Does eviction cause poverty? Quasi-experimental evidence from Cook County, IL*

John Eric Humphries, Nick Mader, Daniel Tannenbaum & Winnie van Dijk†

December 26, 2018

PRELIMINARY DRAFT

[most recent version available here]

Abstract

Each year more than two million U.S. households have an eviction case filed against them. Influential work in sociology has hypothesized that eviction is a cause, and not merely a consequence, of poverty. This paper uses the near-universe of eviction court cases in Cook County, IL for 2000-2016, linked to credit bureau and payday loans data, to provide evidence on the link between eviction and financial strain. We first provide new descriptive evidence showing that there is significant distress and an increase in demand for high interest loans in the run-up to eviction court for both evicted and non-evicted tenants. We then estimate the causal effect of an eviction order on financial strain, proxies for durable goods consumption, household moves, and neighborhood quality, leveraging the random assignment of cases to judges in eviction court for identification. The effects are small relative to the financial strain experienced by tenants in the run-up to eviction court.

*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, Emily Kristine Metz and Ruth Coffman at UChicago Urban Labs, Joe Altonji, Luis Bettencourt, Angela Denis Pagliero, Manasi Deshpande, Michael Dinerstein, Bill Evans, Alex Frankel, Pieter Gautier, Michael Greenstone, Jim Heckman, Ali Hortaçsu, Peter Hull, Louis Kaplow, Ezra Karger, Paymon Khorrami, Scott Kominers, Thibaut Lamadon, Jeff Lin, Maarten Lindeboom, Sarah Miller, Magne Mogstad, Derek Neal, Matt Notowidigdo, Ed Olsen, Azeem Shaikh, Beth Shinn, Jim Sullivan, Chris Taber, Alex Torgovitsky, Bas van der Klaauw, Chris Walters, 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 applied microeconomics seminar, SOLE, the Philadelphia Fed, and the 2018 NBER Summer Institute, and the University of Oslo for helpful discussion. Ella Deeken, Deniz Dutz, Michael Harvey, Katherine Kwok, Ezra Max, Sebastian Seitz, Diego Suarez Touzon, and Yao Xen Tan provided excellent research assistance. Any errors are our own.

†Humphries: Department of Economics, Yale University (e-mail: johneric.humphries@yale.edu). Mader: Chapin Hall at the University of Chicago (e-mail: nmader@chapinhall.org). Tannenbaum: Department of Economics, University of Nebraska - Lincoln (e-mail: dtannenbaum@unl.edu). van Dijk: Department of Economics, University of Chicago (e-mail: wlvandijk@uchicago.edu).

... 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.[1] 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. The high incidence and geographic concentration of evictions have motivated pioneering ethnographic and survey-based research asking whether housing instability, and eviction in particular, is responsible for increasing the financial strain on low-income families.[2]

This hypothesis is challenging to evaluate because, until very recently, little or no data has been collected on evictions. In addition, eviction is likely to occur in conjunction with other adverse life events, such as job loss, financial shocks, or deteriorating health, making it difficult to isolate its causal impact on outcomes associated with poverty.

In this paper, we present new evidence on the link between eviction and measures of financial distress. In the first part of the paper, we provide descriptive evidence on financial strain and housing instability surrounding the eviction filing. In the second part, we assess the causal link between eviction and subsequent outcomes, using a judge instrumental variables design. Overall, we aim to test the hypothesis that eviction is a driver of urban poverty and to examine the potential channels through which eviction affects families. There are several reasons eviction may generate significant costs for tenants. First, the disruption and relocation of the home environment is costly, and tenants may have to relocate farther from their workplace. Second, eviction court records are publicly observable, which may negatively impact a tenant's ability to secure a new lease or access credit. Third, tenants may be cut off from or become ineligible for rental assistance following an eviction. On the other hand, an eviction may not be without some benefit for tenants, since they are released from the obligations of their lease and may relocate to a lower-rent unit, increasing their ability to pay balances on existing loans.

We begin our analysis by documenting a series of descriptive facts about eviction in Cook County, IL, which includes the city of Chicago. Drawing on a newly-assembled linked administra-

[1] Although data on the annual number of eviction cases in the U.S. are not collected or reported by statistical agencies, between 2.3 and 2.7 million cases were filed in 2016; approximately 900,000 of them ended in eviction orders (Marr, 2016; Desmond et al., 2018a). For comparison, there were 2.9 million foreclosures in the peak year of the great recession.

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

tive data set that includes the near-universe of eviction court cases in Cook County, we provide new evidence on the scope and incidence of evictions. We document that evicted households are negatively selected when compared to observationally similar households from the neighborhood that do not have an eviction case filed against them; these comparisons are common in the literature and are often interpreted in the literature as evidence of eviction's harmful effect. However, we show that this comparison can be misleading because of selection into eviction court. For instance, the difference in credit scores observed in the 13-36 months after eviction is reduced by more than 75 percent when the comparison is between evicted and non-evicted tenants in eviction court, rather than between evicted tenants and observationally similar tenants from the neighborhood.

Throughout the remainder of our analysis, we focus on tenants with an eviction case filed against them, and define the treatment as an eviction order issued by a judge. In the Cook County Circuit Court, this is called an "order for possession," which grants the landlord the legal authority to have the sheriff change the locks on the unit and reclaim the property. Our analysis studies the effect of such a court-ordered eviction against the counterfactual of dismissal of the eviction case.[3]

We next present new descriptive evidence, using an event study design, characterizing the evolution of financial strain in the run-up and aftermath of eviction court for both winners and losers of eviction cases. Our main outcomes are credit score, unpaid bills, an indicator for having an open auto loan or lease, which serves as a proxy for durable goods consumption (Dobkin et al., 2018), and high-interest loan inquiries and openings.[4] We also study the evolution of the tenant's neighborhood quality, measured at the zipcode level, and study the cumulative number of zipcode-level moves.

The event studies reveal four important findings. First, regardless of the outcome of the case, households in eviction court show signs of financial strain 2-3 years prior to having a case filed against them, with credit scores falling and unpaid bills rising. Second, the comparison of evicted and non-evicted tenants reduces the selection bias considerably; nonetheless, there remains selection on observables, with evicted tenants having lower credit scores and more debt prior to the court cases. Third, the event studies do not lend support to the hypothesis that an eviction order triggers a downward spiral in financial health, but they do suggest that an eviction exacerbates the initial decline and slows the recovery. Finally, eviction filings coincide with a reduction in durable goods consumption, and a large increase in unpaid bills, for both

[3]We emphasize that the treatment we study in this paper is more narrow than an "involuntary move," terminology which is sometimes used in the sociology literature on eviction. As such, our analysis is not directly informative on cases in which tenants are coerced or illegally forced to move, and which operate outside the legal system. Our focus is on court-ordered eviction, which is well-defined by the legal system and has a clear, policy-relevant counterfactual.

[4]The high-interest loans are single payment microloans that originate either from traditional storefront or online subprime lenders. This data was collected and provided to us by the largest subprime credit bureau in the U.S. For brevity, we refer to these loans throughout the paper as "payday loans," even though not all of these microloans are required to be repaid on the individual's payday.

evicted and non-evicted tenants. For instance, both winners and losers in eviction court have on average more than $3,000 of collections debt in the 1-2 years prior to the eviction filing, and this debt increases by $500-1000 for both groups in the 1-2 years after filing and does not return to pre-eviction levels in the 7 years after filing.

We next turn to an analysis of the causal effect of eviction, using a quasi-experimental research design that leverages random assignment of court cases to judges who differ in their stringency toward tenants, which we measure by their tendency to grant eviction orders. Our analysis is able to recover the local average treatment effect: the causal effect of an eviction for the group of tenants whose case outcome is affected by the stringency of the judge.[5] The linked credit bureau data allows us to evaluate the costs as well as the potential benefits of eviction. Our findings show that eviction has a small negative effect on short- and long-run credit scores, and a relatively small and statistically insignificant positive effect on collections balances. We do find a large negative effect on the probability of having an auto loan or lease. This effect persists for several years, and, taken together with the results from the event studies, suggests a long-lasting negative effect on consumption.

Our research speaks to an active policy debate on how, if at all, the government should address the high frequency of evictions. For example, New York City recently introduced legal aid for tenants in eviction court, and Richmond, Virginia is considering a diversion program to place tenants on a court-supervised payment plan in lieu of eviction.[6] Our work develops critical input into this policy debate by providing an assessment of the costs of eviction on tenants. The eviction court process is important for a well-functioning rental market, but, given the prevalence of evictions, it is important to know how tenants might be affected by policies that make eviction court proceedings more lenient toward them. Our results speak directly to this question. Given the financial strain experienced by tenants in the run-up to eviction and the modest additional effect of an eviction order, one broad takeaway for policy design is that intervening after a case has been filed may be too late to help prevent the large increase in financial strain associated with eviction.

This paper contributes to the growing body of work in sociology on eviction. Recent studies find that eviction has a negative association with 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, and material hardship (Desmond and Kimbro, 2015), suicide (Fowler et al., 2015), job loss (Desmond and Gershenson, 2016), 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 data on evictions, the evidence is largely based on ethnographic research and short-term surveys of households at risk of eviction, including the influential Milwaukee Area

[5]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. (2016); Mueller-Smith (2016); Dobbie et al. (2018).

[6]See Appendix B for a review of recently proposed or passed reforms.

Renters Study. We contribute to this literature by assembling a large-scale administrative data set of eviction cases in Cook County that are linked to a panel of credit reports, including payday loan account openings and inquiries. We provide new descriptive evidence on the evolution of financial strain surrounding an appearance in eviction court, and study the causal effect of an eviction order using a quasi-experimental design. Our study presents some of the first evidence on the effects of eviction that addresses the endogeneity resulting from correlated unobservables and simultaneity. One closely related study is Collinson and Reed (2018), an independent and contemporaneous working paper that studies recipients of public assistance appearing in eviction court in New York and examine the effect of eviction on income, mental health, public assistance, and homelessness using a similar judge design.[7] In addition, Desmond et al. (2018a) has recently assembled and made 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.

Our paper also contributes to the economics literature on housing instability and economic mobility. Several studies of housing vouchers and the Moving to Opportunity program have found small benefits of moving to a better neighborhood for adults, and much larger effects for children (Kling Jeffrey et al., 2007; Gennetian et al., 2012; Chetty et al., 2016; Chyn, 2018). Evans et al. (2016) shows that emergency financial assistance is a cost-effective tool for reducing homelessness. There is surprisingly little economics research on evictions, although the social costs have the potential to exceed the amount under dispute.[8] We aim to fill this gap by studying the dynamics of financial strain surrounding an eviction filing and by bringing a rich research design to identify the causal effect of an eviction order. Our credit panel allows us to assess the evidence for the Desmond hypothesis by studying several stress points on which eviction is likely to leave an empirical signature.[9] One advantage of our credit bureau data is that we can follow individuals across neighborhoods, not only within Cook County but throughout the US, which is typically difficult in studies of the urban poor. Another feature of the data is we are able to observe a tenant's interaction with subprime lenders, which we believe is a first in the housing instability literature, and allows us to observe demand for credit.

The remainder of the paper is organized as follows. Section 2 provides background information on evictions in Cook County and a detailed description of the institutional setting. Section 3 describes our data collection and linking process and detailed description of the population

[7]There are important distinctions in the design worth highlighting. First, the choice of treatment in Collinson and Reed (2018) is the City Marshal carrying out the eviction order. Second, their specification differs in that it is based on courtroom leniency rather than judge leniency.Despite these differences, Collinson and Reed (2018) find a similar pattern of small causal effects,and we view their results as complementary to those presented in this paper.

[8]In the literature on foreclosure and bankruptcy, for example, researchers have studied the costs of default, and the potential for improving the contracting environment (Campbell et al., 2011; Molloy and Shan, 2010; Dobbie et al., 2017). The household finance literature more generally has focused on home ownership, rather than tenancy.

[9]Several recent papers use credit bureau data in the context of studying the economic costs of hospitalization (Mazumder and Miller, 2016; Dobkin et al., 2018), and bankruptcy (Dobbie et al., 2017).

of tenants in our baseline sample. Section 4 provides new descriptive evidence using an event study design. Section 5 formalizes our research design and tests the key underlying assumptions. Section 6 presents the main findings of the causal impact of eviction and a discussion of the mechanisms. Section 7 concludes.

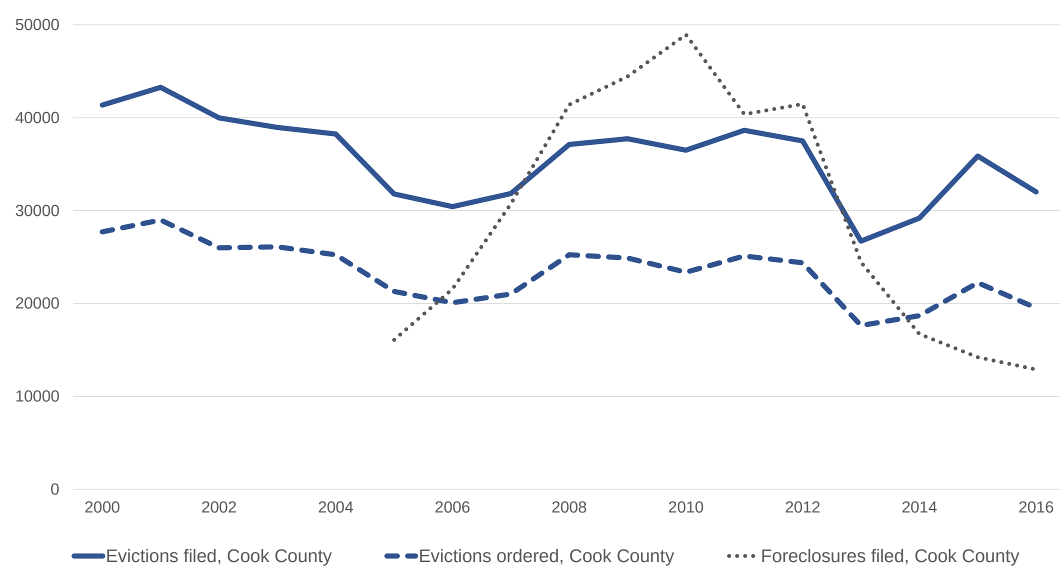
2 Background and institutional details

This section first provides aggregate empirical facts about eviction court cases in Cook County and the city of Chicago based on our newly assembled data, and then describes the details of the eviction process.

2.1 Evictions in Cook County: 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 evictions ordered by a judge in Cook County from 2000 to 2016. There is a 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. For comparison, Figure 1 also shows the number of foreclosure filings between 2005 and 2016, which only surpasses eviction filings during the financial crisis.

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).

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 the cases occur in Census tracts with more than 20 percent of residents living below the poverty line, and more than 22 percent of cases occur in Census tracts with more than 30 percent of residents living below the poverty line This finding is consistent with Desmond (2012) and Desmond and Shollenberger (2015), who find that eviction is a relatively common occurrence in poor communities in Milwaukee. In the city of Chicago, the highest concentration census tracts have more than 10 percent of rental units with an eviction filing per year, or more than four times the 2.4 percent filing rate for the city as a whole.

2.2 The court process

This section summarizes the eviction process.[10] To begin an eviction, the landlord must serve the tenant a written termination notice, which includes the reason for termination of the lease, and the requisite number of days until the lease will terminate. The termination notice must name all tenants whose names are on the lease and will typically also refer to tenants who are not on the lease as "any and all unknown occupants." The termination period varies depending on the reason for terminating the lease; for instance, non-payment of rent has a 5-day notice period, as does using the property for the furtherance of a criminal offense, whereas breaking a rule in the lease such as a prohibition of pets has a 10 day notice period. Our data does not allow us to observe the reason for eviction; however, studies in other cities have found non-payment of rent the most common reason for eviction (Desmond et al., 2013).[11]

Whether to serve a termination notice is entirely at the landlord's discretion. Chicago has no grace period on the number of days a tenant can be late on the rent before the eviction process can be initiated.

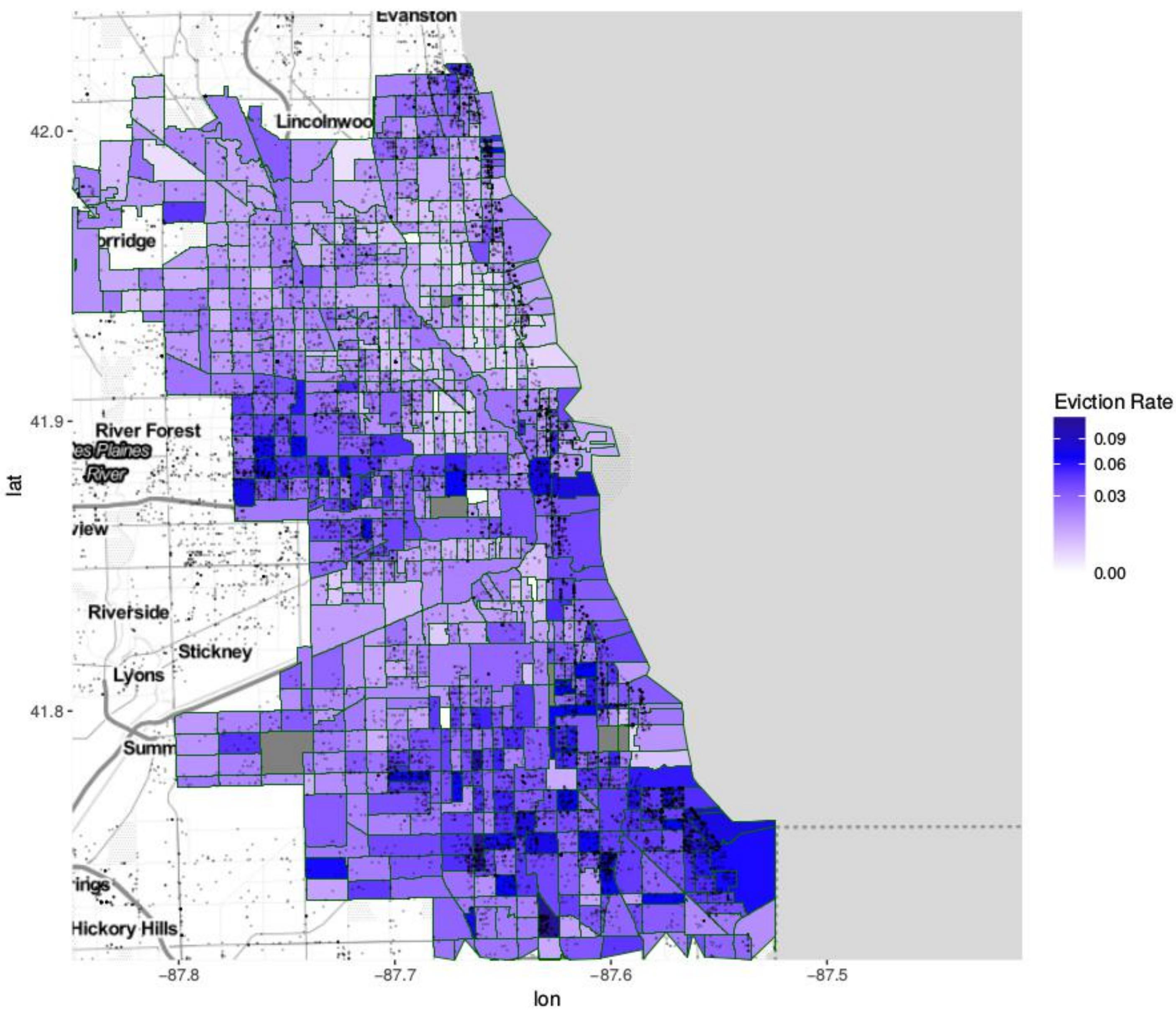
Once the number of days in the written termination notice have elapsed without resolution, the landlord has a right to take legal action and file an eviction court case with the clerk in the Circuit Court of Cook County; the case filing is the starting point from which we observe evictions in our study and is our unit of analysis. The landlord must file the case in the district where the property is located.[12] At filing, the landlord must decide whether to file a *single action* case, in which she seeks only possession of the property, or a *joint action* case, in which she seeks

[10]We provide more detail in Appendix A. See also the Municipal Code of Chicago Residential Landlords and Tenants Ordinance (RLTO) and the Illinois Compiled Statutes (ILCS).

[11]Following the notice, if the tenant pays the full amount, 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.

[12]See Appendix A.2 for a map of the six districts in Cook County.

Figure 2: Eviction rate in 2010.



Notes: This figure depicts the locations of properties for which the court ordered an eviction in Chicago, along with the rate of eviction orders by census tract. 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. Overall, we find that 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 very conservative estimate. There is substantial heterogeneity across tracts, with 9 tracts having eviction rates above 10 percent.

both possession and a money judgment. At the time of filing, the landlord or her attorney can select a return date, which in Chicago is 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 randomized to a courtroom on the selected date by computer assignment, according to the Cook County Circuit Court official policy, which became effective June 2, 1997 (General Order 97-5). The landlord receives a court room and time to which the case has been randomly assigned but does not know the name of the judge who will preside over the hearing. Our data allows us to recover the initial judge assignment, which we use to construct our instrument.[13] We formally test for balance on observables in Section 5.3.

We use the initial judge assignment for our instrument construction; however, cases may end up being ruled on by a different judge for three reasons. First, if the defendant has not been successfully served a court summons by the date scheduled for the first hearing, a new attempt must be made to serve the tenant, and the first hearing is rescheduled.[14] In some cases, this means that the case is assigned to a different judge; for example, if the currently assigned judge is transferring out of evictions court or is on leave. Second, either party has a right to request a new judge assignment once, which is then re-randomized. Third, either party can request a trial by jury, which will result in the assignment of a jury trial judge, but these cases are rare and occur in approximately 3 percent of cases. Because requests for a new judge may be endogenous to the initial judge assignment, we use the the initial judge assignment to construct our leniency measure.[15]

Eviction hearings are fast. Court observation studies have found that the average eviction hearing is completed in less than 2 minutes in Cook County (Doran et al., 2003), a finding that is consistent with studies of eviction court hearings elsewhere in the United States. The judge's main decision is whether to grant the landlord an order for possession. In joint action cases, in which the landlord is also seeking a money judgment, the judge also decides the amount of the award. If the judge decides against an eviction order, the case is dismissed, and no money judgment is awarded.

Figure 3 provides an overview of possible trajectories an eviction case can take through the court, as well as a breakdown of the fraction of cases that follows each path in our data. About two in three cases decided by a judge result in an order for eviction.

If an eviction order is granted, the landlord then files the order for possession with the sheriff's office and pays a non-refundable $60.50 administrative fee. The sheriff will then execute the eviction order, which involves changing the locks and removing the tenants' possessions. The

[13]The name of the judge potentially could be deduced in advance from public information on room assignments, but to receive a new judge assignment, the landlord would need to appear at the first hearing and request a new judge, which in practice rarely occurs.

[14]If the tenant cannot be served after multiple attempts, the judge can allow the case to proceed without the defendant, though in that case no money judgment can be awarded. In about 10 percent of cases, the case is withdrawn 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.

[15]We provide further details in Appendix C.3 describing how we derive this information from the case histories.

sheriff does not execute all orders for possession, because occupants can voluntarily leave after the judge's order, or because the landlord may neglect to pay the sheriff or sign the required paperwork. Only 50 percent of the cases in our data with an eviction order are then filed with the sheriff's office, and 54 percent of those are executed. Executed evictions take place on average two months after the eviction order is filed with the sheriff's office, though tenants do not know the exact date the eviction will be executed. The Cook County sheriff's office publishes their planned schedule only 3 days in advance.[16]

2.3 How representative is Cook County?

Cook County's eviction process is broadly similar to the eviction process in many other eviction courts across the United States (Krent et al., 2016). Cook County is similar to other county courts in how long the eviction process takes, with the process from filing an eviction case to the execution of an eviction order taking several months. The high proportion of unrepresented tenants and swiftness of eviction hearings is also similar to other courts where data is available (Desmond, 2012; Seedco Policy Center, 2010). Cook County also has minimal public- or rent-controlled housing which can change landlords' incentives to evict (Diamond et al., 2017). Like most cities, Cook County does not have a comprehensive social safety net aimed at preventing housing instability or homelessness. For example, Cook County does not have a "right to shelter" which would guarantee recently evicted individuals access to a homeless shelter.[17]

One point of distinction between Cook County and other urban settings is that it is legal for a landlord to decline to extend a lease beyond its stated termination date without a specific reason.[18] A second point of distinction is that in Cook County eviction court records are publicly observable even if the case ends in dismissal, whereas some courts limit the observability of tenants' names in these cases.

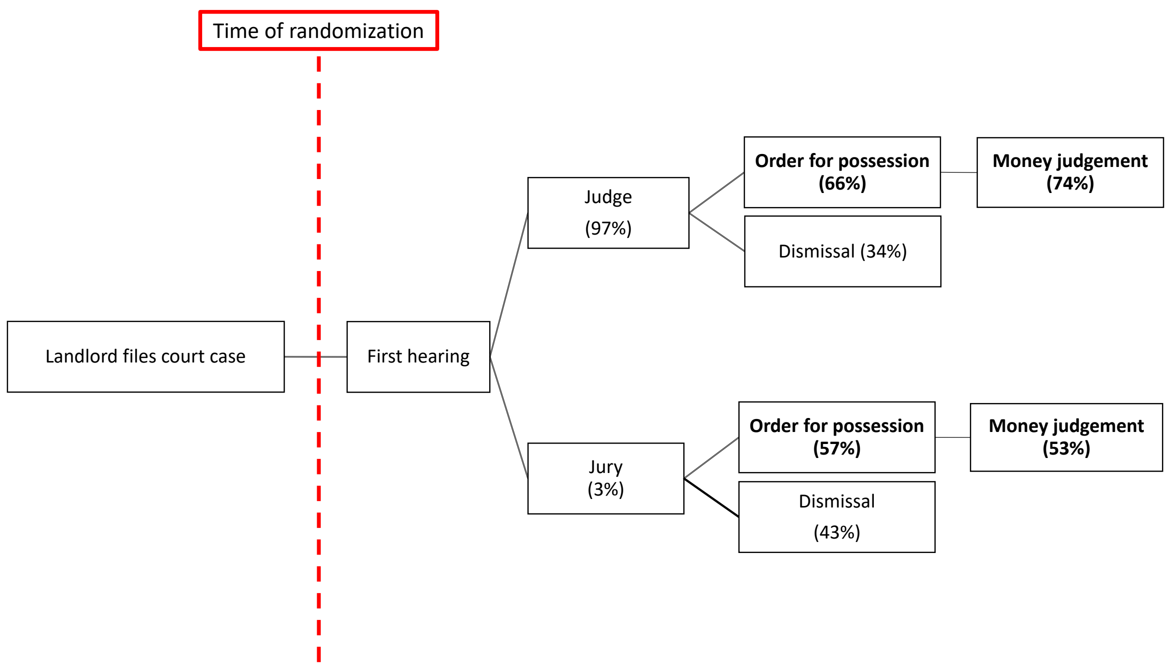
The backlog in the execution of eviction orders is a common feature of eviction courts in large metropolitan areas. Unlike warmer climate settings, however, Cook County may exhibit a more seasonal pattern of delays until the execution of eviction orders due to restrictions on evictions on cold weather days. Appendix A.2 provides further detail on Cook County's eviction process.

[16] Delays are around 1 month in the summer and around 3 months in the winter. Delays are determined by the capacity constraints of the sheriff's office and by the fact that evictions are not executed if the temperature is below 15 degrees, and typically no evictions are executed between Christmas and New Years, though the number of days for which evictions are halted varies by year.

[17] One way to characterize 'generous' homelessness policy is by whether a place has a 'right to shelter': a legal mandate for local government to provide temporary homes to those who would otherwise sleep on the street. At the moment, only D.C., Massachusetts, and New York City have RTS. In New York City, the right to shelter has been in place for 40 years.

[18] This is sometimes called a "no cause" eviction and is allowed in most U.S. cities. However, places that prohibit or place restrictions on "no cause" evictions include Seattle, San Francisco, Oakland, Berkeley, LA, San Diego, New York City, New Jersey, and New Hampshire.

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.

3 Data

This section describes our data sources, linkage procedures, and sample selection criteria.[19]

3.1 Court records

Our data set on court histories includes the near universe of single action and joint action eviction cases filed in Cook County in the years 2000-2016. We assembled this data through collection of public records, supplemented with data sourced from Record Information Services (RIS), a private company that compiles public records in Illinois. The resulting case-level data set includes the defendant's name and address, which we use for linking, and includes detailed histories of the events that were entered into the electronic case file, beginning with the filing by the landlord or his attorney. Case details include information on the case type (single or joint action), filing date, plaintiff name, defendant name, attorney name (on both sides), judge name, presence of legal assistance, the amount of money claimed by the plaintiff in a joint action case, details of each

[19] Further details on data cleaning and linkage are available in Appendix C.3.

hearing and motion filed throughout the case history – including information such as a request for a trial by jury, and the judge ruling at the conclusion of the case. Importantly, the court records include all eviction orders and all 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 via FOIA request. Using tenant addresses, we also append detailed information on neighborhood characteristics using public-use 2010 census data.

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 includes 772,846 named individuals in 583,871 cases. As laid out in Appendix Table 9, we first drop eviction cases associated with businesses, cases associated with condominiums, cases where the name of the defendant is not provided, and cases involving more than $100,000 in claimed damages. After these restrictions, we are left with 729,125 named individuals across 560,670 cases, which represent our main sample of court records. We use this sample to construct leniency measures and link to credit bureau data. For our IV analysis, we further restrict cases to those seen by judges who oversaw at least 10 cases that year and to weeks in which at least two judges were assigned to cases in that district. This sample of cases includes 615,965 named individuals in 477,919 cases, and includes 251 judges who see more than 10 cases in any year, and nearly 750 judge-year stringency observations. The average judge in our sample oversees more than 700 cases per year.

Conceptualizing treatment and counterfactual

We define a case as ending in eviction if the judge issues an order for possession. An order for possession is the legal authorization for the landlord to have the sheriff execute a lockout and remove the tenant from the unit. Throughout the paper, we will refer to an order for possession as an 'eviction order' for clarity of exposition.[20]

Cases that do not end in an eviction order are dismissals. Importantly, there are multiple types of dismissals. A dismissal "with prejudice" means the landlord is not allowed to bring another eviction case with the same allegations against the tenant; a dismissal "without prejudice," on the other hand, permits the landlord to bring the case again.[21]

Dismissals have many benefits over eviction for the tenants. Tenants whose cases end in dismissal avoid the stigma associated with an eviction order, such as additional screening or penalty in future rental contracts. Tenants may additionally avoid having a civil judgment on

[20] Our definition of an eviction order is similar to the one used by Desmond et al. (2018b) which estimates the number of evictions by county.

[21] Dismissals may also be recorded as "dismissed by stipulation or agreement," which may involve settlements in which the tenant agrees to move out or a payment agreement is made. A dismissal does not necessarily mean the tenant will continue to live in the unit.

their credit report, which occurs if there is a money judgment in a joint action case. Finally, dismissals make it more likely that the tenant will be able to remain in their current residence, though this is not guaranteed. For example, cases can be dismissed without prejudice due to the landlord and the tenant reaching an agreement where the tenant moves out. Table 10 decomposes cases by their outcomes and shows that less than 3 percent of cases end with a verdict for the defendant or the case being dismissed with prejudice, the outcomes that are most likely to allow the tenant to stay in the unit.

We explore robustness to our definition of the binary treatment in Appendix F.5. In particular, we consider three alternative definitions of the treatment: (1) cases with either a dismissal with prejudice or a verdict for the defendant, in which the tenant is more likely to remain in the unit (2) an eviction order that is also sent to the sheriff's office and executed. Approach (2) is not our preferred empirical approach, however, because the exclusion restriction under this specification is unlikely to hold, since only 50 percent of eviction orders are filed with the sheriff's office, and only 27 percent are filed with the sheriff's office and executed. Orders filed with the sheriff's office conditional on the judge granting an order for possession are not random, and we do not have an additional source of exogenous variation to identify the separate effects of sheriff's office filing or execution.

3.2 Credit bureau records

Our primary measures of financial strain come from credit files held by Experian, one of the 3 largest credit bureaus in the United States. Avery et al. (2003) provide a detailed overview of this data. Our credit report data are biennial snapshots from March of 2005 to 2017, and September of 2010. For our analysis of residential moves, we exclude the September 2010 sample for ease of interpretation so that move probabilities are defined over constant two-year intervals. We have two credit panels. One is the panel that is linked to our courtroom sample. The second is a panel of a 10 percent random sample of individuals from Cook County zipcodes.

We analyze the following sets of financial outcomes: overall financial health, unpaid bills, and an indicator for having an open auto loan or lease, which serves as a proxy for durable consumption. We also study the effect on home ownership, which we observe as an open mortgage trade in the credit file. We also study demand for high interest loans using subprime borrowing data, described below.

The outcome measures are described in detail in the Appendix 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; scores under 600 are considered subprime. We measure unpaid bills using total balances in collection. 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). Our proxy for durable goods consumption is any positive balance on an auto loan or lease, following an approach similar to Dobkin et al. (2018) and Dobbie et al. (2018). We also measure the presence of any open mortgage trade. An individual will appear in the data as having a mortgage trade if they are a signatory on any mortgage loan with a positive balance, whether it is for their primary residence, an investment property, or the co-signed loan of a parent, relative, or partner.

The credit bureau has supplementary information including zip code of residence and demographic information including gender and year of birth.[22] We use zipcode of residence to track residential moves; a nice feature of the data is that it allows us to track moves throughout the United States and its territories, which has not been done in the eviction setting. There are 215 zipcodes in Cook County, and hence we observe a relatively fine level of geography.[23]

Our credit bureau data is also linked to the largest database of subprime borrowing behavior, which includes over 62 million consumers. We measure demand for high interest loans using 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; we observe subprime account openings for all months between January 2010 and November 2018. We emphasize that we only observe subprime borrowing activity for consumers who have a record in our main credit file. Approximately 32 percent of our credit record sample have a payday loan account inquiry at some point in the sample period, and about 6 percent open an account. These data are described in more detail in Appendix C.2.

3.3 Data linkage and sample restrictions

Evictions case data were linked by Experian to credit report archives by searching name and address identifiers against its master file that includes a name and address history. The overall match rate is 61.2 percent, which is slightly lower but in the range of match rates of studies that use a similar strategy to link administrative data sources.[24] 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.

Our sample population are those who are "credit visible," meaning they have a credit record.

[22]Landlords do not observe evictions directly on a credit report, but evictions may be included in the aggregated category of civil judgments if the eviction order includes a money judgment. In those cases, credit bureaus report the presence of a money judgment and the judgment amount awarded by the court.

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

[24]For example, Dobkin et al. (2018) are able to match 72 percent of their Medicaid sample when additionally using SSNs for matching, and the Oregon Health Experiment has a match rate of 68.5 percent.

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 far higher in our population, since having a utility bill alone would be enough to generate a credit record, and individuals with their name on a lease are likely to have had a utility bill.[25]

To better understand our sample and how it relates to the overall population of tenants in eviction court, we explore characteristics of credit record matches in Appendix D. Appendix Table 12 shows that evicted tenants are 1.8 percentage points less likely to be matched to a credit record, male tenants are less likely to be matched, and tenants without legal representation are 1.5 percentage points less likely to be matched, while tenants in richer neighborhoods are more likely to be matched.

We explore attrition in Appendix Table 11. We show that, conditional on being matched to a baseline year t credit bureau file, the probability of appearing in a subsequent credit bureau file is over 99 percent in each of the subsequent file years. We also show 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 that has been reweighted to match the neighborhood distribution of eviction cases (column 3).

There are a few interesting patterns to highlight. First, the eviction court sample is far more likely to be female (61 percent compared to 50 percent) and is slightly younger than the random sample (38 years old, on average, compared to 44.8 in the random sample). 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 first name, last name, and neighborhood, and adopt an 85 percent probability threshold.

Note that the neighborhood characteristics are similar to the random sample by construction, because of reweighting. It is striking how much debt tenants in eviction court are carrying relative to the random sample from the neighborhood: the average collections balance for tenants in eviction court is over $3,000, compared to about $1650 for the random sample. The event studies presented in the next section will show that most of this debt originates from before the

[25]Up until 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 only 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 comparable cities in other states.

Table 1: Summary statistics: IV sample

	Evicted		Not Evicted		Random sample	
Person characteristics						
Age at case	38.321	(14.520)	38.269	(14.893)	44.790	(17.496)
Female	0.612	(0.487)	0.609	(0.488)	0.502	(0.500)
Black	0.527	(0.499)	0.490	(0.500)		
Case Characteristics						
Eviction order	1.000	(0.000)	0.000	(0.000)		
Ad Damnum Amount (1000s)	2.585	(3.976)	2.275	(4.094)		
Joint Action	0.838	(0.368)	0.800	(0.400)		
Tenant Pro Se	0.970	(0.170)	0.945	(0.229)		
Landlord Pro Se	0.254	(0.435)	0.246	(0.431)		
Neighborhood Characteristics						
Median household inc. (1000s)	47.171	(17.635)	49.555	(18.960)	47.704	(17.753)
Poverty Rate	18.644	(9.736)	17.673	(9.759)	18.401	(9.721)
Median Rent	961.337	(168.940)	983.322	(192.677)	967.762	(177.051)
Pct. White	0.351	(0.289)	0.382	(0.292)	0.358	(0.290)
Pct. Black	0.492	(0.375)	0.456	(0.372)	0.483	(0.374)
Subsequent Outcomes (12-36 mo.)						
Credit Score	526.856	(66.656)	545.247	(79.027)	618.154	(103.580)
Total bal. collections	3,814.293	(4,752.086)	3,088.753	(4,379.230)	1,137.576	(2,693.680)
Any Auto Loan or Lease	0.138	(0.341)	0.197	(0.394)	0.118	(0.299)
Num. Payday Inquiries	1.129	(6.576)	1.190	(6.991)	0.623	(8.008)
Num. Payday Accounts Opened	0.033	(0.356)	0.047	(0.432)	0.023	(0.510)

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 has been reweighted to match the distribution of individuals across neighborhoods in the eviction sample. See notes in the text for the sample restrictions. Race was imputed using last name and Census track (Imai and Khanna, 2016; Khanna et al., 2017).

eviction filing.

One strength of our data is that we can follow individuals across different eviction cases throughout our 2000-2016 sample period, using our anonymized credit bureau identifiers. The table reports the fraction of our eviction sample with a subsequent eviction case, at the same address and at a different address. Eviction tenants are likely to appear again in eviction court: a tenant who is evicted has a 4 percent chance of appearing in eviction court again within 36 months at the same address, and a 17 percent chance of appearing in eviction court at a different address, compared to 12 percent and 18 percent, respectively, for the non-evicted tenants. Note that tenants who are evicted by the court may have a future case at the same address, if, for example, the landlord decides to allow the tenant to remain in the unit after the first eviction order or does not file the order with the sheriff.

4 Descriptive evidence: selection and the evolution of financial strain

This section provides new descriptive evidence about selection into eviction court, the impact of eviction, and how financial health evolves both before and after the eviction case is filed.

4.1 Selection into eviction

Our research design aims to address two forms of selection bias present when studying evictions. First, selection into eviction court is not random and, second, the outcome of the court case is not random.

The empirical approach that has been available to researchers studying eviction thus far is a comparison of evicted tenants to observationally similar renters in the neighborhood (Desmond and Gershenson, 2016; Desmond and Shollenberger, 2015; Desmond and Kimbro, 2015). This literature uses multivariate regression or propensity score matching to estimate the effect of eviction, assuming that, conditional on observables, eviction is effectively random. This assumption is quite strong, because evicted individuals and individuals not facing an eviction case may differ in unobservable ways – such as their health or job prospects – which are likely correlated with eviction and likely to affect future outcomes. We now document the importance of selection into eviction court and show how the effect attributed to eviction changes when the comparison group is non-evicted tenants rather than renters from the neighborhood.

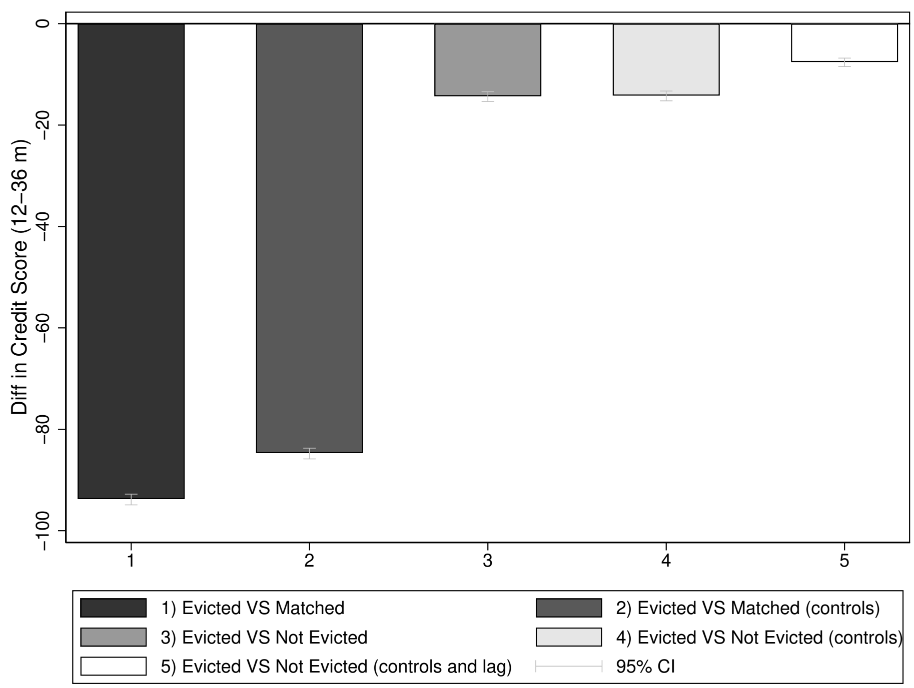
For our first analysis, we append the 10 percent random sample from Cook County to our court sample, restricting the random sample to those over age 21 who are non-homeowners (i.e. with no open mortgage trade) and randomizing a placebo filing month. We also re-weight the regression sample so that the random sample of individuals matches our eviction court sample in their distribution across neighborhoods.

Comparing our eviction court sample to the random sample, we can replicate the large negative effect of eviction found in the literature. The first bar of Figure 4 shows the average credit score difference between evicted individuals and individuals from similar neighborhoods and shows a nearly 100 point difference. 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 and does little to close this gap. These analyses suggest large differences in level between evicted individuals and similar individuals from the neighborhood, but do little to capture selection into eviction court on unobservables such as recent financial or labor market shocks.

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 30 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 baseline levels further reduces the gap by approximately half, demonstrating that there is selection into the eviction decision even once we restrict our comparison to those in eviction court.[26]

These results suggest that there are two important sources of selection that must be addressed when considering the causal impacts of eviction: selection into eviction court and selection into the eviction order. 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 a judge-IV strategy which we describe in more detail in Section 5. 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.

Figure 4: Selection into eviction court



Notes: This figure depicts selection into eviction court, and selection into an eviction order, conditional on having an eviction filing. Column 1 plots the difference in credit score at 12-36 months after filing, for the court sample v. 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 12-36 months after filing, for evicted v. 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.

[26] Appendix Figure 12 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.

4.2 Event studies

This section presents the event study analysis, which is based on comparisons of court-ordered evictions to dismissals, for the sample of tenants who appear in court. This analysis allows us to study the dynamics into and out of eviction court separately by the outcome of the case. We use the following regression, where r indexes the month relative to the eviction case filing:

$$y_{it} = \gamma_t + \delta \times E_i + \sum_{r=S}^{F} \beta_r + \sum_{r=S}^{F} \delta_r \times E_i + \epsilon_{it} \tag{4.1}$$

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-evict mean in $S = -42$ so that the magnitudes are easy to interpret.[27]

Overlaid on these nonparametric event studies, we depict a parametric specification of Equation 4.1, 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. Note that we require the polynomials on either side of the eviction filing to meet at $r = 0$, a choice motivated by the non-parametric 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 13 reports difference-in-difference estimates of the parametric specification at different time horizons, relative to $r = -12$. As above, Figure 6 shows that tenants who appear in eviction court have very poor credit in the run up to eviction and that those whose case ends in eviction have worse credit than the non-evicted group in the baseline. Both groups experience deteriorating credit scores in the 24 months prior to the filing date. Remarkably, the two groups' credit scores remain broadly parallel throughout the sample period, suggesting that eviction does not have an additional scarring effect on credit score for the evicted group. We explore this hypothesis further in the instrumental variables analysis. It is notable, however, that it takes 4-5 years for the two groups to return to their pre-eviction peak.

The top right panel depicts a different pattern for total balances in collections. Collections represent unpaid debt, such as credit card balances, which the original lender decides to turn

[27] There is a tradeoff between including a longer time series and introducing composition effects in the coefficients. Appendix Table 14 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.

over to a collections company following a period of delinquency (typically at least 30 days).[28] The top right panel of Figure 6 shows that in the run-up to eviction court, both groups have high amounts of collections debt and experience rising balances in collections that are approximately parallel. After eviction, the evicted group experiences a steeper rise in balances in collection. The difference at 36 months is 159 dollars, a small difference relative to the average ad damnum amount sought by the landlord. The difference is also small when compared to tenants' average pre-filing balance in collections. Appendix Figure 13 further disaggregates the collections into the four largest collections categories, revealing that utilities and retail debt represent the biggest increases, while medical debt increases only slightly.

Evictions may be mechanically related to collection 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 13, we explore this possibility by presenting the collections event study separately by case type. This figure shows a broadly similar evolution of collections debt in cases for which the plaintiff seeks no money judgment, suggesting the apparent effect is not mechanical. Note that the average collections debt never returns to the pre-eviction court average.

The next two panels of Figure 6 depict the results for durable goods consumption and homeownership.[29] The auto loan variable exhibits a flat or slightly increasing trends in the run-up to eviction court, followed by a drop after filing, along with a widening between evicted and non-evicted tenants. This suggests a decrease in expenditures or consumption. Note that even non-evicted tenants exhibit this pattern of decreased consumption following the filing, which reflects that both groups of tenants are experiencing strain that coincides with the filing. We will show in the next subsection that they both experience high move rates.[30] With the mortgage indicator, the most striking pattern is the steep drop in evicted tenants' probability of having a mortgage loan following eviction, from 8 percent to 4-5 percent, and this drop persists for the entire 72 month period.

We now turn to subprime borrowing behavior, which includes single payment microloan inquiries and account openings. Our data includes both online and storefront loans, and provides 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

[28]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.

[29]Following Dobbie et al. (2017) and Dobkin et al. (2018), we proxy durable good consumption with the presence of an auto loan or lease

[30]Note that these descriptive results are not sensitive to how we define a court-ordered eviction. In Appendix Table 17 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 facts.

the 3 years leading up to eviction filing, from 0.8-1 percent per month to 1.6 percent per month, followed by an immediate decline for both groups after the filing date.

A parallel increase in the number of payday loan openings occurs in the run-up to eviction court. After the eviction filing, the non-evicted group has higher long-run levels of payday loan openings relative to the evicted group. Taken together, the two panels suggest a surge in the demand for short-term cash. Given that inquiries fall in parallel but openings remain higher for non-evicted tenants suggests that 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 when we restrict the comparison to tenants in eviction court, we find that evicted tenants are negatively selected, 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 order leads households to large and lasting financial strain, but instead suggest that eviction may exacerbate the initial decline and slow the recovery.

Appendix Figures 14-16 reproduce the regressions behind Figure 6 in order to contrast these descriptive patterns for several subgroups: (i) joint versus single action cases, (ii) multi- versus single-headed households (as determined by the number of defendant names in the case), (iii) individuals without prior eviction cases versus individuals with prior cases. These subgroups look similar to the main effects with a few notable differences. First, those without prior eviction cases have a big spike in demand for payday loans compared to those with prior cases. They also appear to be harder hit by the eviction event in terms of revolving balances and the presence of an auto loan. Similarly, single-headed households also have a much bigger spike in demand for payday loans and appear to be harder hit in terms of durable goods consumption.

Analysis of residential moves

For the analysis of residential moves, we modify Equation 4.1 so that r is now measured in years relative to the filing year, rather than months relative to the filing month. We also drop the September 2010 period, so that sample years are March 2005-2017 biennially, which allows us to interpret residential zipcode moves over constant 24 month intervals. In addition, we include only individuals who are observed 5 years prior to eviction filing, $r = -5$. We choose the baseline (omitted) year $r = -5$, and define a zipcode move as a change in 5-digit zipcode relative to the observed zipcode 24 months prior.[31] Thus, the coefficient on β_r in Equation 4.1 is the probability of moving from year $r-2$ to year r for the non-evicted group. In these figures, as before, we add the non-evict mean in the base period to the coefficients for each of interpretation.

[31] These results are similar when restricting to individuals observed 3 years prior to eviction filing.

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, which are indicated in the figure with open markers. The first result is that 2-year zipcode move rates are high. The non-evicted group has a nearly 37.5 percent move probability from the period 5 years prior to filing to 3 year prior to filing, while the evicted group has around 40 percent move probability over the same period. As a comparison, the percent of renters that moved 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.[32]

Not surprisingly, the probability of moving spikes 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 between 1 year after filing to 3 years after – a gap of around 5 percentage points, which is about 13 percent of the non-evicted group mean.

Again, these estimates reflect zipcode-level moves, and may mask significant differences in move probabilities between evicted and non-evicted groups within zipcodes. The figure does, however, suggest that both evicted and non-evicted groups are experiencing relatively high move probabilities in both the run-up and aftermath of eviction court. The figure also suggests that difference between the groups appears overall somewhat minor in magnitude.

The right panel of Figure 8 re-estimates the event study but with a measure of neighborhood quality as the outcome, the zipcode-level poverty rate. The main takeaway from this panel is that households from higher poverty neighborhoods are more likely to be evicted and the gap between evicted and non-evicted widens slightly following eviction, but that the overall trend is downward for both groups after year 3, indicating that over time both groups are able to relocate to neighborhoods with slightly lower poverty rates.

5 IV analysis

In this section, we develop an instrumental variables 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 several empirical tests of these assumptions.

5.1 Empirical framework

As we saw in the previous section, defendants in eviction cases have strong pre-trends in financial strain prior to the case regardless of the case outcome. Moreover, defendants who are evicted

[32] We would like to make the comparison of move rates to the random sample, but unfortunately the random sample does not include the time-varying zipcode of residence.

have notably worse financial strain prior to the case filing than defendants who are not evicted. To address the identification challenge of correlated unobservables and simultaneity, we exploit the random assignment of judges to eviction cases. Let E_i be an indicator equal to 1 if the judge orders eviction for household i, $Z_{j(i)}$ the stringency measure of judge j assigned to i's case, 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, and W_i is a set of second-stage controls.

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 is a meaningful predictor of of the case outcome. Second, we need exogeneity of the instrument, i.e. that $Z_{j(i)}$ and η_i are independent after conditioning on controls X_i. This assumption implies that judge leniency affects outcomes Y_i only through the eviction order decision E_i.

If the effect of eviction is heterogeneous across individuals, we also require monotonicity: that those who are evicted would also be evicted by a stricter judge, and that those who are not evicted would also not be evicted by a more lenient judge.

Under these assumptions the estimation will recover the local average treatment effect (LATE) of an eviction order: the effect of an eviction order for tenants who would not be evicted by the most lenient judge but who would be evicted by the strictest judge (Imbens and Angrist, 1994). This group of tenants has a great deal of policy relevance because changes in policy are likely to affect those marginal cases for which the judge's discretion makes the difference. In addition, many recent policy proposals explicitly target the eviction court setting.

5.2 Measuring judge stringency

We construct our judge stringency instrument by computing the leave-out mean eviction rate for the initial judge assignment over the filing year, and then residualizing it of year-by-district fixed effects. We use a residualized stringency measure to account for differences in the type of cases across districts or changes in regulation over time. Our 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. Section F.1 below provides additional robustness, showing that the first stage does not notably change when using an alternative procedure for assigning judges to cases and when increasing or decreasing the minimum number of cases a judge must see in a given year for the case to be included in the sample.

5.3 Validating the empirical design

This section provides empirical evidence to support the assumptions necessary for our research design. In particular, we evaluate the random assignment of cases as well as the relevance, exclusion, and monotonicity of the instrument.

Random assignment. As discussed above, court cases are randomly assigned a room and time at the time of filing by court order. Here we provide additional empirical evidence that random assignment holds in practice. Table 2 shows that case characteristics and defendant characteristics predict eviction status but do not predict the residual stringency of the judge assigned to the case. The first column presents results from a regression of eviction ruling on case and defendant characteristics and finds that all are statistically significant predictors of eviction ruling. The second column presents results from a regression of judge stringency on the same covariates and finds that all covariates have very small and statistically insignificant effects on the stringency of the judge assigned to the case. Note that the first row, the coefficient on 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 with experience landlords are unable to select a favorable judge.

Relevance. Next we demonstrate that instrumental relevance holds. The black line in Figure 5 shows the result of a local linear regression of 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.

The third columns of Table 28 shows results for the corresponding linear regression. Stringency has a large and statistically significant impact on eviction, with a p-value of less than 0.001. The F-statistic for the full first stage is 91, while the partial F statistic for stringency is 1132, suggesting that the stringency instrument passes the standard rule-of-thumb tests for weak instruments.[33]

In Appendix F.1 we provide additional robustness checks on the first stage. In particular, we show that the first stage is not sensitive to our sample selection criteria, nor is it sensitive to controls for additional judge stringency measures (e.g. residual judge stringency in case length, granting continuances, judgment amount in joint action cases, and granting stays). In the third row of Table 15, we construct an alternative judge stringency measure based on the first judge

[33] These results use all cases meeting the sample restrictions laid out in Section 3.3 and not just those cases matched to credit bureau records. Figures 31 and 32 in the Appendix provide the first-stage results for the linked sample.

Table 2: **Random Assignment of Judges**

	(1) Evicted	(2) Judge Stringency
Landlord number of cases	-0.00255***	0.00004
	(0.00046)	(0.00002)
Joint Action	0.04540***	-0.00016
	(0.00677)	(0.00032)
Ad Damnum Amount (1000s)	0.00386***	0.00003
	(0.00037)	(0.00002)
Age at case	-0.01109***	0.00003
	(0.00192)	(0.00011)
$Age^2/1000$	0.22683***	-0.00018
	(0.04188)	(0.00245)
$Age^3/1000$	-0.00145***	-0.00000
	(0.00029)	(0.00002)
Female	-0.00658***	-0.00007
	(0.00210)	(0.00013)
Black	0.04028***	-0.00006
	(0.00299)	(0.00015)
Missing female	-0.01355***	-0.00014
	(0.00449)	(0.00025)
Missing age	-0.21798***	0.00053
	(0.02951)	(0.00179)
Number of observations	232,971	232,971
Joint F-Test Stat.	44.741	0.904
p-value	0.000	0.513

Notes: The left column shows the regression of eviction status on case and defendant characteristics. The right column shows the regression of residual stringency on case and defendant characteristics. Both columns use our linked IV sample.

observed in the case history rather than the judge assigned at filing. Lastly, we also 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 our robustness checks, we find that the coefficient on residual stringency remains similar to the main specification, remains positive, and remains statistically significant with small standard errors. These robustness checks demonstrate that our first stage is robust to additional controls, different sample-selection criteria, different construction of first judge, and split-sample estimation of stringency.

Exclusion. In addition to random assignment, a requirement for the causal interpretation of our results is that a judge's stringency affects tenant outcomes only through the eviction order. As discussed above, in the eviction court setting, there are two main areas of discretion for the judge, the eviction order, and in joint cases in which the tenant is seeking a money judgment, the judgment amount. Such "multi-dimensional sentencing" makes it more challenging to isolate the role of different components to the sentence (see, e.g., Mueller-Smith (2016) and Bhuller et al. (2016)). We present our main results separately for joint and single action cases, since in single action cases the judge has only one main area of discretion, although we note that these cases may differ, e.g., in the reason for the eviction filing.

As a first step toward exploring the multi-dimensional aspect of sentencing, we correlate judge stringency with money judgment awards (conditional on an eviction). Table 26 shows regressions of log judgment amount regressed on our judge stringency instrument and the log ad damnum for the case for joint action cases ending in eviction. The first two columns exclude cases where no money was awarded while the second column includes the extensive margin. Overall, we find no evidence of a statistically significant relationship between ruling amount and judge stringency.

In the results section, we perform several exercises to explore the sensitivity of our estimates to the exclusion restriction, following the approach of Bhuller et al. (2016). Using our two stringency measures – our main eviction order stringency and our judgment amount stringency – we perform three exercises: first, we control for this second stringency measure in both first and second stages of our main IV regressions; second, we perform reduced form regressions of our financial strain outcomes on our two stringency measures; and third, we we allow for two endogenous regressors (eviction order and judgment amount) and instrument for these two measures using the two stringency measures. The main takeaway from these exercises, as will be shown below, is that the relationship between our main stringency measure and the eviction order is largely unchanged by including the second stringency measure as a control, and the latter two exercises show our main IV estimates are largely insensitive to including the judgment stringency.

We note that aside from the two main areas of judge discretion, judges may have discretion over other more minor aspects of the case, such as whether to grant a continuance to allow the defendant additional time to find legal assistance, and whether to grant a stay of the eviction order, to provide evicted tenants more time to find a new housing arrangement before the order may be carried out by the sheriff. Judges may also differ in how long their cases last, which may give the defendant more time to come up with the rent or prompt the plaintiff to settle.

As a final check on exclusion, we construct three additional residual stringency measures for these minor areas of discretion: judge stringency in propensity to grant continuances, propensity to grant stays, and overall case length. Appendix Table 16 evaluates the correlation between our five residual measures of judge stringency. Overall, we find that the correlations between the various residual stringency measures are small, with two exceptions. First, residual eviction stringency has a correlation of -0.326 with residual case length. This may suggest that more stringent judges allow cases to advance more quickly, or simply that more time is associated with cases that end up being dismissed. Second, residual case length and residual granting of continuances have a correlation of 0.160, which would be expected as granting a continuance directly increases the length of the case. Lastly, the first column of Appendix Table 17 shows the first stage regression controlling for the four other measures of residual judge variation. As shown in the table, none of the other residual judge leniencies are statistically significant, and the coefficient on our main judge stringency measure changes very little.

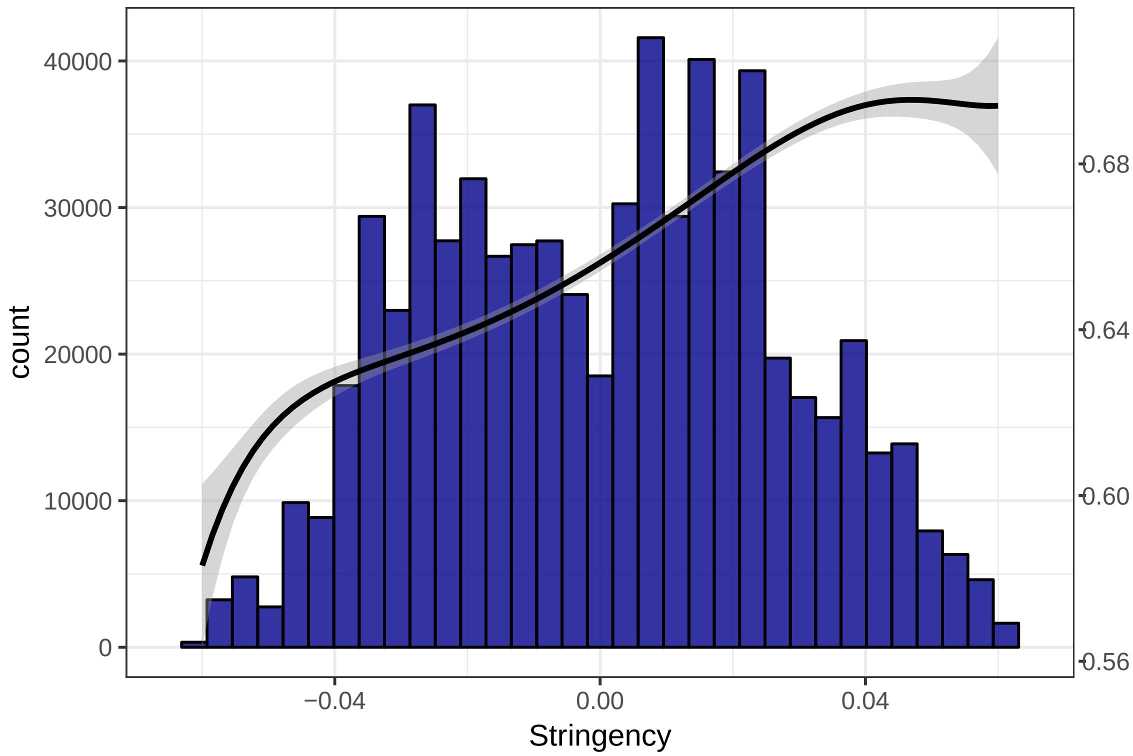
Monotonicity. For our IV estimates to be interpreted as local average treatment effects, we need the monotonicity assumption to hold. In our context, monotonicity requires that any defendant who is not evicted would also not be evicted by a more lenient judge, or, conversely, that any defendant who is 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 sub-sample, e.g. by race or neighborhood income quartile. Our data allows detailed sub-samples, including interactions between judge characteristics and individual characteristics.

Table 22 presents the slope estimate from a regression of eviction on residual stringency and the controls used in Table 28, but restricted to several different sub-populations. If judge stringency for any sub-population 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 main sample, while the remaining rows show the coefficient by case type, gender, attorney status, race, and landlord size. Across all sub-samples the coefficient on stringency keeps the same sign and does not vary widely, providing evidence that monotonicity is not violated.

In Section F.4 of the Appendix, we provide additional tests of monotonicity by conditioning on both judge characteristics and defendant characteristics for the judges who see the most cases in our data. For this exercise, we hand collected additional demographic and background information for more than 100 judges who oversee the most eviction cases in our sample. As shown in Table 20, the coefficient is positive for all but two of these two-way interactions, which provides additional evidence that monotonicity is satisfied in this setting.[34]

[34]The two instances in which the coefficient is not positive, the coefficient is statistically insignificant. Both of these

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 dependant variable, a local linear polynomial in judge stringency, and court × 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. The two vertical gray lines show the 10th and 90th percentile of judge leniency.

6 Results: IV regressions

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.

Financial strain

Table 3 presents the main results of the effects of eviction on financial strain for both the short run (panel I) and the long run (panel II).

The OLS estimates presented in columns 1-2 reflect the cross-sectional differences observed in the event study figures: tenants following eviction are more distressed than those who are

cases involve Hispanic judges, for which we only have 8 and for which we have substantially smaller sample sizes. See Section F.4 of the Appendix for detail.

not evicted. Column 1 includes controls for case type, gender, an indicator for the tenant being black, and a cubic in age at case and shows a gap of 16.4 credit score points (or about 0.2 of a standard deviation). Column 2 controls for additional pre-filing measures of financial strain; these are the individual means, over their pre-filing observations, mean credit score, collections debt, and an indicator for having an auto loan. 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 re-weights the OLS regression so that the regression sample matches the distribution of compliers based on observables.[35] 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 small causal effect of receiving an eviction order on credit score (12.6 points), and both groups on average remain the subprime category of creditworthiness. These results are similar for balances in collection: with the full set of controls, the OLS results show evicted tenants to have 433 dollars more in collections debt 13-37 months after eviction, the point estimates in columns 4 and 5 are 116 and 182 dollars, respectively, which is small relative to the non-evicted mean collections balances of $3,000.

An important exception to this pattern of small effects on financial strain is durable goods consumption, which we proxy using an indicator for having an auto loan or lease. The IV results show that eviction causes a decline in the probability of having an open auto loan or lease. The magnitudes are large: column 5 indicates a decline of 7.5 percentage points, relative to a non-evicted mean of 20 percent. These results recall one of the key takeaways of the event studies; consumption appears to decline, as indicated by the event studies of auto loans. The mortgage results are somewhat similar. While cross-sectional comparisons between evicted and non-evicted tenants show a deep scarring effect of eviction on the probability of a future mortgage, the IV results are not significant, and we can reject effect sizes of .08 of a standard deviation.

We next turn to payday loan inquiries and openings. The OLS result in column 2 reveals that evicted tenants have a statistically significant 1-1.2 percent *fewer* inquiries over the short-run outcome period, relative to a non-evicted mean of 14.5 percent. This result may reflect composition effects – for instance, evicted tenants may be less sophisticated in navigating high-interest lenders – or it may reflect that evicted tenants have lower demand for higher interest loans following eviction, due to moving to lower-rent units. The IV results are statistically insignificant, although the confidence intervals are somewhat wide. For instance, the short-run effect has a confidence interval of $[-2.76, 11.99]$, meaning we can rule out an effect size of .34 of a standard deviation increase or larger. The results on account openings are similar: the OLS estimates show that evicted tenants open 0.5 to .6 percent fewer accounts in the 13-36 months

[35]Following Bhuller et al. (2016) 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 F.7 for more details.

following eviction court, off a baseline non-evicted mean of 2.3 percent. This represents about a 20 percent difference, which is quite large. The IV estimate switches signs and is statistically significant, suggesting an increase short term borrowing. This contrasts with the event study result, which showed that on average evicted tenants have reduced payday borrowing than non-evicted tenants following eviction court, highlighting the importance of a causal empirical design in this setting.

Tables 5 and 6 considers heterogeneity along several key dimensions: (i) joint action v. single action, (ii) multi-headed households v. single-headed households, (iii) those without prior cases v. those with a prior case, and (iv) above median poverty neighborhood of the unit v. below median poverty. Somewhat surprisingly, the negative effect of eviction on credit score is stronger among single action cases compared to joint action; the IV estimate is -29 credit points relative to -7. This result is surprising because single action cases are those in which the landlord seeks to remove the tenant from the residence without seeking to recover back rent, while the joint action seeks to recover back rent in addition to the removal of the tenant. The consumption effects are stronger, however, for joint action cases, which suggests greater strain on households of losing a joint action case. Similarly the negative consumption effects are stronger for those with a prior case and those in higher poverty neighborhoods.

Given the small estimated effects, it is important to consider whether the effect sizes depend on how we define the treatment. If dismissals are typically situations in which tenants are moving out voluntarily, the comparison of treatment and control may be compressed. Hence, as a robustness check, we redefine the treatment as an indicator for the tenant having the legal right to remain in the unit. Specifically, we look for case outcomes that end in “Dismissed for Want of Prosecution,” “Dismissed With Prejudice,” or “Verdict for Defendant.” Importantly, in all three of these outcomes, the landlord may not bring the same case with the same allegations against the tenant. The instrument is also redefined, so that residualized stringency is based on dismissals with prejudice. Appendix Table 29 shows the re-estimated results, with the estimates obviously changing sign due to the re-defined instrument. The effects are larger in magnitude, with the results broadly similar. One exception is the auto loan or lease, which has a very large effect size: having a case dismissed with prejudice causes a tenant to have a 24.8 percentage point increase in having an auto loan or lease, off a base of 16 percent.

As a final robustness exercise, we explore the sensitivity of the IV results to the exclusion restriction. Recall the IV regression requires an assumption that judge stringency affects the financial strain outcomes only through the order of possession. Since judges may decide also 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. (2016), we construct a second stringency measure, one 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 we allow for two endogenous regressors (eviction order and judgment amount) and instrument for these two measures using the two stringency measures.

Table 31, 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 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 judgment stringency. The analysis of auto loans (columns 5 and 6) shows quite clearly that the eviction order itself, rather than the judgment, is the driver of reduced auto consumption.

Residential moves

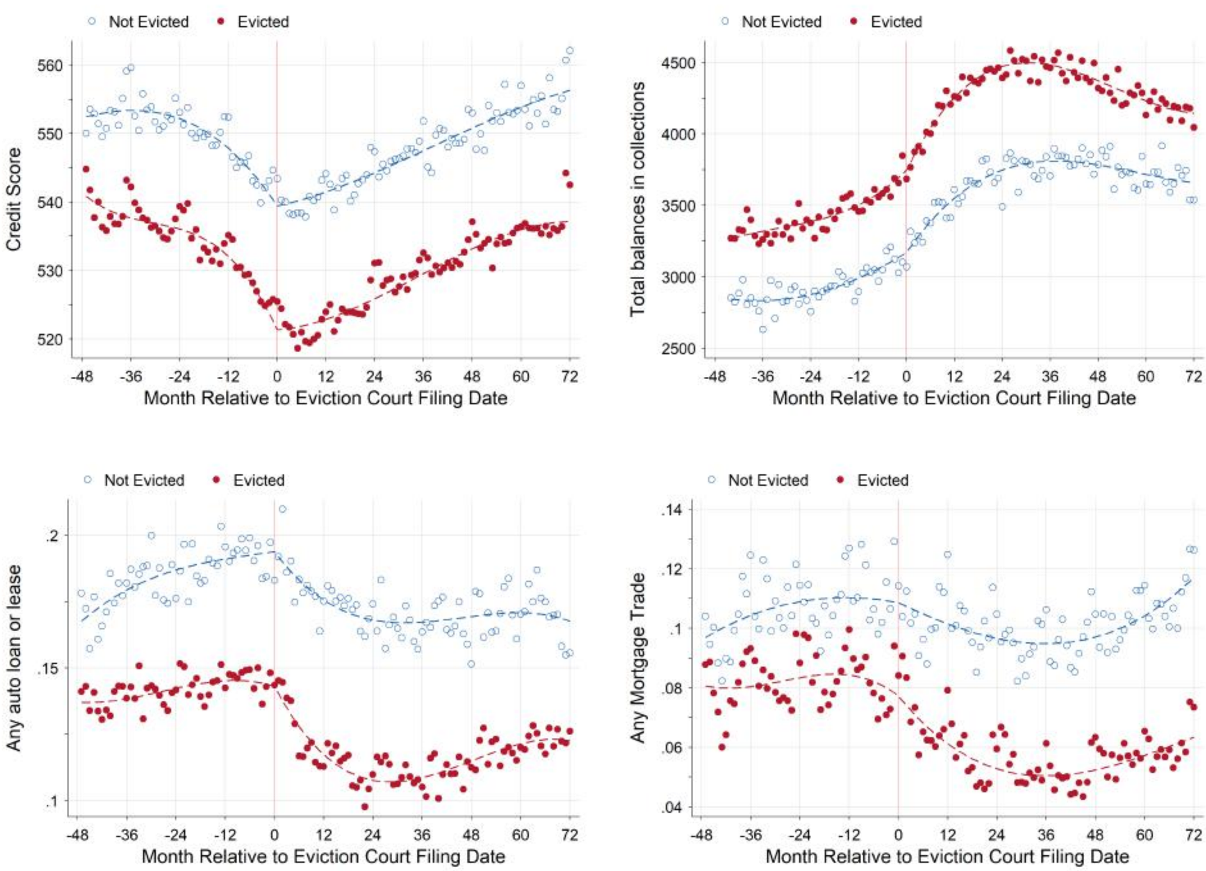
Table 4 presents the analysis of the effect of eviction on residential moves. We use the sample used for event studies of residential moves, again restricting to a balanced panel of individuals observed 3 years prior to eviction and at least 5 years following eviction.

The dependent variable "Cumulative Moves" is the number of cumulative zipcode changes since the year prior to the eviction case. Just as we found in the event study analysis, evicted tenants have a higher probability of moving: by year 3, relative to year -1, they are 0.06 to 0.08 more cumulative moves, off a base of 0.98, which is the non-evicted group mean. The LATE estimate is negative and statistically indistinguishable from 0, suggesting that eviction does not cause an increase in moves rate across zipcodes, at least for the group affected by the stringency of the judge assigned to the case. This result is aligned with the event study, which shows high move rates for both evicted and non-evicted tenants.

The OLS estimates show that evicted tenants relocate to higher-poverty neighborhoods, and this is true both at 3 years after eviction, and at 5 years after eviction, but again the IV results are not statistically significant. The large standard errors means that the 95 percent confidence interval of the 3-year estimate cannot rule out a 3.11 unit increase in the poverty rate, which is roughly 0.3 of a standard deviation, which must be kept in mind when interpreting the results. The OLS results line up with Desmond and Shollenberger (2015), who study survey data collected in Milwaukee and find that renters who experienced a forced move (including eviction) relocate to poorer neighborhoods than those who move voluntarily.

One of the virtues of the linked credit bureau data is being able to follow individuals across eviction cases, despite many of the defendants changing names due to marriage or divorce, or having a first name that exhibits slight variations across cases (e.g. "Jim Smith" v. "James Smith"). The last two rows of each panel of Table 4 examines whether receiving an eviction order has a causal effect on a future eviction filing. Eviction has a strong negative causal effect on having an eviction filing in the next 3 years from any address, but this effect is driven by the reduced probability of having another eviction filing from the same address . The final row of each panel shows the effect of eviction on the probability of a future eviction filing from a different address, finding no statistical effect. The tenant's probability of having a future eviction filing over the 60 month horizon is reduced by 0.14, off a base of 0.27, although the standard errors are again quite large. This provides some evidence against the idea that eviction contributes to a "slipperiness" of housing, in that an eviction order does not appear to beget future filings.[36]

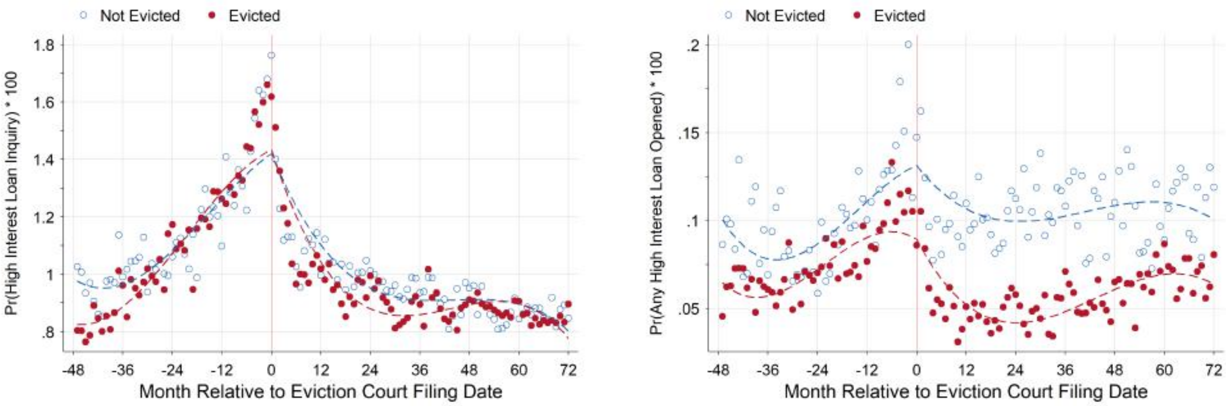
Figure 6: Evolution of financial strain relative to the eviction filing month.



The figure plots estimates of $\{\beta_r\}$ and $\{\delta + \delta_r + \beta_r\}$ from the regression: $y_{it} = \gamma_t + \delta \times evict_i + \sum_{r=S}^{F} \beta_r + \sum_{r=S}^{F} \delta_r \times evict_i + \epsilon_{it}$. 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.

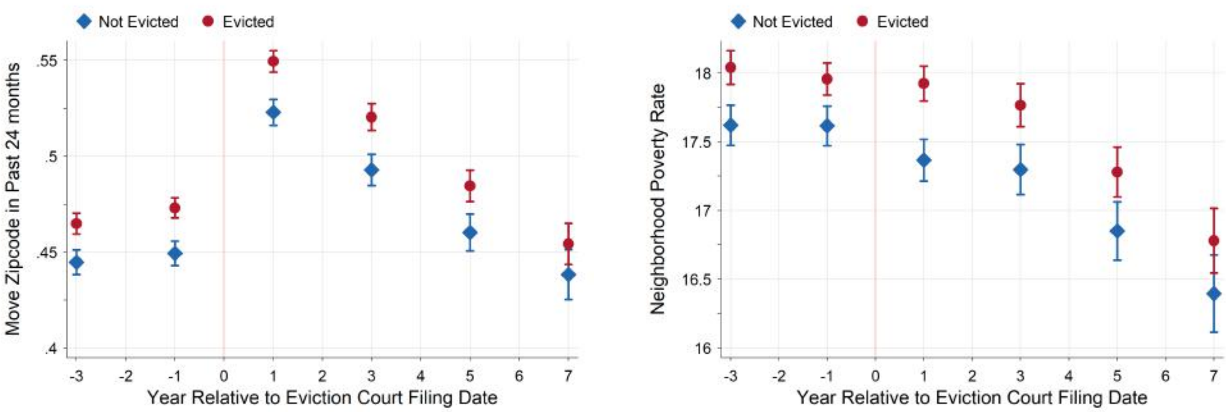
[36]Note that if eviction makes it harder for tenants to get a new lease, tenants may be less likely to be the signatory on a new lease, which would put downward pressure on the probability of a future eviction for the evicted group.

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: $y_{it} = \gamma_t + \delta \times evict_i + \sum_{r=S}^{F} \beta_r + \sum_{r=S}^{F} \delta_r \times evict_i + \epsilon_{it}$, 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 year is -3, and for this regression $S = -1$ and $F = 7$. 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, which are indicated in the figure with open markers.

Table 3: 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	545.246	-16.437***	-8.685***	-8.437***	-8.027	-12.590*
	(79.016)	(0.409)	(0.360)	(0.357)	(4.902)	(7.577)
Total bal. collections	3,088.756	650.454***	437.745***	433.005***	116.045	181.762
	(4,379.229)	(24.029)	(22.265)	(21.663)	(299.321)	(468.808)
Any Auto Loan or Lease	0.197	-0.061***	-0.041***	-0.040***	-0.048**	-0.075**
	(0.394)	(0.002)	(0.002)	(0.002)	(0.022)	(0.035)
Any mortgage	0.090	-0.040***	-0.021***	-0.020***	-0.001	-0.002
	(0.285)	(0.002)	(0.001)	(0.001)	(0.016)	(0.025)
Any Payday Inquiry $\times$ 100	14.525	-1.001***	-1.228***	-1.253***	2.900	4.618
	(35.236)	(0.202)	(0.201)	(0.199)	(2.368)	(3.763)
Any Payday Account $\times$ 100	2.334	-0.633***	-0.605***	-0.574***	1.860*	2.935*
	(15.098)	(0.072)	(0.071)	(0.067)	(0.955)	(1.524)
II. *Financial Strain: 37-60 mon.*						
Credit Score	553.735	-15.805***	-7.786***	-7.644***	-8.792*	-13.715*
	(80.749)	(0.441)	(0.394)	(0.386)	(5.035)	(7.880)
Total bal. collections	3,003.685	519.256***	373.357***	367.698***	-170.049	-265.237
	(4,425.770)	(26.928)	(25.286)	(25.398)	(354.087)	(550.377)
Any Auto Loan or Lease	0.198	-0.055***	-0.041***	-0.040***	-0.050*	-0.078*
	(0.394)	(0.002)	(0.002)	(0.002)	(0.029)	(0.046)
Any mortgage	0.089	-0.041***	-0.024***	-0.023***	0.017	0.026
	(0.283)	(0.002)	(0.002)	(0.001)	(0.016)	(0.025)
Any Payday Inquiry $\times$ 100	13.200	-0.408**	-0.746***	-0.785***	-8.116***	-13.079***
	(33.849)	(0.169)	(0.170)	(0.171)	(2.654)	(4.278)
Any Payday Account $\times$ 100	2.370	-0.466***	-0.437***	-0.432***	-0.530	-0.831
	(15.211)	(0.069)	(0.068)	(0.066)	(0.957)	(1.497)
Additional controls			Yes	Yes	Yes	Yes
Complier re-weighted				Yes		

The table 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 rulings in other cases, as described in the test. All specifications control for district-year fixed effects. Robust standard errors are clustered at the judge-year level.

Table 4: 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 at 3 Years*						
Cumulative Zipcode moves	0.98	0.080∗∗∗	0.060∗∗∗	0.059∗∗∗	−0.072	−0.113
	(0.77)	(0.007)	(0.008)	(0.008)	(0.104)	(0.163)
Poverty rate (×100)	17.09	0.886∗∗∗	0.501∗∗∗	0.499∗∗∗	−0.421	−0.657
	(10.17)	(0.085)	(0.090)	(0.086)	(1.233)	(1.921)
Any Eviction Case (36 mo.)	0.30	−0.096∗∗∗	−0.105∗∗∗	−0.105∗∗∗	−0.138∗∗∗	−0.216∗∗∗
	(0.46)	(0.005)	(0.004)	(0.005)	(0.043)	(0.069)
Eviction Case at Dif. Address (36 mo.)	0.18	−0.009∗∗	−0.018∗∗∗	−0.017∗∗∗	−0.004	−0.006
	(0.38)	(0.004)	(0.004)	(0.005)	(0.039)	(0.060)
II. *Outcomes at 5 Years*						
Cumulative Zipcode moves	1.41	0.112∗∗∗	0.082∗∗∗	0.082∗∗∗	−0.080	−0.125
	(1.00)	(0.011)	(0.011)	(0.010)	(0.123)	(0.191)
Poverty rate (×100)	16.91	0.845∗∗∗	0.453∗∗∗	0.445∗∗∗	−1.401	−2.192
	(10.19)	(0.106)	(0.109)	(0.105)	(1.259)	(1.987)
Any Eviction Case (60 mo.)	0.37	−0.091∗∗∗	−0.103∗∗∗	−0.103∗∗∗	−0.141∗∗	−0.263∗∗
	(0.48)	(0.004)	(0.004)	(0.004)	(0.069)	(0.129)
Eviction Case at Dif. Address (60 mo.)	0.27	−0.022∗∗∗	−0.036∗∗∗	−0.036∗∗∗	−0.065	−0.137
	(0.44)	(0.005)	(0.005)	(0.005)	(0.071)	(0.149)
Additional controls			Yes	Yes	Yes	Yes
Complier re-weighted				Yes		

The table reports OLS and two-stage least squares results of the impact of eviction on measures of moves and neighborhood quality. Column 4 presents an OLS regression on residual stringency. The analysis sample has $N = 70,816$. 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 test. All specifications control for district-year fixed effects. Robust standard errors are clustered at the judge-year level.

6.1 Discussion

One of the key findings of our analysis is that losing an eviction case has a relatively modest effect on tenant outcomes in marginal court cases. Both winners and losers in eviction court experience significant strain in the years preceding the eviction filing, including high move rates, large amounts of collections debt, and a spike in the demand for credit. Both groups also experience significant strain in the aftermath of eviction court, including a spike in move rates and a surge in unpaid bills.

Returning to Desmond's hypothesis, our results are nuanced. On the one hand, the difference between winners and losers in eviction court are not that large, with the exception of auto loans. We find small causal effects on credit score and unpaid bills, suggesting that indebtedness or a "scarring" effect on access to credit is not a key channel through which an eviction eviction order deepens poverty. Most of the decline in credit score happens prior to the filing, and the rise in indebtedness accelerates after filing for both winners and losers of their eviction cases. Even the potential mechanical effect of eviction on collections debt appears to be minor.

We do find persistent negative effects on the probability of having an auto loan. These effects are large in magnitude, and are consistent with tenants having lost income such as labor earnings or rental assistance as a result of eviction. For low-income families, having access to a car may be the difference between being able to get to and from a job or dropping children off at daycare or school.

We also find small causal effects of an eviction order on move rates, on neighborhood poverty rates, and on the probability of a future eviction case are not statistically significant. We emphasize that these results do *not* imply that housing is not important, nor do they imply that the involuntary loss of one's home is not a major disruption with economic consequences. Our research instead speaks to the more narrow but well-defined policy question: what if we made the eviction court process more lenient toward tenants; what would their financial strain look like? Our answer is that, measured by their financial strain, tenants receiving a marginal dismissal under such a policy change, absent equilibrium effects, would not look dramatically different.

The broader financial strain experienced by both winners and losers in eviction court do suggest that policies targeting tenants when they appear in eviction court – a natural target of policymakers because millions of cases pass through the court, which is often the first point of contact between distressed tenants and the legal system – might be too late. Moreover, our finding that even tenants who win eviction cases experience rising indebtedness and high move rates in the aftermath suggests that policies such as legal aid, which have been shown to effectively raise tenant win rates in eviction court, may not be enough to stabilize tenant housing or financial circumstances.

We also emphasize that there are important dimensions that we do not speak to in this paper, including employment, mental health, and homelessness. We cannot rule out that the causal effect of an eviction order may be larger for these outcomes, and we leave this examination for future research.

7 Conclusion

This paper presents an analysis of the causal effect of eviction on the financial health of households. Using a novel, large-scale data set compiled from several public and proprietary sources, this paper makes two main contributions: First, we use new data to document the scope and incidence of eviction in Cook County and the city of Chicago, and use our long time series of linked administrative data to provide a descriptive analysis of the evolution of financial health in the run-up and aftermath of eviction court, for up to 4 years prior and 7 years following the case. Second, we use randomized assignment of judges to estimate the causal effect (LATE) of eviction on financial strain.

Our main goal is to assess the evidence for the hypothesis that eviction is not just a condition of poverty, but is a cause of poverty. We find mixed evidence for this hypothesis. We do find causal evidence that eviction increases financial strain – lowering credit scores and increasing unpaid bills, although the magnitudes are relatively modest. We also find that, perhaps surprisingly, losing an eviction case does not appear to increase the frequency of moves across neighborhoods – move rates are high for both groups.

We also find that eviction appears to reduce consumption, proxied by the likelihood of having an automobile. Our results are not able to speak directly to welfare, as the marginal utility of consumption may differ for evicted and non-evicted tenants who move to different neighborhoods, as in the health setting (Dobkin et al., 2018).

While we find moderately sized causal effects for eviction, the prolonged increase in financial distress for both those who are and are not evicted suggests that policies that target the eviction court setting may not be enough.

Table 5: Heterogeneity in the effect of eviction (12-36 months)

Panel I.	Joint Action		Single Action	
	(OLS)	(IV)	(OLS)	(IV)
Credit Score	-8.979***	-7.178	-7.267***	-28.968***
	(0.421)	(9.063)	(0.711)	(9.481)
	[543.16]		[553.84]	
Total bal. collections	462.342***	26.766	333.543***	605.286
	(25.930)	(536.632)	(40.267)	(676.252)
	[3,162.02]		[2,839.17]	
Any auto loan or lease	-0.044***	-0.105**	-0.026***	0.003
	(0.002)	(0.046)	(0.004)	(0.053)
	[0.21]		[0.16]	
Payday Inquiries	-0.134**	3.411	-0.134	-2.541
	(0.063)	(2.831)	(0.113)	(2.726)
	[1.53]		[1.00]	
Payday Accounts Opened × 100	-1.083***	10.677*	-0.260	-6.089
	(0.299)	(6.468)	(0.339)	(7.292)
	[4.36]		[2.47]	
Panel II.	Multi-headed household		Single-headed household	
	(OLS)	(IV)	(OLS)	(IV)
Credit Score	-8.687***	-16.959	-8.580***	-10.596
	(0.714)	(13.798)	(0.380)	(7.715)
	[559.03]		[541.00]	
Total bal. collections	356.651***	-1.3e+03	457.006***	622.830
	(45.383)	(955.361)	(22.928)	(560.896)
	[2,815.75]		[3,185.03]	
Any auto loan or lease	-0.028***	-0.046	-0.044***	-0.087**
	(0.004)	(0.082)	(0.002)	(0.038)
	[0.21]		[0.20]	
Payday Inquiries	-0.061	3.196	-0.160**	1.518
	(0.124)	(7.796)	(0.067)	(1.849)
	[1.28]		[1.48]	
Payday Accounts Opened × 100	-0.998**	-13.302	-0.884***	12.775**
	(0.469)	(12.710)	(0.303)	(5.804)
	[3.93]		[4.01]	
Panel III.	No prior cases		Prior case	
	(OLS)	(IV)	(OLS)	(IV)
Credit Score	-9.614***	-20.361*	-6.632***	-3.070
	(0.467)	(10.741)	(0.464)	(7.886)
	[554.45]		[528.69]	
Total bal. collections	445.162***	711.553	411.148***	-436.192
	(26.253)	(586.222)	(36.134)	(680.196)
	[2,892.36]		[3,469.72]	
Any auto loan or lease	-0.044***	-0.019	-0.035***	-0.135***
	(0.002)	(0.054)	(0.003)	(0.046)
	[0.21]		[0.18]	
Payday Inquiries	-0.117	3.115	0.009	0.628
	(0.079)	(3.888)	(0.066)	(1.588)
	[2.01]		[0.39]	
Payday Accounts Opened × 100	-1.059***	11.244	-0.237	1.032
	(0.377)	(9.154)	(0.191)	(3.566)
	[5.72]		[0.87]	
Panel IV.	Above median poverty neighborhood		Below median poverty neighborhood	
	(OLS)	(IV)	(OLS)	(IV)
Credit Score	-6.330***	-15.744**	-10.523***	-9.965
	(0.399)	(7.326)	(0.519)	(13.790)
	[528.26]		[561.31]	
Total bal. collections	366.466***	664.763	536.220***	-508.001
	(23.605)	(488.849)	(36.023)	(855.720)
	[3,162.20]		[3,034.47]	
Any auto loan or lease	-0.039***	-0.087**	-0.039***	-0.069
	(0.002)	(0.042)	(0.003)	(0.074)
	[0.17]		[0.23]	
Payday Inquiries	-0.226***	5.161*	-0.008	-3.061
	(0.069)	(3.130)	(0.096)	(3.340)
	[1.51]		[1.35]	
Payday Accounts Opened × 100	-0.534*	5.965	-1.218***	5.353
	(0.282)	(5.661)	(0.386)	(11.584)
	[3.42]		[4.51]	

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 v. single action cases, (II.) multi-headed households v. 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 6: Heterogeneity in the effect of eviction (37-72 months)

Panel I.	Joint Action		Single Action	
	(OLS)	(IV)	(OLS)	(IV)
Cumulative Zipcode moves	0.057***	-0.281	0.064***	0.260
	(0.009)	(0.198)	(0.013)	(0.198)
	[0.98]		[0.94]	
Poverty rate (×100)	0.629***	0.712	-0.101	-3.479
	(0.104)	(2.723)	(0.222)	(2.746)
	[16.67]		[18.72]	
Any Eviction Case (36 mo.)	-0.111***	-0.265***	-0.080***	-0.109
	(0.004)	(0.097)	(0.010)	(0.090)
	[0.31]		[0.23]	
Eviction Case at Dif. Address (36 mo.)	-0.018***	-0.030	-0.015*	0.044
	(0.005)	(0.084)	(0.009)	(0.091)
	[0.18]		[0.15]	
Panel II.	Multi-headed household		Single-headed household	
	(OLS)	(IV)	(OLS)	(IV)
Cumulative Zipcode moves	0.054***	-0.372	0.061***	-0.017
	(0.014)	(0.248)	(0.008)	(0.181)
	[0.95]		[0.98]	
Poverty rate (×100)	0.456**	0.254	0.508***	-1.162
	(0.225)	(3.292)	(0.084)	(1.917)
	[14.91]		[17.72]	
Any Eviction Case (36 mo.)	-0.092***	-0.072	-0.109***	-0.273***
	(0.008)	(0.162)	(0.005)	(0.072)
	[0.25]		[0.31]	
Eviction Case at Dif. Address (36 mo.)	-0.008	0.076	-0.020***	-0.041
	(0.007)	(0.148)	(0.005)	(0.061)
	[0.14]		[0.19]	
Panel III.	No prior cases		Prior case	
	(OLS)	(IV)	(OLS)	(IV)
Cumulative Zipcode moves	0.058***	-0.071	0.062***	-0.146
	(0.008)	(0.222)	(0.011)	(0.201)
	[0.96]		[0.99]	
Poverty rate (×100)	0.520***	-0.670	0.410***	-0.662
	(0.111)	(2.535)	(0.100)	(2.366)
	[16.18]		[18.69]	
Any Eviction Case (36 mo.)	-0.094***	-0.211**	-0.133***	-0.199**
	(0.005)	(0.097)	(0.007)	(0.093)
	[0.24]		[0.41]	
Eviction Case at Dif. Address (36 mo.)	-0.022***	-0.041	-0.018***	0.048
	(0.005)	(0.092)	(0.006)	(0.078)
	[0.14]		[0.25]	
Panel IV.	Above median poverty neighborhood		Below median poverty neighborhood	
	(OLS)	(IV)	(OLS)	(IV)
Cumulative Zipcode moves	0.054***	-0.311	0.071***	0.137
	(0.012)	(0.209)	(0.008)	(0.246)
	[0.99]		[0.96]	
Poverty rate (×100)	0.073	-0.115	0.338***	-2.042
	(0.086)	(2.324)	(0.119)	(2.499)
	[22.80]		[11.68]	
Any Eviction Case (36 mo.)	-0.110***	-0.319***	-0.100***	-0.106
	(0.006)	(0.098)	(0.005)	(0.113)
	[0.33]		[0.27]	
Eviction Case at Dif. Address (36 mo.)	-0.025***	-0.034	-0.011**	0.028
	(0.005)	(0.084)	(0.005)	(0.093)
	[0.21]		[0.15]	

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 v. single action cases, (II.) multi-headed households v. 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.

References

Aizer, A. and J. J. Doyle (2015, feb). Juvenile Incarceration, Human Capital, and Future Crime: Evidence from Randomly Assigned Judges*. *The Quarterly Journal of Economics*, qjv003.

Avery, R. B., P. S. Calem, and G. B. Canner (2003). An overview of consumer data and credit reporting. *Federal Reserve Bulletin* (Feb), 47–73.

Berube, D. A. and D. P. Green (2007, jul). The Effects of Sentencing on Recidivism: Results from a Natural Experiment. {SSRN} {Scholarly} {Paper} ID 999445, Social Science Research Network, Rochester, NY.

Bhuller, M., G. Dahl, K. Løken, and M. Mogstad (2016, sep). Incarceration, Recidivism and Employment. *NBER Working Paper No. 22648*.

Brevoort, K. P., P. Grimm, and M. Kambara (2015, May). Data Point: Credit Invisibles. Technical report, Consumer Finance Protection Bureau Office of Research.

Burgard, S. A., K. S. Seefeldt, and S. Zelner (2012, dec). Housing instability and health: findings from the Michigan Recession and Recovery Study. *Social Science & Medicine (1982) 75*(12), 2215–2224.

Campbell, J. Y., S. Giglio, and P. Pathak (2011, aug). Forced Sales and House Prices. *American Economic Review 101*(5), 2108–2131.

Chetty, R., N. Hendren, and L. F. Katz (2016). The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment. *American Economic Review 106*(4), 855–902.

Chyn, E. (2018). Moved to Opportunity: The Long-Run Effects of Public Housing Demolition on Children. *American Economic Review*.

Collinson, R. and D. Reed (2018). The Effects of Eviction on Low-Income Households. *Working Paper*.

Crane, M. and A. M. Warnes (2000). Evictions and Prolonged Homelessness. *Housing Studies 15*, 757–773.

Dahl, G. B., A. R. Kostøl, and M. Mogstad (2014, nov). Family Welfare Cultures. *The Quarterly Journal of Economics 129*(4), 1711–1752.

Desmond, M. (2012). Eviction and the Reproduction of Urban Poverty. *American Journal of Sociology 118*(1), 88–133.

Desmond, M. (2016). *Evicted: Poverty and Profit in the American City.* Crown Books.

Desmond, M., W. An, R. Winkler, and T. Ferriss (2013). Evicting Children. *Social Forces 92*(1), 303–327.

Desmond, M. and M. Bell (2015, nov). Housing, Poverty, and the Law. *Annual Review of Law and Social Science 11*(1), 15–35.

Desmond, M. and C. Gershenson (2016). Housing and Employment Insecurity among the Working Poor. *Social Problems*.

Desmond, M., A. Gromis, L. Edmonds, J. Hendrickson, K. Krywokulski, L. Lillian, and A. Porton (2018a). Eviction Lab National Database: Version 1.0. Online database, Princeton University.

Desmond, M., A. Gromis, L. Edmonds, J. Hendrickson, K. Krywokulski, L. Lillian, and A. Porton (2018b). Methodology Report: Version 1.0. Methodology report, Princeton University.

Desmond, M. and R. T. Kimbro (2015, sep). Eviction's Fallout: Housing, Hardship, and Health. *Social Forces 94*(1), 295–324.

Desmond, M. and T. Shollenberger (2015). Forced Displacement From Rental Housing: Prevalence and Neighborhood Consequences. *Demography 52*, 1751–1772.

Diamond, R., T. McQuade, and F. Qian (2017). The Effects of Rent Control Expansion on Tenants, Landlords, and Inequality: Evidence from San Francisco. *Working Paper*.

Dobbie, W., J. Goldin, and C. S. Yang (2018, feb). The Effects of Pre-Trial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges. *American Economic Review 108*(2), 201–240.

Dobbie, W., P. Goldsmith-Pinkham, and C. S. Yang (2017, dec). Consumer Bankruptcy and Financial Health. *The Review of Economics and Statistics 99*(5), 853–869.

Dobbie, W. and J. Song (2015). Debt relief and debtor outcomes: Measuring the effects of consumer bankruptcy protection. *The American Economic Review 105*(3), 1272–1311.

Dobkin, C., A. Finkelstein, R. Kluender, and M. J. Notowidigdo (2018, feb). The Economic Consequences of Hospital Admissions. *American Economic Review 108*(2), 308–352.

Doran, K., J. Guzzardo, K. Hill, N. Kitterlin, W. Li, and R. Liebl (2003). No Time for Justice: A Study of Chicago's Eviction Court. Technical report.

Evans, W. N., J. X. Sullivan, and M. Wallskog (2016, aug). The impact of homelessness prevention programs on homelessness. *Science 353*(6300), 694–699.

Fowler, K. A., R. M. Gladden, K. J. Vagi, J. Barnes, and L. Frazier (2015, feb). Increase in Suicides Associated with Home Eviction and Foreclosure During the US Housing Crisis: Findings from 16 National Violent Death Reporting System States, 2005-2010. *American Journal of Public Health 105*(2), 311–316.

Frier, B. W. (1980). Appendix A: An Egyptian "Eviction Notice". In *Landlords and Tenants in Imperial Rome*. Princeton University Press.

Gennetian, L. A., M. Sciandra, L. Sanbonmatsu, J. Ludwig, L. F. Katz, G. J. Duncan, J. R. Kling, and R. C. Kessler (2012). The Long-Term Effects of Moving to Opportunity on Youth Outcomes. *Cityscape 14*(2), 137–168.

Green, D. P. and D. Winik (2010, may). Using Random Judge Assignments to Estimate the Effects of Incarceration and Probation on Recidivism Among Drug Offenders. *Criminology 48*(2), 357–387.

IHS (2018). Institute for Housing Studies: Foreclosure Filings Data.

Imai, K. and K. Khanna (2016). Improving Ecological Inference by Predicting Individual Ethnicity from Voter Registration Records. *Political Analysis*.

Imbens, G. W. and J. D. Angrist (1994). Identification and Estimation of Local Average Treatment Effects. *Econometrica 62*(2), pp. 467–475.

Khanna, K., K. Imai, and J. Hubert (2017, September). Who are You? Bayesian Prediction of Racial Category Using Surname and Geolocation. Technical report, The Comprehensive R Archive Network.

Kling, J. R. (2006, jun). Incarceration Length, Employment, and Earnings. *The American Economic Review 96*(3), 863–876.

Kling Jeffrey, J. Liebman, and L. Katz (2007). Experimental Analysis of Neighborhood Effects. *Econometrica 75*(1), 83–119.

Krent, H. J., P. Cheun, K. Higgens, M. McElwee, and A. McNicholas (2016). Eviction Court and a Judicial Duty of Inquiry. *Journal of Affordable Housing 24*(3), 547–564.

Lee, D. and W. van der Klaauw (2010, nov). An Introduction to the FRBNY Consumer Credit Panel. *SSRN Electronic Journal*.

Maestas, N., K. J. Mullen, and A. Strand (2013). Does Disability Insurance Receipt Discourage Work? Using Examiner Assignment to Estimate Causal Effects of SSDI Receipt. *American Economic Review 103*(5), 1797–1829.

Marr, T. (2016, dec). Millions of Renters Face Eviction: Why Today's Housing Market is Partially to Blame.

Mazumder, B. and S. Miller (2016). The Effects of the Massachusetts Health Reform on Household Financial Distress. *American Economic Journal: Economic Policy 8*(3), 284–313.

Molloy, R. and H. Shan (2010). The Post-Foreclosure Experience of U.S. Households in the Current Housing Market Downturn. *Working Paper*.

Mueller-Smith, M. (2016). The Criminal and Labor Market Impacts of Incarceration. *American Economic Review, forthcoming*.

PERC (2006, June). Giving Credit Where Credit is Due. Technical report, Political and Economic Research Council.

Phinney, R., S. Danziger, H. A. Pollack, and K. Seefeldt (2007). Housing Instability Among Current and Former Welfare Recipients. *American Journal of Public Health 97*(5), 832–837.

Sandel, M., R. Sheward, S. de Cuba, S. M. Coleman, D. A. Frank, M. Chilton, M. Black, T. Heeren, J. Pasquariello, P. Casey, E. Ochoa, and D. Cutts (2018). Unstable Housing and Caregiver and Child Health in Renter Families. *Pediatrics*.

Seedco Policy Center (2010, June). Housing Help Program: Homelessness Prevention Pilot Final Report. Technical report.

Turner, M., A. Lee, R. Varghese, and P. Walker (2008, July). You Score, You Win: The consequences of Giving Credit Where Credit is Due. Technical report, Political and Economic Research Council.

Turner, M., P. Walker, S. Chaudhuri, and R. Varghese (2012, June). A New Pathway to Financial Inclusion: Alternative Data, Credit Building, and Responsible Lending in the Wake of the Great Recession. Technical report, Political and Economic Research Council.

A Appendix: Institutional details

A.1 Introduction

This appendix contains detailed descriptions of the institutional details surrounding residential eviction procedures in Cook County, IL. The following sources form the basis for this appendix:

- the relevant legislative codes;[37]
- observation of court cases by the authors in the years 2016-2018;
- written reports on observation studies of the Cook County eviction court and newspaper articles for earlier years;
- discussions with legal experts, including attorneys for plaintiffs and defendants;
- discussions with judges and staff of the Cook County Circuit Court;
- several 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 via a FOIA request.

The observations in this document do not represent the opinions of any 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.

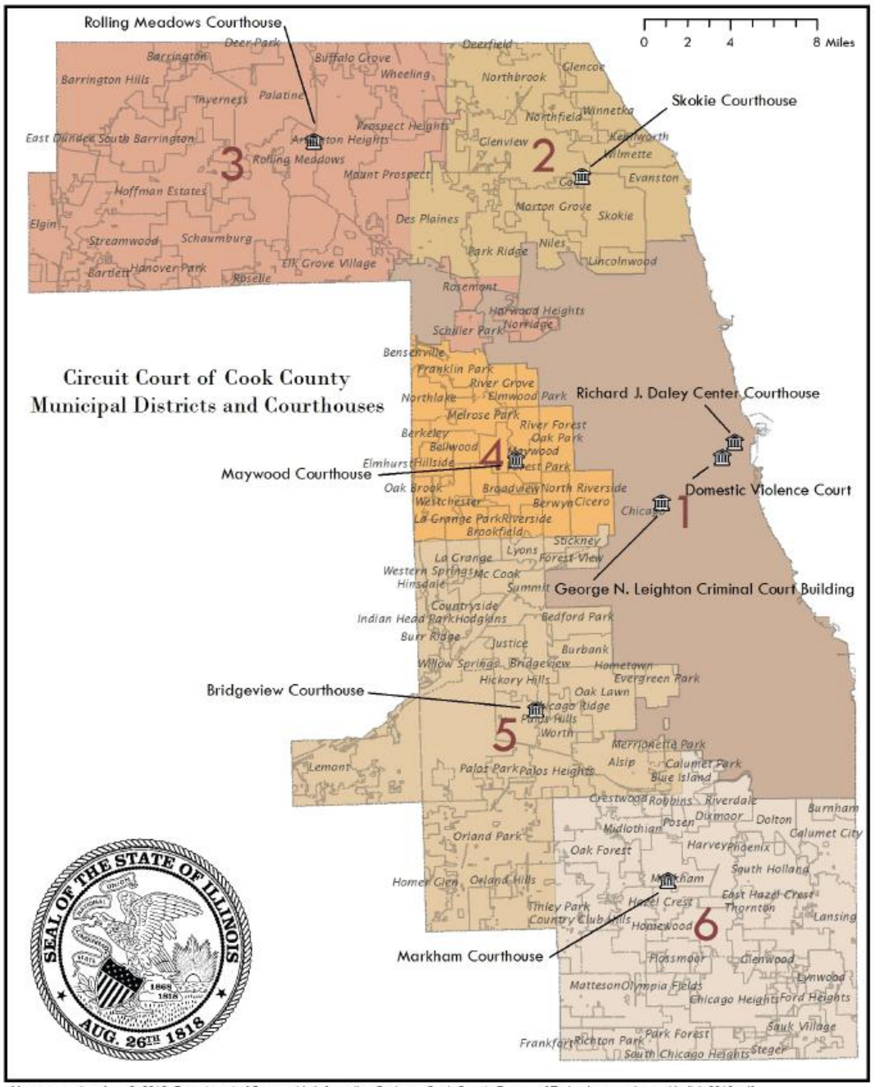
A.2 Legal context: tenant-landlord law in Cook County

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 court 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 9 presents a map of the court districts. Our data set spans all six districts, however not all districts are included in the analysis, as explained in the main body of the paper. 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'.

[37] Specifically, the Illinois Compiled Statutes (ILCS) and the Municipal Code of Chicago Residential Landlords and Tenants Ordinance (RLTO).

Figure 9: 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.
©2012 Cook County Government
You are not permitted to repackage, resell, or distribute this map without the written permission of the Cook County Board of Commissioners

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.

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 Statuses. 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, most relevant are the Forcible Entry and Detainer Act (735 ILCS 5/9) and the Civil Practice Act (735 ILCS 5/2).[38]

The legal framework for the rental housing market in Cook County

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

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, this is interpreted by judges to imply that the only defense a tenant could mount against an eviction case related to nonpayment of rent was proof of payment 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 building or health code violations.

1972 IL Supreme Court rules, in *Jack Spring, Inc v. Little*, that there is an implied warranty of habitability for leased residential premises. As such, failing to keep the unit up to standard is a breach of the lease, and conditions of premise, failing utilities, failure to repair faulty conditions and other complaints are valid arguments in eviction court. This increases the scope of possible defenses for tenants. Before, they could only dispute the claim of non-payment of rent, argue retaliatory motive, 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) release a report based on monitoring of eviction court cases, highlighting the limited amount of time available to make judgments. The report prompts 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 landlord-tenant ordinance (RLTO), which further expanded on tenants' defenses by providing tenants additional rights (for example, if landlord accepts even partial rent after posting 5 day notice, he forgoes 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.[39]

[38] 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.

[39] The presence or absence of rent control regulation is important for predicting the market-level effects of changes to

Comparison of Cook County evictions court proceedings to other counties

There are some differences, but the general gist is consistent throughout most courts.

Differences from other courts. The main departures from the national norm are found in Chicago's RLTO, which is considered to lean somewhat tenant-friendly. Specifically, Chicago's rule that if the landlord even accepts partial payment of past rent, even after the 5 day notice expires, then he loses his right to evict, is unusual. Also, the fact that landlords cannot include a clause in the lease that requires the tenant to cover legal fees in the case of a successful eviction is unusual.

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, almost all states require some type of summons and complaint serving to the tenant after the landlord goes to the clerk to obtain a court date, though the method of delivery may differ but tends to involve, as in IL, that a third party serves the summons (in CA, landlord himself serves the summons). Although there are differences in steps leading to the trial, most processes are relatively similar and involve a method for the tenant to request a jury trial (in TX, must request jury trial within 5 days of receiving the summons and complaint, in IL, you may request it before the judge, etc), and a requirement that the tenant shows up on the court date lest the judge enters a default in favor of the landlord. The trial process is generally similar in most states, but the final ruling in favor of the landlord differs. In some states, including PA and MD, the judge's ruling in favor does not automatically include an Order for Possession similar. Rather, the landlord must request an Order for Possession if the tenant has not vacated the property within a certain amount of days following the end of the trial (for example, 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 mostly similar in most states and involves the landlord paying a certain fee to

eviction laws, as landlords are expected to respond differently depending on the degree to which they are able to adjust rent prices. 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.

the sheriff (or Constable in many cities such as New York City and LA) who then performs the eviction after a certain time frame elapses.

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 that outlines the reason for termination as well as provides the number of days the tenant has before the termination goes into effect. Upon providing notice to terminate the lease, if the issue is not addressed and the required number of days expired, the landlord can file an eviction case against the tenant to reclaim his property if the landlord has not yet vacated. The legally permitted reasons to terminate and the required associated number of days notice are as follows:

1. Illinois/Cook County

non-payment of rent → 5-day notice, with right to file for eviction void only if accept **all** 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, which de-facto are a continuation of the leases agreed upon with the previous landlord prior to the change of ownership. The rules regarding when the purchaser may terminate the bona fide lease depend on the lease:
- weekly or monthly lease → 90-day notice (ILCS 5/9-207.5)
- any other lease → 90-day notice and may not terminate before the end of the term 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, Chicago laws can only be stricter.
- for 5-day notice, the right to file for eviction is voided if tenant accepts **any part** of the rent

- for 10-day notice, the right to file for eviction is voided if tenant fixes issue within the 10 days
- for foreclosures, the owner of the foreclosed property must pay a relocation assistance fee of $10,600 or the option the renew or extend the current lease with some restrictions on the price of that contract[40]

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 which defenses are germane has been relatively stable. The most common germane defenses in eviction court are, according to the 2001 Eviction Court Bench Book (Residential Tenancies):

- 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 landlord in response to certain actions by the tenant, such as notifying the authorities of a building code violation on the landlord's property
  - The plaintiff is discriminating against the defendant on an unlawful basis (in Cook County, discrimination based on income is also prohibited, which in particular protects tenants from being evicted for being a voucher recipient
  - 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 was required to 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 (i.e. landlord gave a 5-day notice for issues that fall under a 10-day notice)
  - The plaintiff filed the eviction action before the statutorily required notice period ended

[40] 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."*

- 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 landlord refused to accept it
  - The plaintiff's failure to maintain the premises in substantial compliance with applicable municipal building codes reduced its value by an amount that 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 that the landlord covers costs related to various utilities if 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 defense 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 has violated the RLTO's prohibitions against retaliation, which expands on the Retaliatory Eviction Act by broadening it by prohibiting, for example, that the eviction was the result of asking the landlord to make necessary repairs or because the tenant 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 teannt of his right to cure the lease violation

A.3 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 or his attorney (the plaintiff) must provide the clerk of the Circuit Court of Cook County with a complaint form and a summons form, and pay the filing fee.

The complaint form includes the address of the tenant, the reason the Plaintiff is claiming action, and, in the case of joint action court case, the amount of rent and/or damages claimed. The summons form is the form that the sheriff serves to the tenant that alerts him of the eviction court case, and also provides information about when and where the court case will take place. 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 has changed from 2000-2016, and also depends on whether the plaintiff is filing for possession of property or possession of property and rent totalling up to $15,000, or if the plaintiff is filing for possession of property and rent totalling over $15,000. The former cost $106 in 2000 and $268 in 2016, while the latter cost $255 in 2000 and $463 in 2016.

Randomized case assignment

Once the plaintiff provides the required eviction filing forms and pays the filing fee with the clerk, he he is given a range of dates to choose from (usually these dates are between 2-4 weeks from the filing date). All possible dates will be some weekday at 9:30am. Once the plaintiff chooses the date and the clerk enters it (and payment is made), the plaintiff gets back, along with the paperwork, the date he choose as well as courtroom number where he has to be present at 9:30am on the date he choose (the process is analogous if plaintiff does an e-filing). Because judges are assigned to courtrooms in Cook County, the random assignment of courtroom is synonymous with the random assignment of judge. The plaintiff can easily determine the judge who is sitting on the courtroom number he is given by either looking it up or by asking the clerk (either in person or by phone call). As such, if the plaintiff wishes to determine the judge who he was randomly assigned to, he can obtain that information relatively easily. However, the plaintiff may not simply ask for another date, or claim he "gave the wrong date" in an effort to somehow have a chance to change courtrooms and thus judges. As such, though the judge name can be found after filing a case, the plaintiff cannot change the judge he is assigned by attempting to re-file.

Court proceedings

Though there is great heterogeneity in the court process, the general process can be surmised as follows:

Unless under rare circumstances, the landlord (usually the landlord is represented by the landlord's attorney but may be pro se) will be present on the return date provided at the time of filing. Depending on whether the tenant was successfully served, he 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 day. At this point, the landlord will have to re-attempt to serve the tenant, and may attempt to do so through a special process server, and the landlord is given a new date to come back. The judge will usually give the tenant a few attempts to be served before deciding that the landlord made a good-faith attempt at serving the tenant and granting a default Order for Possession to the landlord.

If and when the tenant does show up, he can request a continuance to find an attorney. [41] Usually, the tenant is given one week. The tenant can also, at any point prior to the bench trial, request a trial by jury, and the case is moved to a jury courtroom, which takes additional time. Alternatively, before moving to a bench trial, the landlord and tenant may agree to a settlement order [42] which allows the landlord and tenant to agree to 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. If the tenant fails to adhere to the settlement, the landlord can simply return to court after the conditions are violated and receive an immediate Order for Possession. Finally, the case may also be dismissed by the landlord for a variety of reasons. Reasons include the landlord realizing he made a mistake in how he filed, the tenant left the premises so the landlord no longer needed to obtain an Order for Possession, the landlord and tenant came to an out of court understanding,etc. These typically result in the case being dismissed without prejudice, which allow 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, who then makes a ruling to either grant an Order for Possession (and/or back rent if joint action) or to dismiss the case in favor of the tenant (this is usually a dismissal with prejudice, which does not allow the landlord to re-file for the same reasons).

What gets recorded in the court records?

The case docket records the filing date, ad damnum amount, 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. The docket then records key case events including attempts (and successes) at serving the defendant as well as motions and proceedings. It the judge makes a money judgment, the judgment amount is also included in the docket.

[41] Tenants are encouraged to do this by pro bono legal aid helpers at the court because the judge is very likely to grant this. Some judges even automatically provide this option to the tenant.

[42] See settlement form.

After a judge grants an order for eviction

After a judge grants an order for eviction[43], they can also grant a "stay" of eviction order alongside it, 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.[44]

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. At this point, enforcement of the Order by the sheriff may occur as soon as 24 hours after an order has been placed with the sheriff's evictions office, though realistically this takes at least a few weeks (based on our data, we find it takes about 2 months).

After filing, the tenant will receive a letter from the Cook County sheriff's office at the mailing address listed on the Order, which provides the tenant with a notification that an Order has been placed with the sheriff and that he may be evicted within 24 hours. Importantly, this is the only notification that the tenant gets regarding the eviction. However, every day, the eviction notice schedule is posted and reflects both that day's and the following day's evictions, so the tenant can check that to get a 48 hour notice regarding when the eviction will be carried out. 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 get a date on which the eviction will be carried out and can check in the same way as the tenant does. However, the sheriff's office will call the landlord or his attorney about 24 hours before the notice is scheduled to indicate a four-hour window during which one of the deputies 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 that the temperature is above 15F, when the deputy shows up to the residence to perform the eviction, the landlord or his representative is required to greet the deputy at the property with a locksmith alongside. After signing papers authorizing the sheriff's deputy to use force if necessary, the deputy will enter the property and remove any occupants listed in the Order[45] and wait while the landlord changes the locks to the door. This completes the eviction process. Importantly, the deputy is not in charge of removing personal belongings, which must be separately worked out between the landlord and the tenant.

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

[44] 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.

[45] Importantly, if an occupant is on the premises that is not listed in the Order, including occupants who may not even be on the lease, which are usually covered by listing "any and all unknown occupants" in the eviction complaint, the deputy will stop the eviction process and the landlord may have to file a new complaint seeking to evict the previously unnamed occupants.

Money judgments

If the landlord filed a joint action case, in addition to ruling on whether to grant the landlord with an Order for Possession, the judge will also make a money judgment ordering how much, if any, the tenant must pay to the landlord for back rent and damages sought in the case. 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 very plausible in joint action cases for the judge to grant the landlord an Order for Possession but no money order. Indeed, ILCS generally forbids the judge from making a money judgment without the tenant showing up to court, though the judge can and often will grant an Order for Possession if the tenant never shows up to court provided that the landlord made a good-faith effort to serve the tenant. Additionally, landlords will often either reach a settlement order with the tenant in which the tenant agrees to leave the property by a certain date provided that the landlord drops the money judgment, or will mention during court that they are willing to drop the money judgment and that all they are truly seeking is an eviction order.[46] On the other hand, it is rare for the landlord to obtain a money order but no Order for Possession.

Once and only when a landlord obtains a money judgment against a tenant (the debtor), his options to collect are:[47]

Citation to discover assets: debtor testifies as to amount and location of assets, and Court can order any such assets to be liquidated or turned over to Landlord to satisfy judgment (ignoring will result in debtor being arrested for contempt of court. Note that the debtor can claim some exemptions, such as $4000 in value of personal property or $2400 of a motor vehicle).

Wage garnishment: wage garnishment can be filed and served on the employer of the debtor, who has to deduct payments from each paycheck (some restrictions apply, ie can only deduct up to 15% of debtor's gross income, and withholding can't leave debtor with less than 45 time the state minimum wage as weekly take-home pay).

Non-wage garnishment: if debtor has a bank account, for example, assets can be garnished for the full amount.

Levy and execution: if the debtor owns assets free of any secured debts, an order can be obtained to have the sheriff seize the assets and liquidate it to satisfy the debt.

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

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

Figure 10: 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

A.4 Comparison to eviction rates in other counties

Estimates of the annual number of eviction cases nationwide

Although data on the number of annual eviction cases in the U.S. are not collected or reported by statistical agencies, available estimates for 2016 are between 2.1 and 2.7 million cases annually, as can be seen in Table 7. Estimates for the annual number of evictions performed are even more difficult to obtain. The numbers based on Desmond et al. (2018a) are lower bounds: they were compiled by the current authors 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 data are not available after 2006, suggesting these estimates provide a lower bound.

Table 7: 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	

Comparing Cook County eviction rates to other large counties

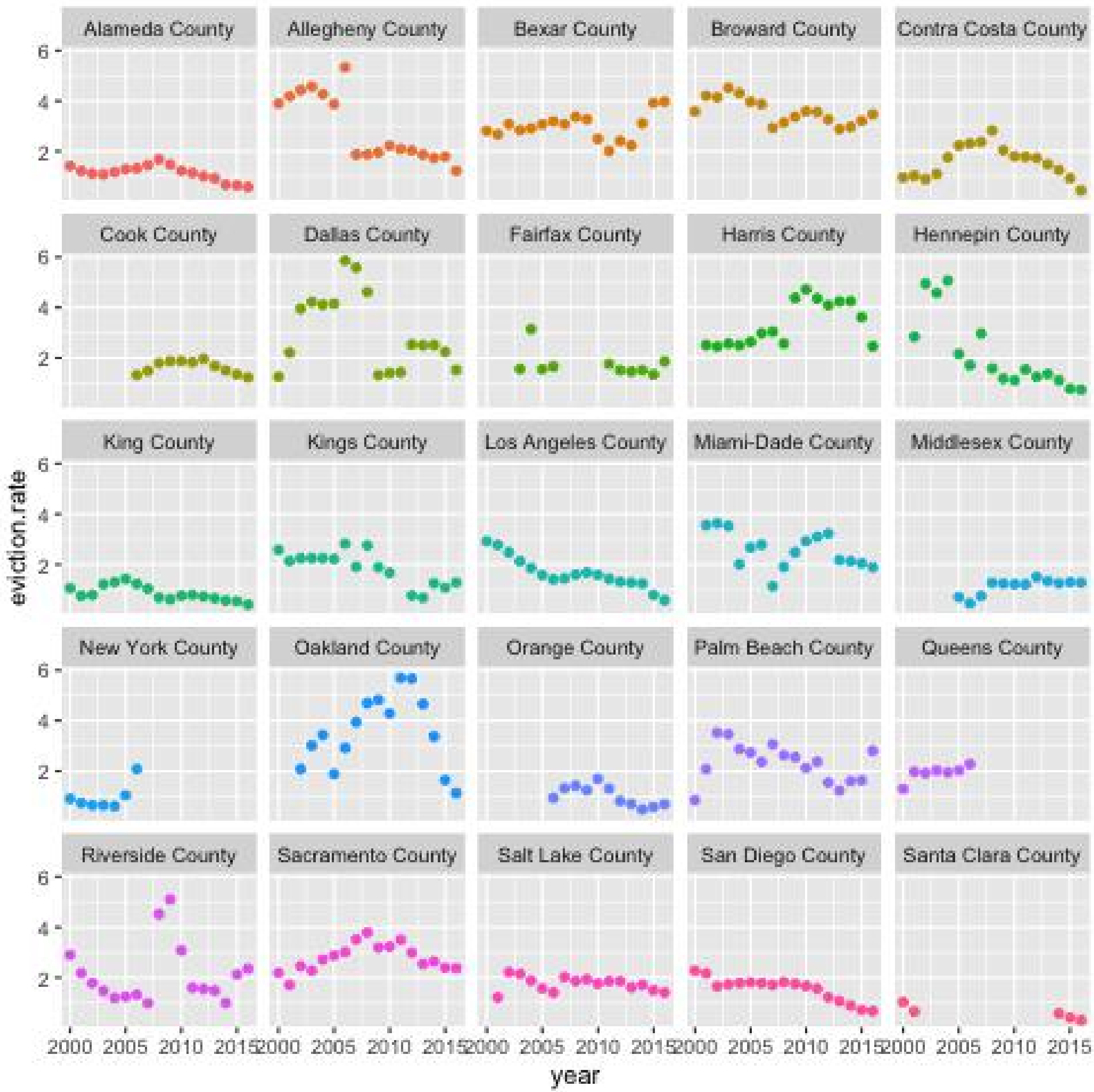
Based on Eviction Lab data (Desmond et al., 2018a), Cook County's eviction rate in 2016 was 1.22%, and it's eviction filing rate was 3.42%. Out of counties with populations greater than 1 million, Cook County ranked 11th out of 34 counties represented in the database in terms of lowest eviction rate.[48] We stress again that it is important to keep in mind that Desmond's data covers most, but not all, counties in the U.S., so this ranking isn't a perfect statistic.[49]

Cook County's eviction rate, as in most counties, seems to be increasing from 2000 to 2009-10 before dipping back down starting from 2011 (probably because of the recession). Most large counties have similar or slightly lower eviction rates in 2016 than they did in 2000. See Figure 11 for time series of eviction rate by year for the 25 counties with lowest average eviction rates among the 37 large counties as defined in the answer above.

[48]The ranking, from lowest 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.

[49]As mentioned above, data for New York County and Queens County are missing after 2005.

Figure 11: 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. Desmond's data has a total of 34 large counties. Data from The Eviction Lab (Desmond et al., 2018a).

A.5 Eviction and government housing assistance in Cook County

If tenants currently receive unit-based housing assistance, e.g., they live in public housing or receive a unit-tied voucher, then they would obviously lose this type of housing assistance once evicted. If a tenant currently receives a person-based housing choice voucher (HCV) and live in the private rental market, they have a fixed time window to find a new place to live that satisfies the associated quality standards, and they may have trouble doing so and lose the voucher as a result. If an evicted tenant tries to access new housing assistance, e.g. public housing or affordable housing or a new HCV, an eviction history can count against them.

From 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."

"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."

B Appendix: Recent eviction reforms

New York, NY Intro. 214-B was signed into law in August of 2017. It provides all low-income tenants facing eviction with legal representation. This was the first law to establish right to counsel in eviction cases in the country. Under the law, low-income is defined as earning 200% of the federal poverty level or less. The City has allocated $155 million over five years to this law.

San Francisco, CA Proposition F, a ballot measure, passed and became law in June 2018. It gives tenants facing eviction lawsuits the right to tax-funded legal assistance. This program is estimated to cost the city $5.6 million a year. Additionally, the program applies to renters of all income levels, not just low-income households.

Denver, CO In January 2018, ten Denver City Council members, through donations from office budgets and personal contributions, pooled together $123,600 to help start an eviction legal defense program. This program would be run through Colorado Legal Services.

In October 2017, the mayor launched a series of programs through the Office of Housing and Opportunities for People Everywhere (HOPE) and the Office of Economic Development, with the help of the Denver City Courts and Denver Human Services. They created a Landlord-Tenant Guide which clearly outlines the rights and responsibilities of both parties and provides a list of resources to help with conflict resolution before court action is taken. The city also put mediation services in place to resolve landlord-tenant conflicts before and after an eviction process begins.

Table 8: Recent Changes to Eviction Policy

Location	Year	Summary	Implemented?
Austin, TX	2018	Developers must pay tenants' relocation causes and evictions require "good cause."	Yes
Boston, MA	2018	H.4142: all landlords must notify the city when they begin an eviction.	No
California	2018	AB2343 would make landlords wait 10 days after rent is late to begin eviction.	No
Denver, CO	2018	Eviction legal defense program.	No
Maryland	2018	Judges can toss out eviction requests form landlords who fail to pass lead inspections.	No
North Carolina	2018	S.244 allows landlords to recover attorney's fees and filing fees incurred from a tenant during the eviction process.	Yes
Oakland, CA	2018	Extends "just cause" eviction protections to tenants living in owner-occupied duplexes and triplexes.	No
Philadelphia, PA	2018	Philadelphia Eviction Project provides legal services for tenants facing eviction.	Yes
Portland, OR	2018	Landlords must pay renters' moving costs when evicted without cause or due to a rent increase.	Yes
Richmond, VA	2018	Eviction Diversion Program	No
San Francisco, CA	2018	Proposition F gives all tenants the right to tax-funded legal assistance.	Yes
Santa Monica, CA	2018	Provides protection from eviction during the school year for educators and families with school age children.	Yes
Washington, D.C.	2018	Eviction notices must have a set date and that date must be at least 2 weeks in the future, evictions will occur by changing the locks.	Yes
Berkeley, CA	2017	Tenant Protection Ordinance prohibits landlords from conducting evictions using misleading information or coercive conduct.	Yes
Boston, MA	2017	Landlords of a certain size must notify the city when they begin an eviction.	Yes
Denver, CO	2017	Mediation services, Landlord-Tenant Guide, and financial support to low- and moderate-income households in crisis.	Yes
Detroit, MI	2017	Prevents landlords from collecting rent if they haven't passed city inspections.	Yes
Durham, NC	2017	Eviction Diversion Program	Yes
New York, NY	2017	Intro. 214-B provides all low-income tenants facing eviction with legal representation.	Yes
Philadelphia, PA	2017	Bill 170854 requires "good cause" for evictions; tenants must be notified 30 days in advance.	No

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

This mediation process aims to make the eviction process more balanced by allowing tenants access to financial resources to stay in their homes. This financial support is being provided by the Temporary Rent and Utility Assistance (TRUA) program, which launched in November 2017. This is available to low- and moderate-income households in crisis scenarios. TRUA provides up to 6 months of rent assistance and $1,000 in utility payments for households experiencing a "current financial or other housing crisis."

Philadelphia, PA The Philadelphia Eviction Protection Project was implemented in January 2018. It provides new and improved legal services for tenants facing eviction, including on-call legal assistance in the courtroom twice a week, a new tenant aid hotline, a website answering common legal questions, and full-time service in a Landlord-Tenant Help Center in the courtroom. It came out of the work of the Eviction Task Force, which was formed last year to help come up with solutions to solve the city's eviction problem.

Bill 170854 was introduced to the City Council in October 2017, passed through committee, and is still awaiting a final vote. If signed into law, it would require "good cause" for evictions. "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," they must notify tenants at least 30 days before the eviction date, and the tenant would have the right to contest the "good cause" by filing a complaint with the Fair Housing Commission.

Furthermore, in July of 2017, the City allocated $500,000 to help renters facing eviction receive legal assistance. It was unclear how exactly these funds would be distributed.

Richmond, VA In Richmond, planning began in June on the implementation of an Eviction Diversion program, which would mediate and provide alternatives to eviction for tenants. This would be similar to ones currently existing in Durham, NC and Kalamazoo, MI. Landlords would have to agree to participate in the program since no Virginia state codes are in place that can legally require them to participate.

Durham, NC In late 2017, Durham implemented an Eviction Diversion Program similar to the ones that are in place in Michigan. It aims to mediate and to find alternatives to eviction, working in partnership with Duke Law. It was announced in June 2018 that the program will receive $200,000 from the City of Durham.

Washington, D.C. The Eviction Reform Emergency Amendment Act of 2018 passed in June and took effect August 13. Some important reform measures included in the bill are that eviction notices must include a scheduled eviction date, while giving the tenant the notice at least two weeks prior. This contrasts the current schedule where eviction notices are given the day before the eviction. Additionally, the actual eviction will now happen simply by changing the locks on the property, rather than by removing the tenant's property and placing it on the street. The goal of this reform measure is to have tenants voluntarily evict themselves, resulting in fewer belongings left on the roads and no need for an eviction crew.

Santa Monica, CA Santa Monica City Council approved an ordinance that gives protections to tenants with school-age children or to tenants who are educators in May of 2018. This defense will apply in the case of no-fault evictions with the eviction date occurring during the school year. This aims to prevent evictions from disrupting the school year for both students and teachers.

Portland, OR The Portland City Council voted to implement a program that requires landlords to pay renters' moving costs when they are either evicted without cause or when they are forced to move due to a rent increase of 10% or more. It had existed in a trial basis for a year besides it was implemented permanently in March of 2018. Additionally, in May of this year, the Mayor put forth a draft for another set of reforms that would require landlords to serve tenants on a first-come, first-serve basis to ensure equitability within a frame of standardized metrics.

Oakland, CA The Oakland City Council voted unanimously in July 2018 for a proposed eviction protection measure that will be on the ballot for voters to decide on in November. The bill extends "just cause" eviction protections to tenants living in owner-occupied duplexes and triplexes. Currently, tenants residing in these buildings are not given the same protections as other tenants. These protections only allow landlords to evict tenants for specific reasons, including failure to pay rent or violating the lease.

Austin, TX The Austin City Council passed a collection of housing reforms in April 2018, which included some points on eviction. Namely, developers must pay tenants' relocation costs when they are displaced, landlords must have "good cause" for evictions, and Section 8 vouchers must be accepted in all units in city incentive programs.

California Bill AB2343, which is currently pending in the state legislature, would make landlords wait longer before evicting their tenants. The bill was introduced in February 2018. It would give landlords 10 days, upped from 3 originally, before allowing a late rent payment to initiate the eviction process. Additionally, tenants would have 14 days to respond to the landlord's eviction court filing, as opposed to the original five. While this bill is still awaiting a vote, two other eviction reform bills introduced at the same time were recently voted down.

Detroit, MI The Detroit City Council enacted a law in October 2017 that prevents landlords from collecting rent if they haven't passed city inspections, with lead inspections being an important driving factor in this initiative. In 2015, only 1 out of every 13 evictions cases in Detroit was filed on an address legally registered in the city. Under the law, after a 6-month phase-in period, tenants who live in units that haven't passed inspection can put their rent in an escrow account for 90 days. If, after that period of time, the landlord still has not obtained a city inspection certificate, the tenant can have the money back. Multifamily properties must be inspected every 2 years, one- to two-family properties must be inspected every 3 years.

Maryland Currently, there is a pending bill in the Maryland Senate passed in the House of Delegates in a similar form would allow judges to toss out eviction requests from landlords who fail to verify that their properties are safe from hazardous, lead-based paint. It was introduced in March of 2018. Under the bill, judges can either dismiss or delay the cases if there is evidence that the landlords are not complying with state lead paint rules. The two chambers' versions of the bill differ slightly. The House of Delegates version states that judges "may" toss out the eviction requests, whereas the Senate version requires judges to toss them out. The Senate version is favored by low-income housing advocates.

Berkeley, CA In March of 2017, Berkeley passed the Tenant Protection Ordinance, which is a document that prohibits landlords from conducting illegal evictions using fraudulent/misleading information or intimidating/coercive conduct. Landlords must now give a copy of the Ordinance to tenants when they begin renting, and also must include it with any eviction notice.

Boston, MA H.4142, the "Jim Brooks Stabilization Act," was introduced in the Massachusetts legislature in January of this year, and in July was referred "to study," which does not bode well for the bill's future. This bill, while introduced in the state legislature, would apply in Boston. It would provide the city with a way to track evictions in real time and allow the city to notify all tenants of their rights when they have been given an eviction notice. All landlords would have to notify the city when they issued eviction notices. However, being referred "to study" in the Massachusetts Legislature is how many bills die.

Before this, the Boston City Council passed an ordinance in October 2017 that required landlords of a certain size to notify the city when they begin an eviction process. This allows the city to inform tenants of their rights and connect them with community groups to help them. H. 4142 seems to take this ordinance and apply it to all landlords, regardless of size.

North Carolina S.244 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. It became law because, after passing both state chambers, it remained unsigned by the governor for 10 days.

C Appendix: Data

C.1 Financial strain outcomes: Detailed descriptions

This section describes key financial outcomes in the credit report data, which fall into five categories: (i) overall financial health (ii) access to credit, (iii) borrowing, (iv) unpaid bills, (v) consumption. We include zeros in the outcome measures.

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.[50] 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." Not everyone in our data has a 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 by using tools of machine learning. 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 individiuals do not have a score due to not enough information.

Borrowing

Our main measure of borrowing is *total revolving balances*, which represent the total balance on open revolving trades. The majority of revolving trades are credit cards, which can carry a balance across months. Our utilization measure captures the overall balance to credit amount ratio on open trades reported in the last 6 months with a positive credit amount. Note that credit amount is Experian's alternative measure of credit limit. For revolving trades, the credit limit is the maximum dollar amount a consumer can borrow without incurring additional fees. If the limit is not provided to Experian by the lender, Experian populates the credit amount with substitute values, in the following priority order: original credit amount, credit limit, high balance, charge-off amount. If none of the information is available, then the credit amount value defaults to 0.

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

Unpaid bills

We measure unpaid bills in two ways. The first is *total balance on collections*. Collections remain on a credit report up to 7 years from the time it is first placed in collections. The second is the *total number of occurrences of 30 days delinquency* in the last 12 months on trades excluding collections and other unsatisfied derogatory.

Durable goods consumption

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 consumption.

Zipcode 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 Zipcode of residence is not the most recent address, but the 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.

C.2 Subprime borrowing data

The subprime borrowing data comes from Clarity, a part of Experian, which is an FCRA-regulated credit reporting agency that maintains the largest subprime database of over 62 million unique consumers. 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 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%), 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%), 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%), Storefront Installment (1.5%), which together constitute 96 percent of tradelines opened. Internel SPML and Storefront SPML would constitute payday loans.

C.3 Details on data cleaning and linkage

Data restrictions on the court sample

Table 9: Summary of data restrictions.

Sample	Named Individuals	Cases	Judges
Full	772,846	583,871	313
No businesses or condos	743,024	567,401	313
Non-missing names	722,893	559,292	312
Damages <$100,000	720,110	557,503	311
Non-missing judge	719,043	556,692	310
Judge sees more than 10 cases per year	653,913	507,694	262
Multiple judges per week	610,777	476,648	251
In Experian sample	210,604	185,487	168

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.

Defining case outcomes

For each eviction case we have a record of each event associated with the 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”

though in over 99% of cases we classify as ending in eviction had an “ORDER FOR POSSESSION” ruling and results are robust to using an alternate indicator of eviction for if there is an oder for posession issued by the judge.

In addition to determining if a case ended in eviction, we study if the case resulted in voluntary dismissal, involuntary dismissal, or ruling for the defendant. Rulings for the defendant include:

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

Voluntary 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 WITHOUT PREJUDICE -ALLOWED”

Involuntary dismissal codes include:

- “CASE DISMISSED WITH PREJUDICE - ALLOWED”,

Table 10: Breakdown of case outcomes.

Case Outcome	Proportion
Evicted	0.60
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 where the 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 where the defendants' 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 not included in the analysis.

Standardizing addresses

Addresses were first checked for common misspellings, typos, or 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 first judge from the raw data

The judge (and their associated leniency) assigned based on the randomized court room assigned to the case at filing. Once a case is filed we see the date, time, and court room assigned to the case. Using this information we assign the judge presiding in that court room at that date and time. Note that this assigns a judge to the case even if the case is withdrawn before the first hearing (which for experience plaintiffs could be influenced by the judge overseeing the case).

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 properly 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, with the most common reason for differences being cases where the tenant is never served and the case is withdrawn in which case our alternative measure of judge is missing.

D Appendix: Sample linkage

Table 11: Sample attrition and judge stringency.

	2007	2009	2010	2011	2013	2015	2017
2005	0.00035	-0.00458	-0.00192	0.00745	0.00099	0.00081	0.00037
	(0.00871)	(0.00958)	(0.01081)	(0.01001)	(0.01008)	(0.01015)	(0.00840)
	[0.9961]	[0.9962]	[0.9956]	[0.9951]	[0.9941]	[0.9918]	[0.9919]
2007		-0.00367	-0.00407	0.00784	0.00370	0.00212	0.00263
		(0.01032)	(0.01145)	(0.01073)	(0.00994)	(0.00913)	(0.00802)
		[0.9962]	[0.9955]	[0.9950]	[0.9939]	[0.9917]	[0.9918]
2009			-0.00078	0.00908	0.00458	0.00116	0.00240
			(0.00905)	(0.00987)	(0.00965)	(0.00853)	(0.00772)
			[0.9961]	[0.9953]	[0.9939]	[0.9917]	[0.9919]
2010				0.00907	0.00405	0.00341	0.00146
				(0.00580)	(0.00891)	(0.00827)	(0.00759)
				[0.9975]	[0.9944]	[0.9921]	[0.9919]
2011					0.00194	0.00219	0.00091
					(0.00887)	(0.00805)	(0.00764)
					[0.9947]	[0.9923]	[0.9920]
2013						0.00008	0.00214
						(0.00694)	(0.00704)
						[0.9933]	[0.9923]
2015							-0.00027
							(0.00608)
							[0.9937]

Table 12: Exploring characteristics of matches.

Evicted	-0.019***
	(0.002)
Pred. Af-American	0.223***
	(0.015)
Pred. White	0.178***
	(0.016)
Pred. Hispanic	0.049***
	(0.015)
Pred. Asian	0.108***
	(0.017)
Ad Damnum	-0.004***
(1000s)	(0.000)
Joint Action	0.072***
	(0.002)
Defend. Pro Se	-0.017***
	(0.004)
Med. Household Inc. (1000s)	0.001***
	(0.000)
Pct. Af.-Am	-0.008**
	(0.004)
Filing Year 2006	0.008*
	(0.004)
Filing Year 2007	0.013***
	(0.004)
Filing Year 2008	-0.002
	(0.004)
Filing Year 2009	0.020***
	(0.004)
Filing Year 2010	0.038***
	(0.004)
Filing Year 2011	0.053***
	(0.004)
Filing Year 2012	0.059***
	(0.004)
Filing Year 2013	0.054***
	(0.005)
Filing Year 2014	0.053***
	(0.004)
Filing Year 2015	0.048***
	(0.004)
Filing Year 2016	0.029***
	(0.004)
Number of observations	366,138
R^2	0.0194
Mean of dep. var.	0.6080

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 event study analysis sample restrictions imposed, and where we keep only post-March 2005 cases (the only allowable matches to have a post-credit file case). The dependent variable is an indicator for a successful match to any credit file. The controls include neighborhood median household income and percent African-American (at the tenant ZCTA), predicted race of the tenant (ranging from 0 to 1, and with omitted category "Other"), dummies for filing year, and case characteristics.

E Appendix: Event studies

For the initial analysis of selection we append the 10 percent random sample from Cook County to our court sample, restricting the random sample to those over age 21 who are non-homeowners (i.e. with no open mortgage trade), and randomizing a placebo filing month. We also re-weight the regression sample so that placebo individuals' distribution across zipcodes 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:

$$y_{it} = X_{it}\tilde{\alpha} + \tilde{\gamma} \times court_i + \tilde{\delta} \times evict_i + \sum_{r=S}^{F} \tilde{\beta}_r + \sum_{r=S}^{F} \tilde{\gamma}_r \times court_i + \sum_{r=S}^{F} \tilde{\delta}_r \times evict_i + \tilde{\epsilon}_{it} \quad \text{(E.1)}$$

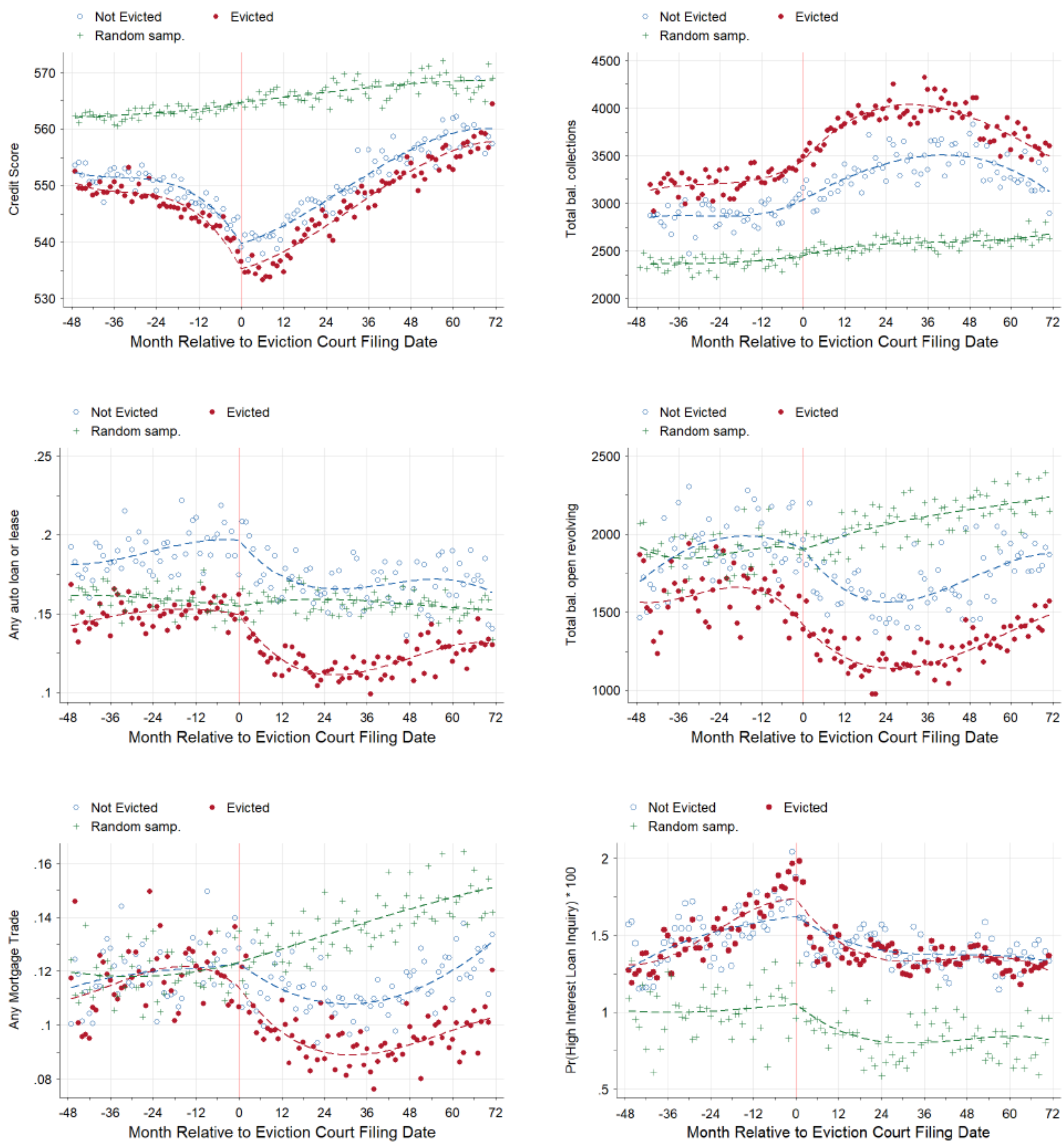
where r represents the month relative to the case filing month. The vector X_{it} represents individual controls, including age at filing month, gender, and baseline credit score. The variable $court_i$ is an indicator for the individual being in our eviction court sample, and 0 if the person is in the placebo group; $evict_i$ 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 E.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.

Table 13: Event study: main estimates.

	Credit score	Total Collections	Any Auto Loan	Revolving Balances
	(1)	(2)	(3)	(4)
12-Month Effect	-2.835***	191.777***	-0.012***	-103.796***
	(0.369)	(22.972)	(0.002)	(27.360)
36-Month Effect	-2.172***	158.517***	-0.013***	-117.439***
	(0.386)	(27.424)	(0.002)	(28.919)
60-Month Effect	-2.118***	-0.843	-0.003	-108.758***
	(0.451)	(31.353)	(0.003)	(32.131)
Baseline non-evict mean	552.22	2,674.15	0.18	1,950.67
Number of individuals	250,934	252,660	254,585	177,437
Number of observations	1,302,965	1,310,089	1,320,357	907,492

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

Figure 12: Selection into eviction court



The figure above plots estimates of Equation E.1. 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.

Table 14: Event study: estimates under alternative specifications

	Baseline	Indiv. f.e.	Balanced Panel	No prior cases
	(1)	(2)	(3)	(4)
12-Month Effect	-2.835***	-2.478***	-2.856***	-3.556***
	(0.369)	(0.359)	(0.538)	(0.484)
36-Month Effect	-2.172***	-1.222***	-4.168***	-2.820***
	(0.386)	(0.373)	(1.257)	(0.503)
60-Month Effect	-2.118***	-0.829*		-3.142***
	(0.451)	(0.434)		(0.582)
Baseline non-evict mean	552.22	552.22	558.28	568.07
Number of individuals	250,934	250,934	66,924	153,602
Number of observations	1,302,965	1,302,965	392,471	797,257

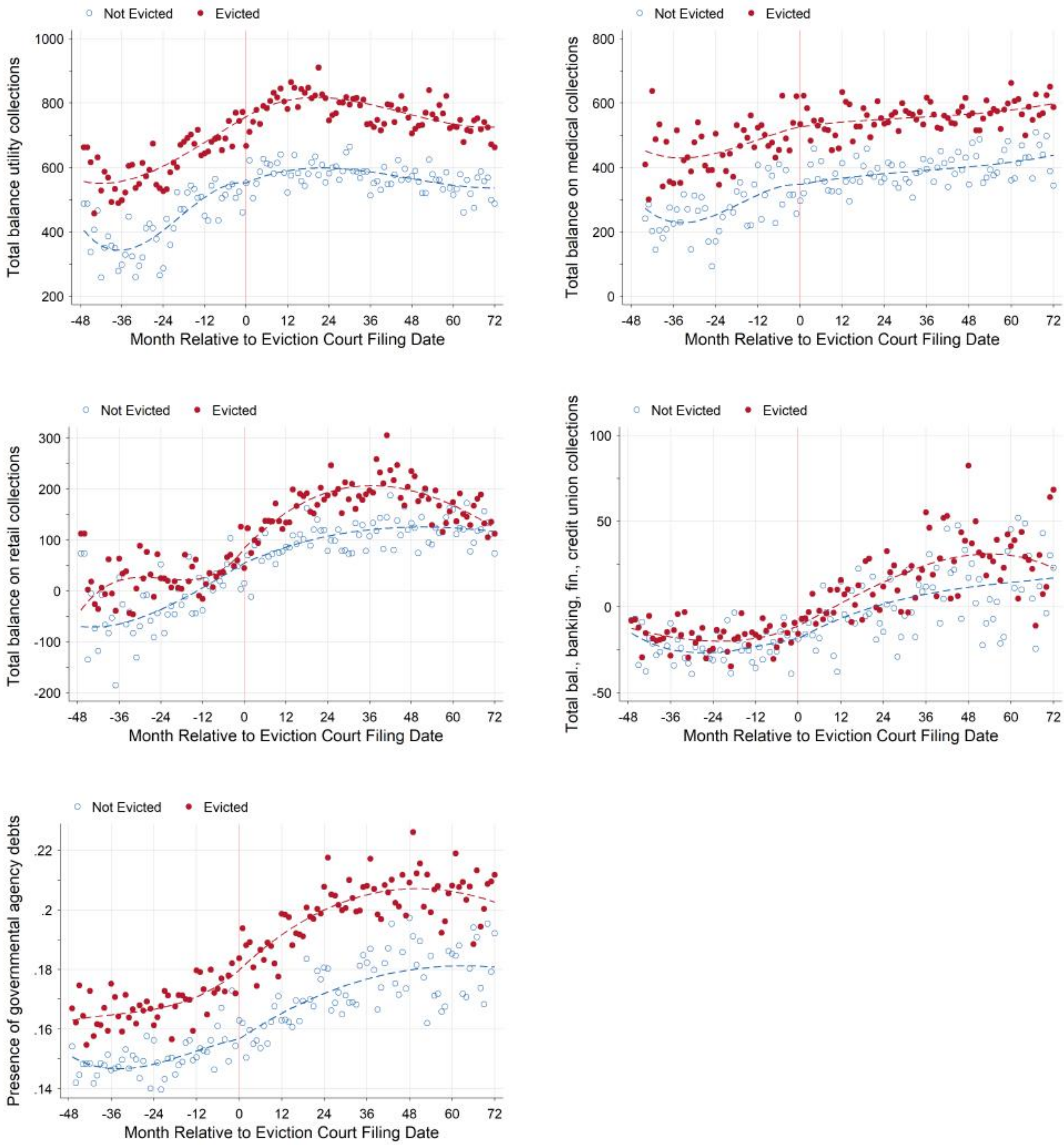
	Baseline	Indiv. f.e.	Balanced Panel	No prior cases
	(1)	(2)	(3)	(4)
12-Month Effect	191.777***	162.831***	187.558***	234.443***
	(22.972)	(21.974)	(34.164)	(28.236)
36-Month Effect	158.517***	126.007***	237.743***	261.526***
	(27.424)	(27.126)	(79.384)	(33.183)
60-Month Effect	-0.843	-61.723*		92.924**
	(31.353)	(31.657)		(37.976)
Baseline non-evict mean	2,674.15	2,674.15	2,421.23	2,148.45
Number of individuals	252,660	252,660	66,968	155,222
Number of observations	1,310,089	1,310,089	392,947	803,935

	Baseline	Indiv. f.e.	Balanced Panel	No prior cases
	(1)	(2)	(3)	(4)
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)
Baseline non-evict mean	0.18	0.18	0.20	0.19
Number of individuals	254,585	254,585	67,544	156,385
Number of observations	1,320,357	1,320,357	396,186	810,375

	Baseline	Indiv. f.e.	Balanced Panel	No prior cases
	(1)	(2)	(3)	(4)
12-Month Effect	-103.796***	-69.971***	-139.720***	-169.647***
	(27.360)	(25.124)	(45.945)	(37.032)
36-Month Effect	-117.439***	-73.571***	-108.597	-172.048***
	(28.919)	(27.392)	(81.524)	(38.416)
60-Month Effect	-108.758***	-87.436***		-169.123***
	(32.131)	(32.614)		(42.280)
Baseline non-evict mean	1,950.67	1,950.67	2,481.30	2,460.55
Number of individuals	177,437	177,437	43,007	110,120
Number of observations	907,492	907,492	252,073	562,640

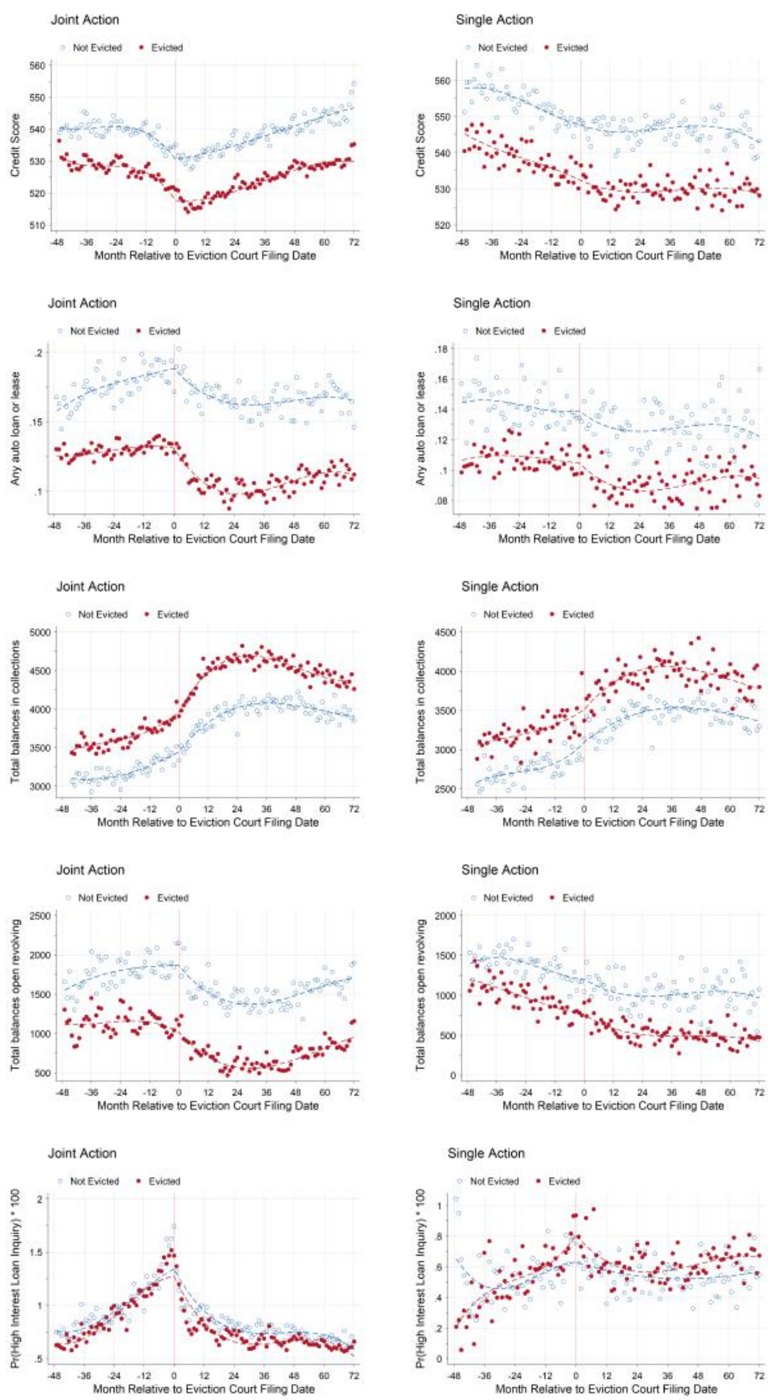
The table above re-estimates the regressions of Table 13 under alternative specifications. Column 1 presents the baseline specification of Table 13, Column 2 includes individual fixed effects, Column 3 limits the sample to a balanced panel (cases from March 2008 to March 2014 and with relative month $r = -36$ to 36), and Column 4 restricts the sample to individuals with no prior eviction cases.

Figure 13: Components of unpaid bills: types of collections and government agency debt.



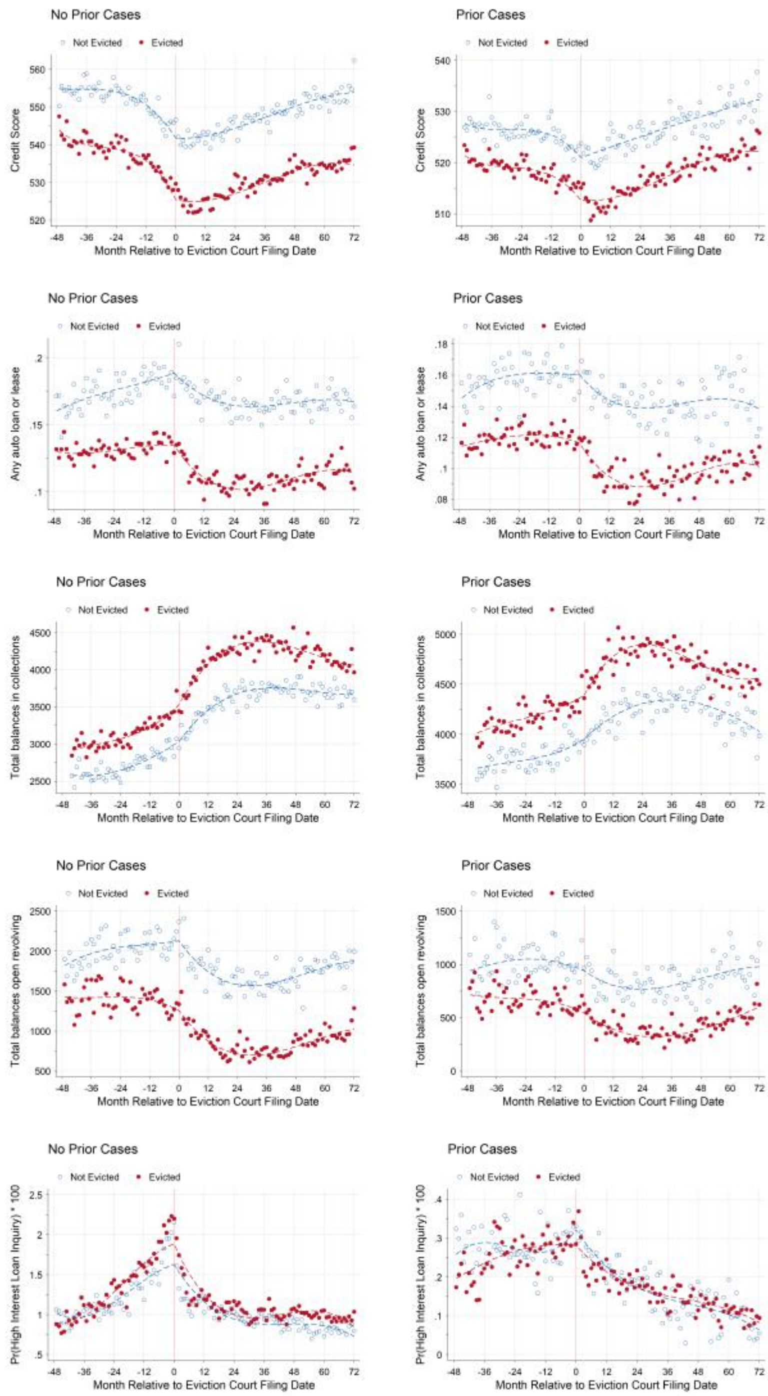
The figure is constructed as in Figure 6. The top four panels display four types of collections debt (utilities, medical, banking/financial/credit union, retail). Medical collections are only available as a separate category from years 2013, 2015, 2017. The bottom left panel reflects the presence of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

Figure 14: Event studies: heterogeneity by case type



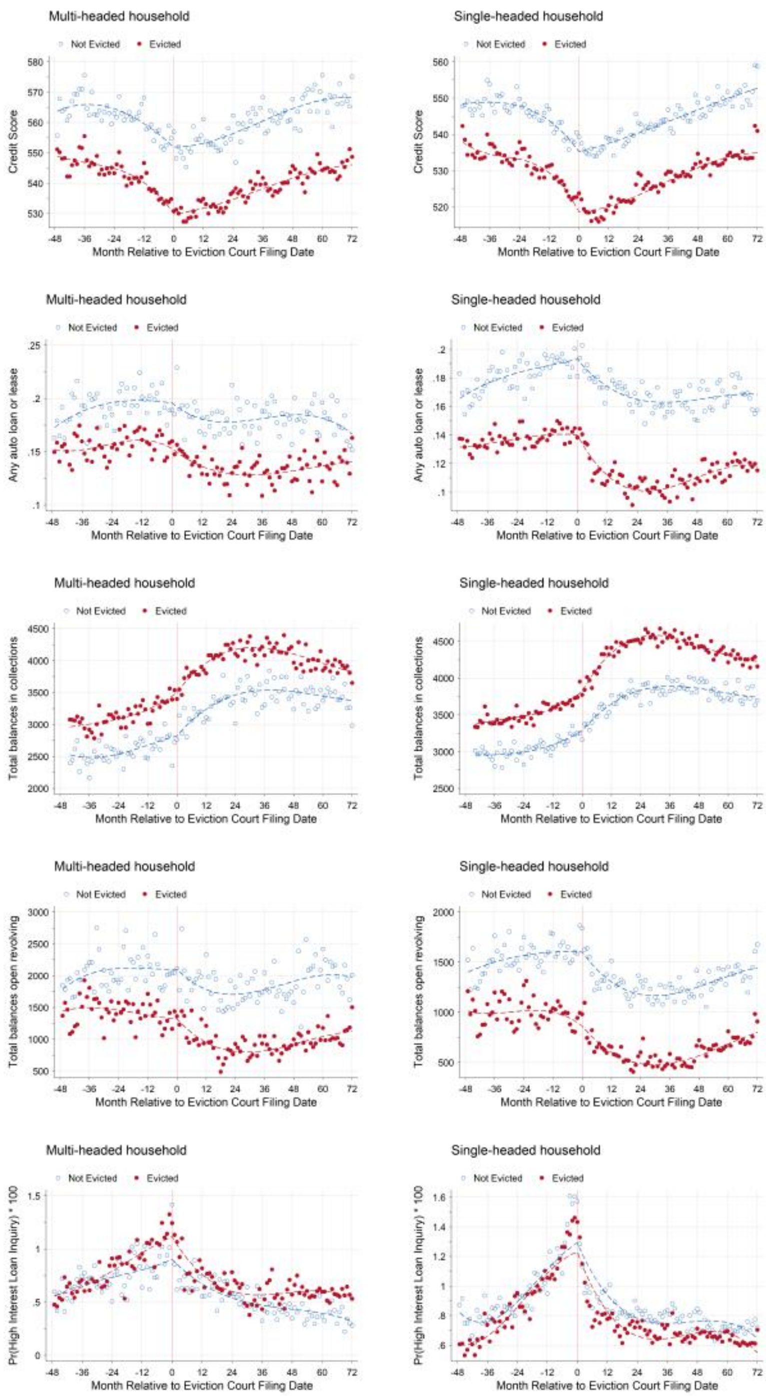
The figure is constructed as in Figure 6, separately by case type.

Figure 15: Event studies: heterogeneity by prior cases



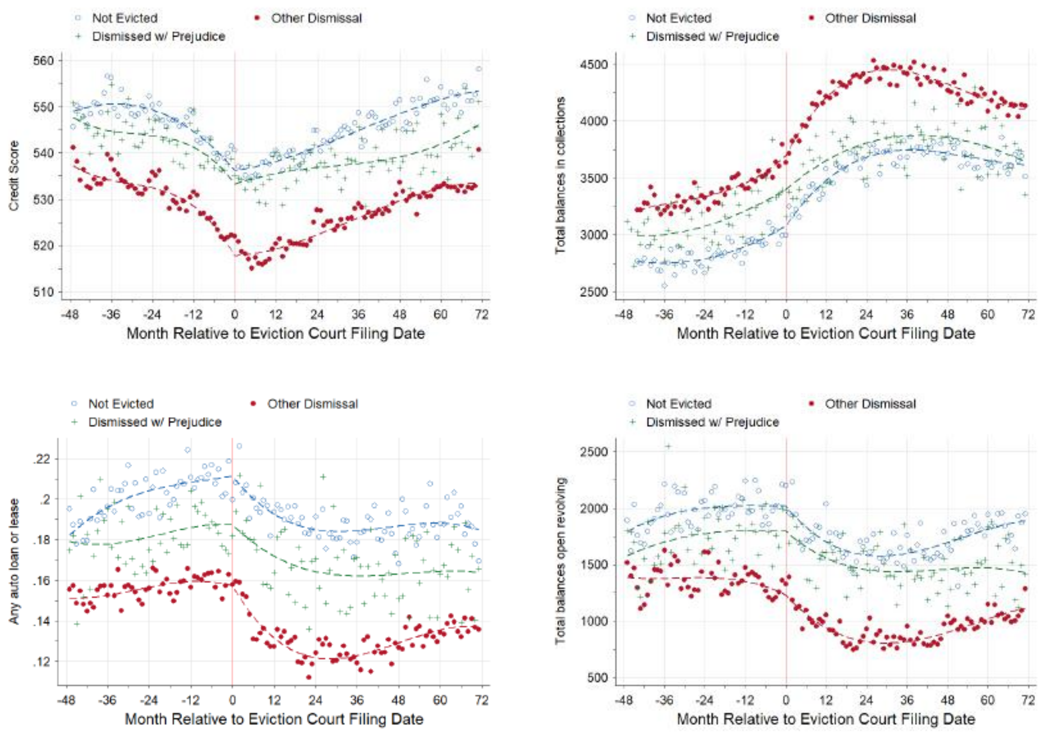
The figure is constructed as in Figure 6, separately by whether the individual had a prior eviction case or not.

Figure 16: Event studies: heterogeneity by multi v. single-headed household



The figure is constructed as in Figure 6, separately 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 17: Event studies: disaggregated case outcomes



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

F Appendix: IV analysis

F.1 Robustness checks for the first stage

This section provides a number of additional robustness checks to the sample selection and modeling assumptions made above. Table 15 documents how the first-stage regression of residual judge stringency changes under a number of alternative specifications. The first two rows add additional controls to the first stage. The first row controls for case length, while the second row controls for residual judge stringency in case length, 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 do not always correspond to the same judge. In the third row of Table 15, 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 6 use alternative sample selection criteria. Row 4 restricts cases to judges who see more than 100 cases that year, row 5 includes all judges rather than those who see more than 10 cases, row 6 includes all cases, including cases against businesses and row 7 excludes cases where the court record does not show the tenant ever being served.[51]

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, remains positive, and remains statistically significant with small standard errors. In total, these robustness checks demonstrate that our first stage is robust to additional controls, different sample-selection criteria, different definition of first judge, and split-sample estimation of stringency.

F.2 Exclusion restrictions checks

We explore robustness to our definition of the binary treatment, and the corresponding definition of stringency. In particular, we consider three alternative definitions of the treatment: (1) a "win" for the defendant, meaning either a dismissal with prejudice or a verdict for the defendant, (2) an eviction order that is also sent to the sheriff's office, and (3) an eviction order that is also sent to the sheriff's office and executed. Table 24 shows the correlations between constructions of judge stringency based on these outcomes, alternative first-stages based on these case outcomes, and alternative IV results based on these case outcomes. Judge's residual stringency for a defendant "win" has only a correlation of -0.221 with our stringency measure and very low correlations

[51]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.

Table 15: Specification check on stringency in first stage.

Sample	Coefficient	Standard Errors	P-Value	Observations
Main	0.710	0.027	0.000	516, 771
Controlling for case length	0.661	0.028	0.000	516, 771
Controlling for other judge chars.	0.726	0.030	0.000	515, 167
Alternate first judge construction	0.722	0.023	0.000	506, 603
All cases	0.630	0.027	0.000	576, 042
Excluding cases never served	0.722	0.026	0.000	470, 244
Exludes judges <100 cases	0.797	0.025	0.000	506, 141
Includes judges <10 cases	0.668	0.028	0.000	517, 380
Single-Action stringency on Joint-Action	0.280	0.026	0.000	415, 650
Joint-Action stringency on Single-Action	0.935	0.085	0.000	107, 828

Notes: Table shows the coefficient on our measure of residual judge stringency in 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.

with stringency measures based on the sheriff's office. Table 25 provides IV results on credit score using these alternative outcomes and shows that a defendant "win" has a large and positive effect on credit score but which is not statistically significant.

An eviction order that is also filed with the sheriff's office is associated with a reduction in credit score of almost twice the size of the estimates from our main specification. This is not our preferred empirical approach, however, because the exclusion restriction under this specification is unlikely to hold, since only 50 percent of eviction orders are filed with the sheriff's office, and only 27 percent are filed with the sheriff's office and execute. Orders filed with the sheriff's office conditional on the judge granting an order for possession are not random and we do not have an additional source of exogenous variation to identify the separate effects of sheriff's office filing or execution.

F.3 Judge characteristics

Table 16: Correlations between residual stringency measures.

	Stringency	Continuance	Amount	Stays
Residual stringency	1	-0.069	0.103	0.066
Residual continuance	-0.069	1	0.035	-0.004
Residual judgment amount	0.103	0.035	1	0.087
Residual stays granted	0.066	-0.004	0.087	1

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

Table 17: Case outcomes on judge stringency measures.

	Evicted	Continuance allowed	Case length	Stay Allowed	Ruling amount - ad damnum	
	(1)	(2)	(3)	(4)	(5)	(6)
Eviction Stringency	0.830***	−0.013	68.142	−0.011	716.510*	255.604**
	(0.030)	(0.010)	(49.230)	(0.010)	(383.583)	(115.351)
Continuance Stringency	−0.013	0.880***	11.298	0.003	306.895	254.417*
	(0.032)	(0.047)	(80.345)	(0.013)	(488.168)	(146.199)
Case Length Stringency	−0.00000	−0.00001	0.807***	−0.00001*	0.255*	0.228***
	(0.00002)	(0.00001)	(0.059)	(0.00001)	(0.139)	(0.088)
Judgment Amount Stringency	−0.00000	−0.00000	0.0002	0.00000	0.012**	0.001
	(0.00000)	(0.00000)	(0.0002)	(0.00000)	(0.005)	(0.001)
Stay of Evic. Stringency	−0.049	0.064**	−98.728	0.731***	202.501	39.502
	(0.054)	(0.032)	(88.754)	(0.090)	(306.676)	(254.267)
Observations	462,938	462,938	462,938	281,424	267,561	199,447
R^2	0.027	0.044	0.065	0.054	0.062	0.081

Note: *p<0.1; **p<0.05; ***p<0.01

Notes: Table shows various case outcomes regressed on residual stringency measures of the judge assigned to the case. The last three columns are restricted to cases ending in eviction. Column 6 adds the restriction that the judgment amount is not 0 to column (5) to evaluate the importance of the extensive margin. Robust standard errors clustered at the judge-year level are reported in the parentheses. Controls and district by year fixed effects omitted from table.

Table 18: Unique judges seeing 10 or more cases by year and district.

Year	District					
	1	2	3	4	5	6
year	1	2	3	4	5	6
2000	22	4	2	3	5	7
2001	21	6	3	5	6	9
2002	28	6	4	3	4	7
2003	26	5	3	5	4	7
2004	26	6	5	6	4	6
2005	26	4	4	6	3	4
2006	19	5	5	6	6	4
2007	23	6	3	4	6	7
2008	23	4	5	5	5	6
2009	27	4	4	4	7	6
2010	28	4	4	6	5	5
2011	27	4	8	7	6	5
2012	24	6	6	5	6	5
2013	25	5	3	4	2	6
2014	29	4	3	4	3	5
2015	25	4	7	5	5	6
2016	28	5	4	5	7	6

Table 19: **Judge characteristics breakdown**

Sample	Male	Female	White	Black	Hispanic
Number of Judges	110	64	77	30	8
Number of Total Cases	366780	149784	234390	196694	19930
Stringency Percentage Point Difference (90^{th} and 10^{th} Percentiles Judges)	0.071	0.079	0.072	0.078	0.035

F.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 sub-samples, including interactions between judge characteristics and individual characteristics.

Basic monotonicity checks Table 22 shows the coefficient for stringency[52] 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 was **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 sub-samples the coefficient on stringency keeps the same sign, providing evidence that monotonicity is not violated.

Judge-tenant interaction monotonicity checks Our data also enables us to study interactions between judge characteristics and tenant characteristics. By allowing judge characteristics to interact with defendant characteristics, we can perform an even richer analysis of our monotonicity assumption.

Table 19 shows the breakdown of judges by race and gender characteristics.[53] 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 sub-samples have at least 30 judges, over total $100,000$ cases, and at least an 7 percentage point difference between the 90^{th} and 10^{th} percentile judges.[54] In contrast, our data only includes 8 Hispanic judges, a combined case-load of less than $20,000$ cases, and a

[52] Specifically, the leave-one-out estimator of a judge's stringency adjusted for year $\times$ cohort trends.

[53] race and gender were imputed based on pictures of the judges

[54] The differences in stringency are comparable to the 7 percentage points difference in stringency observed in the entire judge data set.

judge stringency difference of less than 4 percentage points, suggesting that cross interactions between Hispanic judges and tenant characteristics will most likely suffer from a small sample size.

Table 20 shows the coefficient for stringency from the regression on being evicted 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. As expected, 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 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 sub-samples do indeed violate the judge monotonicity assumption.

A closer look at cross interactions between Hispanic judges and tenant sub-samples with 21 reveals that there are no statistically significant negative stringency coefficient is the interaction between Hispanic judges and tenant sub-samples.

Table 20: Monotonicity checks, coefficient of stringency

Pro se tenants	Male judges	Female judges	White judges	Black judges	Hispanic judges
All	0.710	0.641	0.609	0.720	-0.204
Male	0.639	0.647	0.551	0.732	-0.904
Female	0.764	0.638	0.654	0.712	0.391
White	0.468	0.675	0.403	0.580	-3.238
Black	0.823	0.734	0.748	0.752	0.695
Hispanic	0.794	0.525	0.718	0.758	0.352

Table 21: Monotonicity checks: Hispanic judges

Pro_Se_Tenants	Coefficient	P-Value	Observations
All	-0.204	0.850	19661
Male	-0.904	0.422	8713
Female	0.391	0.702	10948
White	-3.238	0.009	2409
Black	0.695	0.455	10764
Hispanic	0.352	0.676	2663

Table 22: **Testing the monotonicity assumption.**

Sample	Coefficient	Standard Errors	P-Value	Observations
Main	0.711	0.030	0.000	516,771
Joint Action	0.622	0.028	0.000	411,896
Forcible Entry and Detainer	1.036	0.066	0.000	104,875
Males	0.652	0.032	0.000	224,860
Females	0.753	0.032	0.000	291,911
No attorney	0.721	0.029	0.000	497,703
Attorney	0.384	0.132	0.004	19,068
Black	0.803	0.035	0.000	271,216
Hispanic	0.787	0.078	0.000	64,796
Larger landlords	0.533	0.033	0.000	338,299
Smaller landlords	0.953	0.049	0.000	195,619

Notes: Table 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 the regression. "Black" and "Hispanic" are imputed using 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 3 or fewer cases ever appearing in the sample, and "larger landlords" are those with more than 3 cases appearing in the sample.

Conclusion The above monotonicity analysis provides an important insight. Our ability to perform monotonicity checks based on both tenant sub-samples and judge-tenant interacted sub-samples greatly validates our monotonicity assumption. Performing such in-depth checks, especially in the context of court cases, is crucial because interactions between different parties could conceivably uncover violations of monotonicity despite no apparent violations when simply considering a party on its own.

F.5 Alternative definition of treatment

Table 23: Alternative first stage regressions with different case outcomes.

	Outcome: Evicted			
Judge Stringency Meas.	Coefficient	Standard Errors	P-Value	Obs.
Eviction Ruling	0.728	0.025	0.000	522468
Def. Wins	-0.951	0.098	0.000	522468
Shrf. Evict. Ordered	0.724	0.090	0.000	149297
Shrf. Evict. Completed	0.684	0.125	0.000	149297
	Outcome: Defendant Wins Case			
Judge Stringency Meas.	Coefficient	Standard Errors	P-Value	Obs.
Eviction Ruling	-0.206	0.035	0.000	522468
Def. Wins	0.842	0.033	0.000	522468
Shrf. Evict. Ordered	-0.043	0.034	0.218	149297
Shrf. Evict. Completed	-0.009	0.039	0.817	149297
	Outcome: sheriff's Eviction Ordered			
Judge Stringency Meas.	Coefficient	Standard Errors	P-Value	Obs.
Eviction Ruling	0.380	0.060	0.000	149390
Def. Wins	-0.249	0.202	0.217	149390
Shrf. Evict. Ordered	0.454	0.092	0.000	149297
Shrf. Evict. Completed	0.417	0.131	0.002	149297
	Outcome: sheriff's Eviction Completed			
Judge Stringency Meas.	Coefficient	Standard Errors	P-Value	Obs.
Eviction Ruling	0.197	0.037	0.000	149390
Def. Wins	-0.158	0.150	0.291	149390
Shrf. Evict. Ordered	0.263	0.055	0.000	149297
Shrf. Evict. Completed	0.187	0.089	0.035	149297

Notes: Table shows coefficients on four different measures of judge stringency on four different ways of measuring the outcome of the case. All measures of judge stringency are leave-one-out estimates with district-by-year fixed effets removed. Each row shows the coefficient on that given measure of judge stringency. Regression also controls for district by year fixed effects. Standard errors are clustered at the judge-year level. Data from the sheriff's department only available from 2011 to 2015.

Table 24: Correlations between different case outcomes.

	Stringency	Stringency_defendant_wins	Stringency_shrf_evic_ordered
Stringency	1	-0.264	0.711
Stringency_defendant_wins	-0.264	1	-0.138
Stringency_shrf_evic_ordered	0.711	-0.138	1
Stringency_shrf_evic_completed	0.505	-0.048	0.673

Table 25: IV estimates with different measures of case outcomes.

	Credit Score		Credit Score		Credit Score	
	(1)	(2)	(1)	(2)	(1)	(2)
Def. Wins	-20.79	76.18				
	(19.85)	(51.15)				
Shrf's Evic Ordered			-51.37*	-37.24*		
			(23.54)	(18.43)		
Shrf's Evic Completed					7.566	-57.11
					(76.48)	(30.60)
Constant	522.0***	520.8***	571.6***	557.5***	516.4***	548.8***
	(0.665)	(0.872)	(23.62)	(18.53)	(38.33)	(15.90)
Observations	209555	209555	101898	101940	101898	101940

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table shows IV estimates using three alternate definitions of case outcomes. The first two columns show results for the defendant winning the case, the second two columns show results for an eviction order being filed with the sheriff, and the last two columns show results for the sheriff's office completing the eviction. The columns labeled (1) show results using residual judge stringency for this specific outcome as the instrument while the columns labeled (2) show results using the main measure of judge stringency as the instrument. sheriff's records on ordered and completed evictions are for 2011 to 2015. All regressions control for district × year fixed effects. Standard errors are clustered at the judge-year level.

Table 26: Log judgment amount on judge stringency.

	Log Judgment Amount			
	Excluding 0s		Including 0s	
	(1)	(2)	(3)	(4)
Judge Stringency	−0.054	0.006	1.505	1.527
	(0.104)	(0.082)	(1.190)	(1.187)
log ad damnum		0.773***		0.661***
		(0.003)		(0.014)
Constant	7.192***	1.793***	5.030***	0.418***
	(0.011)	(0.023)	(0.084)	(0.129)
Observations	267,813	267,813	362,953	362,953
R^2	0.095	0.618	0.027	0.053
Adjusted R^2	0.095	0.618	0.027	0.052
Observations	263,621	263,621	356,649	356,649
R^2	0.00001	0.587	0.0003	0.038
Note:				*p<0.1; **p<0.05; ***p<0.01

Notes:
Table shows regression of log judgment amount on judge stringency for joint action cases ending in a judgment for eviction. The first two columns show this regression excluding cases where no judgment amount was reported while the second two columns set log of judgment amount to 0. The second and fourth column include controls for the log of Ad Damnum amount plus 1. Standard errors are clustered at the judge-year level. Results are similar when working in levels. Judge stringency has a positive coefficient on judgment amount, but is not statistically significant. District by year fixed effects included but not shown.

Table 27: Summary statistics: tenants facing eviction.

Analysis sample	All	Evicted	Not Evicted
Prop. Black	0.551	0.524	0.565
Prop. Hisp	0.159	0.157	0.160
Prop. White	0.228	0.248	0.217
Prop. Male	0.444	0.445	0.443
Prop. without Lawyer	0.962	0.946	0.971
Avg claimed amount	2,171.818	2,102.229	2,208.569
Median HH Income (tract)	43,508.780	45,084.480	42,675.930
Per-cap Income (tract)	24,244.440	26,028.560	23,301.420
Median Rent (tract)	772.261	789.408	763.201
Pct of House Stock Rented (tract)	0.566	0.564	0.567
Avg. Perc. Vacant (tract)	0.138	0.134	0.139
Avg Pct. Black (tract)	0.497	0.471	0.511
Avg Pct. Hisp (tract)	0.185	0.179	0.189
Avg Pct. Below Pov-Line (tract)	0.216	0.210	0.220
IV sample		Low Stringency	High Stringency
Prop. Black		0.574	0.576
Prop. Hisp		0.158	0.158
Prop. White		0.210	0.208
Prop. Male		0.439	0.441
Prop. without Lawyer		0.961	0.962
Avg claimed amount		2,094.390	2,096.592
Median HH Income (tract)		42,519.020	42,536.480
Per-cap Income (tract)		23,975.790	23,846.650
Median Rent (tract)		764.554	763.797
Pct of House Stock Rented (tract)		0.579	0.576
Avg. Perc. Vacant (tract)		0.142	0.141
Avg Pct. Black (tract)		0.520	0.522
Avg Pct. Hisp (tract)		0.186	0.185
Avg Pct. Below Pov-Line (tract)		0.224	0.223

Notes: The table above shows summary statistics for all defendants who appear in our court records sample from 2000-2016 which are included in our analysis sample. Race and gender are estimates based on the defendant's first name (for gender) and the defendant's last name and census tract (for race). The proportion without a lawyer and the average claimed amount are observed directly in the court records. Neighborhood characteristics are calculated at the tract level using the address associated with the court record. Observations reports the total number of cases in our baseline sample: the defendant was not a business and the judge assigned to the case saw at least 10 cases that same year.

Table 28: Tests of random assignment of judges and first stage.

	Stringency	Eviction	
Stringency			0.710^{***}
			(0.027)
Ad damnum	0.00000	0.00000^{***}	0.00000^{***}
	(0.00000)	(0.00000)	(0.00000)
Median rent	−0.0005	-0.066^{***}	-0.065^{***}
	(0.0004)	(0.006)	(0.006)
Pct black (tract)	0.0004	0.103^{***}	0.103^{***}
	(0.0004)	(0.006)	(0.006)
Male	0.0002^{*}	0.009^{***}	0.009^{***}
	(0.0001)	(0.001)	(0.001)
Black	0.0005	-0.025^{*}	-0.025^{*}
	(0.001)	(0.014)	(0.014)
No attorney	0.0002	0.150^{***}	0.150^{***}
	(0.0003)	(0.006)	(0.006)
Constant	−0.0002	0.495^{***}	0.495^{***}
	(0.010)	(0.018)	(0.016)
Observations	516,771	516,771	516,771
R^2	0.0003	0.017	0.019
F Stat	1.271	82.056	91.608
Partial F Stat			1132

Notes: *p<0.1; **p<0.05; ***p<0.01. The table above demonstrates the exogeneity of judge assignment in the Stringency column, which reports regression estimates of the judge leave-out mean on characteristics of the case, the tenant, and the tenant's neighborhood. Under the Eviction heading we report the first stage of the regression with and without the judge stringency measure. Under the Voluntary Dismissal heading we report the first stage voluntary dismissal among cases where there was not an eviction ruling. All columns include court-year fixed effects. The table reports robust standard errors. The "Partial F-Stat" reported in the last row is for the coefficient on Stringency.

F.6 IV Results: Robustness

Table 29: 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	533.04	5.319***	4.158***	3.920***	41.966
	(71.58)	(0.808)	(0.611)	(0.613)	(28.439)
Total bal. collections	3,578.06	-328.279***	-299.010***	-280.045***	-606.556
	(4,650.89)	(38.040)	(36.167)	(36.339)	(1565.682)
Any auto loan or lease	0.16	0.009***	0.010***	0.010***	0.248**
	(0.36)	(0.003)	(0.003)	(0.003)	(0.126)
Payday Inquiries	1.36	-0.197	-0.171	-0.167	-10.261
	(11.38)	(0.122)	(0.123)	(0.118)	(12.581)
Payday Accounts Opened $\times$ 100	3.27	0.069	0.106	0.164	-25.175
	(46.76)	(0.457)	(0.460)	(0.442)	(20.397)
II. *Financial Strain: 37-60 mon.*					
Credit Score	541.99	2.103**	1.778**	1.574**	39.351*
	(74.01)	(0.962)	(0.742)	(0.733)	(23.779)
Total bal. collections	3,388.18	-240.570***	-231.885***	-221.914***	737.537
	(4,592.04)	(54.978)	(53.512)	(53.254)	(1532.471)
Any auto loan or lease	0.16	0.006	0.008**	0.008**	0.223*
	(0.36)	(0.004)	(0.004)	(0.004)	(0.134)
Payday Inquiries	1.34	-0.187	-0.174	-0.162	-3.958
	(11.03)	(0.125)	(0.125)	(0.119)	(5.455)
Payday Accounts Opened $\times$ 100	3.86	-0.890*	-0.834*	-0.766*	-31.568
	(53.57)	(0.488)	(0.493)	(0.455)	(24.778)
Additional controls			Yes	Yes	Yes
Complier re-weighted				Yes	

The table 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 rulings in other cases, as described in the test. All specifications control for district-year fixed effects. Robust standard errors are clustered at the judge-year level.

F.7 Characterizing compliers

We next explore characteristics of compliers, following Abadie (2003), Dahl et al. (2014), and Dobbie et al. (2017) 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

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

Table 30: 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 at 3 Years*					
Cumulative Zipcode moves	1.03	-0.045***	-0.046***	-0.046***	0.287
	(0.77)	(0.015)	(0.016)	(0.017)	(0.403)
Poverty rate (×100)	17.75	0.625***	0.575***	0.605***	1.653
	(10.12)	(0.178)	(0.166)	(0.176)	(4.759)
Any Eviction Case (36 mo.)	0.23	0.072***	0.070***	0.067***	0.549**
	(0.42)	(0.015)	(0.015)	(0.015)	(0.266)
Eviction Case at Dif. Address (36 mo.)	0.17	0.025***	0.024***	0.023**	0.014
	(0.38)	(0.009)	(0.009)	(0.010)	(0.154)
II. *Outcomes at 5 Years*					
Cumulative Zipcode moves	1.49	-0.055***	-0.055**	-0.055**	0.317
	(0.99)	(0.021)	(0.022)	(0.023)	(0.478)
Poverty rate (×100)	17.54	0.608***	0.578***	0.547***	5.576
	(10.18)	(0.184)	(0.174)	(0.190)	(4.946)
Any Eviction Case (60 mo.)	0.31	0.076***	0.073***	0.073***	0.434
	(0.46)	(0.015)	(0.014)	(0.015)	(0.270)
Eviction Case at Dif. Address (60 mo.)	0.25	0.044***	0.042***	0.042***	0.234
	(0.43)	(0.012)	(0.011)	(0.012)	(0.275)
Additional controls			Yes	Yes	Yes
Complier re-weighted				Yes	

The table 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 analysis sample has $N = 70,816$. 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 test. All specifications control for district-year fixed effects. Robust standard errors are clustered at the judge-year level.

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

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

In Table 33, we calculate 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 our measure of 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 around 64 percent always-takers, 9 percent compliers, and 27 percent never-takers, and that the relative sizes of that these groups are not very sensitive to the choices of specification and stringency threshold.

Next we are interested in the characteristics of compliers, including both individual attributes, but also case characteristics, and neighborhood characteristics. For characteristic x_k, we use the relationship:

Table 31: Testing the exclusion assumption: financial strain

A: First stage

	(1)	(2)
Stringency: eviction order	0.654*** (0.036)	0.656*** (0.035)
Court-by-time F.E.	Yes	Yes
Baseline controls	No	Yes
Number of observations	232,971	232,971
R^2	0.007	0.012
Mean of dep. var.	0.64	

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	-7.980	-5.936	115.533	19.242	-0.047**	-0.045**
	(4.901)	(4.757)	(299.196)	(322.479)	(0.022)	(0.022)
Stringency:judgment amount		-0.544		0.725		0.001
		(0.626)		(40.573)		(0.003)
Panel II: Evicted						
Eviction order	-12.506*	-9.223	180.809	29.864	-0.074**	-0.070**
	(7.569)	(7.406)	(468.218)	(500.186)	(0.035)	(0.034)
Stringency:judgment amount		-0.488		0.538		0.001
		(0.624)		(41.854)		(0.003)
Panel III: Evicted and Judgment Amt.						
Eviction order	-6.772		27.059		-0.078**	
	(8.303)		(626.745)		(0.038)	
Judgment amount (1000s)	-1.115		1.242		0.003	
	(1.349)		(96.602)		(0.006)	

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.

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

We estimate the numerator by first estimating the first stage regression of eviction on judge leniency with district×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 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 32: Testing the exclusion assumption: moves and neighborhood quality

A: First stage

	First stage	
	(1)	(2)
Stringency: eviction order	0.638*** (0.065)	0.647*** (0.071)
Stringency: judgment amount		0.009 (0.007)
Additional controls	Yes	Yes
Number of observations	70,099	

B: OLS and IV with additional stringency

	Cumulative Moves		Neighborhood Poverty Rate		Eviction Case at 3 Years	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel I: Reduced Form						
Stringency: eviction order	-0.072	-0.071	-0.421	-0.892	-0.138***	-0.142***
	(0.104)	(0.106)	(1.233)	(1.360)	(0.043)	(0.048)
Stringency:judgment amount		-0.002		0.041		-0.004
		(0.009)		(0.108)		(0.003)
Panel II: Evicted						
Eviction order	-0.113	-0.110	-0.657	-1.374	-0.216***	-0.219***
	(0.163)	(0.166)	(1.921)	(2.157)	(0.069)	(0.072)
Stringency:judgment amount		-0.001		0.053		-0.002
		(0.010)		(0.126)		(0.003)
Panel III: Evicted and Judgment Amt.						
Eviction order	-0.106		-1.561		-0.212***	
	(0.177)		(2.542)		(0.077)	
Judgment amount (1000s)	-0.002		0.109		-0.004	
	(0.020)		(0.263)		(0.007)	

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 34 presents the relative likelihood of characteristics. Note that at present, the table does not reflect significant differences between the complier population and the overall eviction court population, but this may change in future drafts.

Table 33: Sample shares by compliance type.

	Linear model			Polynomial		
	1%	1.5%	2%	1%	1.5%	2%
Always Takers	0.69	0.69	0.69	0.68	0.69	0.69
Compliers	0.10	0.08	0.08	0.08	0.07	0.07
Never Takers	0.22	0.22	0.23	0.22	0.23	0.23

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 34: Complier characteristics ratios.

	Pr(X=x)	Pr(X=x|complier)	$\frac{Pr(X=x\|complier)}{Pr(X=x)}$
Joint Action	0.827	0.803	0.971
	(0.008)	(0.059)	(0.070)
Defendent w/o Attorney	0.963	0.964	1.002
	(0.002)	(0.021)	(0.021)
Female	0.618	0.612	0.990
	(0.005)	(0.040)	(0.065)
Black	0.440	0.468	1.069
	(0.020)	(0.132)	(0.409)
Age Over 55	0.068	0.063	0.918
	(0.004)	(0.025)	(0.337)

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