which further expanded on tenants' possible defenses by providing them with additional rights. For example, if a landlord accepts even partial rent after posting a termination notice, he forgoes the right to go to court.

1997 IL Rent Control Preemption Act, a law that prohibits municipalities from enacting any form of regulation on residential or commercial rent prices. The presence or absence of rent control regulations is likely important for predicting the market-level effects of changes to eviction laws, since landlords will likely respond differently to policies depending on the degree to which they are able to adjust rents.[46]

Permissible causes for eviction

If a tenant commits certain violations as described in the ILCS and Chicago's RLTO, the landlord can provide the tenant with a notice to terminate the lease. The notice should outline the reason for termination and the number of days the tenant has to remedy the problem. If the issue is not addressed within the stated time period, the landlord can file an eviction case against the tenant to reclaim his property if the tenant has not yet vacated. The legally permitted reasons to terminate and their respective required days of notice are as follows:

1. Illinois/Cook County

- **Nonpayment of rent** → 5-day notice, and the right to file for eviction is void only if the landlord accepts **all** of past, due rent (735 ILCS 5/9-210)
- **Any violation of the lease** → 10-day notice (735 ILCS 5/9-210)
- **Foreclosure of property.** The purchaser who assumes control of the residential real estate in foreclosure enters into bona fide leases with the tenants in the property. This is a de-facto continuation of the leases agreed upon by the previous landlord, prior to the change of ownership. The rules on when the purchaser may terminate the bona fide lease depend on the lease type:

[46] Most states have laws against rent control; the ones that don't (as of 2014) are AK, CA, DE, DC, HI, ME, MD, MT, NE, NJ, NY, NV, OH, PA, RI, WV. These states don't necessarily have rent control in all or even some of their cities – they merely don't have laws *against* rent control. States where some cities currently have implemented a form of rent control include CA, DC, MD, NJ, and NY.

- Weekly or monthly lease → 90-day notice (ILCS 5/9-207.5)
- Any other lease → 90-day notice and the purchaser may not terminate before the end of the bona fide lease (ILCS 5/9-207.5)

- **Ending tenancy at end of lease.**
  - Weekly lease → 7-day notice (735 ILCS 5/9-207)
  - Any lease less than one year (not including weekly lease) → 30-day notice (735 ILCS 5/9-207)
  - Yearly lease → 60-day notice provided before end of lease (735 ILCS 5/9-205)

2. **Chicago** (all Chicago-specific laws come from RLTO, especially 5-12-130)

- All ILCS laws hold, and Chicago laws can only be stricter.
- For 5-day notice, the right to file for eviction is voided if tenant pays **any part** of the rent.
- For 10-day notice, the right to file for eviction is voided if tenant fixes issues within 10 days.
- For foreclosures, the owner of the foreclosed property must pay a relocation assistance fee of $10,600 or provide tenants with the option to renew or extend the existing lease (with some restrictions on the price of that renewed contract).[47]

Lines of defense for tenants in eviction court

According to 735 ILCS 5/9-106, "Except as otherwise provided in Section 9-120, no matters not germane to the distinctive purpose of the [eviction court] proceeding shall be introduced by joinder, counterclaim or otherwise." Although there is a certain ambiguity in interpreting what arguments are germane in eviction court, the general consensus regarding appropriate defenses has been relatively stable. According to the 2001 Eviction Court Bench Book (Residential Tenancies), the most common defenses in eviction court are:

- Potential defenses to any eviction action:
  - The plaintiff is not a proper party or lacks capacity to sue.
  - The defendant has a claim arising under the Retaliatory Eviction Act, which prohibits counter measures by the plaintiff in response to certain actions by the defendant, such as notifying the authorities of a building code violation on the plaintiff's property.

[47] According to RLTO 5-14-050, *"the owner of a foreclosed rental property shall pay a one-time relocation assistance fee of $10,600 to a qualified tenant unless the owner offers such tenant the option to renew or extend the tenant's current rental agreement with an annual rental rate that: (1) for the first 12 months of the renewed or extended rental agreement, does not exceed 102 percent of the qualified tenant's current annual rental rate; and (2) for any 12-month period thereafter, does not exceed 102 percent of the immediate prior year's annual rental rate."*

  - The plaintiff is discriminating against the defendant on an unlawful basis. In Cook County, discrimination based on income is also prohibited, which protects tenants from being evicted for being voucher recipients.
  - After the lease agreement expired or was terminated, the plaintiff recognized the existence of the defendant's tenancy (e.g., by accepting rent that accrued after the date of expiration or termination).

- Potential defenses when the plaintiff fails to properly serve a termination notice:
  - The notice was not served in accordance with applicable law. Note that the defendant's receipt of the notice, however, cures the plaintiff's failure to serve its notice in accordance with the methods set forth in the Forcible Entry and Detainer Act.
  - The notice does not afford the defendant the statutorily required number of days of notice (e.g., the plaintiff gave a 5-day notice for issues that require a 10-day notice).
  - The plaintiff filed the eviction action before the statutorily required notice period ended.

- Potential defenses when the plaintiff served a termination notice regarding nonpayment of rent:
  - The defendant owed no rent.
  - The defendant paid the plaintiff all the rent due before the termination notice expired.
  - The defendant tendered to the plaintiff all the rent due before the termination notice expired, but the plaintiff refused to accept it.
  - The plaintiff failed to maintain the premise in substantial compliance with applicable municipal building codes, such that the resulting drop in the unit's value exceeds the rent demanded in the notice.
  - The rent demanded represents an amount the defendant withheld in compliance with the Rental Property Utility Services Act, which requires the landlord to cover costs related to various utilities listed in the lease.

- Potential defenses when the plaintiff served a 10-day termination notice alleging that the defendant violated the lease agreement:
  - The defendant never committed the alleged violation.
  - The defendant's conduct does not constitute a material lease violation.
  - The plaintiff waived his right to pursue an eviction action based upon the lease violation, by accepting rent that accrued after the plaintiff learned about this violation.

- Potential defenses when the plaintiff served a 7-day or 30-day notice that did not state a reason for terminating the tenancy:
  - The plaintiff accepted rent after the lease terminated, thus creating a new monthly lease.

- Additional defenses that are specific to Chicago, as governed by the RLTO:
  - The plaintiff violated the RLTO's prohibitions against retaliation, which expands on the Retaliatory Eviction Act by broadening the set of prohibited motives for eviction (for example, if the eviction was the result of the defendant asking the plaintiff to make necessary repairs or because the defendant joined a tenants' organization).
  - The plaintiff accepted partial rent after serving the eviction notice.
  - The defendant cured the 10-day notice violation within the 10 days of receiving the notice, or that the notice did not inform the defendant of his right to cure the lease violation.

C.4 Cook County eviction court procedures

Filing an eviction case

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

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

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

Randomized case assignment

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

Court proceedings

Although there are differences between cases, the general eviction court process is as follows.

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

If and when the tenant shows up to court, there are several courses of action he can pursue. He can request a continuance which delays the start of the case to give the tenant additional time to find an attorney or seek legal advice.[48] If granted, the tenant is usually given one week to do so. At any point prior to the bench trial, the tenant can also request a trial by jury, and the case may moved to a jury courtroom, which takes additional time. Alternatively, before moving to the bench trial, the landlord and tenant may agree to a settlement order.[49] This allows the landlord and tenant to negotiate certain binding conditions, which, if adhered to, result in the eviction case being dismissed. Typically, this involves the tenant agreeing to vacate the premises by a certain date and the landlord agreeing to dismiss the case if this is done, or the tenant agreeing to pay a certain amount by a certain date. If the tenant fails to fulfill the settlement conditions, the landlord can return to court and receive an immediate order for possession.

Finally, the case may also be dismissed by the landlord for a variety of reasons. Common reasons include: the landlord realized he made a mistake in how he filed the case, the tenant left the premises so the landlord no longer needed to obtain an order for possession, the landlord and tenant came to an understanding outside of court, etc. This typically results in the case being dismissed without prejudice, which allows the landlord to file a separate case at a later time for the same reasons as before.

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

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[48] Tenants are encouraged to do this by pro bono legal aid helpers at the court because judges usually grant continuances when requested. Some judges will also ask tenants if they would like a continuance.

[49] See settlement form.

After a judge grants an order for eviction

After a judge grants an order for eviction[50], there are still ways to delay or revoke the order. The judge can grant a "stay," which gives the tenant a certain number of days before the landlord can file the order for eviction with the sheriff. Most judges usually give a one week stay. Additionally, before the eviction is carried out, the tenant may also submit a motion to vacate to the Court asking the judge to vacate the eviction order.[51]

Once the order has been entered and any stay periods have expired, the landlord may file the order for possession with the Sheriff's Office for a nonrefundable fee of $60.50. The sheriff may enforce the eviction order as soon as 24 hours after the landlord's filing. Realistically, however, it takes around 2 months (median time of 71 days, based on data from the Sheriff's Office).

Next, the tenant will receive a letter from the Cook County Sheriff's Office notifying them that the landlord has filed an eviction order with the sheriff, and he may be evicted at any time starting 24 hours after the receipt of the letter. Importantly, this is the only notification the tenant receives regarding the eviction. However, the tenant could check the sheriff's eviction schedule everyday to see if he would be evicted within 72 hours. The schedule is posted daily on the sheriff's website, and lists the cases to be enforced within the next 3 days. In most cases however, the initial notice scares the tenant into believing his eviction is imminent, which may compel him to leave before the sheriff shows up weeks later.

Similarly, the landlord does not receive advance notice of the eviction date, though he can check in continually using the sheriff's schedule. On the working day before the scheduled eviction, the Sheriff's Office would call the landlord (or his attorney) to inform him of the time block during which a deputy is likely to appear. At any point leading up to the eviction, the landlord can cancel the eviction, most likely because the tenant already left the premises.

Assuming the temperature is above 15°F on the day of eviction, the landlord (or his representative) is required to greet the sheriff's deputy at the property with a locksmith alongside. Once papers authorizing the deputy's use of force (if necessary) are signed, the deputy would enter the property and remove any occupants listed on the order.[52] Once that is complete, the landlord would change the locks to the door(s). This completes the eviction process. Importantly, the deputy is not in charge of removing personal belongings; this task must be separately worked out between the landlord and the tenant.

[50] To see what the eviction order form looks like, see eviction order.

[51] Though generally not granted, one reason this may be granted is if the tenant was extremely ill and missed the court date during which the judge rules a default order for possession.

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

Money judgments

If the landlord filed a joint action case, the judge must also decide if and how much the tenant owes to the landlord for back rent and claimed damages. For residential evictions, if the complaint is filed properly, the landlord may also claim court fees and rent accrued during the eviction process.

It is plausible in joint action cases for the judge to grant the landlord an order for possession but no money order. In contrast, it is rare for the landlord to obtain a money order but no order for possession. For instance, if the tenant never shows up to court after being served the summons several times, the judge can often grant an order for possession, but the ILCS generally forbids her from making a money judgment in such situations. Also, the landlord and tenant could reach a settlement in which the landlord agrees to drop the money judgment if the tenant moves out by a certain date. Finally, landlords would often mention before the judge that they are willing to drop the money judgment, as their main goal is eviction.[53]

Once the landlord obtains a money judgment against the tenant (the debtor), his options to collect the debt are:[54]

- **Citation to discover assets:** In a supplementary proceeding, the debtor would be summoned to testify the amount and location of his assets. If the debtor fails to appear and testify, he may be arrested for contempt of court. The court can order for any of the declared assets to be liquidated or transferred to the landlord. Parts of the debtor's assets may be exempt from collection, including up to $4000 in equity interest of the debtor's personal property and up to $2400 in equity interest of a motor vehicle.
- **Levy and execution:** If the debtor owns assets without secured debts, the landlord can obtain an order to have the sheriff seize and sell any such assets to satisfy the money order.
- **Wage garnishment:** If the debtor is employed, an order can be filed to the debtor's employer specifying an amount to deduct from each paycheck. Some restrictions apply: the order can only deduct up to 15% of debtor's gross weekly income, and the debtor cannot be left with a weekly take-home pay that is less than 45 times the state minimum hourly wage.
- **Non-wage garnishment:** If the debtor has a bank account or earns commission, the court can order for these assets to be taken as payment.

[53] Though this should theoretically not affect the judge, we have observed this argument numerous times.

[54] Sources: Landlord & Tenant Eviction Handbook, 735 ILCS 5/2-1402.

Figure C.2: Court order for random assignment of cases

STATE OF ILLINOIS)
)
COUNTY OF COOK)

IN THE CIRCUIT COURT OF COOK COUNTY

FIRST MUNICIPAL DISTRICT

GENERAL ORDER 97-5

IT IS HEREBY ORDERED that effective June 2, 1997, the assignment of cases in the Forcible Entry and Detainer Section is re-structured as follows:

(1) Courtroom 1302 will no longer be limited to pro-se plaintiff cases; and,

(2) All non-jury case filings, by pro-se plaintiff cases, will be randomized to Courtrooms 1302, 1402, 1406, 1408, and 1409. All non-jury cases filed by attorneys will be randomized to Courtrooms 1302, 1402, 1406, 1408 only. As many as three cases filed at the same time are to be assigned to the same courtrooms; and

(3) The three case block limitation provided in (2) above does not apply to designated bulk filers. Bulk filers' cases will be assigned to Courtroom 1302, 1402, 1406, and 1408.

(4) All forcible jury cases will continue to be assigned to Courtroom 1409, and,

(5) All previously assigned cases will remain in the courtroom to which they are assigned.

IT IS FURTHER ORDERED that this General Order shall be spread upon the records of the court.

Dated at Chicago, Illinois this 8th day of May, 1997.

ENTER:

Earl E. Strayhorn
Presiding Judge
First Municipal District
Circuit Court of Cook County

C.5 Comparing evictions in Cook County to other counties

Comparison of Cook County eviction court proceedings to other counties

While there are a small number of notable differences, the general legal framework surrounding eviction in Cook County is comparable to that of other counties. The similarities revolve around the rules that landlords must follow to summon their tenants to eviction court, the trial process, and the procedures following an order for possession.

Differences: The main departures from the national norm are found in Chicago's RLTO, which is considered to lean somewhat tenant-friendly. Specifically, Chicago has an unusual rule that the landlord loses his right to evict if he accepts even a partial payment of past rent after the (5-day) notice period expires. In addition, landlords in Cook County cannot include a clause in the lease that requires the tenant to cover legal fees in the case of a successful eviction, which is not the case in may other parts of the country.

Similarities: Almost all states require the landlord to serve some form of a written notice of termination after the tenant has failed to pay rent. The notice gives the tenant a certain amount of days to remedy the situation by paying late rent before the tenant can pursue legal eviction, ranging from 3 days (CA, NY, TX) to 5 days (IL) to 14 days (MA) or longer (interestingly, MD requires no notice period, but this is a rarity). Most states also require that if the tenant can offer full payment of rent owed before the notice expires, the landlord must accept it and loses the right to evict. As noted above, Chicago's rules surrounding partial and late payments causing the landlord to forgo eviction rights is unusual.

Once the notice expires without remedy and the landlord goes to the clerk to obtain a court date, almost all states require some type of summons and complaint to be served to the tenant. The method of delivery may differ, but it tends to involve, as in IL, a third party serving the summons. Exceptionally, in CA, the landlord is required to serve the summons.

Although there are some differences in steps leading to the trial, most processes are relatively similar and involve a method for the tenant to request a jury trial. For instance in TX, tenants must request for jury trial within 5 days of receiving the summons and complaint, while in IL, tenants may request it before the judge. Most counties also require the tenants to show up on the court date, lest the judge enters a default judgment in favor of the landlord.

The trial process is generally similar in most states, with one notable difference in the implications of a favorable ruling for the landlord. In some states, including PA and MD, the judge's ruling in favor does not automatically include an order for possession. In these states, the landlord must request an order for possession if the tenant has not vacated the property within a certain amount of days (in PA, the landlord can only request an order for possession if the tenant does not leave within 10 days of the ruling). Once an order for possession has been obtained, the process is comparable across most states and involves the landlord paying a certain fee to the sheriff (or Constable in many cities such as New York City and Los Angeles), who implements the eviction after a certain time frame elapses.

Comparing Cook County eviction rates to other large counties

Based on Desmond et al. (2018a), Cook County's eviction rate in 2016 was 1.22%, and its eviction filing rate was 3.42%. Amongst the 34 counties in the database with over 1 million in population, Cook County ranked 11th in terms of lowest eviction rate.[55] Again, it is important to keep in mind that Desmond et al. (2018a)'s data cover most, but not all counties in the U.S., so this ranking isn't a perfect statistic.

Cook County's eviction rate, like that of most other counties, seems to be increasing from 2000 to 2009-10 before dipping back down starting from 2011 (probably because of the recession). In general, most large counties have similar or slightly lower eviction rates in 2016 than they did in 2000. See Figure C.3 for a time series of eviction rates by year for the 25 counties with lowest average eviction rates among the 34 large counties as defined in the answer above.

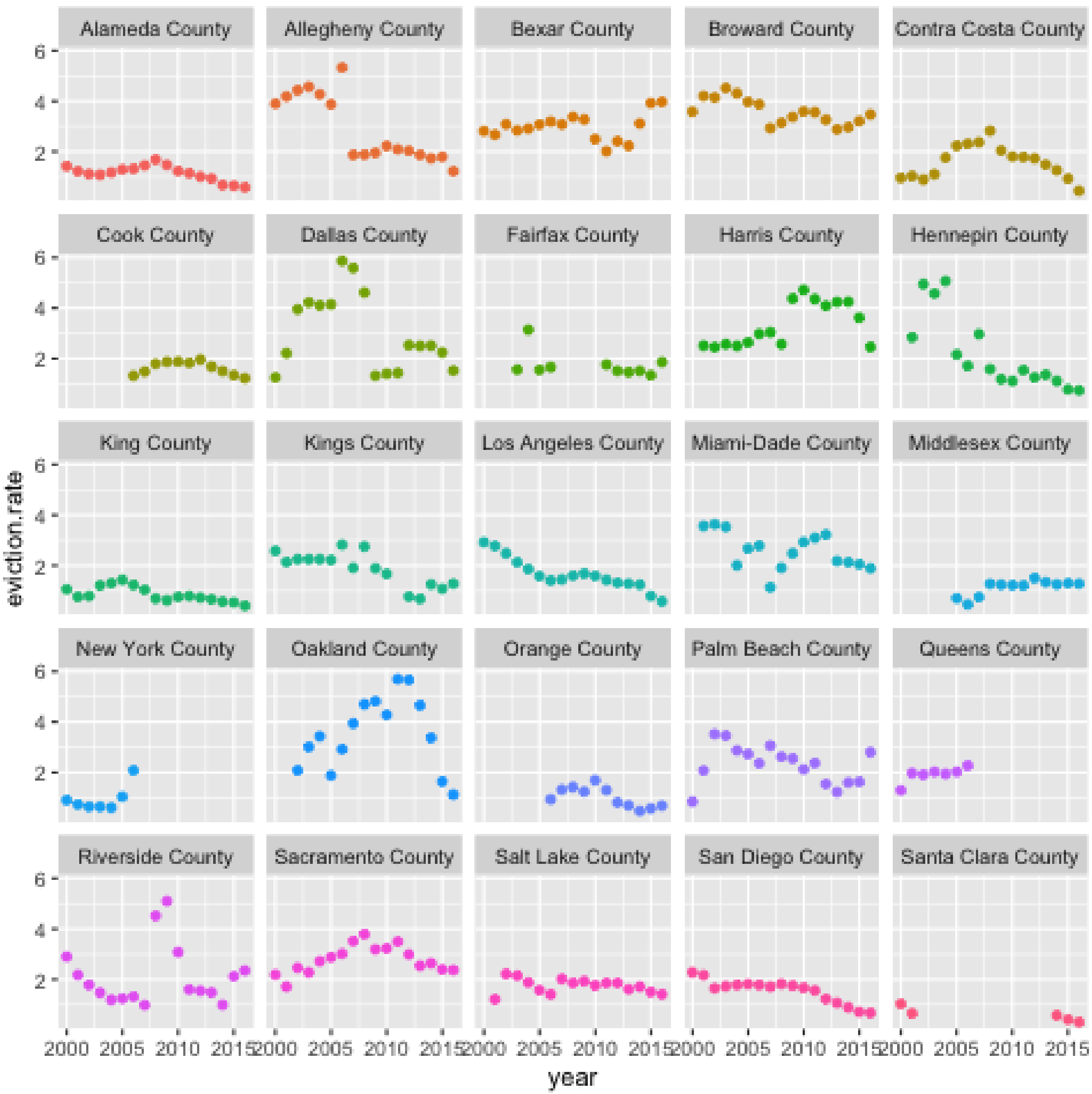
C.6 Eviction and government housing assistance in Cook County

Tenants' ability to receive government housing assistance can be affected if the judge issues an eviction order. If a tenant receives unit-based housing assistance prior to eviction, i.e., they live in public housing or receive a unit-tied voucher, they lose this type of housing assistance once evicted. If a tenant receives a person-tied housing choice voucher (HCV) and lives in a private rental unit prior to eviction, he faces a fixed time window to find a new home that satisfies the HCV-associated quality standards. If the tenant fails to do so, he could lose the voucher as a result.

According the 2016 Chicago Housing Authority Housing Choice Voucher Program Procedure Guide (last revised in 9/12/16), the CHA may terminate a family's assistance if the family (or any family member) is "evicted due to serious or repeated violation of the lease. The CHA is required by HUD to terminate a family's assistance if they do not meet this obligation. See 24 CFR 982.552(b)(2). A family will be considered evicted if the family moves after a legal eviction order has been issued, whether or not physical enforcement of the order was necessary." Additionally, the family (or any family member) must not "Commit any serious or repeated violation of the lease, even if the violation does not lead to eviction. Serious or repeated lease violations will include, but not be limited to, nonpayment of rent, disturbance of neighbors, destruction of property, or living or housekeeping habits that cause damage to the unit or premises and criminal activity."

[55] The ranking, from the county with the lowest eviction rate to Cook County, is: Santa Clara County (CA), King County (WA), Contra Costa County (CA), Los Angeles County (CA), Alameda County (CA), San Diego County (CA), Orange County (CA), Hennepin County (MN), Travis County (TX), Oakland County (MI), and Cook County (IL). New York County and Queens County are not represented in the database for 2016.

Figure C.3: Eviction rate by year for large counties



Notes: Plots include the top 25 large counties (population > 1 million) with lowest average eviction rates between 2000 and 2016. Data are from the Eviction Lab (Desmond et al., 2018a).

A different channel through which access to housing assistance is reduced comes into play if an evicted tenant tries to access new housing assistance, e.g., public housing, affordable housing, or a new HCV. An eviction history can count against them in the application procedure. According the 2016 Chicago Housing Authority Housing Choice Voucher Program Procedure Guide (last revised in 9/12/16): "The CHA will deny assistance to an applicant family if: ... Any family member has been evicted from federally-assisted housing in the last five years."

D Appendix: Data

D.1 Financial strain outcomes: Detailed descriptions

This section describes the key outcomes in the credit report data: (i) credit score, (ii) balance in collections (iii) having a revolving line of credit such as a credit card, (iv) durable consumption proxied by having an auto lease or loan, and (v) ZIP code of residence.

Credit score We interpret credit score as a measure used by lenders for the overall credit-worthiness of the individual borrower and as a proxy for the interest rate faced on new loans. We use VantageScore 3.0, which was provided by the credit bureau and developed as an alternative to FICO; according to the credit bureau, FICO requires a substantial amount of recent data to score an individual.[56] VantageScore 3.0 is on a scale of 300-850. While different lenders impose their own cutoffs for credit quality, the credit tier breakdown provided by Experian is as follows: a score of 300-499 is "deep subprime," 500-600 is "subprime," 601-660 is "nonprime," 661-780 is "prime," and 781-850 is "superprime." Individuals can have a credit report while not having sufficient information to be assigned a FICO credit score. VantageScore was developed as an alternative to FICO in part to be able to score individuals without a substantial amount of recent data, using machine learning tools. Two types of individuals do not have credit scores: deceased individuals, and those who Experian does not have enough information to score. Less than 1 percent of linked individuals are deceased and therefore do not have a score, and an additional 1-2 percent of linked individuals do not have a score due to not enough information.

Balance in collections Credit reports include the total balance in collections. Collections remain on a credit report up to 7 years from the time it is first placed in collections or until paid.

Access to credit Our measure of access to credit is whether the individual has any open revolving account. A revolving account includes any account that begins with 0 balance and borrowers are allowed to carry the balance and do not have to pay it off in full every month. Revolving accounts include credit cards.

Auto Lease or Loan We follow an approach based on Dobkin et al. (2018) and Dobbie et al. (2017), and use an indicator for the individual having a positive balance on an auto loan or lease as a proxy for durable goods consumption.

[56] Several recent studies use VantageScore as a proxy for credit-worthiness, including Dobkin et al. (2018).

ZIP code of residence Addresses are reported to Experian through the inquiry process. The lender has to be verified and the inquiry has to be for a permissible purpose. The ZIP code of residence is not the most recent address, but the modal address of recent inquiries (e.g., last month). Experian does not use the USPS change of address file, but they do use addresses from court records. For example, they use address information from bankruptcies and money judgments, and tax liens.

Subprime borrowing data The subprime borrowing data comes from Clarity, an FCRA-regulated credit reporting agency that maintains the largest subprime database of over 62 million unique consumers and is owned by Experian. Clarity collects data from alternative finance providers, including Online Installment, Online Small Dollar (Single Pay), Storefront Installment, Storefront Small Dollar (Single Pay), Title, Marketplace, Auto, Rent-to-Own, Telecom, Subprime Credit Card, and Collections Records.

The data consists of two data sets: tradelines, which are the monetized loans, and inquiries, which are the borrower's inquiry about getting a loan. Note that there may be one or many inquiries to a tradeline, or a tradeline with no associated inquiries, which may occur on "roll-over" loans or where a borrower is well known to the lender.

From the inquiries file, we keep only new credit inquiries, which excludes inquiries due to collections or leases. The most common inquiries types are Internet Single Payment Micro Loan (SPML) (46.8%), Internet Installment Loans (46.3%), Telecommunications/Cellular (2.5%), Storefront Title Loans (1.2%), and Storefront Installment Loan (0.98%), which together constitute 98 percent of inquiries.

Among tradelines, the most common portfolio types are Single Payment Loans (43.6%), Real-Time Installment loans (35.7%), Installment loans (19.4%), and Line of Credit (0.75%), with Real-Time Line of Credit and Bill Pay being the remaining types and constituting less than half a percent each. The most common account types are Internet SPML (42.6%), Online Installment (32.3%), Unsecured (10.2%), Note Loans (5.0%), Secured (4.2%), and Storefront Installment (1.5%), which together constitute 96 percent of tradelines opened. Internel SPML and Storefront SPML would constitute payday loans.

D.2 Details on data cleaning and sample restrictions

Data restrictions on the court sample

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

Table D.1: Summary of data restrictions

Sample	Named Individuals	Cases	Judges
Full	$772,846$	$583,871$	313
No businesses or condos	$729,379$	$555,164$	313
Non-missing names	$728,603$	$554,856$	313
Damages <$100,000	$707,213$	$546,190$	311
Non-missing judge	$706,141$	$545,378$	310
Judge sees more than 10 cases per year	$640,669$	$495,905$	260
Multiple judges per week	$599,366$	$466,548$	250
In Experian sample	$203,648$	$179,471$	167

Notes: The full sample consists of all cases recorded in Cook County by Record Information Services and not individual people. Our sample includes only individuals who are named on the lease and are in the court filing, and excludes unnamed tenants such as children. The last column includes those matched to the Experian data, and are matched prior to the eviction case. This sample is notably smaller, since the first Experian report is from 2005.

What do court records contain?

The case docket records the filing date, ad damnum amount (i.e., the amount the plaintiff is seeking from the defendant), and information on the defendants, plaintiffs, and their lawyers. When the case is filed, it is assigned a room, date, and time, which are also included in the docket. Finally, the docket shows key case events, including attempts (and successes) at serving the defendant, motions, and proceedings. If the judge makes a money judgment, the judgment amount is also included in the docket.

Defining case outcomes

The court dockets include a detailed history of events and rulings associated with each case. Some events are administrative, while others involve court hearings. For each case, we take the history of events and establish if the case ended in eviction or not. We define cases as ending in eviction if the case has a judge rule for:

- "ORDER FOR POSSESSION"
- "ORDER OF POSSESSION"
- "JUDGMENT FOR PLAINTIFF"

- “SHERIFF EVICTION WORKSHEET FILED”
- “EX PARTE JUDGMENT-PLAINTIFF”
- “VERDICT FOR PLAINTIFF”

Over 99% of cases that we classify as ending in eviction had an “ORDER FOR POSSESSION” ruling, and results are robust to using alternate definitions of eviction.

In addition to determining if a case ended in eviction, we study the various forms of case dismissal. Dismissal codes include:

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

Table D.2: Breakdown of case outcomes

Case Outcome	Proportion
Evicted	0.61
Never Served	0.10
Dismissed without Prejudice	0.08
Dismissed by Plaintiff	0.07
Dismissed by Stipulation or Agreement	0.05
Dismissed for Want of Prosecution	0.03
Dismissed With Prejudice	0.02
Verdict for Defendant	0.01
Other	0.03

Notes: The table above shows the breakdown of case outcomes. Cases classified as “never served” are cases in which the defendant is never successfully served but the case does not proceed ex-parte. Most “never served” cases are dismissed by the plaintiff after multiple attempts at serving the defendant. Case outcomes in the table are mutually exclusive.

Removing businesses and unnamed occupants

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

Standardizing addresses

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

Deriving assigned judge from the raw data

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

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

D.3 Characterizing dismissals using court microfilms

To understand the impact of an eviction, it is essential to characterize the dismissal, the counterfactual outcome in an eviction case. The electronic court docket, from which we collect our main court sample, only records broad dismissal categories.[57] While the dismissal categories provide some insight into how the case was resolved, they do not reveal whether, as part of the dismissal decision, the tenant was required to vacate the unit, or whether the tenant was required to pay a settlement amount.

To provide a richer description of dismissals, we hand-collected court microfilm records for a random sample of court cases ending in dismissal. This sample includes cases from the first district, which is Cook County's largest and includes the City of Chicago. For each type of

[57] The five categories of dismissals that are recorded in the electronic docket are: dismissed by stipulation or agreement, dismissed with prejudice, dismissed without prejudice, dismissed by plaintiff, and dismissed for want of prosecution.

dismissal, we collected court microfilm records for 100 cases, except for "Dismissed for want of prosecution", where we only collected 50 cases, as this dismissal type did not contain information on the terms of the dismissal as expected. Not all of the randomly selected files contained underlying court documents. In total, our sample includes the following types of cases:

- Dismissed by stipulation or agreement: 100 cases (4 missing)
- Dismissed with prejudice: 100 cases (7 missing)
- Dismissed by plaintiff: 100 cases (26 missing)
- Dismissed without prejudice: 100 cases (14 missing)
- Dismissed for want of prosecution: 50 cases (18 missing),

where the number in parentheses indicates the number of cases where there were no underlying court documents available for the dismissal recorded in the court docket.

For each case, all documents relevant to the terms of the dismissal were manually reviewed by at least two researchers for information on whether the agreement allowed the tenant to stay in the unit, whether the agreement required the tenant to pay the landlord, whether the agreement included a payment plan for any payments from the tenant to the landlord, whether the landlord kept the security deposit, and whether the agreement included any payments from the landlord to the tenant. Double-checking the microfilms with the court docket, we found that the form of dismissal in the docket agreed with the form of dismissal implied by the underlying text documents more than 99% of the time. Below, we consider each of these outcomes for the first four types of dismissals as well as a weighted average.[58]

Some cases ending in dismissals do not have any documents associated with the dismissal of the case. Table D.3 summarizes the proportion of cases that have microfilms, and conditional on having microfilms, the proportion of cases that have information on if the tenant did or did not move and if the tenant did or did not make payments to the landlord. The first six rows summarizes the proportions for each case outcome. The "Weighted Average" row displays the average across dismissal types, weighted by the prevalence of each type of dismissal in the court data.

[58] The weights represent the share of each type of dismissal in the full sample.

Table D.3: Details on information contained in case microfilms

	With microfilms	With move-out info.	With payment info
Dismissed by Plaintiff	0.7400	0.5000	0.4595
Dismissed by Stipulation or Agreement	0.9600	0.5938	0.6771
Dismissed with Prejudice	0.9300	0.6774	0.6774
Dismissed without Prejudice	0.8600	0.4651	0.4535
Weighted Average	0.8509	0.5248	0.5266

Notes: The table above reports the proportion of cases that had microfilms of documents associated with the dismissal (column 1), the proportion that specifically included information on whether the tenant agreed to move out or stay (column 2), and the proportion that specifically included information on whether the tenant did or did not agree to make payments to the landlord (column 3). The table is based on a random sample of 400 cases randomly drawn from the district 1 courthouse. The final row provides the weighted average of cases accounting for the fact that some types of dismissals are more common than others.

Dismissals resulting in payment from tenant to landlord On average, 47% of dismissal cases did not contain information on whether the tenant was or was not required to pay rent. For the cases that contained information, approximately half of dismissals stipulated that the tenant make payments to the landlord, with payment being more likely in cases dismissed by stipulation or agreement and much less likely in cases dismissed with prejudice.

Table D.4: Proportion of dismissals involving tenant paying landlord

	No	Yes	NA
Dismissed by plaintiff	0.2297	0.2297	0.5405
Dismissed by stipulation or agreement	0.1667	0.5104	0.3229
Dismissed with prejudice	0.4946	0.1828	0.3226
Dismissed without prejudice	0.2907	0.1628	0.5465
Weighted average	0.2616	0.2649	0.4734

Notes: The table above reports the proportion of dismissals in which the underlying court documents stated the tenant did not owe the landlord money, the tenant did owe the landlord money, or in which no information was available. The "Weighted average" row is the weighted average of the four types of dismissals. Data is based on a random sample of 400 cases from the district 1 courthouse.

Dismissals resulting in the tenant vacating the unit On average, 48% of dismissals did not include information on if the tenant was or was not able to stay in the unit. For cases with information, approximately 54% moved from the unit and 48% stayed. The proportion moving was highest for cases dismissed with prejudice and lowest for cases dismissed by stipulation or agreement.

Table D.5: Proportion of dismissals involving tenant moving

	No	Yes	NA
Dismissed by plaintiff	0.2703	0.2297	0.5000
Dismissed by stipulation or agreement	0.3958	0.1979	0.4062
Dismissed with prejudice	0.0860	0.5914	0.3226
Dismissed without prejudice	0.1512	0.3140	0.5349
Weighted average	0.2387	0.2860	0.4752

Notes: The table above reports the proportion of dismissals in which the underlying court documents stated the tenant did not agree to move out, the tenant did agree to move out, or in which no information was available. Tenants who had already moved out are counted in the "Yes" column. The "Weighted average" row is the weighted average of the four types of dismissals. Data is based on a random sample of 400 cases from the district 1 courthouse.

Was there a payment plan? For cases ending in dismissal with the tenant agreeing to pay the landlord, the court documents sometimes potentially provide additional information on terms of repayment. Such documentation was available in 52.5% of cases in which the microfilms included information on the tenant paying the landlord. Overall, approximately 55% of dismissals that involved the tenant agreeing to pay the landlord involved a repayment plan in which the tenant agrees to pay in installments, with the remainder having already paid, agreeing to pay in lump-sum at a later date, or having partially paid with an agreement to pay in lump-sum at a later date.

Table D.6: Proportion of dismissals involving payments with payment plans

	No	Yes	NA
Dismissed by plaintiff	0.2353	0.7059	0.0588
Dismissed by stipulation or agreement	0.5714	0.4286	0.0000
Dismissed with prejudice	0.4706	0.3529	0.1765
Dismissed without prejudice	0.3571	0.6429	0.0000
Weighted average	0.4245	0.5482	0.0273

Notes: The table above reports the proportion of dismissals where, conditional on the tenant agreeing to pay the landlord, an installment plan was put into place. The "Weighted average" row provides the weighted average across the four types of dismissals weighted by their relative frequency.

E Appendix: Sample linkage robustness

Table E.1 explores the extent of attrition in the credit bureau data and shows that attrition is unrelated to judge stringency. Specifically, we regress an indicator for the individual appearing in the column year on judge stringency, conditional on the individual appearing in the row year. We perform this regression for every pair of years.

Table E.1: Sample attrition and judge stringency

	2007	2009	2010	2011	2013	2015	2017
2005	0.001	-0.003	-0.000	0.007	-0.003	0.003	-0.000
	(0.007)	(0.007)	(0.008)	(0.007)	(0.009)	(0.009)	(0.007)
	[0.9962]	[0.9963]	[0.9956]	[0.9951]	[0.9941]	[0.9918]	[0.9917]
2007		-0.002	-0.003	0.007	0.001	0.005	0.001
		(0.008)	(0.008)	(0.007)	(0.009)	(0.008)	(0.007)
		[0.9963]	[0.9955]	[0.9951]	[0.9939]	[0.9916]	[0.9916]
2009			0.000	0.008	0.001	0.004	0.003
			(0.006)	(0.007)	(0.009)	(0.008)	(0.007)
			[0.9961]	[0.9953]	[0.9938]	[0.9917]	[0.9917]
2010				0.008	0.000	0.005	0.002
				(0.005)	(0.008)	(0.008)	(0.007)
				[0.9976]	[0.9944]	[0.9921]	[0.9917]
2011					-0.000	0.004	0.001
					(0.008)	(0.008)	(0.007)
					[0.9947]	[0.9923]	[0.9917]
2013						0.002	0.003
						(0.007)	(0.006)
						[0.9933]	[0.9921]
2015							0.000
							(0.006)
							[0.9935]

Notes: This table regresses an indicator for the individual appearing in the column year on judge stringency, conditional on the individual appearing in the row year. Standard errors are robust and clustered at the judge-year level.

Table E.2 regresses an indicator for being matched to the Experian data on case, defendant, and plaintiff characteristics. The table suggests that the matched sample is less likely to have their case end in eviction, owe less money, have legal representation, and are from somewhat more affluent neighborhoods.

Table E.2: Exploring characteristics of matches

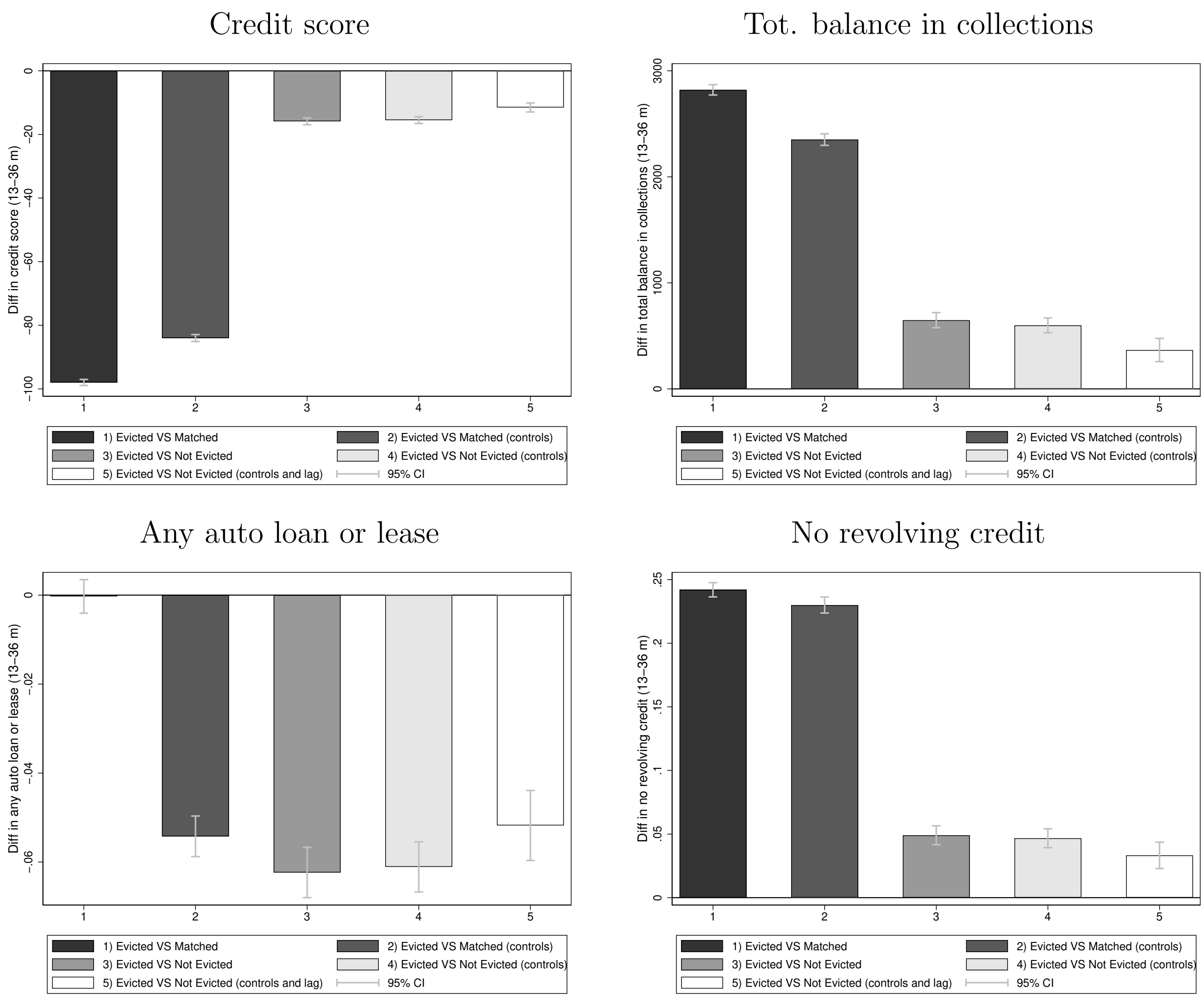
Evicted	-0.019***
	(0.002)
Pred. Af-American	0.237***
	(0.015)
Pred. White	0.194***
	(0.015)
Pred. Hispanic	0.065***
	(0.015)
Pred. Asian	0.128***
	(0.016)
Ad Damnum (1000s)	-0.004***
	(0.000)
Joint Action	0.073***
	(0.002)
Defend. Pro Se	-0.015***
	(0.004)
Plaintiff Pro Se	-0.036***
	(0.002)
Med. Household Inc. (1000s)	0.001***
	(0.000)
Pct. Af.-Am	-0.007*
	(0.004)
Filing Year 2006	0.009**
	(0.004)
Filing Year 2007	0.014***
	(0.004)
Filing Year 2008	-0.001
	(0.004)
Filing Year 2009	0.023***
	(0.004)
Filing Year 2010	0.042***
	(0.004)
Filing Year 2011	0.057***
	(0.004)
Filing Year 2012	0.060***
	(0.004)
Filing Year 2013	0.049***
	(0.004)
Filing Year 2014	0.052***
	(0.004)
Filing Year 2015	0.043***
	(0.004)
Filing Year 2016	0.029***
	(0.004)
Number of observations	390,277
R^2	0.0196
Mean of dep. var.	0.6130

Notes: This table explores individual and case characteristics that predict being able to successfully match to an Experian credit file. The sample is the court file with the IV sample restrictions imposed, and where we keep only post-March 2005 cases (the only cases that may have a pre-case filing match). The dependent variable is an indicator for a successful match to any credit file.

F Appendix: Selection into eviction court

This section presents bar charts analogous to Figure 4 for the other key financial strain outcomes.

Figure F.1: Selection into eviction court



Notes: Column 1 plots the difference in the outcome 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 reproduces column 1 with flexible demographic controls (age, gender, and year). Column 3 plots the difference in the outcome at 13-36 months after filing, for evicted versus non-evicted in the court sample. Column 4 reproduces column 3 with flexible demographic controls. Column 5 reproduces column 4 with an additional control for individual credit score in the pre-filing period.

G Appendix: DiD estimates and event studies

DiD estimates: This section discusses the main DiD estimates of the effect of an eviction. We then present additional event studies, analogous to those presented in the main text but modified to include the random sample from Cook County. This modified regression allows us to depict both selection into eviction court as well as selection into the eviction outcome.

Table 2 in the main text 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 results are estimated from the parametric specification of the event studies depicted in Figure 6. Table G.1 provides additional robustness showing the baseline results, results using individual fixed effects, results using a balanced panel, and results restricted to the first eviction cases only. Across specifications, the estimated causal effects are very similar. Table3 in the main text and and Appendix Table G.2 provide similar estimates for mobility and neighborhood poverty.

Event Studies Figure G.1 presents event studies comparing evicted and non-evicted individuals in the court sample to the 10 percent random sample from Cook County. The random sample is restricted to those over age 21 with no open mortgage trade, and randomizing a placebo filing month. We also reweight the regression sample so that placebo individuals' distribution across ZIP codes matches that of the eviction court sample. We exclude the baseline month from the regression sample since baseline credit score is included as a control. The event study regression is:

$$y_{it} = X_{it}\tilde{\alpha} + \tilde{\gamma} \times court_i + \tilde{\delta} \times E_i + \sum_{r=S}^{F} \tilde{\beta}_r + \sum_{r=S}^{F} \tilde{\gamma}_r \times court_i + \sum_{r=S}^{F} \tilde{\delta}_r \times E_i + \tilde{\epsilon}_{it} \tag{G.1}$$

where r represents the month relative to the case filing month. The vector X_{it} represents individual controls, including age at filing month, gender, and baseline credit score. The variable $court_i$ is an indicator for the individual being in the eviction court sample, and 0 if the person is in the random sample; $evict_i$ represents an indicator for being in the court sample and the case outcome being eviction, $\tilde{\beta}_r$ represents coefficients on indicators for month relative to filing month, $\tilde{\gamma}_r$ are the coefficients on indicators for relative month interacted with the court sample indicator, and $\tilde{\delta}_r$ are the coefficients on relative month interacted with the eviction indicator. For this analysis S=-41 and F=72, and the omitted month is S=-42. Figure G.1 plots $\tilde{\beta}_r$, $\{\tilde{\gamma} + \tilde{\gamma}_r\}$, and $\{\tilde{\delta} + \tilde{\delta}_r\}$. For all series we add in the omitted group mean in time r=-42 so that the magnitudes are interpretable.

Figure G.2 shows event studies for collection debt, decomposed by the type of debt. Figures G.3-G.6 show the event studies conditional on observable case characteristics. Finally, Figure G.7 shows the event studies for the key financial distress outcomes, but splitting the "Not Evicted" groups into those whose case was dismissed with prejudice, and all other forms of dismissal.

Table G.1: DiD estimates under alternative specifications

	Baseline	Indiv. f.e.	Balanced Panel	No prior cases
	(1)	(2)	(3)	(4)
I. *Credit Score*				
12-Month Effect	-2.828***	-2.470***	-2.861***	-3.564***
	(0.369)	(0.359)	(0.538)	(0.484)
36-Month Effect	-2.175***	-1.219***	-4.148***	-2.820***
	(0.386)	(0.373)	(1.257)	(0.503)
60-Month Effect	-2.133***	-0.838*		-3.158***
	(0.451	(0.434)		(0.582)
Number of individuals	251,036	251,036	66,952	153,664
Number of observations	1,302,930	1,302,930	392,458	797,230
II. *Total Collections*				
12-Month Effect	191.218***	162.148***	186.910***	234.466***
	(22.971)	(21.972)	(34.152)	(28.241)
36-Month Effect	157.990***	125.625***	233.577***	262.298***
	(27.426)	(27.127)	(79.416)	(33.189)
60-Month Effect	-0.982	-61.718*		94.218**
	(31.355	(31.659)		(37.981)
Number of individuals	252,718	252,718	67,042	155,233
Number of observations	1,310,057	1,310,057	392,934	803,911
III. *Any Auto Loan*				
12-Month Effect	-0.012***	-0.012***	-0.011***	-0.015***
	(0.002)	(0.002)	(0.003)	(0.003)
36-Month Effect	-0.013***	-0.011***	-0.011*	-0.015***
	(0.002)	(0.002)	(0.006)	(0.003)
60-Month Effect	-0.003	-0.003		-0.006**
	(0.003	(0.003)		(0.003)
Number of individuals	254,578	254,578	67,570	156,379
Number of observations	1,320,322	1,320,322	396,173	810,348
IV. *No Revolving Credit*				
12-Month Effect	0.014***	0.014***	0.009***	0.019***
	(0.002)	(0.002)	(0.003)	(0.003)
36-Month Effect	0.015***	0.014***	0.037***	0.024***
	(0.003)	(0.003)	(0.008)	(0.003)
60-Month Effect	0.009***	0.006**		0.016***
	(0.003	(0.003)		(0.004)
Number of individuals	254,578	254,578	67,570	156,379
Number of observations	1,320,322	1,320,322	396,173	810,348

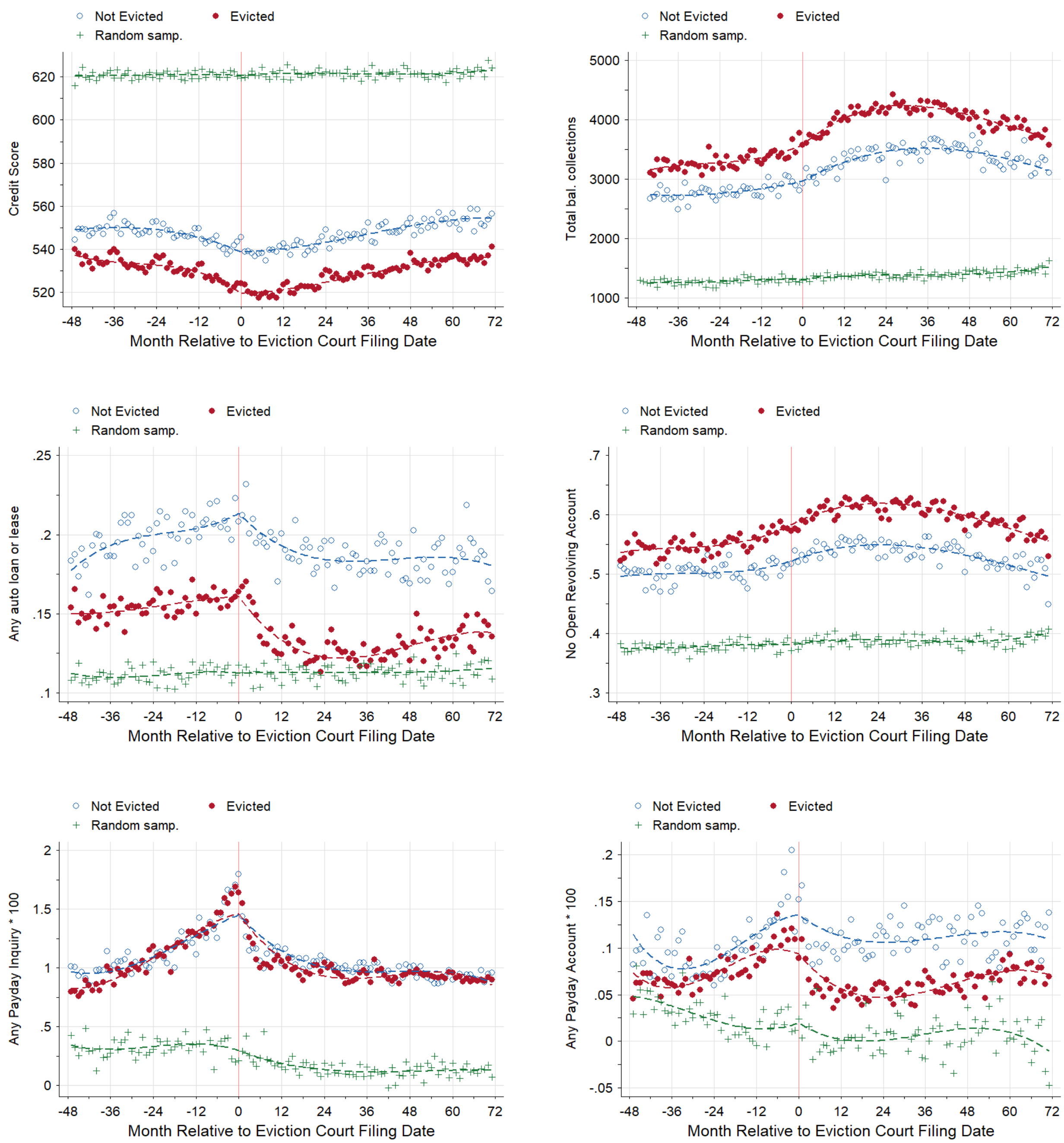
Notes: The table above re-estimates the regressions of Table 2 under alternative specifications. Column 1 presents the baseline specification of Table 2, column 2 includes individual fixed effects, column 3 limits the sample to a balanced panel (cases from March 2008 to March 2014 and with relative month $r = -36$ to 36), and column 4 restricts the sample to individuals with no prior eviction cases.

Table G.2: DiD estimates under alternative specifications: moves and neighborhood quality

	Baseline	Indiv. f.e.	Balanced Panel	No prior cases
	(1)	(2)	(3)	(4)
I. *Move Zipcode*				
1-Year Effect	0.004	0.004	0.014**	0.003
	(0.005)	(0.006)	(0.007)	(0.007)
3-Year Effect	0.003	0.003	0.007	0.002
	(0.006)	(0.006)	(0.008)	(0.007)
5-Year Effect	0.000	-0.004		-0.005
	(0.006	(0.007)		(0.008)
Number of individuals	115,023	115,023	72,849	86,175
Number of observations	770,472	770,472	270,740	455,523
II. *Neighborhood poverty rate (×100)*				
1-Year Effect	0.202***	0.162**	0.223***	0.254***
	(0.066)	(0.068)	(0.084)	(0.084)
3-Year Effect	0.127	0.249***	0.323***	0.133
	(0.085)	(0.082)	(0.109)	(0.108)
5-Year Effect	0.090	0.156*		0.073
	(0.103	(0.092)		(0.130)
Number of individuals	115,022	115,022	72,845	86,174
Number of observations	790,598	790,598	270,669	467,604

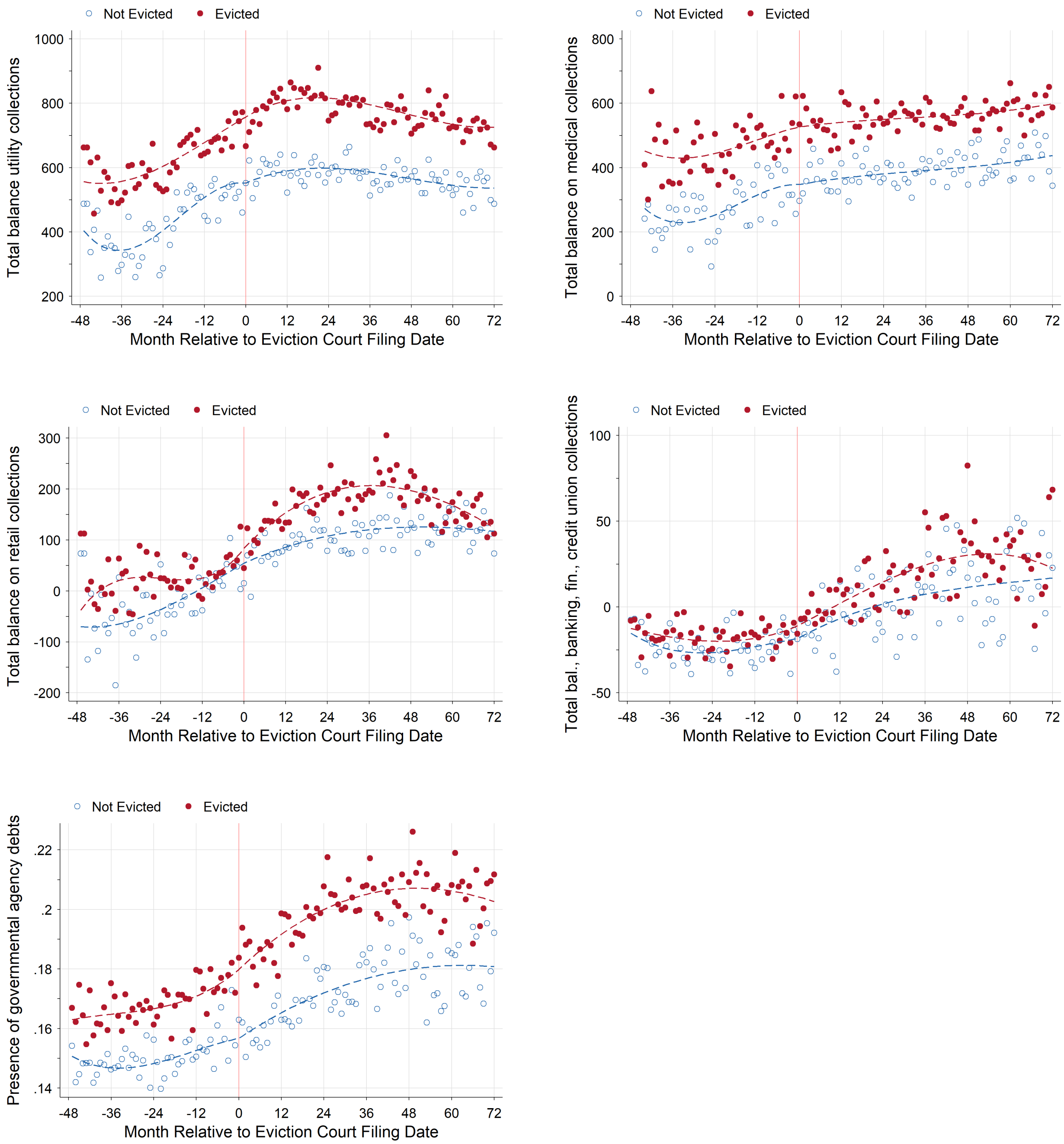
Notes: The table above re-estimates the regressions of Table 3 under alternative specifications. Column 1 presents the baseline specification of Table 3, column 2 includes individual fixed effects, column 3 limits the sample to a balanced panel (cases from March 2008 to March 2014 and with relative month $r = -36$ to 36), and column 4 restricts the sample to individuals with no prior eviction cases.

Figure G.1: Selection into eviction court



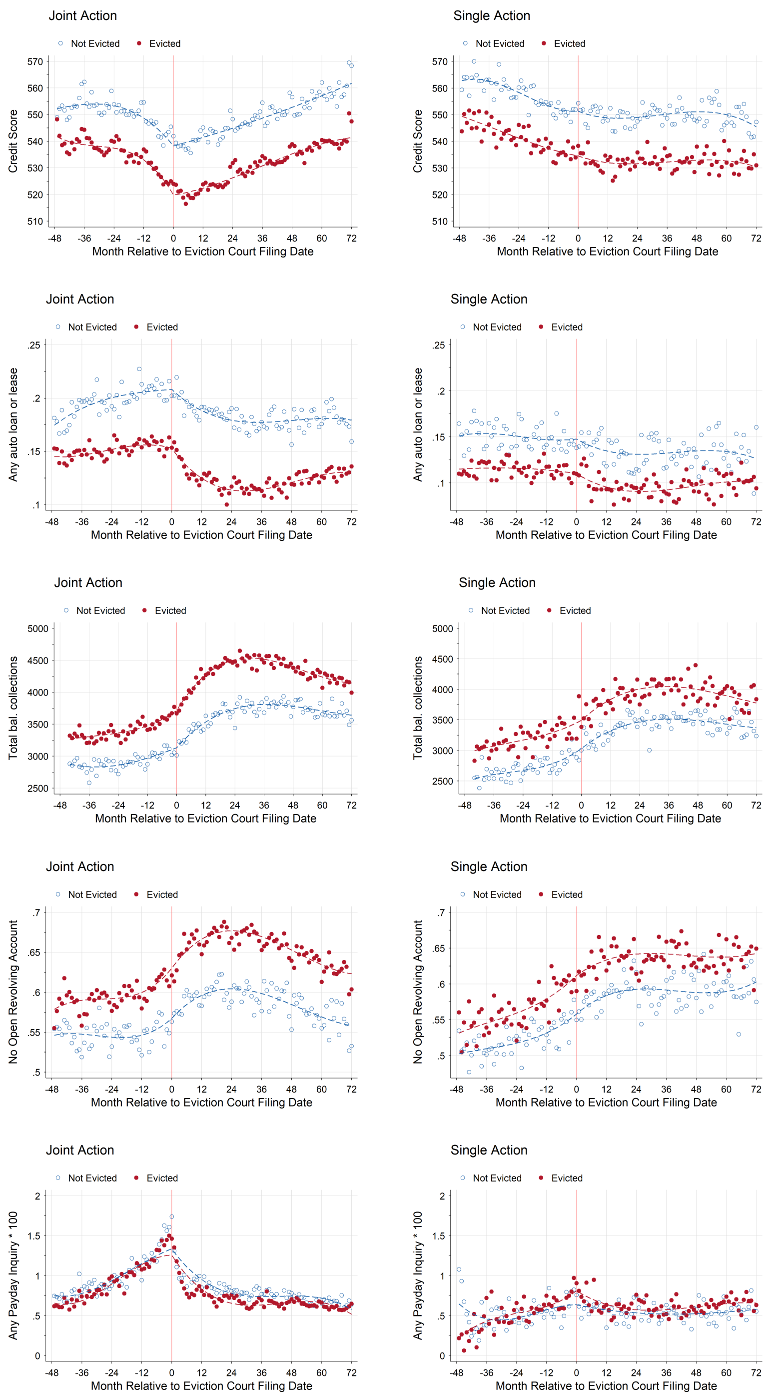
Notes: The figure above plots estimates of Equation G.1. The only control used is calendar year of the credit file. The omitted month is -48. Overlaid is a parametric specification where the right hand side variables include a cubic in relative month in the months leading up to eviction filing ($r < 0$), a cubic in relative month for the months following eviction filing ($r \geq 0$), and these two cubics interacted with eviction case outcome, and these cubics interacted with an indicator for being in the court sample. We use a random 50 percent subset of our 10 percent random sample in the regressions underlying these plots.

Figure G.2: Components of unpaid bills: types of collections and government agency debt



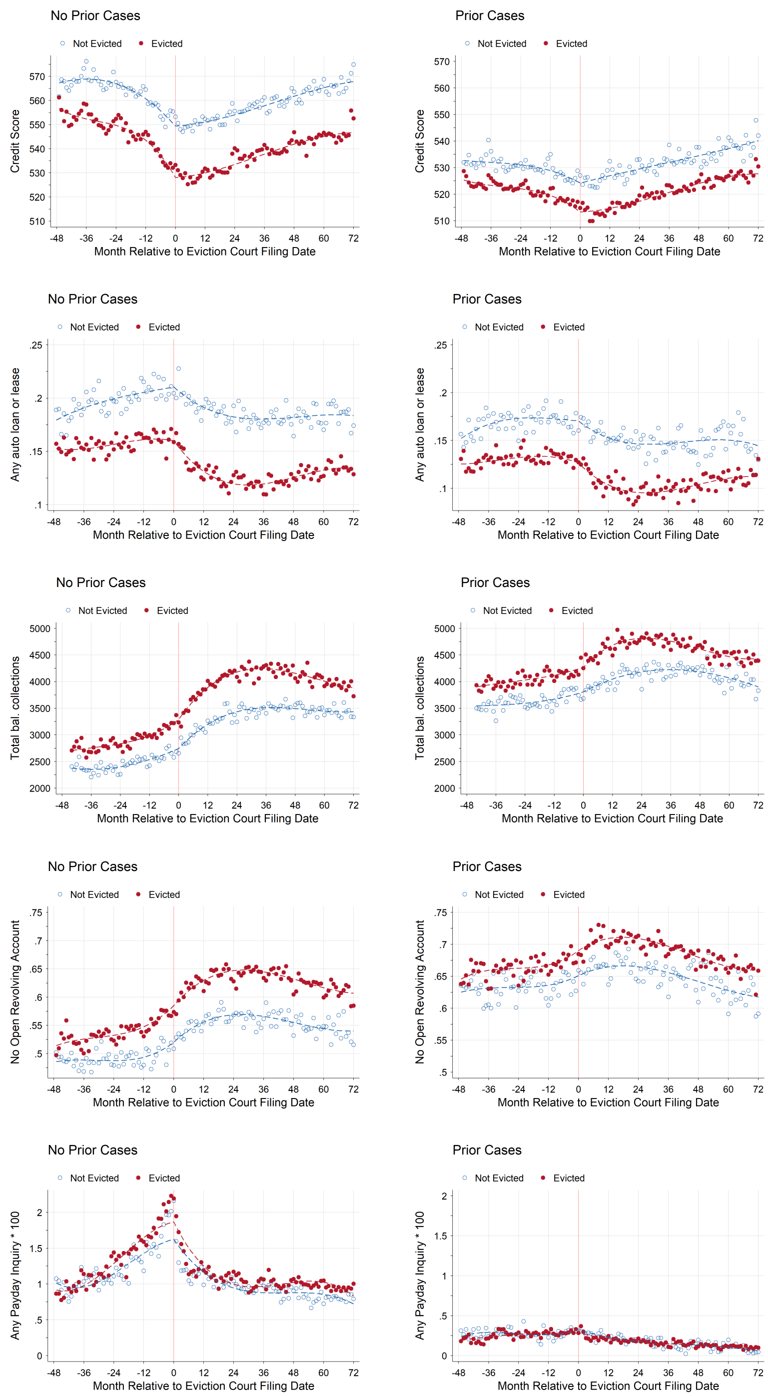
Notes: The figure is constructed as in Figure 6. The top four panels display four types of collections debt (utilities, medical, banking/financial/credit union, retail). Medical collections are only available as a separate category from years 2013, 2015, 2017. The bottom left panel reflects the presence of outstanding government agency debts (including default student loans, tax liens, unpaid child/family support).

Figure G.3: Event studies: heterogeneity by case type



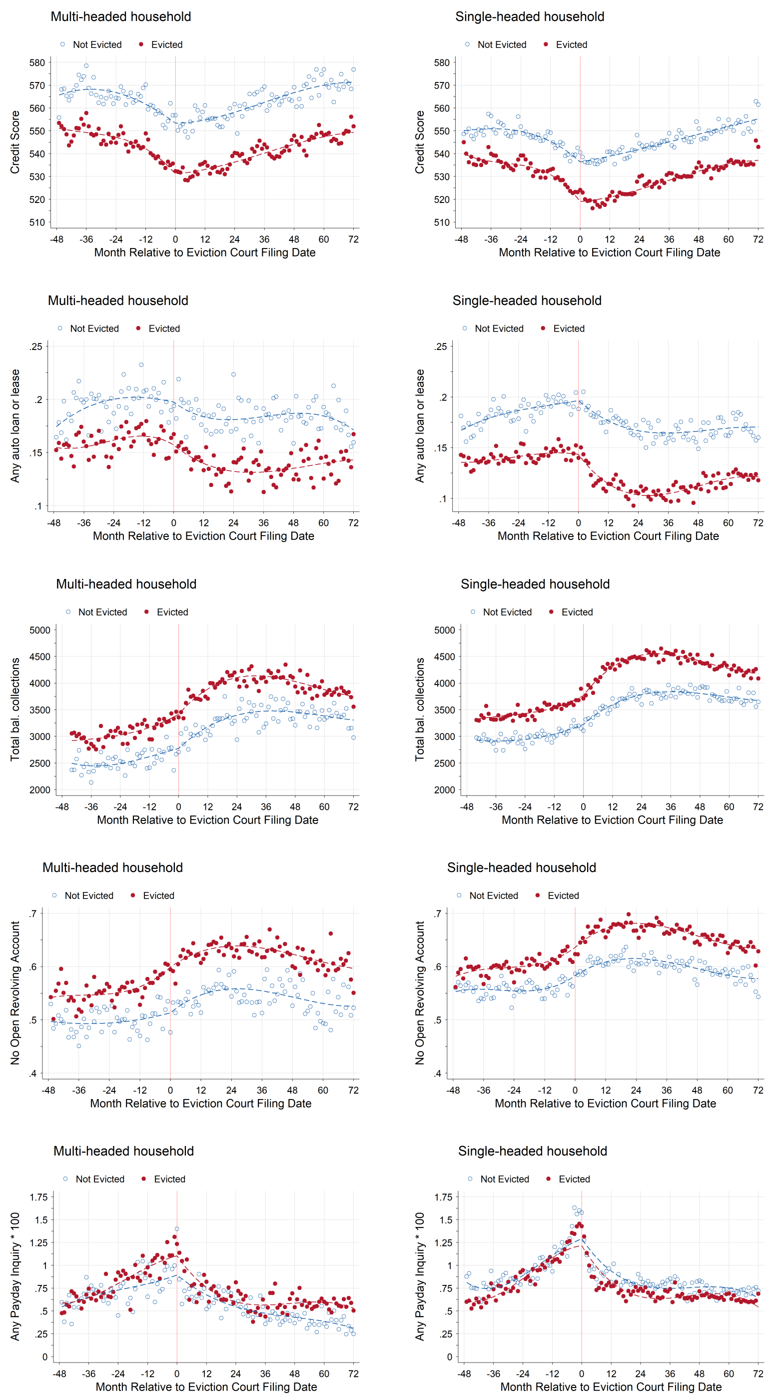
Notes: The figure is constructed as in Figure 6, separated by case type.

Figure G.4: Event studies: heterogeneity by prior cases



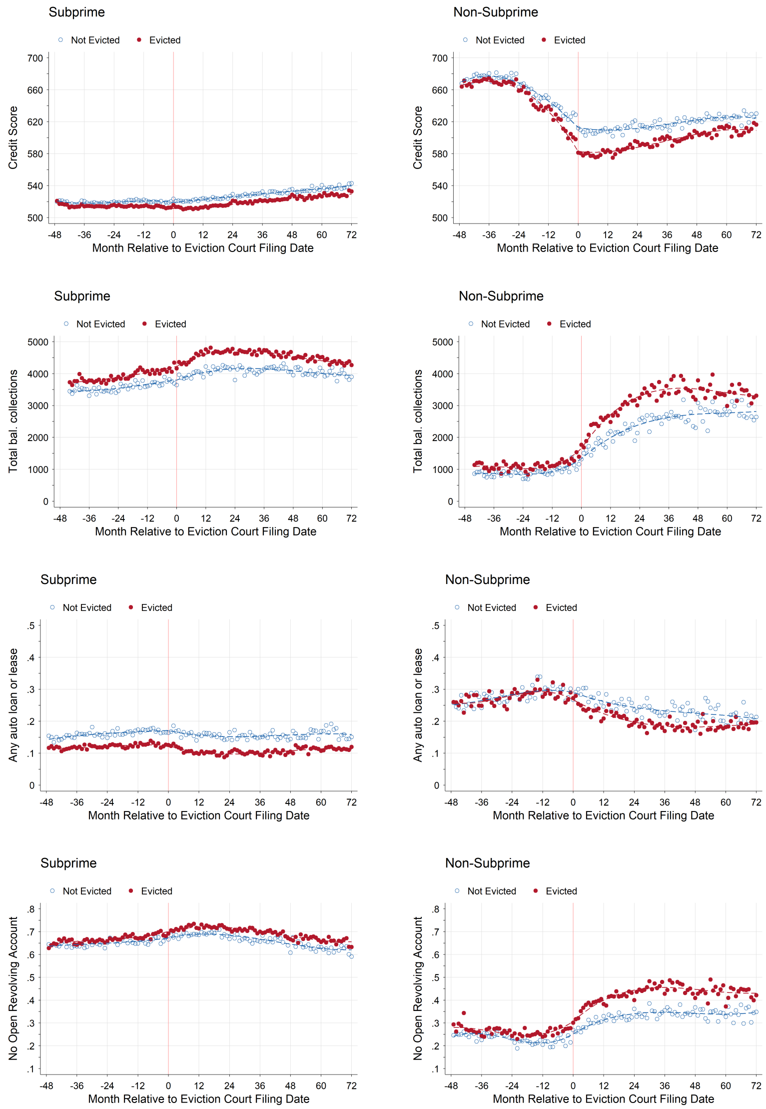
Notes: The figure is constructed as in Figure 6, separated by whether the individual had a prior eviction case or not.

Figure G.5: Event studies: heterogeneity by multi versus single-headed household



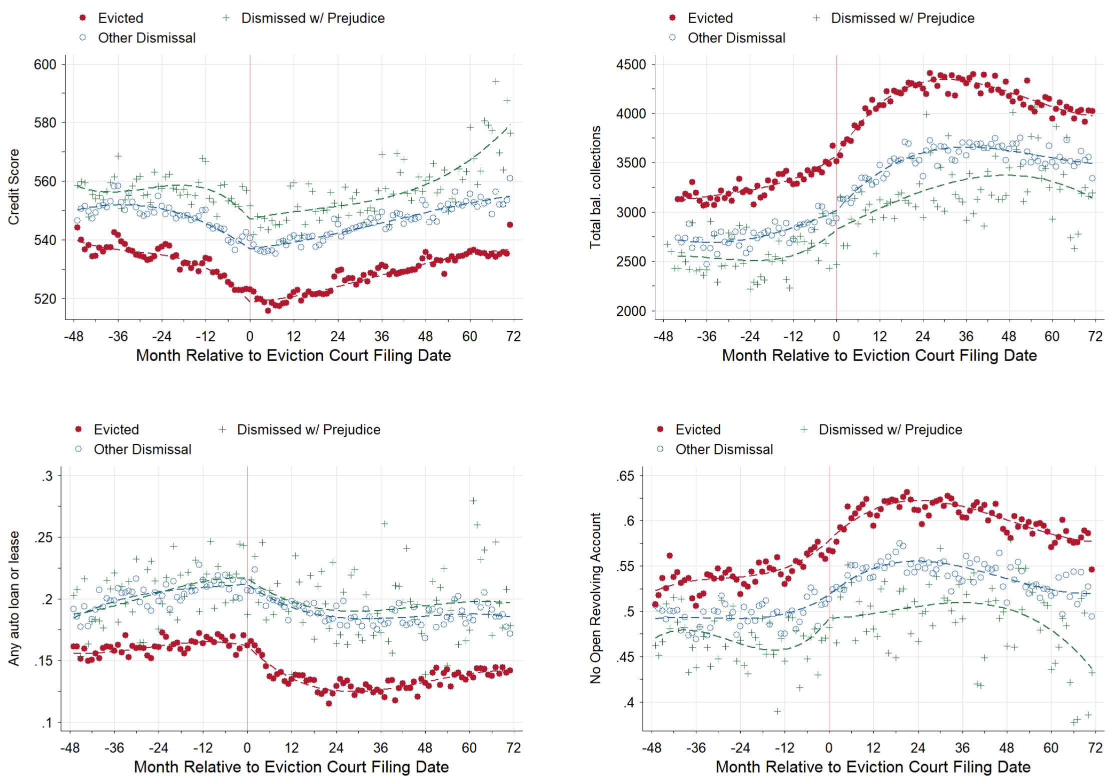
Notes: The figure is constructed as in Figure 6, separated by whether the eviction court case names multiple individuals as signatories on the lease or only one individual. Note the number of signatories of the lease may not correspond exactly to the number of household heads.

Figure G.6: Event studies: heterogeneity by prime versus subprime borrowers at baseline



Notes: The figure is constructed as in Figure 6, separated by subprime versus prime credit score – determined by a credit score cutoff of 600 – in the credit record two periods prior to eviction filing.

Figure G.7: Event studies: disaggregating dismissals by type



Notes: The figure is constructed as in Figure 6, but separating the eviction dismissal outcome into "dismissed with prejudice" and "other dismissal".

H Appendix: IV analysis

H.1 Construction of judge stringency

Sections 3.3 and 6.2 provide details on sample restrictions and the construction of judge stringency used in the paper. This subsection evaluates alternative constructions of the judge stringency variable. All measures are leave-out estimates adjusted for district-year fixed effects and are calculated using the same sample unless otherwise stated. Table H.1 shows the 2SLS estimates of evictions on the outcomes associated with financial strain using five alternative constructions of judge stringency. The first row constructs a rolling average for each judge based on the prior 50 and next 50 cases. The second row calculates stringency by judge-year, but only for judges that see at least 100 cases that year. The third row constructs stringency by judge rather than judge-year. The fourth row calculates judge stringency by judge, year, and case type (single vs. joint action). The final row estimates stringency by judge and case type (single vs. joint action). Across the 5 specifications, the point estimates are broadly similar to the main results of the paper. The main specification also falls in the middle of the alternate estimates except for inquiries 37-60 months out, where the main specification is more negative than other specifications.

While we use a continuous measure of judge stringency throughout the paper, Table H.2 provides several specifications using judge fixed effects directly as our instruments, which, for the most part, produce similar point estimates. The first row uses judge dummies, the second row uses judge-by-year dummies, the third row uses interacted judge-case-type dummies, and the fourth row uses judge dummies with statistically predicted time breaks. There are trade-offs to each of these specifications. Given seventeen years of data, some judges drift in their stringency over time, which will be missed using judge dummies, yet using judge-by-year dummies creates a very large number of instruments, many of which have correlations close to 1. The final specification uses conditional inference trees using filing date to identify partitions of the data that best predict case outcomes, and then includes dummy variables for each judge partition. The last approach substantially reduces the number of instruments compared to the judge-by-year case, while still allowing stringency to potentially vary over time for each judge, but suffers from the fact that it first uses the data to identify partitions. Overall, results are qualitatively similar for all specification except for the specification including judge-by-year dummies which has large standard errors, as would be expected when including many highly correlated instruments.

Table H.1: Robustness of IV estimates to construction of judge stringency

specification	*Cred. Score (13-36)*	*Cred. Score (37-60)*	*Auto (13-36)*	*Auto (37-60)*	*Collections (13-36)*	*Collections (37-60)*	*No Rev. (13-36)*	*No Rev (37-60)*	*Accounts (13-36)*	*Accounts (37-60)*	*Inquiries (13-36)*	*Inquiries (37-60)*
Rolling avg stringency:												
	-12.64	-18.51**	-0.12**	-0.04	-307.26	-1037.34**	0.09*	0.03	-0.38	0.18	0.58	-8.19**
	(7.74)	(6.77)	(0.04)	(0.04)	(482.67)	(493.48)	(0.05)	(0.05)	(1.58)	(1.42)	(4.59)	(3.59)
Stringency (req 100 cases per year)												
	-9.33	-7.69	-0.03	-0.05	-11.42	-622.66	0.09*	0.12*	3**	0.84	4.43	-6.71*
	(8.16)	(9.11)	(0.03)	(0.05)	(471.75)	(582.83)	(0.05)	(0.06)	(1.35)	(1.35)	(3.49)	(3.91)
Stringency (no year):												
	-7.51	-5.26	-0.07*	-0.08	383.08	-247.19	0.12**	0.11*	3.35*	0.74	8.64**	-3.84
	(8.46)	(9.87)	(0.04)	(0.05)	(492.82)	(578.46)	(0.06)	(0.07)	(1.72)	(1.44)	(4.37)	(4.34)
Stringency by case type:												
	-21.85**	-31.61**	-0.02	-0.07**	-150.57	-117.47	0.12**	0.15**	4.61**	1.97	10.77**	-0.93
	(6.62)	(7.33)	(0.03)	(0.03)	(406.62)	(431.78)	(0.05)	(0.05)	(1.21)	(1.31)	(3.86)	(3.42)
Stringency by case type (no year):												
	-16.49**	-22.55**	-0.04	-0.06*	355.4	118.24	0.12**	0.12**	3.6**	2.23*	10.57**	1.19
	(6.28)	(7.27)	(0.03)	(0.03)	(387.52)	(418.48)	(0.05)	(0.05)	(1.35)	(1.23)	(4.19)	(3.43)

Notes: This table shows 2SLS estimates based on alternative constructions of the judge stringency measure. "Rolling avg stringency" calculates judge stringency using a rolling average of the 50 prior cases and the next 50 cases (excluding own case). "Stringency (req 100 cases per year)" requires a judge sees at least 100 cases per year in order for their year-specific stringency measure to be calculated and included in the IV analysis. "Stringency (no year)" calculates a single measure of judge stringency rather than year-specific measures of judge stringency. "Stringency by case type" calculates judge stringency separately for single action and joint action cases each year. "Stringency by case type (no year)" calculates judge stringency separately for single action and joint action cases, with a single number per judge rather than calculating the number for each year. Except for the differences listed above, all regressions otherwise follow the same sample restrictions and specifications as the regressions in Section 7. "Auto" is an indicator for having any auto loan or lease. 'Collections" is the amount in collections. "No Rev." is an indicator for having no revolving line of credit such as a credit card. "Accounts" is the number of sub-prime loans (such as pay-day loans) held by the individuals. "Inquiries" is the number of inquiries for sub-prime loans made by the individual.

Table H.2: Robustness of IV results to using judge fixed effects directly

	Credit (12-36)	Credit Score (36-60)	Auto (12-36)	Auto (36-60)	Coll. (12-36)	Coll. (12-36)	No Rev. (12-36)	No Rev (36-60)
Judge dummies:								
	-7.33	-1.62	-0.04*	-0.07**	4.39	-170.18	0.07*	0.07
	(5.48)	(6.35)	(0.02)	(0.03)	(344.59)	(386.08)	(0.04)	(0.05)
Judge-by-year dummies:								
	-29.57	106.97	-0.56	0.11	4252.5	-4454.35	0.54	0.95
	(70.49)	(144.55)	(0.66)	(0.47)	(6531.08)	(6127.99)	(0.75)	(1.18)
Judge-by-case-type dummies:								
	-14.94**	-14.15**	-0.02	-0.07**	213.18	233.68	0.07**	0.07*
	(4.08)	(5.14)	(0.02)	(0.02)	(280.23)	(298.04)	(0.03)	(0.04)
Judge dummies with time breaks:								
	-8.49*	-9.2	-0.05**	-0.08**	188.77	-46.74	0.06*	0.03
	(4.63)	(6.06)	(0.02)	(0.03)	(246.15)	(322.88)	(0.03)	(0.04)

Notes: The table above reports the coefficient on eviction from the 2SLS regressions using judge dummies in the first stage rather than the continuous measure of judge ruling stringency used in Sections 6 and 7. "Judge dummies" includes a single dummy for each judge. "Judge-by-year dummies" includes judge-by-year dummies. "Judge-by-case-type dummies" includes judge dummies interacted by case type (single or joint action). "Judge dummies with time breaks" first estimates a conditional inference tree (Hothorn et al., 2006) for each judge using a date to predict evictions. Conditional inference trees use a recursive partitioning algorithm similar to CART, but it additionally addresses the tendency of recursive partitioning algorithms such as CART to overfit the data. The algorithm additionally requires bins of at least 10 continuous cases. The first stage is then run using a fixed effect for each identified bin. This allows multiple fixed effects over time for judges whose stringency varies over time, without introducing many highly correlated fixed effects for judges whose stringency does not vary over time as is the case when using judge-by-year dummies.

H.2 Robustness checks for the first stage

This section provides the first stage regression of eviction on judge stringency and a number of additional robustness checks to the sample selection and modeling assumptions made in the paper. Table H.3 reports the first stage regression run on the sample of court cases meeting the IV-analysis requirements laid out in Table D.1.

Table H.4 documents how the first stage regression of residual judge stringency changes under a number of alternative specifications. The first row provides the main specification, the second row controls for residual judge stringencies in granting continuances, judgment amount in joint action cases, and granting stays.

If serving the defendant takes much longer than expected, a new court date can be assigned that does not always correspond to the same judge. In the third row of Table H.4, we construct an alternative judge stringency measure based on the first judge observed in the case history rather than the judge assigned at filing. Rows 4 to 7 use alternative sample selection criteria. Row 4 includes all cases, including cases against businesses, row 5 restricts cases to judges who see more than 100 cases that year, row 6 excludes cases where the court record does not show the tenant ever being served,[59] and row 7 includes all judges rather than those who see more than 10 cases. Lastly, rows 8 and 9 estimate the first stage using a split sample. Row 8 estimates the first stage using stringency constructed from single action cases on eviction in joint action cases, while row 9 does the opposite. Across all of our robustness checks, we find that the coefficient on residual stringency remains similar to the main specification, positive, and statistically significant with small standard errors. These checks demonstrate that our first stage is robust to adding additional controls, imposing alternative sample selection criteria, adjusting the method for identifying the first judge assigned to the case, and estimating stringency using a split-sample approach.[60]

[59]For a case to proceed the tenant must be served, or the judge must allow for an ex-parte judgment to be made after they determine a sufficient effort at serving the defendant has been made. In a number of cases, the plaintiff voluntarily dismisses the case after multiple attempts at serving the defendant, which could happen, for example, if the defendant moves.

[60]Frandsen et al. (2019) establishes that, even if monotonicity is violated, informative causal effects can still be recovered if an "average monotonicity" condition holds, for which they propose regressions similar to those reported in Table H.4.

Table H.3: First stage on full analysis sample

	Eviction
Stringency	0.715***
	(0.027)
Ad damnum	0.00000***
	(0.00000)
Median rent	−0.068***
	(0.006)
Pct black (tract)	0.102***
	(0.006)
Male	0.008***
	(0.001)
Black	−0.017
	(0.014)
No attorney	0.154***
	(0.006)
Constant	0.490***
	(0.016)
Observations	508,543
R^2	0.019
F Stat	86.681
Partial F Stat	1158.9

Notes: *p<0.1; **p<0.05; ***p<0.01. The table above reports the first stage of the regression. Standard errors are robust and clustered at the judge-year level. The regression includes district-year fixed effects, but they are excluded from the table. "Partial F-Stat" reports the F-statistic for the coefficient on stringency.

Table H.4: Specification check on stringency in first stage

Sample	Coefficient	Standard Errors	P-Value	Observations
Main	0.715	0.027	0.000	$508,543$
Controlling for other judge chars.	0.725	0.028	0.000	$506,971$
Alternate first judge construction	0.729	0.024	0.000	$498,559$
All cases	0.630	0.027	0.000	$576,042$
Excluding judges <100 cases	0.805	0.025	0.000	$497,853$
Including cases never served	0.725	0.027	0.000	$458,451$
Including judges <10 cases	0.674	0.029	0.000	$509,169$
Single-Action stringency on Joint-Action cases	0.294	0.027	0.000	$396,819$
Joint-Action stringency on Single-Action cases	0.949	0.092	0.000	$101,460$

Notes: This table shows the coefficient on our measure of residual judge stringency for a number of alternative specifications. For restrictions affecting the sample selection, the stringency measure is recalculated on that sample. Standard errors are clustered at the judge-year level. The "Controlling for other judge chars" specification includes controls for the judge's propensity to grant stays and continuances, average case length, and their average judgment amount difference (which is defined as the ruling amount minus the ad damnum in joint action cases ending in an eviction order). The alternate first-judge construction assigns judge using the first judge named in the docket after the tenant has been served.

H.3 Exclusion restrictions checks

The exclusion restriction requires that judge stringency affects tenant outcomes only through the eviction decision. This assumption could be violated if judges affect tenant outcomes through dimensions other than the eviction order, and these other dimensions are correlated with their tendency to evict. In this section we provide several pieces of evidence that the exclusion restriction is not violated in our setting. The main concern in this empirical setting is that, conditional on an eviction order, judges additionally make decisions about how much the tenant must pay. In addition, judges affect two other potentially relevant aspects of the case: they can grant continuances that provide the tenant additional time to seek legal advice or aid, and they can grant stays of eviction orders, which delay when the landlord can file the eviction order with the Sheriff's Office. For each of these, we construct a measure of residual judge stringency following the same approach we used for eviction stringency with two exceptions. First, residual judgment amount is calculated as the residual variation of the judgment amount minus the ad damnum amount at case filing for joint action cases ending in eviction. Second, residual stays granted is similarly restricted to cases ending in eviction orders.

Table H.5 reports the correlations between the four residual stringency measures. Judge eviction stringency has a low correlation with all four other stringency measures, with the largest correlation being a correlation of 0.098 between eviction stringency and residual judgment amount. This suggests that there is little relationship between the four measures.

Table H.6 provides additional evidence that the three alternative stringency measures have little effect on the case outcome and do not predict eviction stringency. The first column regresses the eviction indicator on all four stringency measures, case and defendant controls, and district-year fixed effects. Eviction stringency remains an important determinant of the case outcome, while judgment amount stringency and continuance stringency are also weakly statistically significant predictors of eviction. The second column runs the same regression, but with eviction stringency as the left-hand-side variable, and we find that none of the other stringency variables are statistically significant predictors. This further suggest that there is no relationship between a judge's propensity to evict, and their impact on other dimensions of the case.

Table H.7 regresses log judgment amount on eviction stringency for joint action cases ending in an eviction with a positive money judgment. The first column runs the univariate regression, the second column additional controls for log ad damnum amount, and the third column additionally controls for case and defendant characteristics. All three specifications control for district-year fixed effects. Across specifications residual eviction stringency is statistically insignificant, suggesting that a judges' residual stringencies do not predict their judgment amounts in joint action cases ending in evictions.

Tables H.8 and H.9 reproduce the IV estimates for a subset of the financial strain outcomes accounting for judgment amount. Panel A shows that controlling for residual stringency in judgment amount has little impact on the first stage. Panel B shows estimates for (i) running the reduced form including judgment amount stringency, (ii) running the IV regressions controlling for judgment amount stringency, and (iii) jointly instrumenting for both the eviction order and judgment amount have little impact on the point estimates.

Table H.5: Correlations between residual stringency measures

	Stringency	Continuance	Amount	Stays
Residual stringency	1	-0.070	0.098	0.051
Residual continuance	-0.070	1	0.039	0.008
Residual judgment amount	0.098	0.039	1	0.081
Residual stays granted	0.051	0.008	0.081	1

Notes: This table shows correlations between various measures of residual judge behaviors. Residuals are leave-out means with district-year fixed effects removed.

Table H.6: Case outcomes on judge stringency measures

	Dependent variable:	
	Evicted	Eviction Stringency
	(1)	(2)
Stringency (eviction)	0.723***	
	(0.028)	
Stringency (amount)	0.00001*	0.00001
	(0.00000)	(0.00001)
Stringency (continuance)	−0.063*	−0.110
	(0.033)	(0.072)
Stringency (stays)	−0.024	0.120
	(0.050)	(0.111)
R^2	0.018	0.018
Adjusted R^2	0.018	0.017
Note:	*p<0.1; **p<0.05; ***p<0.01	

Notes: *p<0.1; **p<0.05; ***p<0.01. The first column shows coefficients from the regression of the eviction dummy on the four judge stringency measures. The second column regresses judge eviction stringency on the three other judge stringency measures. All stringency measures are leave-out averages adjusted by district-year fixed effects. Both regressions include controls for case and defendant characteristics and district-year fixed effects. Robust standard errors clustered at the judge-year level are reported in the parentheses.

Table H.7: Log judgment amount on judge stringency

	Log judgment amount:		
	(1)	(2)	(3)
Stringency	−0.031	−0.032	−0.033
	(0.109)	(0.085)	(0.087)
Log ad damnum		0.772***	0.741***
		(0.003)	(0.003)
Constant	7.186***	1.803***	2.634***
	(0.011)	(0.023)	(0.040)
Observations	255,770	255,770	198,510
R^2	0.095	0.621	0.609
Adjusted R^2	0.094	0.621	0.609
Note:	*p<0.1; **p<0.05; ***p<0.01		

Notes: *p<0.1; **p<0.05; ***p<0.01. This table shows regression of log judgment amount on judge stringency for joint action cases ending in a judgment for eviction. The first column includes only stringency and district-year fixed effects. The second column additionally controls for log ad damnum amount. The third column additionally controls for the standard set defendant characteristics. All results are for joint action cases ending in an eviction. Standard errors are clustered at the judge-year level.

Table H.8: Testing the exclusion assumption: financial strain

A: First stage

	First stage (1)	(2)
Stringency: eviction order	0.646***	0.649***
	(0.035)	(0.037)
Stringency: judgment amount		0.006
		(0.005)
Additional controls	Yes	Yes
Number of observations	232,834	

B: OLS and IV with additional stringency

	Credit Score		Collections Bal.		Auto Loan	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel I: Reduced Form						
Stringency: eviction order	-9.067*	-7.228	133.910	27.903	-0.039*	-0.037*
	(4.822)	(4.887)	(308.343)	(334.861)	(0.022)	(0.022)
Stringency: judgment amount		-0.157		-13.022		0.001
		(0.629)		(37.986)		(0.002)
Panel II: Evicted						
Eviction order	-14.151*	-11.225	209.391	43.445	-0.060*	-0.057*
	(7.459)	(7.660)	(482.336)	(521.178)	(0.035)	(0.034)
Stringency: judgment amount		-0.093		-13.278		0.001
		(0.630)		(39.339)		(0.002)
Panel III: Evicted and Judgment Amt.						
Eviction order	-10.728		117.005		-0.063*	
	(8.974)		(654.878)		(0.038)	
Judgment amount (1000s)	-0.211		-30.688		0.002	
	(1.426)		(91.857)		(0.005)	

Notes: This table tests the exclusion assumption underlying the IV analysis. Panel A, column 1, shows the first stage of the main IV regression and, in column 2, the first stage with the additional stringency measure: residualized judgment amount stringency. Panel B.I shows the reduced form regressions of the outcome measure in the column heading on stringency measures, with all controls and district-year fixed effects. Panel B.II shows the IV regression of the outcome measure in the column heading controlling for the second stringency measure. Panel B.III shows the IV regression with two endogenous variables on the right hand side (eviction order and judgment amount) and the two stringency instruments.

Table H.9: Testing the exclusion assumption: moves and neighborhood quality

A: First stage

	First stage	
	(1)	(2)
Stringency: eviction order	0.631*** (0.038)	0.632*** (0.040)
Stringency: judgment amount		0.005 (0.006)
Additional controls	Yes	Yes
Number of observations	213,729	

B: OLS and IV with additional stringency

	Any Move		Neighborhood Poverty Rate		Eviction Case at Diff. Address (36 mo.)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel I: Reduced Form						
Stringency: eviction order	0.052	0.025	-1.252*	-1.340*	0.004	-0.008
	(0.043)	(0.043)	(0.656)	(0.693)	(0.029)	(0.031)
Stringency: judgment amount		-0.000		-0.009		0.005*
		(0.004)		(0.071)		(0.003)
Panel II: Evicted						
Eviction order	0.084	0.040	-2.027*	-2.150*	0.007	-0.012
	(0.069)	(0.069)	(1.062)	(1.137)	(0.046)	(0.049)
Stringency: judgment amount		-0.001		0.004		0.005*
		(0.004)		(0.079)		(0.003)
Panel III: Evicted and Judgment Amt.						
Eviction order	0.044		-2.169		-0.034	
	(0.077)		(1.392)		(0.055)	
Judgment amount (1000s)	-0.002		0.009		0.011	
	(0.010)		(0.186)		(0.007)	

Notes: This table tests the exclusion assumption underlying the IV analysis. Panel A, column 1, shows the first stage of the main IV regression and, in column 2, the first stage with the additional stringency measure: residualized judgment amount stringency. Panel B.I shows the reduced form regressions of the outcome measure in the column heading on stringency measures, with all controls and district-year fixed effects. Panel B.II shows the IV regression of the outcome measure in the column heading controlling for the second stringency measure. Panel B.III shows the IV regression with two endogenous variables on the right hand side (eviction order and judgment amount) and the two stringency instruments.

H.4 Monotonicity checks

In our context, monotonicity requires that a defendant receiving a stricter judge would have a weakly higher eviction probability. Given that the overall first stage estimate is positive, 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. Our data allows detailed subsamples, including interactions between judge characteristics and individual characteristics.

Basic monotonicity checks Table H.10 shows the coefficient on stringency[61] from the regression on being evicted on stringency and various controls, but restricted to a number of different subpopulations. Specifically, if judge stringency for any subpopulation were negatively related to eviction, it would provide evidence that monotonicity of judge leniency does not hold. The first row shows the coefficient from the full sample, while the remaining rows show the coefficient by case type, gender, attorney status, and race. Across all subsamples the coefficient on stringency keeps the same sign, providing evidence that monotonicity is not violated.

Judge-tenant interaction monotonicity checks To better explore potential monotonicity violations, we hand-collected data on judge characteristics. Using information from judge profiles in Sullivan's Judicial Profiles (2017) supplemented with additional online sources, demographic information was compiled on 176 of the judges who presided the most cases in our sample. Using this data, gender was determined based on their biographies in Sullivan's Judicial Profiles. Sullivan's Judicial Profiles is the only reference guide with profiles for all judges serving in Illinois. For each judge, we determined gender based on the use of feminine or masculine pronouns used in their respective biographies. Based on conversations with the people behind Sullivan's Judicial Profiles, the biographies are created based on surveys they send to the judges, and the gender of the pronouns for each judge's biography is determined based on the judge's self-chosen gender. We were able to identify gender for all 176 judges.

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

[61] Specifically, the leave-out mean of a judge's stringency adjusted for district-year fixed effects.

[62] Puerto Rican is coded as Hispanic.

when pictures were able to be found, and in many cases race was additionally confirmed in the associated text of the selected source. Perceived race was coded for 117 of the 176 judges.[63]

Table H.11 shows the breakdown of judges by race and gender. In particular, we see that sub-sampling judges based on gender and on being white or black provides us with enough data to adequately interact these characteristics with tenant characteristics. Each of these judge subsamples has at least 30 judges and over $100,000$ cases in total. In contrast, the sample only includes 8 Hispanic judges and a combined caseload of less than $20,000$ cases, suggesting that cross interactions between Hispanic judges and tenant characteristics will most likely suffer from a small sample size.

Table H.12 shows the coefficient for stringency from the regression of the case outcome on stringency and various controls, restricted to a number of different subpopulations that now include interactions between tenant and judge characteristics. Restricting our focus to the columns for male, female, white, and black judges, we see that all interactions result in positive judge stringency coefficients (all statistically significant at the 0.01 level), supporting our monotonicity claim. Perhaps unsurprisingly, cross interactions between Hispanic judges and tenant characteristics are mixed. To understand this results, we look more carefully at the p-values and number of observations for Hispanic judges $\times$ tenants in Table H.13. In particular, because we only require coefficients to be non-negative, if the negative coefficients are not statistically significant, we lack sufficient information to argue that these subsamples do indeed violate the judge monotonicity assumption.

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

Table H.10: Testing the monotonicity assumption

Sample	Coefficient	Standard Errors	P-Value	Observations
Main	0.715	0.027	0.000	508,543
Joint Action	0.631	0.028	0.000	406,943
Forcible Entry and Detainer	1.036	0.071	0.000	101,600
Males	0.663	0.033	0.000	219,430
Females	0.753	0.032	0.000	289,113
No attorney	0.726	0.029	0.000	489,844
Attorney	0.376	0.133	0.005	18,699
Black	0.798	0.033	0.000	272,629
Hispanic	0.807	0.076	0.000	63,556
Larger landlords	0.540	0.032	0.000	331,268
Smaller landlords	0.948	0.049	0.000	194,084

Notes: The table above reports the coefficient on judge stringency by subpopulation (robust standard errors clustered at the judge-year level), p-values, and the number of observations included in the regression. "Black" and "Hispanic" are imputed using each defendant's last name and census tract. Imputation defines a tenant as part of the group if the estimated posterior probability of being of that race is greater than 0.75. "Smaller landlords" are those with 5 or fewer cases ever appearing in the sample, and "larger landlords" are those with more than 5 cases appearing in the sample.

Table H.11: Judge characteristics breakdown

Sample	Male	Female	White	Black	Hispanic
Number of Judges	111	65	77	30	8
Number of Total Cases	365655	149056	233754	195965	19898
Stringency Diff. (10-90)	0.071	0.076	0.072	0.075	0.034

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

Table H.12: Monotonicity checks, two-way interactions

Pro Se Tenants	Male Judges	Female Judges	White Judges	Black Judges	Hispanic Judges
All	0.689	0.704	0.613	0.704	-0.372
Male	0.608	0.728	0.558	0.737	-1.082
Female	0.751	0.686	0.656	0.680	0.224
White	0.455	0.591	0.454	0.588	-3.392
Black	0.829	0.754	0.754	0.743	0.617
Hispanic	0.770	0.671	0.773	0.760	0.312

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

Table H.13: Monotonicity checks: Hispanic judges

Pro_Se_Tenants	Coefficient	P-Value	Observations
All	-0.372	0.744	19477
Male	-1.082	0.372	8673
Female	0.224	0.833	10804
White	-3.392	0.005	2529
Black	0.617	0.534	10482
Hispanic	0.312	0.713	2612

Notes: Table replicates the results from Table H.12 for Hispanic judges, additionally providing p-values and observations for each regression.

H.5 IV results by subpopulation

This section provides OLS and IV estimates splitting the data across four different sets of observable characteristics. Table H.14 shows results for outcomes related to financial strain while Table H.15 shows results for residential mobility and neighborhood poverty.

Table H.14: Heterogeneity in the effect of eviction (13-36 months)

Panel A.	Joint Action		Single Action	
	(OLS)	(IV)	(OLS)	(IV)
Credit Score	-9.103***	-9.157	-7.365***	-30.528***
	(0.441)	(8.977)	(0.713)	(9.833)
	[545.294]		[553.754]	
No Open Revolving Account	0.041***	0.084	0.022***	0.093
	(0.003)	(0.061)	(0.005)	(0.108)
	[0.557]		[0.538]	
Total bal. collections	470.755***	-17.067	331.237***	827.413
	(26.478)	(560.276)	(42.593)	(688.794)
	[3,108.304]		[2,834.336]	
Any Auto Loan or Lease	-0.044***	-0.093**	-0.027***	0.022
	(0.002)	(0.045)	(0.004)	(0.053)
	[0.209]		[0.158]	
Any Payday Inquiry × 100	-1.439***	1.386	-0.478	19.276***
	(0.227)	(5.015)	(0.377)	(5.405)
	[15.182]		[12.034]	
Any Payday Account × 100	-0.719***	3.384*	-0.178	1.600
	(0.089)	(1.906)	(0.136)	(1.968)
	[2.528]		[1.630]	

Panel B.	Multi-headed household		Single-headed household	
	(OLS)	(IV)	(OLS)	(IV)
Credit Score	-8.614***	-21.499	-8.785***	-10.897
	(0.719)	(14.490)	(0.396)	(7.378)
	[560.927]		[542.526]	
No Open Revolving Account	0.039***	0.211*	0.037***	0.052
	(0.005)	(0.123)	(0.003)	(0.055)
	[0.508]		[0.567]	
Total bal. collections	355.183***	-1215.897	467.346***	607.596
	(43.894)	(1,013.967)	(24.341)	(581.373)
	[2,761.125]		[3,147.231]	
Any Auto Loan or Lease	-0.029***	-0.024	-0.044***	-0.073*
	(0.004)	(0.085)	(0.002)	(0.039)
	[0.209]		[0.196]	
Any Payday Inquiry × 100	-1.087***	0.085	-1.310***	7.440*
	(0.373)	(8.841)	(0.216)	(4.105)
	[13.938]		[14.708]	
Any Payday Account × 100	-0.576***	1.306	-0.614***	3.382*
	(0.150)	(3.076)	(0.083)	(1.804)
	[2.297]		[2.359]	

Panel C.	No prior cases		Prior case	
	(OLS)	(IV)	(OLS)	(IV)
Credit Score	-9.757***	-21.567*	-6.679***	-5.410
	(0.488)	(11.058)	(0.473)	(7.925)
	[556.302]		[529.541]	
No Open Revolving Account	0.042***	0.086	0.029***	0.085
	(0.003)	(0.072)	(0.004)	(0.077)
	[0.521]		[0.613]	
Total bal. collections	461.068***	789.816	403.165***	-464.856
	(27.903)	(592.953)	(35.999)	(672.496)
	[2,839.541]		[3,455.794]	
Any Auto Loan or Lease	-0.043***	-0.010	-0.035***	-0.116***
	(0.002)	(0.054)	(0.003)	(0.044)
	[0.210]		[0.179]	
Any Payday Inquiry × 100	-0.731***	8.852	-0.332**	0.100
	(0.280)	(6.378)	(0.143)	(2.490)
	[21.039]		[2.770]	
Any Payday Account × 100	-0.767***	5.142*	-0.100**	0.115
	(0.105)	(2.808)	(0.050)	(0.926)
	[3.369]		[0.473]	

Panel D.	Above median poverty neighborhood		Below median poverty neighborhood	
	(OLS)	(IV)	(OLS)	(IV)
Credit Score	-6.608***	-15.073**	-10.426***	-14.610
	(0.399)	(7.196)	(0.545)	(13.295)
	[528.794]		[563.286]	
No Open Revolving Account	0.019***	0.141**	0.056***	0.010
	(0.003)	(0.065)	(0.004)	(0.093)
	[0.621]		[0.492]	
Total bal. collections	372.804***	700.539	540.400***	-507.593
	(25.029)	(500.010)	(35.034)	(903.870)
	[3,142.816]		[2,973.240]	
Any Auto Loan or Lease	-0.039***	-0.103**	-0.039***	-0.011
	(0.002)	(0.040)	(0.003)	(0.075)
	[0.165]		[0.230]	
Any Payday Inquiry × 100	-1.642***	4.078	-0.842***	8.133
	(0.286)	(4.388)	(0.277)	(6.705)
	[15.686]		[13.432]	
Any Payday Account × 100	-0.604***	1.590	-0.583***	4.666
	(0.082)	(1.556)	(0.099)	(2.912)
	[2.320]		[2.370]	

Notes: The table shows OLS and IV estimates of the impact of eviction for four key subsamples indicated in the column heading, including (I.) joint action versus single action cases, (II.) multi-headed households versus single-headed households (based on whether there are one or multiple individuals named on the lease), (III.) whether the individual has a prior case or not, (IV.) whether the tenant's address at eviction is above or below the median neighborhood poverty rate in the sample. All regressions include the full set of controls and district-year fixed effects. Robust standard errors are clustered at the judge-year level. The non-evict mean of the subsample is reported in brackets below the estimates.

Table H.15: Heterogeneity in the effect of eviction (25-47 months)

Panel A.	Joint Action		Single Action	
	(OLS)	(IV)	(OLS)	(IV)
Move Zipcode	0.039***	-0.110	0.038***	0.211**
	(0.003)	(0.068)	(0.006)	(0.099)
	[0.735]		[0.719]	
Neighborhood poverty rate (×100)	0.528***	-2.080	0.188	-0.941
	(0.077)	(1.727)	(0.132)	(1.816)
	[16.219]		[18.474]	
Panel B.	Multi-headed household		Single-headed household	
	(OLS)	(IV)	(OLS)	(IV)
Move Zipcode	0.036***	-0.007	0.041***	-0.021
	(0.006)	(0.113)	(0.003)	(0.066)
	[0.735]		[0.731]	
Neighborhood poverty rate (×100)	0.390***	-1.908	0.488***	-1.627
	(0.111)	(2.458)	(0.069)	(1.277)
	[14.609]		[17.326]	
Panel C.	No prior cases		Prior case	
	(OLS)	(IV)	(OLS)	(IV)
Move Zipcode	0.039***	-0.060	0.042***	0.043
	(0.003)	(0.073)	(0.004)	(0.075)
	[0.735]		[0.727]	
Neighborhood poverty rate (×100)	0.451***	-1.998	0.442***	-1.328
	(0.078)	(1.598)	(0.088)	(1.732)
	[15.816]		[18.294]	
Panel D.	Above median poverty neighborhood		Below median poverty neighborhood	
	(OLS)	(IV)	(OLS)	(IV)
Move Zipcode	0.037***	-0.083	0.045***	0.093
	(0.004)	(0.079)	(0.003)	(0.096)
	[0.730]		[0.733]	
Neighborhood poverty rate (×100)	0.001	-1.095	0.344***	-3.202*
	(0.069)	(1.360)	(0.074)	(1.826)
	[22.457]		[11.544]	

Notes: The table shows OLS and IV estimates of the impact of eviction for four key subsamples indicated in the column heading, including (I.) joint action versus single action cases, (II.) multi-headed households versus single-headed households (based on whether there are one or multiple individuals named on the lease), (III.) whether the individual has a prior case or not, (IV.) whether the tenant's address at eviction is above or below the median neighborhood poverty rate in the sample. All regressions include the full set of controls and district-year fixed effects. Robust standard errors are clustered at the judge-year level. The non-evict mean of the subsample is reported in brackets below the estimates.

H.6 Alternative definition of treatment

Throughout the paper, we define the treatment as an eviction order. Yet, as we show in Appendix Section D.3, many dismissed cases may still involve the tenant paying the landlord or moving. This section provides alternative results which calculate alternative instruments and alternative treatment based on whether the case was dismissed with prejudice. Cases dismissed with prejudice result in no eviction order and bar the landlord from bringing a future case against the tenant related to the same concern or disagreement. Thus, this form of dismissal could be considered a success for the tenant. Similarly, the instrument is recalculated as the leave-out mean of an indicator for the judge dismissing a case with prejudice adjusted for district-year fixed effects.

Tables H.16 and H.17 report alternative IV results for this definition of treatment. The OLS estimates are similar in magnitude but, as expected, have the opposite signs; having a case dismissed with prejudice increases the tenant's credit score, decreases their balance in collections, and decreases their access to revolving credit. The IV results are imprecise with no statistically significant results, likely driven by the fact that only a small fraction cases are dismissed with prejudice.

Table H.16: The effect of the case being "dismissed with prejudice" on financial strain

	Non-evicted mean	OLS: Dismissed with Prejudice			IV: Dismissed with Prejudice
		(1)	(2)	(3)	(4)
I. *Financial Strain: 13-36 mon.*					
Credit Score	534.393	12.645***	6.629***	6.434***	-42.510
	(72.662)	(1.349)	(0.929)	(0.917)	(68.893)
No Open Revolving Account	0.602	-0.064***	-0.042***	-0.040***	0.255
	(0.485)	(0.007)	(0.006)	(0.007)	(0.432)
Total bal. collections	3,558.757	-616.286***	-423.355***	-433.074***	-7628.850
	(4,676.298)	(56.896)	(49.503)	(49.418)	(4,626.774)
Any Auto Loan or Lease	0.160	0.027***	0.018***	0.016***	0.048
	(0.363)	(0.005)	(0.005)	(0.005)	(0.287)
Any Payday Inquiry × 100	13.919	-0.389	-0.145	-0.014	-11.573
	(34.615)	(0.483)	(0.471)	(0.453)	(32.439)
Any Payday Account × 100	1.898	-0.017	-0.020	-0.014	-2.212
	(13.645)	(0.206)	(0.205)	(0.194)	(15.448)
II. *Financial Strain: 37-60 mon.*					
Credit Score	543.815	10.858***	4.497***	4.614***	38.108
	(75.340)	(1.731)	(1.235)	(1.230)	(65.330)
No Open Revolving Account	0.584	-0.045***	-0.027***	-0.025***	-0.539
	(0.488)	(0.009)	(0.008)	(0.008)	(0.473)
Total bal. collections	3,366.803	-516.756***	-374.954***	-370.938***	1,247.582
	(4,613.546)	(75.081)	(67.754)	(69.487)	(5,890.321)
Any Auto Loan or Lease	0.164	0.026***	0.020**	0.018**	0.324
	(0.366)	(0.008)	(0.008)	(0.007)	(0.403)
Any Payday Inquiry × 100	13.054	0.006	0.317	0.416	10.086
	(33.689)	(0.496)	(0.489)	(0.466)	(29.878)
Any Payday Account × 100	2.075	0.011	0.016	0.012	-9.584
	(14.253)	(0.201)	(0.199)	(0.192)	(11.179)
Additional controls			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. Column 4 presents an OLS regression on residual stringency. The analysis sample has $N = 225,794$. The dependent variable is listed in each row. Two-stage least squares models instrument for eviction using the judge stringency measure based on decisions in other cases, as described in the text. All specifications control for district-year fixed effects. Robust standard errors are clustered at the judge-year level.

Table H.17: The effect of the case being "dismissed with prejudice" on moves and neighborhood quality

	Non-evicted mean	OLS: Dismissed with Prejudice			IV: Dismissed with Prejudice
		(1)	(2)	(3)	(4)
I. *Outcomes 13-36 months after filing*					
Move Zipcode	0.606	-0.020**	-0.019**	-0.018**	-2.080
	(0.489)	(0.008)	(0.008)	(0.008)	(2.337)
Neighborhood poverty rate (×100)	17.597	0.131	0.381***	0.342***	50.233
	(10.033)	(0.136)	(0.126)	(0.128)	(50.097)
II. *Outcomes 37-60 months after filing*					
Move Zipcode	0.762	-0.019*	-0.016	-0.014	0.253
	(0.426)	(0.010)	(0.010)	(0.010)	(0.939)
Neighborhood poverty rate (×100)	17.366	0.098	0.359**	0.371**	27.082
	(10.086)	(0.165)	(0.149)	(0.156)	(25.470)
III. *Future Eviction Cases*					
Any Eviction Case (36 mo.)	0.237	-0.006	0.001	-0.002	2.251
	(0.425)	(0.009)	(0.009)	(0.009)	(1.408)
Eviction Case at Dif. Address (36 mo.)	0.172	-0.006	0.001	-0.000	-0.110
	(0.377)	(0.007)	(0.007)	(0.007)	(0.738)
Additional controls			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 number of moves and neighborhood quality. Column 4 presents an OLS regression on residual stringency. The dependent variable is listed in each row. Two-stage least squares models instrument for eviction using the judge stringency measure based on rulings in other cases, as described in the text. All specifications control for district-year fixed effects. Robust standard errors are clustered at the judge-year level.

H.7 Characterizing compliers

We next explore characteristics of compliers, following Abadie (2003) and Dahl et al. (2014), to interpret our LATE estimates.

In the eviction setting, compliers are those whose eviction court judgment would have been different had their case been assigned to the most lenient instead of the most strict judge. By the monotonicity and independence assumptions,

$$\pi_c \equiv Pr(E_i|Z_i = \bar{z}) - Pr(E_i|Z_i = \underline{z}) = Pr(E_i(\bar{z}) - E_i(\underline{z}) = 1),$$

where $\bar{z}$ represents the strictest judge, and $\underline{z}$ represents the most lenient judge, and E_i is an indicator for being evicted.

The always takers represent the tenants who would be evicted, even by the most lenient judge, hence

$$\pi_a \equiv Pr(E_i = 1|Z_i = \underline{z}) = Pr(E_i(\bar{z}) = E_i(\underline{z}) = 1)$$

and the never takers are those who would not be evicted, even by the most strict judge,

$$\pi_n \equiv Pr(E_i = 0|Z_i = \bar{z}) = Pr(E_i(\bar{z}) = E_i(\underline{z}) = 0).$$

Table H.18 shows the share of tenants in eviction court in the three categories. Following Dahl et al. (2014), we use a flexible analog of the first stage regression, where we perform a local linear regression of the eviction indicator on estimated judge stringency, including a vector of interacted district and year fixed effects. Our baseline choice of $\bar{z}$ is the top 1 percentile of judge stringency and of $\underline{z}$ is the bottom 1 percentile of judge stringency.

Columns 1-3 adopt a linear specification of the first stage regression. Under this specification, we calculate $\hat{\pi}_a = \hat{\alpha} + \hat{\gamma}\underline{z}$ and $\hat{\pi}_n = 1 - \hat{\alpha} - \hat{\gamma}\bar{z}$ and $\hat{\pi}_c = \hat{\gamma}(\bar{z} - \underline{z})$. We find approximately 59 percent always-takers, 10 percent compliers, and 31 percent never-takers. The relative sizes of these groups are not highly sensitive to the choices of specification and stringency threshold.

Next we study the characteristics of compliers, including individual, case, and neighborhood characteristics. For characteristic x_k, we use the relationship:

$$\begin{aligned}\frac{Pr(x_{ki} = 1|e_i(\bar{z}) - e_i(\underline{z}) = 1)}{Pr(x_{ki} = 1)} &= \frac{Pr(e_i(\bar{z}) - e_i(\underline{z}) = 1|x_{ki} = 1)}{Pr(e_i(\bar{z}) - e_i(\underline{z}) = 1)} \\ &= \frac{E[e_i|Z_i = \bar{z}, x_{ki} = 1] - E[e_i|Z_i = \underline{z}, x_{ki} = 1]}{E[e_i|Z_i = \bar{z}] - E[e_i|Z_i = \underline{z}]}.\end{aligned}$$

We estimate the numerator by first estimating the first stage regression of eviction on judge leniency with district $\times$ year fixed effects for the sample with $x_{ki} = 1$, and constructing $\hat{\beta}(\bar{z} - \underline{z})$ using the coefficient estimate on judge leniency, $\hat{\beta}$, and using values $\bar{z}$ and $\underline{z}$ from the local linear model shares with 1 and 99 percent judge leniency thresholds. The denominator is constructed analogously, but with $\hat{\beta}$ obtained from the first stage regression without the $x_{ki} = 1$ sample restriction.

Table H.19 presents the relative likelihood of characteristics. The table does not reflect significant differences between the complier population and the overall eviction court population.

Table H.18: Sample shares by compliance type

	Linear model			Polynomial		
	1%	1.5%	2%	1%	1.5%	2%
Always Takers	0.59	0.59	0.60	0.59	0.59	0.60
Compliers	0.10	0.09	0.08	0.09	0.08	0.08
Never Takers	0.31	0.32	0.32	0.31	0.32	0.32

Notes: The table above depicts the shares of tenants in eviction court who are always takers, never takers, and compliers. The three left columns adopt a linear model for estimating the effect of judge stringency on eviction, and the right columns adopt a third-order polynomial specification. The column headings indicate the threshold for determining the most stringent and least stringent judge.

Table H.19: **Complier characteristics ratios**

	Pr(X=x)	Pr(X=x|complier)	$\frac{Pr(X=x\|complier)}{Pr(X=x)}$
Joint Action	0.829	0.747	0.901
	(0.001)	(0.019)	(0.023)
Defendent w/o Attorney	0.961	0.964	1.003
	(0.000)	(0.011)	(0.011)
Female	0.539	0.555	1.030
	(0.001)	(0.021)	(0.039)
Black	0.351	0.397	1.132
	(0.001)	(0.019)	(0.053)
Age 21 to 39	0.517	0.535	1.034
	(0.001)	(0.026)	(0.049)
Age 40 to 59	0.369	0.377	1.022
	(0.001)	(0.023)	(0.062)
Age 60 and up	0.073	0.060	0.818
	(0.001)	(0.013)	(0.184)
Neigh. Above Median Pov.	0.506	0.622	1.230
	(0.001)	(0.028)	(0.056)
No prior cases	0.614	0.556	0.906
	(0.001)	(0.023)	(0.038)

Notes: This table presents the sample distribution, complier distribution, and relative likelihood for different subgroups. Bootstrapped standard errors in parentheses are obtained using 500 replications.

I Appendix: MTE analysis

This section estimates the marginal treatment effects of evictions to better understand how effects depend on the unobservables affecting the eviction decision. Consider the selection model $Y = (1-E)Y(0) + EY(1)$ where $Y(j) = \mu_j(X) + U_j$ and the eviction indicator is given by $E = \mathbb{1}\{\mu_E(X,Z) - U_E \geq 0\}$. X are observable characteristics, Z is the judge stringency instrument, and the error terms are continuous random variables. As discussed in Heckman and Vytlacil (2007), we can normalize the distribution of U_E to be uniformly distributed between 0 and 1 conditional on X, which implies that $\mu_E(X,Z)$ is equal to the propensity score $p(X,Z) \equiv Pr(E = 1|X,Z)$. We can write the marginal treatment effect as:

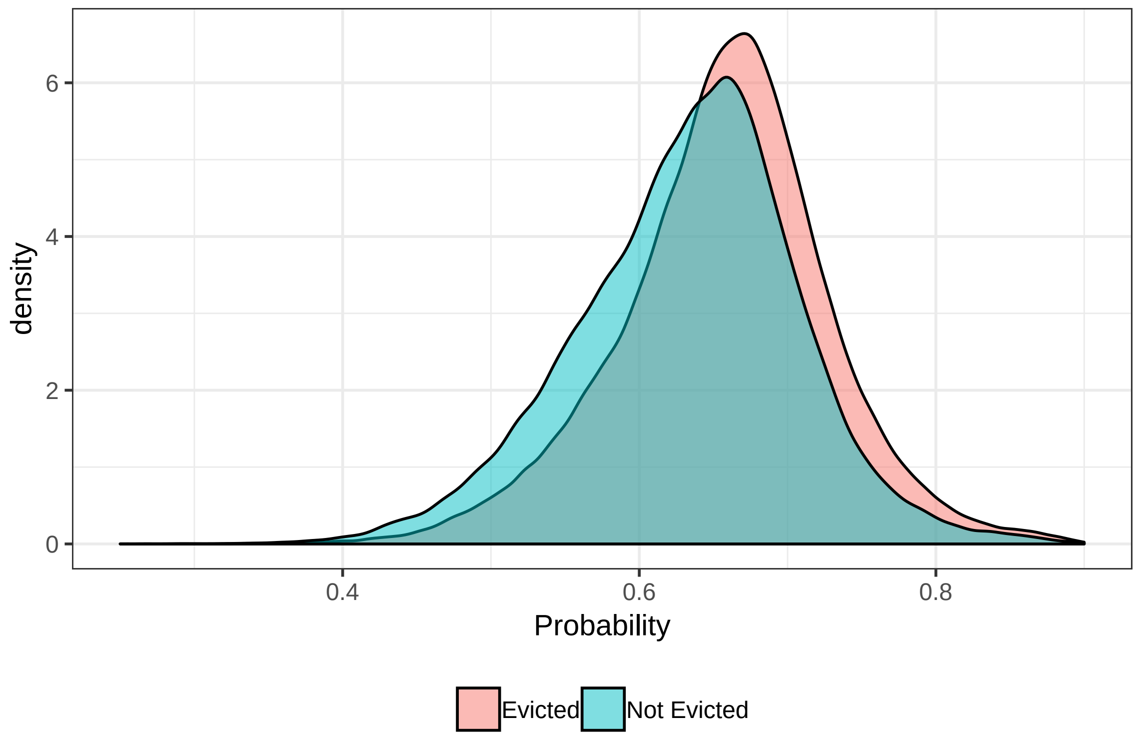
$$MTE(x,p) = E[Y(1) - Y(0)|U_E = p, X = x],$$

where U_E represents unobserved characteristics of the defendant that affect the court ruling. Following Brinch et al. (2017) and Bhuller et al. (2019), we assume that the treatment effects are separable into observed and unobserved components which, combined with the assumptions regarding the instrument maintained in this paper, implies that the MTE is point identified over the support of $p(X, Z)$.

Figure I.1 shows the estimated distributions of eviction probabilities for those who were and were not evicted, where the probability is estimated using a flexible logit specification described in the figure notes. Following Bhuller et al. (2019), we calculate the MTE only over the range of probabilities where there is support in both distributions by restricting the analysis to probabilities between the 1st percentile of the evicted distribution and the 99th percentile of the not evicted distribution.

Figures I.2 and I.3 plots the marginal treatment effects by eviction probability or, equivalently, the effect by latent resistance to eviction (conditional on appearing in eviction court) using local cubic polynomial regression. While the estimates are noisy, we see that the marginal treatment effects on credit scores, having no revolving credit, and amount in collections are largest for individuals with unobservables that make them more likely to be evicted, though our estimates are negative over the full distribution for which their is common support in both the evicted and not-evicted populations. For the other outcomes considered, the MTEs are largely flat, and somewhat imprecisely estimated.

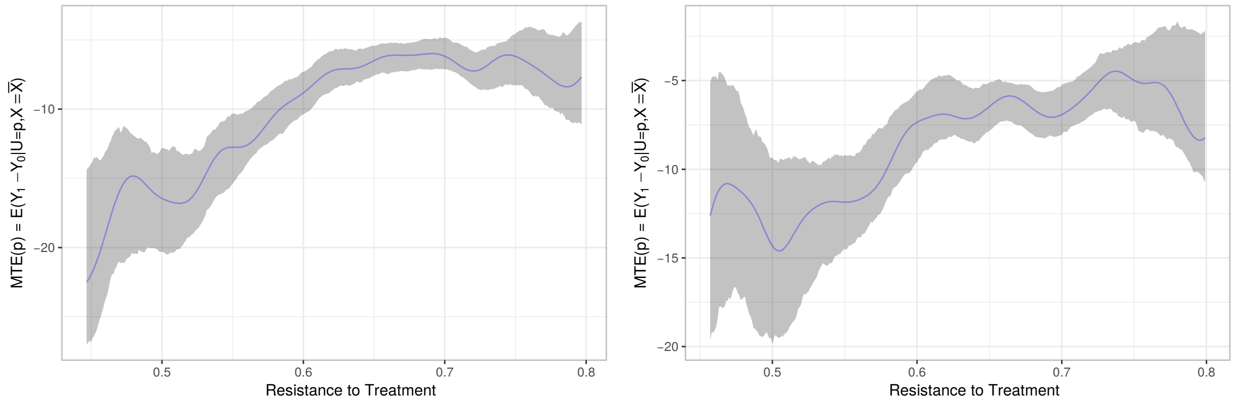
Figure I.1: Common support of eviction propensity scores



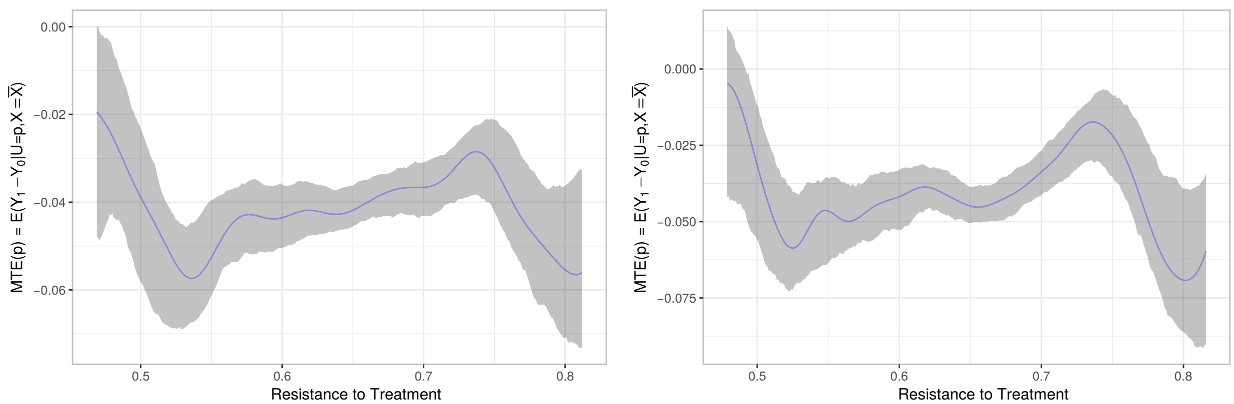
Note: Probabilities are estimated using a logistic regression of case out come on a polynomial of age, gender, an indicator for being African American, an indicator for being a joint action case, the ad damnum amount, lagged credit score, lagged amount in collections, and lagged indicator for having any auto loan or lease plus interactions between female, African American, joint action case, and ad damnum amount. All these covariates are interacted with judge stringency, and we additionally control for stringency squared, stringency cubed, and district-year fixed effects. The model is estimated on the sample of cases linked to credit reports used in the IV analysis. The blue density represents the density of estimated eviction probabilities for those not evicted while the red density represents the density for those evicted.

Figure I.2: Marginal Treatment Effects

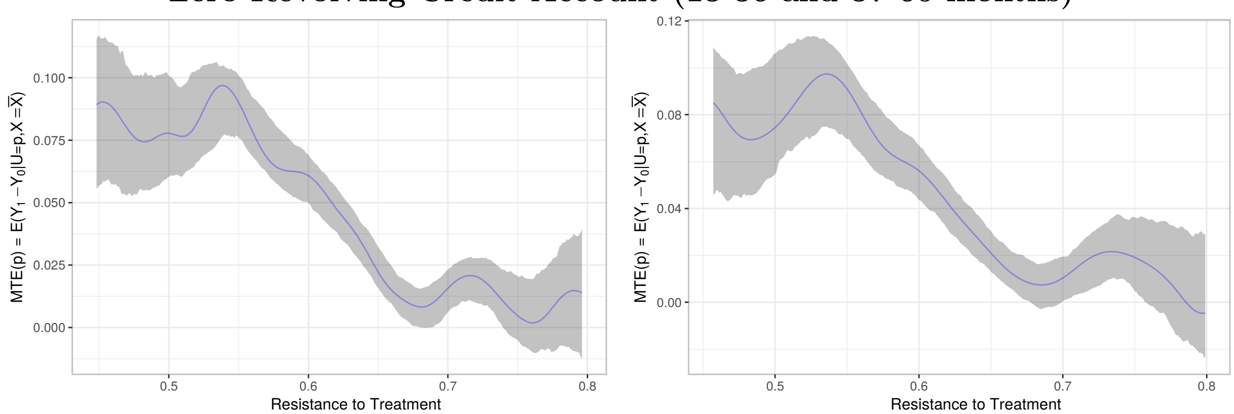
Credit Score (13-36 and 37-60 months)



Any Auto Loan or Lease (13-36 and 37-60 months)

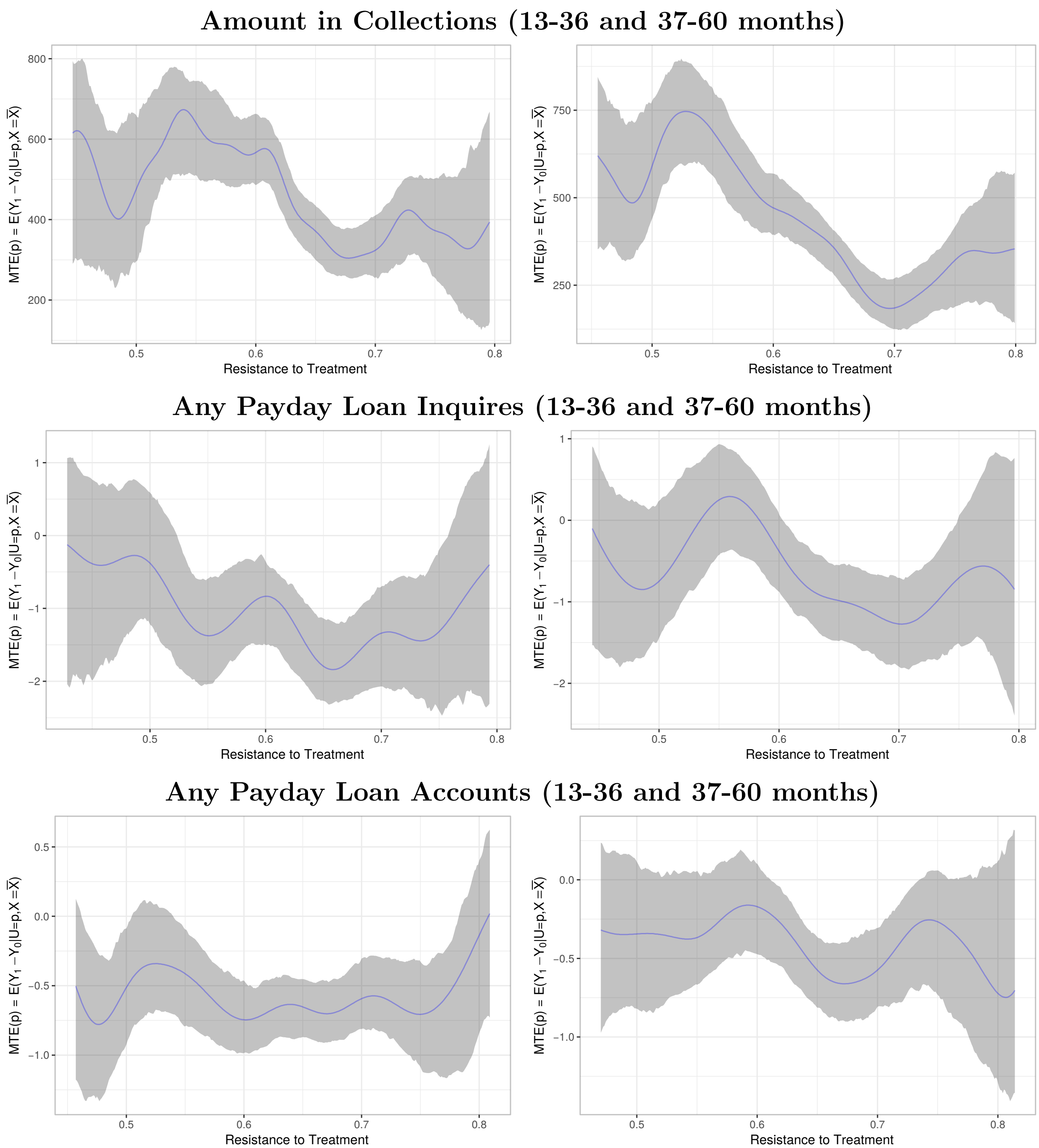


Zero Revolving Credit Account (13-36 and 37-60 months)



Note: Figure shows the treatment effects by latent resistance to treatment (eviction). Higher probabilities represent those with unobservables that make them less likely to be evicted (i.e., more resistant to treatment). The MTE estimates are based on the local IV approach of Brinch et al. (2017) using a local poly cubic polynomial specification. The bandwidth for the local polynomial regression is estimated following Ruppert et al. (1995). The grey ribbons in the figure show 90% confidence intervals based on 200 bootstraps. The model is estimated on the sample of cases linked to credit reports used in the IV analysis restricted to a trimmed sample with common support.

Figure I.3: Marginal Treatment Effects



Note: Figure shows the treatment effects by latent resistance to treatment (eviction). Higher probabilities represent those with unobservables that make them less likely to be evicted (i.e., more resistant to treatment). The MTE estimates are based on the local IV approach of Brinch et al. (2017) using a local poly cubic polynomial specification. The bandwidth for the local polynomial regression is estimated following Ruppert et al. (1995). The grey ribbons in the figure show 90% confidence intervals based on 200 bootstraps. The model is estimated on the sample of cases linked to credit reports used in the IV analysis restricted to a trimmed sample with common support.

J Appendix: Alternative measures of residential mobility

The mobility results in Section 7 use data from Experian, which are measured at the ZIP code level. This appendix section provides an alternative analysis using InfoUSA data on address histories. InfoUSA is a commercial database that includes address data and basic information about residents. InfoUSA collects data from different marketing sources including real estate and tax assessments, utility connections, voter registration files, and other commercial sources. Addresses are standardized and verified using the Delivery Sequence File. Kennel and Li (2009) compares the coverage of the Census Bureau's Master Address File (MAF) and InfoUSA address data and finds that 16.1% of the addresses in the MAF do not match an exact address in InfoUSA and 5.7% of InfoUSA addresses do not match an address in the MAF.

The advantages of InfoUSA data are that it includes exact address, a unique individual ID, and the exact timing at which InfoUSA obtained information on the address of an individual (either confirming their current address or providing a new address). The main disadvantages are that address information is infrequently updated for our sample and has a relatively low match rate with our court sample. Using snapshots from 2006 to 2017, we observe the last confirmed address as well as the date at which that address was last confirmed. In a given year, the InfoUSA data contains an updated address information confirming a resident's address or providing a new address in a given year 41.3% of the time.

Matching: An individual from InfoUSA was considered a match with an individual from the eviction data set if the InfoUSA individual: (1) had matching addresses (same ZIP code, street name, and street number as listed in eviction filing records), (2) had same first and last name, and (3) the InfoUSA data was within 48 months of the eviction case date. In order to account for potential differences in spelling of names as provided to InfoUSA and to the eviction court, we fuzzy matched names using Jaro-Winkler distance, and required that the distance be less than 0.2. We then kept the match with the smallest last name distance, and, in the event of a tie, the smallest first name distance.

After implementing the matching procedure described above, we are able to match $134,621$ individuals from the eviction data set with InfoUSA, of which $116,756$ are in our IV sample of the eviction data set. Thus, we match approximately 30% of the evictions IV sample with court addresses in 2006 or later, which is when the InfoUSA data begins. Although the match rate may seem low, we stress that the population of individuals facing eviction are a highly marginalized population that is difficult to track. As such, the problem may not be with the quality of the InfoUSA data, but rather due to the simple difficulty of tracking these marginalized individuals.

Moves within ZIP codes: Using the InfoUSA data, we estimate what proportion of observed moves occur within ZIP code. Using the match sample, we find that 26.7% of observed moves were within ZIP code for the sample over the time window.[64] This suggests that the main analysis, which considers ZIP code mobility, underestimates overall mobility.

Tracking populations at risk of eviction: Tracking the mobility of low-income households is difficult because of the limited sources of data. Moreover, low-income households may be less likely to update their address with the United States Postal Service and more likely to have alternative living situations where they may not be listed on a lease or utility bill. Using the linked eviction records and a random sample of individuals from Cook County, we find that households with eviction cases filed against them are 37% less likely to have updated address information in a given year than a random sample from Cook County, with this number being slightly larger for individuals whose cases ended in eviction. Overall, this highlights the difficulty of tracking individuals at risk of eviction.

Alternative estimates of eviction on mobility Exact addresses allow us to better measure unit-level mobility, but the InfoUSA data is also much more likely to not be updated in any given year. To address missingness in the data, we define three outcomes: "New address" which is an indicator for whether there is any new address information, "Same address" which is an indicator for whether there is any confirmation that the individual is at the same address, and "No update" which is an indicator for whether there is no updated information of either kind. For the first two outcomes, we additionally construct an imputed version which assumes an individual has moved out if we see a new household move in to the exact unit.

Looking at the OLS estimates, evicted households are two percentage points less likely to be observed at the same address, eight percentage points more likely to be observed at a new address, and 6 percentage points more likely to not have no updated address information. Imputed results are similar, but with a much higher baseline move rate for the control group of 70 percent. OLS results are similar when looking over a longer time horizon, though evicted tenants are not statistically significantly more likely to be seen at a new address. The IV results using judge stringency are largely uninformative because of large standard errors. Overall, these results suggest that evicted individuals are somewhat more likely to move, but that they are also more likely to not have new address information, which could suggest alternative living arrangements such as staying with a friend or at a shelter.

[64] We find that 28% of observed moves prior to the eviction filing are within ZIP code and 25% of moves after the eviction filing are within ZIP code. Numbers are similar regardless of the outcome of the eviction case.

Table J.1: The effect of eviction on mobility (InfoUSA)

	Baseline			Imputed		
I. *Outcomes (1-24 months)*:	C. Mean	OLS	IV	C. Mean	OLS	IV
New address	0.212	0.022***	-0.034	0.702	0.036***	-0.114
		(0.003)	(0.062)		(0.003)	(0.072)
Same address	0.335	-0.084***	-0.014	0.146	-0.046***	0.056
		(0.004)	(0.069)		(0.003)	(0.045)
No update	0.472	0.058***	0.019			
		(0.004)	(0.077)			
II. *Outcomes (1-48 months)*:	C. Mean	OLS	IV	C. Mean	OLS	IV
New address	0.250	0.005	-0.058	0.796	0.026***	-0.022
		(0.004)	(0.066)		(0.003)	0.065)
Same address	0.368	-0.086***	-0.014	0.110	-0.034***	0.036
		(0.004)	(0.071)		(0.002)	(0.039)
No update	0.425	0.064***	0.024			
		(0.004)	(0.074)			

Notes: Table shows mobility results using InfoUSA data. “New address” is an indicator for any new address in the time window, “Same address” is an indicator for any confirmation of same address in the time window, and “No update” is an indicator of no updated address information in the time window. “C. Mean” is the control mean. The “Imputed” columns use additional information on if a new household moves into the exact address, indicating that the prior household moved. Standard errors are clustered at the judge-year level.