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NONPAYMENT AND EVICTION IN THE RENTAL HOUSING MARKET

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ABSTRACT

Recent research has documented the prevalence and consequences of evictions, but our understanding of underlying drivers of the eviction rate and the scope for policy to affect it remains limited. In this paper, we study landlords' decisions to evict tenants and how these decisions may be influenced by policy. We combine novel lease-level ledger data from low-income rental markets with a model of the landlord's eviction decision to characterize the persistence of shocks to tenant default risk, landlords' information about these shocks, and landlords' cost of eviction. Our data show that nonpayment is common, is frequently tolerated by landlords, and is often followed by recovery, suggesting that landlords face a trade-off between initiating a costly eviction or waiting to learn whether a tenant can continue paying. Our dynamic discrete choice model of the eviction decision captures this tradeoff. Estimates indicate that filing an eviction costs landlords the equivalent of 2-3 months of rent, and that the majority of evictions involve tenants who are unlikely to pay going forward. This implies that uniformly applied policies can generate additional forbearance for tenants, but they do not prevent most evictions. We find that 15% of those evicted would have resumed paying rent, suggesting a role for more targeted interventions. Among the policy instruments we consider, direct financial incentives for landlords—such as taxes and subsidies—are more likely to durably prevent evictions than procedural delays.

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1. Introduction

Eviction is common in the United States: five to six percent of renter households have a case filed against them in a typical year, usually for nonpayment, with about half of these court filings resulting in an eviction order (Gromis et al., 2022). Eviction is also consequential, generating both adverse consequences for tenants and negative externalities for their communities (e.g., Desmond, 2016; Collinson et al., 2024b, 2025). While recent years have seen an increase in policy proposals aimed at reducing evictions, there is limited empirical evidence on the underlying drivers of eviction to guide the design of these interventions. In particular, we know little about how landlords decide whom to evict, or when. If landlords primarily evict tenants with a structural inability to pay, such evictions can be delayed, but they are unlikely to be prevented by procedural delays or temporary financial support. On the other hand, these interventions can help preserve stable housing situations if tenants facing eviction are able to resume payment in the future.

This paper uses novel data to study landlords' decisions to evict tenants, and combines the data with a model to understand how these decisions may be influenced by policy changes. We make three contributions. First, we collect unique data from landlord ledgers to study the relationship between tenant nonpayment and landlord eviction decisions in a sample of tenancies in low-income rental markets. We find that rent nonpayment rates can be high, with half of tenants in our sample falling 30 days behind at some point, but that nonpayment comes in different forms: some tenants are persistently delinquent, while others are intermittently delinquent and recover. Landlords typically evict only after observing more persistent nonpayment. Second, we develop and estimate a dynamic model of tenant nonpayment and the landlord's eviction decision to characterize the mechanisms driving evictions. Our estimates reveal a fundamental trade-off: landlords must weigh substantial eviction costs—equivalent to 2-3 months of rent—against the uncertain prospect that a delinquent tenant will resume payment. This uncertainty resolves gradually as landlords observe payment patterns and update their beliefs about tenant risk. Third, we use the model to study commonly proposed eviction-prevention policies in counterfactual simulations. While uniformly applied policies can generate additional forbearance for tenants, they do not prevent most evictions. At the same time, we find that 15% of those evicted would have resumed paying rent, suggesting a role for more targeted interventions. Among the policy instruments we consider, direct financial incentives for landlords—such as taxes and subsidies—are more likely to durably prevent evictions than procedural delays.

Researchers' efforts to understand the drivers of evictions have been constrained by a lack of data on nonpayment in the rental housing market. We fill this data gap by assembling lease-level ledger data from landlords that operate in low-income neighborhoods across several U.S. cities. These data contain a complete record of landlord-tenant transactions by month, the duration of occupancy and subsequent vacancies, and the timing of eviction decisions. To our knowledge, this study is the first to use data linking detailed payment histories to landlords' eviction behavior.[1] Our data has three key advantages. First, it allows us to quantify the prevalence of nonpayment even when nonpayment does not culminate in an eviction filing. Second, observing the timing of eviction filings enables us to use a revealed preference approach to empirically study landlords' decisions about when and whether to evict. Third, the data cover a low-income segment of the rental market where eviction rates are high. This segment of the rental market is particularly difficult to study as standard data sources like credit bureaus or property management platforms often have limited coverage (Witzen, 2025; Calder-Wang and Kim, 2023).

Our analysis proceeds in three steps. We begin by documenting descriptive facts that create a window into the relationship between nonpayment and eviction in low-income rental markets. Our data are characterized by high rates of nonpayment, but also considerable forbearance by landlords. 50% of tenants default on rent at some point and, for tenants who do not receive rent subsidies, total rent payments are only 86% of rent due—a loss rate that exceeds what is typically seen for U.S. credit cards, mortgages, and high-risk bonds (e.g., Federal Reserve, 2024). However, not all of these defaults lead to eviction; landlords frequently tolerate nonpayment, typically forbearing two or three months of default before eviction filing. Descriptive patterns suggest one reason for waiting to file an eviction is that landlords value waiting to learn which tenants are likely to recover. Among tenants who fall one month behind, 39% fully repay their balance at some future date.

In the second step of our analysis, we develop and estimate a dynamic discrete choice model of the landlord's eviction decision (Rust, 1987; Aguirregabiria and Mira, 2010, following). Because landlords frequently tolerate nonpayment and many delinquent tenants eventually catch up, we model the landlord's eviction decision as a choice between bearing the costs of removing a delinquent tenant immediately and waiting to learn if the tenant will recover. In the model, the landlord observes the history of a tenant's rent payments each month and updates their belief about the tenant's likelihood of paying rent next month, where tenant payment probabilities are modeled as latent types that evolve according to a Markov process. Filing an eviction is an irreversible decision that involves two costs. First, landlords pay a fixed cost of filing that includes expected legal costs, expected damage to the

[1] Survey data such as the New York City Housing and Vacancy Survey, the Survey of Income and Program Participation, or the Milwaukee Area Renters Survey (e.g., Desmond and Shollenberger, 2015; Pattison, 2024) contain detailed information on renters, but include relatively few evictions and do not have data on payment histories at a higher frequency than yearly. Moreover, these data are self-reported, making them susceptible to misreporting, especially when respondents are asked to recall information over such a long period of time (see e.g., Meyer et al., 2015, for a discussion of misreporting in household surveys). Studies that have used administrative rent payment and delinquency data do not have information on eviction behavior (Ambrose and Diop, 2021; Agarwal et al., 2022; Bèzy et al., 2024).

unit caused by tenants facing eviction, and any hassle, time, or psychic costs from the eviction process. Second, it takes time for an evicted tenant to leave the unit and additional time to find a new tenant. The key model parameters are landlords' filing costs, the Markov process governing tenant payments, and the rates at which evicted and non-evicted tenants move out and vacant units are filled. These parameters determine landlords' willingness to tolerate nonpayment and the responsiveness of their eviction decisions to tenant protection policies.

We use the ledger data to estimate the model parameters by maximum likelihood. The tenant type process is identified from the payment data, accounting for censoring due to evictions. Eviction filing costs are identified from how the probability of eviction varies with the difference in expected rental income from evicting rather than keeping the current tenant. Our baseline model assumes that landlords learn about tenant types over time only through the tenant's rent payment history, which we observe. This is a plausible lower bound on their actual information, and is a conservative assumption vis-à-vis our main conclusions. We also estimate alternative models in which the landlord perfectly observes the tenant's current payment probability.

Our model estimates reveal that landlords' direct costs of filing an eviction equal 2 to 3 months of rent for an average apartment in our data. Landlords additionally pay an indirect cost of, on average, 2 months of rent due to vacancy after an eviction. Correspondingly, landlords often wait to evict until they believe tenants' odds of paying rent in the future are low. Landlords evict tenants for whom their median posterior belief places roughly 75% probability on the tenant being a "low" type who, in expectation, pays rent less than one month out of the next twelve. For non-evicted tenants in default, the corresponding median posterior belief is about one-third. Thus, while many default spells are temporary, a significant majority of *evicted* tenants would pay very little going forward.

In the third step of our analysis, we use the estimated model to provide ex-ante evaluations of commonly-proposed policy instruments intended to reduce evictions. Interest in eviction-related policy has grown rapidly in recent years, with hundreds of eviction-related bills introduced each year since 2018 at the U.S. state and federal levels.[2] These include a variety of interventions, from short-term rental assistance to legal aid for evicted tenants, which influence landlords' incentives to evict in economically distinct ways. Yet there is limited empirical evidence on how alternative policy instruments compare in the same setting. We consider three distinct policy levers: (1) taxes on or fees for eviction filings ("Tax"); (2) procedural interventions that create delays in the eviction process ("Delay"); and (3) rent subsidies for tenants in arrears, or short-term rental assistance ("SRA"). To facilitate comparison across policies, we calibrate each policy's parameters to achieve the same decrease in eviction rates as

[2] See Appendix Figure 1. The number of eviction-related federal and state bills per year has exceeded 400 every year since 2020.

a \$250 eviction tax, and compare other outcomes. A tax of this magnitude reflects a significant but not unrealistic policy change; it would be equivalent to nearly doubling the filing fee in Cook County during our sample period, a roughly three-standard-deviation increase that would make Cook County an outlier among the U.S. housing courts covered by Gomory et al. (2023).

Eviction rates fall by 5% under these three counterfactuals. Some tenants avoid eviction entirely, but the majority of tenants whose outcomes change are still evicted several months later. Of the tenants whose evictions are delayed or prevented, a minority would have resumed paying consistently—between 12 and 22% could have paid at least 10 of the next 12 months' rent had they stayed. Despite having modest overall impacts on eviction rates, we find that the three policy instruments differ substantially in the types of evictions that are avoided, and the associated costs to landlords and the government. A higher eviction tax disproportionately prevents evictions for tenants who are most likely to recover, since these tenants are more likely to be marginal to a uniform increase in the cost of filing. In contrast, delays in the eviction process increase the effective cost of filing the most for the tenants with the lowest ability to continue paying rent, so these tenants disproportionately avoid eviction. An eviction tax is the least costly of the three policy instruments we study, both in terms of fiscal cost and in terms of the total cost summed across the government and landlords. Delay is the most costly policy to landlords per eviction avoided.

The same policies could have substantially different effects if we had found different model parameters. We use the model to show that if we had estimated that tenant types were less persistent and landlords' eviction costs were lower, the same policies would lead to larger reductions in evictions as well as more long-term recovery. Furthermore, the differences across policy instruments in the types of evictions averted would be much smaller, because landlords would be similarly pessimistic about all evicted tenants' odds of paying in the future. In this sense, two key features of our empirical estimates drive our findings: first, the high persistence in tenants' probability of payment; second, the high eviction costs for landlords. These findings also imply that the policies we consider could have different impacts in other settings with different regulatory and market conditions.

We also consider how other equilibrium responses not accounted for in our baseline model might impact our findings. We show in additional simulations that our main findings are qualitatively robust to both rental price changes if landlords pass the costs of regulation on to tenants, and to changes in payment rates due to strategic default by tenants.[3] Using a subset of our data, we also show that landlords may have limited scope to increase their screening criteria in response to stronger protections.

[3]Evidence from Collinson et al. (2024a) suggests landlords pass on to tenants between \$20 and \$40 per month of the costs induced by a legal aid program designed to prevent or delay evictions. We are not aware of any direct evidence that tenants' rent payments are responsive to tenant protections, though Ambrose and Diop (2021) find in a cross-sectional regression that more heavily regulated rental markets have lower rates of tenant nonpayment.

This evidence suggests that our main insights into landlord eviction behavior would not be sensitive to these responses, though they could have important welfare implications for landlords and tenants.

Our analysis elucidates the mechanisms driving eviction rates and takes a step toward understanding the impacts of eviction-protection policy proposals. Specifically, our results provide three main insights about the role of policy in preventing evictions. First, due to the costs of the eviction process for landlords and the persistence of nonpayment among tenants, commonly proposed policies can generate additional forbearance for tenants, but they do not prevent most evictions. Second, we estimate that 15% of evicted tenants would have paid 10 out of the next 12 months' rent if not evicted, suggesting that well-targeted policies could prevent evictions at a lower cost than the broad-based policies we consider here. Third, the choice of policy instrument matters for both the types of evictions prevented, and the costs to landlords and the government. Because landlords are relatively well-informed about tenant nonpayment risk at the time of eviction, policies that provide direct financial incentives for landlords—such as taxes and subsidies—are more likely to durably prevent evictions than procedural delays. At the same time, direct financial incentives for landlords are less likely to affect the timing or likelihood of eviction for tenants with the lowest likelihood of paying, who may have the highest marginal utilities of consumption.[4]

Related Literature. Relatively few papers have studied landlord eviction decisions or how they respond to eviction protections implemented at scale.[5] Recent papers that have addressed these questions using structural models have done so without detailed rent payment data. Corbae et al. (2024) embed landlord eviction decisions in a model with housing search frictions and neighborhood spillovers to study eviction policy and alternative rental contracts. Similar macroeconomic modeling and calibration strategies are employed in Abramson (2024) and Abramson and van Nieuwerburgh (2024), which focus on tenant strategic default, homelessness, and rental housing supply while treating landlord eviction decisions as exogenous given tenant default. We complement these papers by providing direct empirical evidence on landlords' financial incentives to evict using detailed, high-frequency microdata on nonpayment.[6] Our structural model highlights how landlords' eviction decisions depend on mechanisms

[4] A full welfare analysis would incorporate these tradeoffs, as well as any externalities and fiscal costs generated by evictions, and tenants' ex-ante willingness-to-pay for additional protections. We see this as an important direction for future work.

[5] This is true despite a growing body of work on the correlates and consequences of evictions in the United States. Following early evidence from Desmond (2012), researchers have documented the overall prevalence and types of renters most at risk of eviction (Gromis et al., 2022; Graetz et al., 2023). Collinson et al. (2024b) provide quasi-experimental evidence on the harms to tenants from eviction using the random assignment of judges. Combined with qualitative and ethnographic work in Desmond (2016), this work has helped inform a broader national discussion on eviction and growing interest in policies to protect low-income tenants.

[6] While we focus on the financial drivers of eviction, other work highlights alternative explanations such as distance to the courthouse (Hoffman and Strezhnev, 2023), misperceptions and social preferences (Rafkin and Soltas, 2024), and

not previously studied, including the dynamics of learning over time about tenant nonpayment risk.

Our structural approach also complements quasi-experimental studies of existing eviction protections. The staggered introduction of New York City's Universal Access to Counsel program has been used to study legal aid's effects on eviction filings, tenant court outcomes, rent prices, and landlord screening (Ellen et al., 2021; Cassidy and Currie, 2023; Collinson et al., 2024a). Other quasi-experimental evaluations of large-scale policies study settings with very strong tenant protections, including France (Bèzy et al., 2024) and the U.S. during the COVID-19 pandemic's eviction moratoria (Arefeva et al., 2024). Other studies have used cross-sectional policy variation (Gomory et al., 2023; Ambrose and Diop, 2021; Merritt and Farnworth, 2021; Coulson et al., 2024) or between-tenant variation in benefit receipt, such as research on short-term rental assistance by Rafkin and Soltas (2024) and Dutz et al. (2024). Our descriptive and structural analyses provide insight into the mechanisms underlying the high eviction rates and policy responses documented in this reduced-form literature. The structural model also allows us to conduct out-of-sample policy counterfactuals and to compare multiple policy instruments in the same setting.

Eviction protections are one of many ways in which government policy shapes the supply side of the low-income segment of the rental housing market. Other research has studied the impacts of rent control (Glaeser and Luttmer, 2003; Diamond et al., 2019; Favilukis et al., 2023),[7] lead paint and habitability regulations (Vigdor and Williams, 2022), and banning short-term rentals (Calder-Wang, 2022); the impacts of affordable housing policy through developer subsidies (Sinai and Waldfogel, 2005; Baum-Snow and Marion, 2009; Diamond and McQuade, 2019; Soltas, 2024), the Section 8 voucher program (Collinson and Ganong, 2018; Phillips et al., 2022; Song and Blanco, 2024), and public housing (Blanco, 2023); and the effects of land use regulation on rental housing supply (e.g. Glaeser et al., 2005; Kulka et al., 2023; Song, 2022). We extend this literature by showing how landlords' endogenous willingness to forbear unpaid rent is an important supply-side behavior, and by developing an empirical framework to understand how it will respond to rental market policy.

Methodologically, our analysis uses empirical techniques from the dynamic discrete choice literature (Rust, 1987; Hotz and Miller, 1993; Arcidiacono and Ellickson, 2011). Other research on housing markets has used dynamic discrete choice models to study extensive-margin supply (Murphy, 2018) and demand for housing and neighborhoods (Bayer et al., 2016; Almagro and Dominguez-lino, 2024). Our work also connects to econometric models of learning, which have been used to explain dynamic behavior in markets such as advertising (Erdem and Keane, 1996; Ackerberg, 2003), employee com-

discrimination by landlords (Lodermeier, 2024).

[7] Interestingly, several papers have highlighted evictions as a margin of adjustment to rent control (Asquith, 2019; Gardner, 2022; Geddes and Holz, 2022).

pensation (Lange, 2007; Kahn and Lange, 2014), credit scoring (Chatterjee et al., 2023; Blattner et al., 2022), pharmaceutical demand (Crawford and Shum, 2005; Dickstein, 2021), and college major choice (Larroucau and Rios, 2022), among others. We argue that landlords' incomplete information and ability to learn about a tenant's evolving ability to pay rent are key to understanding both the drivers of eviction and the responsiveness of eviction to policy.[8]

2. Data and Background

2.1 Landlord Ledger Data

Our analysis is based on a unique dataset of privately owned rental properties located in low-income neighborhoods in the Midwestern United States.[9] The data cover the payment histories of almost 6,000 tenants between 2015 and 2019 and offer a uniquely detailed view into low-income rental markets: we observe the timing and amount of nonpayment for each tenant, security deposits and late fees, the duration of occupancy and subsequent vacancies, and the filing dates of eviction cases. These data are shared with us by property management firms that either directly own the properties they manage or, based on our conversations with the firms, have broad discretion over how units are managed, including the eviction decision. We refer to these firms as "landlords" throughout the paper.

Given that the landlords in our sample are property management firms, our findings apply primarily to professionally managed units in the low-rent market segment. A limitation of our data is that it does not include units owned by "mom and pop" landlords who self-manage their properties rather than outsourcing management to a third party. However, a back-of-the-envelope estimate based on data from the Residential Household Finance Survey suggest that most units in the rental market are professionally managed: we estimate the share of professionally managed units to be approximately 60 percent in the overall market, and about 73 percent in the low-rent segment (US Census Bureau, 2018).[10]

[8]By studying an endogenous decision to end a repeated relationship, our setting also shares features with analyses of labor market separations (Lazear, 1986, 1990; Hopenhayn and Rogerson, 1993), mortgage foreclosure (Foote et al., 2010; Adelino et al., 2013; Kytömaa, 2023; Aiello, 2022; Agarwal et al., 2011; Ganong and Noel, 2020; Cherry et al., 2021; Kim et al., 2022), bankruptcy filings (Dou et al., 2021; Antill, 2022), and terminations of firms' supplier relationships (Harris and Nguyen, 2024), among other contexts. We build on this literature's insights that policy constraints on ending a repeated relationship affect the entire course of such a relationship (e.g., as in Hopenhayn and Rogerson (1993)'s analysis of labor firing restrictions), and that the potential for recovery after an adverse shock can moderate the effects of such policy (e.g., as in Foote et al. (2010)'s analysis of mortgage foreclosure). Extending this literature, we believe we are the first to estimate structurally how the efficacy of relationship-termination policy is determined by market participants' learning about a dynamically evolving match quality.

[9]The majority (56%) of tenancies in our analysis sample are for properties in Chicago, IL. The two cities with the next largest share of tenancies are Detroit, MI (7%) and Milwaukee, WI (7%). The remaining tenancies are in 172 other cities, each representing less than 4% of the sample.

[10]The Residential Household Finance Survey (RHFS) is a nationally representative survey that provides data at the property level on property size and whether or not a third-party manager manages the property. To address the fact that

From the raw ledger data, we construct two samples for our analysis.[11] The first, which we call our "analysis sample," is created using only basic data cleaning steps such as deduplicating, removing nonresidential properties, and removing leases for which the tenant never moved in. We also restrict to tenancies that began during the 2015-2019 sample period. We use this sample for the descriptive evidence presented in Section 3.

The second sample, which we refer to as our "model estimation sample," is used for the structural analysis in Section 5. For this sample, we restrict to tenants without a Section 8 housing voucher who pay monthly rent between $600 and $1,000 in Cook County, IL cities where our sample has greatest coverage; this includes leases for properties in Chicago and some of its closest suburbs (Des Plaines, Northlake, Oak Lawn, and Maywood). Because Cook County shares a common court system, this restriction ensures that institutional details such as court filing fees, local ordinances, and court administrative procedures are held constant, and that the units are relatively homogeneous in terms of monthly rent.

Both samples consist of tenants in neighborhoods that have low rent prices, high poverty rates, and a high share of non-White households. Appendix Table 1 provides ZIP-level characteristics for the tenancies in our analysis (column 1) and model estimation (column 2) samples and compares them to averages for Cook County (column 3), where the majority of the units in our analysis sample and all units in our model estimation sample are located. Median monthly rents in the analysis and model estimation samples are quite similar—$954 and $952, respectively—and 19% below the Cook Country average. Median household monthly incomes in the two samples are $2,800 and $2,544, respectively; the latter is 41% below the Cook County average. Unemployment and poverty rates are substantially higher than the Cook County benchmark, and so is the share of non-White households. For the model estimation sample, we can additionally compute eviction filing rates based on Cook County eviction court records (as in Collinson et al., 2024b). As expected given the higher poverty rates, we find a ZIP-level eviction filing rate of 5.6% for our sample, which is 66% higher than the county-wide rate. Lastly, the neighborhoods in which these properties are located are experiencing lower rent growth than the county as a whole: in the model analysis sample's ZIP codes, annualized rent growth is 1.61%, which is about two-thirds of the Cook County average.[12]

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the RHFS only records the presence of external (third-party) managers, we estimate the share of professionally-managed units, we assume that properties with 8 or more units are always professionally managed, while for smaller properties we assume that only the 21 percent of units that have an external property manager (according to the 2018 RHFS) are professionally managed (US Census Bureau, 2018). We obtain a subsample of low-rent properties from the RHFS by filtering for properties where the maximum rent across units is below $1,000, and recalculate our estimate.

[11] See Appendix F for details on sample construction and definitions of key variables.

[12] In addition to Appendix Table 1's comparison of the analysis sample and model estimation sample using ZIP characteristics, Appendix Tables 2 and 3 and Appendix Figures 4 and 5 repeat analyses from Section 3 on the model estimation sample rather than the analysis sample. Descriptive patterns across the two samples are quite similar.

Cook County, IL is a useful setting for studying the low-income rental market because it is broadly similar to rental markets in many large urban areas in the United States. Appendix Figure 2 compares eviction filing rates, rental vacancy rates, home ownership rates, and median rent prices across such areas. It shows, for example, that Cook County's eviction filing rate (adjusted for serial filing) in 2015 was 3.79%, which is close to the median across large counties.[13] Similarly, median rent prices, rental vacancy rates, and home ownership rates in Cook County lie close to the median for large counties. These large counties represent over 90% of all eviction filings nationwide (calculated using counties for which 2015 data was available). Although there is considerable variation in eviction filing rates and rent prices across locations, counties similar to Cook County represent a large share of national filings: 55% of evictions are from counties within one standard deviation of median rent from Cook County and 51% are within one standard deviation of Cook County's filing rate. When focusing on the larger counties included in Appendix Figure 2, these numbers are 64% and 55% (The Eviction Lab, 2019; Manson et al., 2023a).

2.2 The Eviction Process and the Landlord's Eviction Decision

The legal process for eviction varies across jurisdictions but typically includes the following steps. First, the landlord must provide the tenant with a written notice that indicates the intention to file an eviction court case and the reason for doing so. The landlord can file an eviction court case against the tenant after a fixed time period that is determined by the reason for eviction. For example, the notice period is typically shorter if the eviction is filed for nonpayment, and longer if it is filed for other lease violations. An eviction filing typically involves the landlord paying a filing fee to the court. Once a case is filed, the tenant must be served a court summons that informs the tenant of the date of the initial hearing. A case may have a single court hearing or multiple hearings depending on the complexity of the case and the actions taken by the plaintiff and defendants. If a judge grants an order for eviction, the tenant has lost the right to remain in their unit. Depending on the case, there may also be additional rulings, such as a money judgment for past rent or damage to the property. Although tenants may leave after the eviction order, to enforce the order, the landlord must file paperwork with the Sheriff's or Marshall's office, who will then execute the eviction.

In this paper, we focus on the landlord's decision to *file* a case in eviction court. The filing decision likely reflects a strong intent to evict the tenant. To file an eviction, the landlord must pay a filing fee of several hundred dollars.[14] Earlier actions such as giving notice are non-binding and, based on our

[13]We report the filing rate for 2015 as this is the latest year for which county-level estimates from Princeton's Eviction Lab were available for Cook County, IL, and because it corresponds to the beginning of our sample.

[14]Filing fees vary over time and across cities but can be substantial. For example, in 2019, filing an eviction case cost $287, and filing a joint action case (which also seeks a money judgment) cost $379-$388 in Cook County, IL. A landlord

conversations with landlords, frequently do not lead to a court case.[15] The decision to file a case is also the point at which the landlord has the most agency. Once a case is filed, subsequent events, such as the duration of the case, whether it ends in an eviction order, and whether the order is enforced all depend in part on the court proceedings and on whether the tenant chooses to move out before an eviction is enforced. Hence, the filing decisions we observe in our data likely reflect contemplated, costly actions by the landlords. As such, they are informative of the landlords' perceived relative payoff from eviction.

3. Descriptive Evidence

We use our ledger data to document several pieces of new evidence about nonpayment and eviction in the low-income rental housing market. Because detailed data on rent nonpayment are scarce, little is known about the prevalence of nonpayment in the low-income rental market and how nonpayment covaries with eviction. We present five descriptive facts that are of interest in their own right and that we use to inform key features of the structural model.

Rent nonpayment rates can be high for landlords operating in low-income rental markets. Table 1 provides summary statistics for the tenants in our sample. Column (1) describes the full analysis sample. On average, 9.1% of rent due goes unpaid. The average rent owed at the time of move-out is \$1,252, or more than 1.25 months' rent out of an average tenure of 15.3 months. The rate of nonpayment is even larger for the majority of tenants who do not have a Section 8 housing voucher, described in column (2). These unsubsidized tenants miss 13.8% of rent due and owe nearly two months' rent (\$1,579) on average at move-out. Thus, landlords' losses from nonpayment are substantial—as large as losses from the time the unit is vacant after a tenant moves out, which lasts two months on average.

Underlying these averages is substantial heterogeneity in nonpayment across tenants. Figure 1 panel (a) plots the distribution of tenant balances at moveout. Half of tenants are current when they move out, but there is a long tail of tenants owing substantial sums of unpaid rent. Over 30% of tenants owe two or more months, and 14% owe at least five months. In principle, this variability in arrears could be explained by how long tenants spent in the unit before moving out. But the variability remains large holding fixed the length of time over which losses are measured. Appendix Figure 3 shows the distribution of the share of rent that goes unpaid over a 12-month period due to either vacancy or nonpayment, across all unit-years—i.e., all units and all possible 12-month windows—in our analysis

will also typically incur the cost of hiring a lawyer to handle the case.

[15] For one landlord, we observe both when notice is given and when a case is filed. Twice as many tenants are given notice as eventually have a case filed against them.

sample. While a large minority of unit-years have full payment for all 12 months, and nearly 60% of 12-month rent receipts are above 90%, the average unit-year has over 2 months of rent unpaid, and the 90th percentile unit-year has over 6 months of rent unpaid.[16]

Table 1 – Descriptive Statistics on Tenancies and Nonpayment

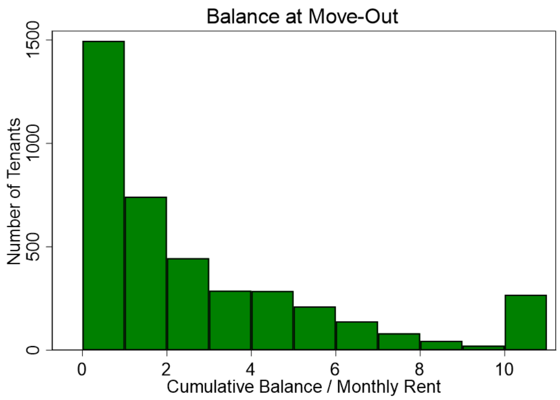
Statistic	All (1)	No Voucher (2)	Voucher (3)	Not Evicted (4)	Evicted (5)
Monthly Rent ($)	920	834	1,052	935	871
Share of Rent Collected (%)	90.9	86.2	96.7	95.6	73.2
Balance at Moveout ($)	1,252	1,579	611	630	3,229
Evicted (%)	23.9	30.1	11.9	0.0	100.0
Months Tenure	15.3	14.0	17.8	15.7	13.9
Months Vacant after Moveout	2.0	2.0	2.1	1.8	2.4
Tenancies	5,809	3,847	1,962	4,418	1,391
Units	3,937	2,705	1,638	3,305	1,204

Notes: Statistics based on lease-months in the 2015-2019 analysis sample. A lease refers to a specific tenant in a specific unit. Tenure is measured as the number of months from move-in date to month the tenant moves out or December 2019, whichever is earlier. 1,779 tenants were still active at the end of December 2019. Evicted refers to an eviction court filing or landlord's notice to attorney to file eviction, regardless of whether the tenant moved out after. Voucher holders include tenants with at least one rental assistance charge or payment. For tenants with vouchers, (i) subsidy payments are treated as deterministic: for each observed subsidy charge, it is assumed that an equivalent rental assistance payment has been deposited; and (ii) rent and share of rent collected include the subsidy as well as the tenant's portion of the rent. Share of rent paid is rent-weighted and is computed using equation (13). Vacancy duration is the number of months a unit is unoccupied between two tenancy spells. Vacancy periods lasting 12 or more months are excluded. Buildings that exit the sample before the end of the sample period are excluded when calculating the tenure and vacancy statistics.

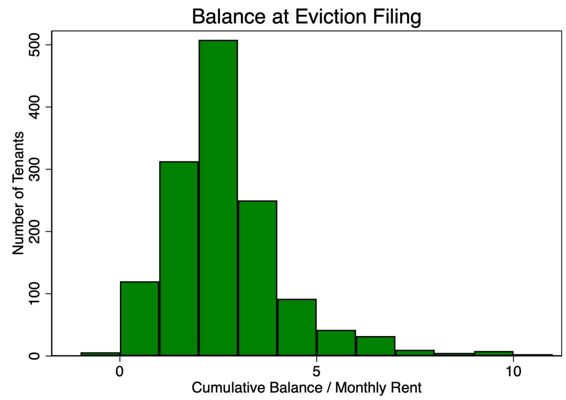
Eviction almost always follows nonpayment, but nonpayment does not always lead to an eviction. 23.9% of the tenants in the full analysis sample, and 30.1% of unsubsidized tenants, are evicted (columns (1) and (2) of Table 1, respectively). Most evictions are filed against tenants who are in arrears. Panel (b) of Figure 1 plots the distribution of tenant balances during the month an eviction is filed. 90% of evicted tenants owe at least one month of rent. This is consistent with evidence from eviction court data that the large majority of cases are filed for nonpayment (Collinson et al., 2024b; Desmond et al., 2013). Further, based on the timing of default in the ledger data, nonpayment does

[16] Much of this variability remains when comparing different tenants in the *same unit* over time, suggesting landlords face considerable uncertainty in the returns to operating a rental unit. Landlords' uncertainty about a new tenant's future payment behavior is a key feature of our model. We abstract away from landlord risk aversion in the structural model given that we study mid-size landlords managing hundreds of units. This idiosyncratic risk is, however, an important feature of this market, as it is relevant for the sizeable share of landlords who operate smaller portfolios and are unable to otherwise insure this risk.

Figure 1 – Nonpayment Risk



(a) Months Behind at Move-Out



(b) Months Behind at Eviction Filing

Notes: Distribution of balances owed at move-out and at eviction filing for tenants in the 2015-2019 analysis sample. Panel (a) includes evicted and non-evicted tenants. Move-out refers to the last month of observed rent charges or payment. Eviction is measured as the month of eviction court filing or landlord's notice to attorney to file eviction, regardless of whether the tenant moves out after. Cumulative balance in a given month is the sum of differences between rent charges and payments since move-in. Balances are top-coded at 10 months of rent.

not appear to just be a pretext for eviction; in the three months prior to eviction, 81% of tenants miss rent at least once, and on average 48% of their total balance was accrued during that time.

Though a strikingly high proportion of tenants are evicted, it is much lower than the proportion (1 in 2) who default. Panel (b) of Figure 1 shows that landlords usually wait until a tenant owes two or more months of rent before filing. As a result, tenants who are not evicted account for a significant share of nonpayment. Columns (4) and (5) of Table 1 split our analysis sample into not-evicted and evicted tenants. Evicted tenants owe over $3,000 (3.5 months' rent) at the time of move-out, and over 25% of rent from ultimately evicted tenants goes unpaid by the end of the tenancy. However, non-evicted tenants still owe considerable amounts: $630, or two-thirds of a month's rent, on average. This means that 38% of landlords' losses from nonpayment are from non-evicted tenants.

Tenants often recover after default. The previous observations raise the question of why landlords tolerate nonpayment, given that defaulting tenants often continue missing rent and many end up being evicted. Part of the answer is the costs of the eviction process—both direct costs from court and legal fees, and indirect costs from the time it takes to remove a tenant and fill a subsequent vacancy. Another reason to delay eviction is that a tenant might recover after defaulting, by either continuing to pay rent or even repaying what they owe.

Table 2 assesses the scope for recovery by analyzing outcomes for tenants whose balances reach different levels for the first time. As a baseline, the first row describes outcomes for tenants from the time they move in (with zero balance); the next row summarizes the same outcomes for tenants after they default for the first time (reaching a 1-month balance); and the remaining rows do the same from when tenants first fall additional months behind. For each group, the table summarizes how long tenants stay and their payment rates beginning from the relevant month.

While a tenant's balance is strongly predictive of subsequent default, many defaulting tenants recover. Among the 50% of tenants (2,902 of 5,809) who default for the first time, 38.7% fully recover—continue paying and pay back the rent they owe in some later month. If a landlord always evicted a tenant after the first default, they would be getting rid of many tenants who could otherwise have stayed in the unit and continued paying. Expected outcomes for tenants in this group are nonetheless worse than for new tenants. The average first-time defaulter pays 78% of the rent thereafter and stays in the unit for 9 months, compared to 88% and 15 months for new tenants. As a measure of whether the tenant successfully stayed in the unit and resumed paying rent in the remainder of the tenancy, column (5) of Table 2 reports whether the tenant was still in the unit 12 months later and missed at most 2 months of rent during that time.[17] This share is 20.5% for first-time defaulters, compared to 44.2% for new tenants.

Recovery becomes much less likely as tenants fall further behind. In the third row of Table 2, 28.9% of tenants fall two months behind. These tenants pay 58.8% of subsequent rent and stay for only 5.5 months. 10.6% of these tenants fully recover, and only 7.7% satisfy the criterion in column (5) of staying and resuming payment. The remaining rows of the table show that these numbers continue to fall for tenants who reach even higher balances.

Landlords appear uncertain about their tenants' future default. The fact that landlords wait multiple months to file an eviction against tenants who (sometimes) continue to miss rent suggests that they cannot perfectly forecast a tenant's future payments. For example, by the time they were evicted, 21% of evicted tenants had defaulted for at least three consecutive months. Even more strikingly, a small minority (3%) of tenants move in and *never* pay rent, and landlords on average wait four months to evict these tenants. It is difficult to believe that landlords anticipated that these tenants would move in and never pay, but rented to them anyway. The landlords in our study also told us directly that it was hard for them to predict how much rent a tenant would pay after they defaulted. We therefore believe landlord uncertainty is the most reasonable explanation for the patterns we document.[18] The

[17] The landlords providing us with the data described this criterion as indicating a relatively desirable tenant. This payment rate also closely matches the 86% share of rent paid on average by all unsubsidized tenants.

[18] In principle, if landlords' preferences are unrestricted, their eviction decisions can always be rationalized by a model in which they perfectly forecast a tenant's future payments. However, the behavior we observe would imply that if landlords

Table 2 – Scope for Recovery

Statistic	Leases (1)	Ever Recovered (%) (2)	Share Paid (%) (3)	Tenancy (4)	Stayed 12mo Paid 10mo (%) (5)
New Tenants	5,809	–	88.1	15.0	44.2
1 month behind	2,902	38.7	78.1	9.1	20.5
2 months behind	1,680	10.6	58.8	5.5	7.7
3 months behind	1,082	3.2	41.5	3.9	3.6
4 months behind	680	1.0	27.6	3.3	1.8
5 months behind	415	0.7	26.9	3.3	2.2
6 months behind	247	0.8	26.7	3.4	2.4

Notes: Statistics based on the 2015-2019 analysis sample. Values are calculated from the first month in which a tenant's cumulative balance reaches N months' rent through the remainder of the tenancy. Cumulative balance is the sum of monthly differences between rent charges and payments from move-in through the current month, normalized by current rent. Ever Recovered measures whether a tenant subsequently paid back their full balance. Share Paid, the share of rent collected, is calculated using equation (13). Tenancy is the number of the months the tenant remained in the unit. Share Paid and Tenancy are computed at the tenant level and then averaged across tenants. Stayed 12mo Paid 10mo is the share of tenants who subsequently remained in the unit for at least 12 months and missed at most 2 months' rent.

structural model will make specific assumptions about landlords' information and assess robustness of our findings to those assumptions.

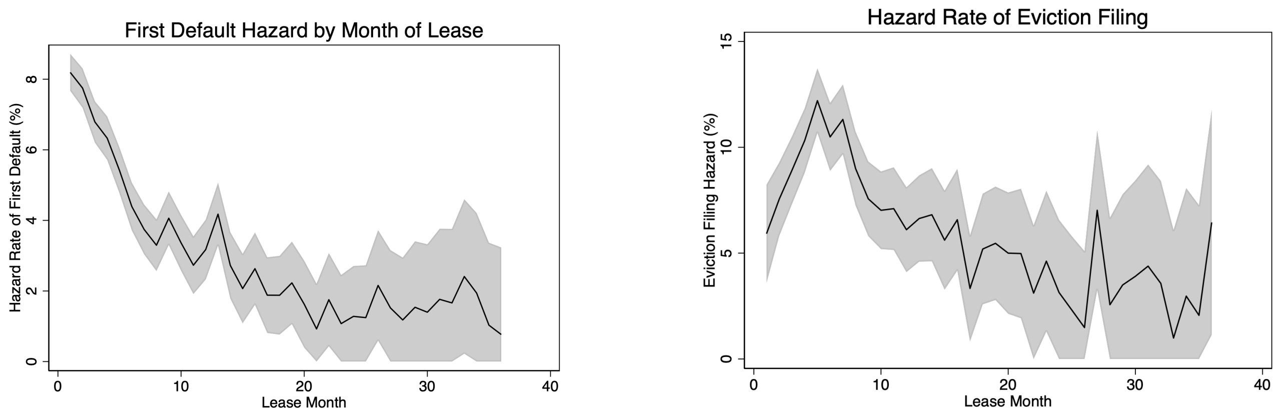
This uncertainty creates option value for the landlord from delaying an eviction filing, but also entails risk. On one hand, a tenant who has defaulted for the first time may go on to be a successful tenant and even repay what they owe. In this case, waiting and ultimately deciding not to evict a tenant avoids a costly eviction process. On the other hand, if the tenant continues defaulting, the landlord incurs greater losses from nonpayment by waiting to file.

The timing of eviction is consistent with landlords learning about tenants' evolving ability to pay. We present further evidence on nonpayment dynamics and eviction behavior that guides our modeling choices in the rest of the paper.

We begin with evidence suggesting that tenants' nonpayment risk evolves over time, and that landlords learn about this risk gradually. Figure 2 Panel (a) plots the probability that a tenant defaults for the first time (if they have not yet defaulted) against the number of months they have been in the unit. This first-default hazard rate is 8% in the first month of tenancy, and then declines over the next 12 months before stabilizing at a monthly hazard well above zero. Figure 2 Panel (b) then shows landlords' eviction filing hazards over the same tenancy months for tenants at least one month

have perfect foresight, they also have very strong, persistent preferences to *not* evict some of their worst-performing tenants.

Figure 2 – Default and Eviction over Time



(a) First Default Hazard by Month of Lease **(b)** Eviction Filing Hazard for Delinquent Tenants

Notes: Hazard rates of first default (Figure 2a) and eviction filing on delinquent tenants (Figure 2b) based on the 2015-2019 analysis sample. First default is default among the set of tenants with no prior default. Default is defined as payment rate below 50%, where share of rent paid is computed using equation (13). Eviction filing month is measured as eviction court filing or landlord's notice to attorney to file eviction, regardless of whether the tenant moved out after.

behind on rent. These eviction hazards are initially low, rise sharply to about 11% from the fifth to the seventh month of a tenancy, and then fall gradually before stabilizing at around 4%.

These patterns have several implications. First, the declining first-default hazards suggest there is persistent heterogeneity in tenants' default risk; the line would instead be flat if all tenants had the same constant monthly probability of default. Second, both the first-default and eviction hazards remain well above zero, even for tenants who have rented their units for more than two years. This suggests that tenants' default risk also changes over time, as tenants who missed no payments for two years later go on to default and be evicted. Together with the evidence on tenant recoveries in Table 2, default patterns are best explained by tenants facing a mix of transitory and relatively persistent changes in their nonpayment risk.

A more detailed analysis of landlords' eviction behavior reveals patterns consistent with their learning about a tenant's evolving nonpayment risk. Table 3 presents estimates from linear probability models predicting whether the landlord files an eviction in a given month as a function of the tenant's payment history. We estimate specifications of the following form:

$$\text{Evict}_{it} = \alpha_t + \alpha_{l(i)} + \alpha_{\tau(i,t)} + \sum_b \beta_b \mathbf{1}_{\{\text{Bal}_{it}=b\}} + \gamma B_{it} + \delta B_{it} 1_{\{\text{Tenure}_{it}>12\}} + \epsilon_{it} \, . \tag{1}$$

The outcome variable is an indicator for whether tenant i has an eviction filed against them in month t.

On the right-hand side are fixed effects for calendar year and month t, months of tenure τ, and landlord l; a set of indicators for whether the tenant has cumulative balance of $b \in \{1, 2, \ldots, 4+\}$ months; the tenant's *recent* cumulative balance B_{it} over the past three months; and B_{it} interacted with tenure. The last two regressors allow us to test whether landlords are more responsive to recent defaults, and whether this differential responsiveness is equally strong for tenants with longer tenure.

Table 3 presents the results. Column (1) only includes the tenant's cumulative balance and fixed effects. Consistent with our earlier evidence, eviction rates rise sharply once a tenant owes two or more months of rent. A balance of 2 or 3 months predicts eviction hazards 16.4 and 23.9 percentage points higher than for non-delinquent tenants, respectively. In contrast, tenants with a balance of only 1 month face an eviction hazard only 2.5 percentage points higher than for non-delinquent tenants. Column (2) adds the tenant's recent balance to the specification; conditional on a tenant's total balance, each additional month accrued during the last three months (instead of further in the past) predicts a 3.7 percentage point higher eviction hazard. This behavior is consistent with landlords understanding that recent defaults are more predictive of future nonpayment than defaults further in the past, given that tenants' default risk can change. Column (3) adds the interaction between recent balance and tenure. The responsiveness to recent information is still positive, but muted for tenants who have been renting for more than one year. This behavior is consistent with landlords accumulating richer information about their tenants over the course of a lease and therefore putting less weight on recent information.[19]

Taken together, these patterns indicate a rental market characterized by heterogeneous tenants facing persistent, yet evolving, nonpayment risks, where landlords learn about these risks and adapt their eviction decisions based on forecasts of tenants' future payment behavior.

4. Model

Guided by the descriptive evidence in the previous section, we develop a partial equilibrium model of the landlord's eviction decision to predict owners' responses to eviction protections and elucidate the drivers of eviction. We estimate this model in the next section.

[19] When viewed in a Bayesian framework, another potential explanation for why the weight on recent default could vary with tenure is if the informativeness of recent default about future default changes over time. However, for the results discussed here, we find in a simple AR(1) model with the same covariates as in Table 3 that the predictiveness of current default for future default is stable or modestly increasing over tenure. We also found similar qualitative patterns controlling for unit fixed effects, suggesting that these patterns are not driven by unit-specific heterogeneity.

Table 3 – Landlord Eviction Decisions

Dependent Variable: Eviction filed in Current Month	(1)	(2)	(3)
Cum. Balance 1 Months	0.0251***	0.0118***	0.0124***
	(0.00156)	(0.00186)	(0.00185)
Cum. Balance 2 Months	0.164***	0.130***	0.130***
	(0.00832)	(0.00826)	(0.00820)
Cum. Balance 3 Months	0.239***	0.179***	0.178***
	(0.0151)	(0.0156)	(0.0155)
Cum. Balance 4+ Months	0.169***	0.0817***	0.0770***
	(0.0138)	(0.0140)	(0.0138)
3-Month Balance		0.0372***	0.0428***
		(0.00327)	(0.00387)
3-Month Balance × Tenure > 1 Year			-0.0173**
			(0.00536)
Constant	0.00298	0.00166	0.00112
	(0.00242)	(0.00240)	(0.00248)
Month and Year FEs	X	X	X
Firm FEs	X	X	X
Tenure Month FEs			X
Observations	81,598	81,598	81,598

Notes: Estimates from a linear probability model of eviction filing. An observation is a month of a specific lease. The sample includes all lease-months during 2015-2019 from the 5,809 tenancies in the analysis sample, up to and including the first month an eviction was filed. All specifications include fixed effects for year, month-of-year, and landlord. Specification (1) controls for indicators for the tenant's cumulative balance. Specification (2) adds cumulative balance over the past 3 months ("3-Month Balance"). Specification (3) adds interactions with tenure. Cum. Balance is the cumulative balance, divided by the current rent, rounded to the nearest integer. Balance includes the current month's (non-)payment. Tenure > 1 Year is an indicator for whether the tenant has been in the unit for more than 12 months. Standard errors are clustered at the tenancy level.
*p<0.05; **p<0.01; ***p<0.001

4.1 Model Setup

Time is discrete. At the start of a period (month) t, each unit can be in one of three states s_t: (i) vacant ($s_t = v$), (ii) occupied by a unitary household ("tenant") who has been evicted ($s_t = e$), or (iii) occupied by a tenant who has not yet been evicted ($s_t = o$). Consider first the latter case ($s_t = o$), when the eviction filing decision is relevant. The following steps occur each month:

1. The tenant draws an unobserved type $\omega_t \sim F(\cdot \mid \omega_{t-1})$ with support on $[0, 1]$. The type ω_t

determines both the tenant's probability of paying (and repaying) in month t and, through $F(\cdot \mid \cdot)$, the distribution of their type next month.

2. The tenant then pays rent with probability $\theta(\omega_t)$. Conditional on paying rent, tenants who carry a balance of past unpaid rent also repay a month of their balance with probability $\mu(\omega_t)$.[20] We denote rent payment as $y_t \in \{0, 1, 2\}$, with 1 corresponding to paying (only) the current month's rent and 2 corresponding to paying both current and one month of past-due rent. The tenant's balance b of past-due rent evolves as $b_t = b_{t-1} + 1 - y_t$.

3. The landlord observes the full payment history $h^t \equiv (y_1, ..., y_t)$ and updates their beliefs $\pi_t(\omega_t \mid h^t)$ over the tenant's current type.

4. The landlord then makes a decision $e_t \in \{0, 1\}$ of whether to begin evicting the tenant. Eviction incurs a one-time fixed cost C_e paid at the time of filing.

5. At the end of the month, the tenant experiences a mobility shock with probability δ_d. After a mobility shock, the tenant vacates the apartment and makes no further payments to the landlord.

If the landlord files an eviction and the tenant does not leave at the end of the month, the unit enters month $t + 1$ occupied by the same tenant with eviction proceedings initiated ($s_{t+1} = e$). The landlord no longer has the option to evict, and the tenant's type continues to evolve according to F as before eviction, but payment and repayment probabilities are reduced proportionally after eviction by a factor ϕ_1. In addition, eviction accelerates the tenant's moveout: with probability δ_e the tenant is removed or leaves. If the tenant moves out, the unit begins the next period vacant. Otherwise, the apartment begins next month occupied with an in-progress eviction.

A vacant apartment ($s_t = v$) is filled with probability δ_v at the start of the month. When a new tenant moves in, their initial type ω_1 is drawn from an initial type distribution $\alpha(\cdot)$. Both the vacancy filling rate and the initial type distribution can be thought of as generated by landlord screening. As such, $\alpha(\cdot)$ may differ from the invariant distribution generated by the Markov process governing tenant type transitions $F(\omega_t \mid \omega_{t-1})$.

[20] While we do observe cases of partial payment, the vast majority of payments are very close to one or two months' rent. We therefore abstract away from the intensive margin of payment and focus on landlords' beliefs about whether a tenant will pay (or repay) at all. We likewise abstract away from late fees, which we find are only 1.5% of total rent charges and frequently go unpaid by tenants.

4.2 Payoffs and Value Functions

Each month the unit is occupied, the landlord receives rental income $R\,y_t$ and pays a maintenance cost c.[21] The landlord also pays the fixed cost C_e if they file an eviction, and receives decision-specific payoff shock $\epsilon_t(1)$ if $e_t = 1$ and $\epsilon_t(0)$ if $e_t = 0$. Their net flow payoff in the occupied ($s_t = o$) state is

$$u(y_t, e_t) = R\,y_t - e_t C_e + \epsilon_t(e_t) - c\,. \tag{2}$$

The landlord's eviction decision maximizes the expected net present value of revenue net of costs. We define continuation values corresponding to the three unit states $s_t \in \{o, e, v\}$.

While renting to a tenant who has not been evicted ($s_t = o$), the landlord chooses whether to evict after observing the tenant's payment history h^t up to and including the current month. Their value function also depends directly on the unit's occupancy state s_t. The ex-ante value function, including this month's rent payments, is

$$V(h^t, e^t; o) = R\,y_t - c + \mathbb{E}_\epsilon \left[\max_{e_t \in \{0,1\}} -e_t C_e + \epsilon_t(e_t) + \beta \left(\delta_d V_v + (1-\delta_d)\mathbb{E}[V(h^{t+1}, e^{t+1}; s_{t+1}) \mid h^t, e_t] \right) \right], \tag{3}$$

where V_v is the value of a vacant unit and e^t is the tenant's eviction history.

After filing an eviction, if the tenant has not left ($s_t = e$), the landlord's continuation value is

$$V(h^t, e^t; e) = R\,y_t - c + \beta\, \left(\delta_e V_v + (1-\delta_e)\, \mathbb{E}[V_e(h^{t+1}, e^{t+1}; e) \mid h^t; e] \right)\,. \tag{4}$$

The decision to evict takes into account how eviction accelerates tenant exit (governed by the difference between δ_e and δ_d), how rent payments fall while the evicted tenant is still in the unit, and the value of a vacancy. Because eviction directly affects payment rates, and thus the information that any given payment history conveys about a tenant's current type, the continuation value depends on when the eviction was filed (hence the eviction history e^t) as well as the payment history.

When the unit is vacant, the owner either finds a new tenant immediately or continues searching next month:

$$V_v = \delta_v\, \mathbb{E}\left[V(h^1, e^1; o) \mid \alpha(\cdot)\right] + \beta\,(1-\delta_v)\,V_v\,. \tag{5}$$

The value of vacancy therefore depends on the vacancy fill rate δ_v and the expected value of a new

[21] The maintenance cost c represents the cost of maintaining an occupied unit relative to a vacant unit. Any fixed or sunk costs, such as property taxes, are not included.

tenant. The latter integrates over whether the tenant pays the first month, which depends on the initial type distribution $\alpha(\cdot)$.

This model captures several components of the effective cost of evicting a tenant. First are the expected legal, hassle, (apartment) depreciation, and psychic costs captured by C_e. Second, evicting the tenant causally reduces rent payments by factor ϕ_1 until the tenant leaves. Third, finding a new tenant takes time, and the new tenant might also default.

At the same time, waiting to file an eviction on a tenant who has defaulted has potential option value. If the tenant repays their balance or can at least pay future rents, the landlord can avoid the direct and indirect costs of eviction and replacing the tenant. Further, the tenant may move out on their own without an eviction. Of course, waiting to file risks retaining a tenant who continues to default. The model clarifies that it may well be in a landlord's interest to tolerate some nonpayment, even when the direct costs of filing are low, especially if tenants who default have a significant chance of recovering.

4.3 Belief Updating

A landlord's decision to evict depends on their belief about the tenant's future probability of payment. In the baseline model, the landlord learns about the tenant's underlying type, which may itself be evolving, through the realized rent payments. This section derives a recursive expression for landlord beliefs π_t and uses it to reduce the dimensionality of the state space in their decision problem. Since ω_t follows a first-order Markov process, the landlord's posterior belief about ω_t fully captures their beliefs about how the tenant's type will evolve going forward.

Let $\omega^t = (\omega_1, ..., \omega_t)$ denote a tenant's full type history. By Bayes' Rule, the landlord's posterior belief about ω^t given payment history h^t can be written

$$\Psi(\omega^t \mid h^t) = \frac{p(h^t \mid \omega^t)P(\omega^t)}{\int p(h^t \mid \omega^t)P(\omega^t)d\omega^t},$$

where $P(\omega^t)$ is the probability density function of the full type history implied by $\alpha(\cdot)$ and $F(\cdot \mid \cdot)$, and $p(h^t \mid \omega^t)$ is the probability the payment history h^t was realized given the type history:

$$p(h^t \mid \omega^t) = \Pi_{s=1}^{t}(1-\theta(\omega_s))^{1_{y_s=0}}(\theta(\omega_s)(1-\mu(\omega_s)^{b_s>0}))^{1_{y_s=1}}(\theta(\omega_s)\mu(\omega_s)^{b_s>0})^{1_{y_s=2}}.$$

Integrating over all type histories ending with $\omega_t = \omega$, we obtain the posterior probability that $\omega_t = \omega$:

$$\pi_t(\omega \mid h^t) = \frac{\int_{\omega^t:\omega_t=\omega} p(h^t \mid \omega^t)P(\omega^t)d\omega^t}{\int p(h^t \mid \omega^t)P(\omega^t)d\omega^t}.$$

This formulation requires integrating over all possible type histories and conditioning on the full payment history. Even in a parsimonious model with discrete types, enumerating all possible type and payment histories quickly becomes prohibitive. We can instead write their posterior π_t recursively as a function of (i) their posterior last month (π_{t-1}), and (ii) this month's payment outcome y_t, and (iii) the balance b_{t-1} from last month:

$$\begin{aligned}\pi_t(\omega \mid y_t, b_{t-1}, \pi_{t-1}) &= \frac{p(y_t \mid b_{t-1}, \omega)\tilde{\pi}(\omega)}{\int p(y_t \mid b_{t-1}, \omega)\tilde{\pi}(\omega)d\omega} \\ \tilde{\pi}(\omega) &= \int f(\omega \mid \omega')\pi_{t-1}(\omega')d\omega' \,.\end{aligned} \tag{6}$$

The interim posterior $\tilde{\pi}(\cdot)$ accounts for the fact that the tenant's type may change from months $t-1$ to t. The landlord combines this with the realized payment y_t (and the scope for repayment, governed by b_{t-1}) to form their posterior belief π_t.

This recursive representation of beliefs allows us to transform the state entering the landlord's continuation value. Instead of conditioning on the full payment history h^t, we need only condition on the landlord's posterior π_t, along with the realized payment y_t (for accounting purposes) and balance b_t (which determines the scope for future repayment).[22] The rest of the paper will express the state as (π_t, y_t, b_t). Equation (3) can be rewritten as

$$\begin{aligned}V(\pi_t, y_t, b_t; o) = R\,y_t - c \\ + \mathbb{E}_\epsilon \left[\max_{e_t \in \{0,1\}} -e_t C_e + \epsilon_t(e_t) + \beta\left(\delta_d V_v + (1-\delta_d)\mathbb{E}[V(\pi_{t+1}, y_{t+1}, b_{t+1}; s_{t+1}) \mid \pi_t, b_t, e_t]\right)\right],\end{aligned} \tag{7}$$

and similarly for equations (4) and (5).

4.4 Discussion

The goal of the structural model is to both recover the primitives governing landlords' incentives to evict, and to predict how eviction protections will interact with these incentives. Several assumptions in the above model warrant discussion.

First, the model focuses on the decision of an individual landlord to file an eviction case, but holds constant other possible equilibrium responses by landlords and tenants to eviction policy. In Section 6.3, we explicitly consider how two such responses would impact our findings: (1) if landlords pass the costs of stronger protections on to tenants through higher rents, and (2) if tenants strategically reduce their payment rates due to moral hazard. Our main findings regarding eviction behavior are qualitatively robust to allowing for these additional responses.

[22] See Chatterjee et al. (2023) for a similar transformation of (loan) payment histories into a posterior about consumer types.

Landlords could also respond to stronger protections by screening prospective tenants more aggressively, or by adjusting other rental contract features such as security deposits. Fewer than 1% of leases in our model estimation sample include security deposits (perhaps because of binding liquidity constraints among their tenants), suggesting that responses on this margin may be limited. To assess the scope for screening responses, Appendix Section B reports an analysis of a subset of our data containing tenant-screening reports. The information in these reports—which includes past evictions, credit histories, income, and criminal backgrounds—is highly predictive of whether an applicant signs a lease (meaning the landlord approved them), but not of default among tenants who sign a lease and move in. This suggests that it may be difficult to further distinguish between higher- and lower-risk tenants among the set of currently approved tenants. While this analysis has limited power due to a relatively small sample of tenants with linked screening reports, it is consistent with limited screening responses to policies of the magnitudes we consider.

Second, we treat eviction as the outcome of a single-agent decision problem rather than strategic interactions between the landlord and tenant. Our conversations with landlords suggest that the combination of asymmetric information and limited commitment on the part of tenants (who cannot commit to paying rent in the future) commonly prevents cooperative solutions, such as agreeing on a repayment schedule instead of a formal eviction.

Finally, we discuss in Section 5.2 our assumptions governing the landlord's information about the tenant's ability to pay.

5. Estimation

This section describes our estimation procedure to recover the parameters governing payments, landlord costs, and unit transitions. We estimate the rates of tenant departure and vacancy filling offline, and then jointly estimate the type process and cost parameters by maximum likelihood (Rust, 1987).

For our model estimation sample, we focus on the relatively homogeneous Cook County, IL subset of our sample presented in Section 3. In particular, this sample restricts to rental units with monthly rent between $600 and $1,000 where the tenant does not have a housing voucher. Appendix F provides additional details on our sample criteria. Importantly, these units share a common regulatory environment, including rules surrounding the eviction process and other tenant protections. This leaves us with 1,814 distinct tenancies covering 2015 - 2019.

5.1 Parameters and Likelihood

We parameterize tenant types as following a discrete Markov process with elements $\{1, ..., K\}$. Let $\theta_{it} \in \{\theta_1, ..., \theta_K\}$ denote tenant i's probability of paying some rent in month t given their current type ω_{it}, and $\mu_{it} \in \{\mu_1, ..., \mu_K\}$ the probability of repayment (when behind) conditional on paying. Let M denote the $K \times K$ Markov matrix governing type transitions, and $\alpha = \{\alpha_1, ..., \alpha_K\}$ the initial probabilities that a new tenant is each type at $t = 1$. The proportional reduction in payment probabilities post-filing is governed by a common parameter ϕ_1. We also allow for a proportional reduction in default in the first month, denoted ϕ_2.[23] This reflects that landlords usually require first month's rent to be paid before the tenant moves in. We allow the departure and vacancy filling rates $\delta_d, \delta_e, \delta_v$ to vary across landlords and rent categories, separating units renting for \$600-800 and \$800-1,000 per month. Estimates are reported in Appendix Table 6. We assume the filing cost C_e is the same for all units, and calibrate the maintenance cost c to 10% of the monthly contract rent.[24]

We assume the decision-specific errors $\epsilon_t(1)$ and $\epsilon_t(0)$ are drawn i.i.d. across units and months from a Type-1 Extreme Value distribution. This yields closed-form conditional choice probabilities for the eviction decision once we have calculated the landlord's choice-specific conditional value function in each state. The conditional choice probability of filing an eviction given history h^t is

$$Pr(e_t = 1 \mid \pi_t, b_t) = \frac{1}{1 + e^{\bar{v}^{e=0}(\pi_t, b_t) - \bar{v}^{e=1}(\pi_t, b_t)}}\,, \tag{8}$$

where $\bar{v}^{e=0}$ and $\bar{v}^{e=1}$ are the choice-specific conditional value functions:

$$\bar{v}^{e=0}(\pi_t, b_t) = \beta \left(\delta_d V_v + (1 - \delta_d)\mathbb{E}\left[V(\pi_{t+1}, y_{t+1}, b_{t+1}; o) \mid \pi_t, b_t\right]\right) \tag{9}$$

$$\bar{v}^{e=1}(\pi_t, b_t) = -C_e + \beta \left(\delta_d V_v + (1 - \delta_d)\mathbb{E}\left[V(\pi_{t+1}, y_{t+1}, b_{t+1}; e) \mid \pi_t, b_t\right]\right)\,. \tag{10}$$

Let i denote a specific tenant and t a month in their tenancy. We observe data $\{(y_{it}, e_{it})\}_{i=1,...,N}^{t=1,...,T_i}$ on payments and filing decisions for every month a tenant occupies a unit. We also observe months in which each unit is vacant.

Let $\Gamma = (\alpha, M, \theta, \mu, \phi, C_e)$ denote the model parameters to be estimated. Consider a particular

[23] The probability a tenant of type ω defaults at $t = 1$ is therefore $(1 - \phi_2)(1 - \theta(\omega))$, and $(1 - \theta(\omega))$ thereafter while the tenant has not been evicted.

[24] In the Census's 2018 Rental Housing Finance Survey, landlords' average monthly expenditure on building maintenance is 8.5% of monthly rent receipts for apartments with rent between \$600 and \$1,000. Our counterfactual results are qualitatively not sensitive to whether this maintenance cost is calibrated to 10% or 0% of monthly contract rent.

tenant who rents for T_i months. The likelihood of observing $\{(y_t, e_t)\}_{t=1}^{T_i}$ is

$$L(\Gamma \mid h^{T_i}, e^{T_i}) = \Pi_{t=1}^{T_i} Pr(y_t \mid \pi_{t-1}, b_{t-1}; \Gamma) Pr(e_t \mid \pi_t, b_t; \Gamma)\,, \tag{11}$$

where $Pr(e_t \mid \pi_t, b_t)$ is given by Equation (8) and

$$Pr(y_t \mid \pi_{t-1}, b_{t-1}; \Gamma) = \sum_{k=1}^{K} [M\pi_{t-1}]_k (1-\theta_k)^{1_{y_t=0}} (\theta_k(1-\mu_k 1_{b_{t-1}>0}))^{1_{y_t=1}} (\theta_k \mu_k)^{1_{b_{t-1}>0} 1_{y_t=2}}\,, \tag{12}$$

with $[M\pi_{t-1}]_k \equiv \tilde{\pi}_t(\omega_k)$ being the belief about the tenant's type *before* payment is observed.

We estimate the unit transition probabilities $(\delta_d, \delta_e, \delta_v)$ offline by calculating the mean hazard rate among at-risk units of each observable type (i.e., the categories of rent level $\times$ landlord). Then, we jointly estimate the parameters governing tenant types and landlord costs by maximizing the likelihood in Equation (11), solving the value function and forming the likelihood for each candidate value of model parameters.[25] In this sense, our estimator takes a "full-solution" approach to estimating the dynamic discrete choice model (Rust, 1987). In our baseline model in which the landlord only observes payments, we could instead estimate the parameters governing the tenant type process separately using the payment data alone (without solving the landlord's problem), and then estimate the cost parameters in a second step given the type parameter estimates. We report jointly estimated parameters both for efficiency and to accommodate alternative specifications in which the landlord has more information about the tenant's type, which introduces persistent unobserved heterogeneity for the econometrician. We get similar results when estimating the model with the two-step approach.

5.2 Identification

We rely on several assumptions for identification that are standard in the dynamic discrete choice literature (Rust, 1987; Hotz and Miller, 1993). The distribution of choice-specific payoff shocks and the discount factor are assumed known. The landlord's payoffs are interpreted relative to the value of keeping their unit vacant forever, which we assume is invariant to our counterfactuals.

A key identification challenge is the fact that the landlord's filing decision censors payment histories. Eviction patterns suggest that landlords largely choose whether to file based on their beliefs about a tenant's future probability of payment, but such payments are only observed for tenants who are *not* evicted. In our baseline model, we assume landlords only learn about a tenant's type through payments, which we also observe. This assumption implies that, conditional on a tenant's full payment

[25] We solve for the value functions in equations (3)-(5) on a dense grid of beliefs for each combination of payment and balance (y, b), and approximate the value function at other points using linear interpolation. Further details are in Appendix C.2.

history, eviction is uncorrelated with the tenant's future ability to pay. The tenant type transitions and the landlord's eviction costs are then identified from the observed payment and eviction patterns (Magnac and Thesmar, 2002).[26] In particular, the tenant type process is identified from the payment data accounting for conditionally random censoring due to evictions. Eviction filing costs are identified by how the probability of eviction varies with the difference in expected rental income from evicting rather than keeping the current tenant. If landlords wait to file eviction until tenants appear extremely unprofitable to keep—even accounting for delays in the eviction process and rental losses due to vacancy after the tenant moves out—then this suggests eviction costs are high.

It is also possible that landlords have additional information about their tenants not captured in the payments, and choose to evict based on it.[27] Such information would introduce persistent unobserved heterogeneity into the model. Testing whether the landlord has additional information requires instrumental variables that impact the filing decision without affecting the distribution of payments.[28] Furthermore, such a strategy can only provide a lower bound on the landlord's information; landlord behavior can always be rationalized by perfect foresight if arbitrary payoff structures are admitted. To the extent that landlords have information beyond payments, the assumption of symmetric information places a plausible lower bound on the information landlords have and, consequently, an upper bound on the likelihood that evicted tenants would have continued paying.

Since we cannot identify the landlord's information set, to assess sensitivity of our estimates and counterfactual predictions to this informational assumption, we also report estimation and counterfactual results from an alternative version of the model assuming the landlord perfectly observes the tenant's current type. This "full-information" model reinterprets the landlord's filing decision as reflecting their knowledge of the tenant's true type as well as the payment history we observe. Identification of this model relies on parametric assumptions about the tenant type process, and in particular on the number of distinct types. Most of our findings are qualitatively robust to this alternative model.

[26] Although the tenant's payment type is unobserved in this model to both the landlord and the econometrician, the landlord and econometrician have symmetric information. As a result, this model has the structure of a standard "Rust" model with no persistent unobserved heterogeneity, with the tenant's full payment history as the observed state. The tenant's unobserved payment type simply parameterizes how the payment history evolves.

[27] For example, the landlords in our data often tried to contact tenants who are behind on rent to gauge their ability to (re)pay. However, they often found it was difficult to reliably ascertain a tenant's current or future financial health.

[28] As one candidate instrumental variables strategy, we investigated events in which entire buildings are sold to new owners. While such buildings leave our sample after the sale, the sales are marked in the data. We observe a decrease in both payment and eviction rates leading up to sales. Unfortunately, an instrumental variables strategy using this variation is underpowered to reject either our baseline model of landlord information or the full-information model.

5.3 Results

Parameter Estimates. We present model parameter estimates in Table 4. Column (1) reports estimates from our baseline model with 3 tenant types.[29] Subsequent columns explore robustness to alternative models, including environments with 2 types and where landlords have complete information about tenants' current types.

Starting with the baseline model in column (1), we estimate three highly distinct payment types. The highest-quality tenants ("type H") have a monthly payment probability of 98.6%, middle-quality tenants ("type M") have a monthly payment probability of 78.6%, and the lowest-quality tenants ("type L") have a monthly payment probability of just 4.0%. All three types have lower *re*payment probabilities. The medium and low types together create a non-trivial inference problem for a landlord who observes their tenant in default. The tenant may only be experiencing a temporary default, but they might also be a persistent nonpayer.

At the start of a new lease, we estimate that about 5.1% of tenants are the lowest-quality type, 52.1% are the highest-quality type, and the rest are middle types. These types then evolve according to the Markov transition probabilities in Table 4. The highest- and lowest-quality types are more persistent than the middle type: high types remain high types in any given month 98% of the time while low types remain low types 96% of the time. Nonetheless, type transitions play an important role in explaining the nonpayment dynamics in our data; for example, a high-type tenant has a 24% chance of becoming a medium or low type within the next 12 months, and a medium-type tenant has a greater than one-third chance of becoming a low type in the next 6 months. Thus, when a landlord observes their tenant periodically missing rent, part of their concern is that the tenant may become a persistent nonpayer. On the other hand, even the lowest-type tenants have a 20% chance of reverting to being a medium type in the next 6 months.

Turning to the bottom of the table, we estimate landlords' filing costs to be about $2,000. These costs capture both direct legal costs such as court fees and lawyers' fees—which are typically over $1,000 in Cook County—and other pecuniary costs such as expected damage to the unit caused by tenants facing eviction,[30] as well as any hassle or psychic costs from the eviction process.

Parameter estimates are broadly similar across the remaining columns of Table 4. Column (2) allows 2 payment types while still in an incomplete information environment. This model rationalizes the data with a high type who usually pays and a lower-quality type who pays 22% of the time.

[29] We were unable to reliably estimate models with more than three tenant types. Monte Carlo simulations suggest that doing so would require a much larger sample of tenants.

[30] Property damage is a primary cost to mortgage foreclosure (Campbell et al., 2011), and theoretical work indicates that such agency costs can be a key friction in leasing markets more generally (Eisfeldt and Rampini, 2009).

Table 4 – Parameter Estimates

Model Parameter	Learning 3 Types (1)	Learning 2 Types (2)	Full Information 3 Types (3)	Full Information 2 Types (4)
Payment Parameters (%)				
Pmt. boost in month 1	17.9 (5.6)	18.6 (5.1)	7.6 (7.0)	7.1 (6.6)
Prop. change in pmt. post-filing	86.0 (2.1)	75.6 (2.2)	78.7 (2.2)	74.0 (2.1)
Type H	98.6 (0.1)	95.9 (0.2)	98.8 (0.2)	94.4 (0.2)
Type M	78.6 (1.1)	–	79.7 (1.0)	–
Type L	4.2 (0.8)	22.8 (0.9)	4.6 (0.7)	13.2 (0.8)
Repayment param. (%)				
Type H	0.0 (0.1)	3.1 (0.4)	0.1 (0.3)	4.9 (0.4)
Type M	8.9 (0.7)	–	9.3 (0.7)	–
Type L	28.7 (6.3)	17.2 (1.3)	29.6 (5.5)	23.8 (2.3)
Initial type shares (%)				
Type H	52.1 (2.1)	87.5 (1.0)	50.2 (2.3)	91.0 (1.0)
Type M	42.7 (2.2)	–	45.0 (2.3)	–
Type L	5.1 (0.8)	12.5 (1.0)	4.8 (0.8)	9.0 (1.0)
Transition prob. (%)				
H → H	97.8 (0.2)	95.7 (0.2)	97.1 (0.3)	96.0 (0.2)
H → M	1.8 (0.3)	–	2.9 (0.4)	–
H → L	0.5 (0.2)	4.3 (0.2)	0.0 (0.3)	4.0 (0.2)
M → H	1.9 (0.3)	–	2.3 (0.4)	–
M → M	90.4 (0.6)	–	87.7 (0.8)	–
M → L	7.8 (0.4)	–	10.0 (0.6)	–
L → H	0.0 (0.5)	4.6 (0.5)	0.0 (0.6)	3.2 (0.7)
L → M	3.6 (1.1)	–	0.5 (0.8)	–
L → L	96.4 (0.9)	95.4 (0.5)	99.5 (0.7)	96.8 (0.7)
Cost param. ($)				
Eviction cost	1,977 (173)	719 (126)	3,550 (341)	1,900 (245)
Maintenance cost	0.1×R	0.1×R	0.1×R	0.1×R
S.D. epsilon	581 (34)	389 (23)	537 (57)	470 (49)
Model fit				
Log likelihood	-10,361	-10,728	-10,361	-10,739

Notes: Estimates based on the 2015-2019 model estimation sample, which includes 1,814 non-voucher tenancies in Cook County, IL, with monthly rent $600-1,000. Columns (1) and (2) report parameter estimates from the baseline 3- and 2-tenant type learning model, which assumes that the landlord's information set is only payments. Columns (3) and (4) include estimates from the full-information model, which assumes that landlord's information set is both payments and tenant types. The maintenance cost is set to 10% of rent (R). The Payment Parameters govern the probability that each tenant type pays some rent in that month. The Repayment Parameters govern the probability that each type repays conditional on paying. Pmt. boost in month 1 is the proportional reduction in default in a tenant's first month; Prop. change in pmt. post-filing is the proportional change in the probability a tenant pays some rent after an eviction has been filed against them.

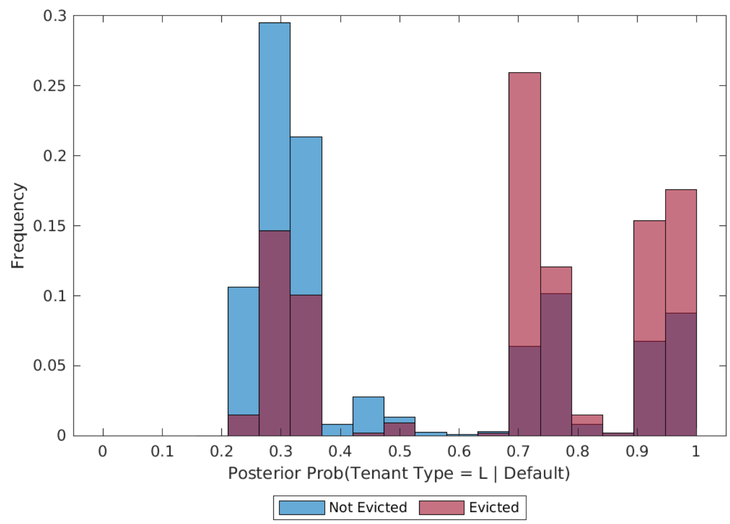
There are also more frequent transitions between types than in the estimated 3-type model in order to rationalize the frequency of temporary defaults. Landlords' eviction costs are estimated to be lower in the 2-type model, in part helping the model rationalize why many high types get evicted.

Columns (3) and (4) of Table 4 present estimates from an alternative model, formalized in Appendix Section C.1, in which landlords observe their tenants' current types. The payment probabilities for each type, the initial type shares, and the Markov process for type transitions are similar to columns (1) and (2). In this sense, our findings about the distribution and dynamics of tenants' nonpayment risk appear robust to alternative assumptions about how well landlords are informed about their tenants' types. However, there are some quantitative differences. The landlord eviction cost is estimated to be about twice as high under complete information. These higher costs are an artifact of the strong assumption that landlords fully know tenant types: to help the model rationalize how landlords are slow to evict nonpaying tenants—among whom the estimated Markov process predicts a substantial share must be the low type—filing an eviction needs to be quite costly. For a similar reason, tenants are more likely to transition from higher to lower types in the complete information model, and are less likely to recover after becoming a low type.

Model Fit. Appendix Section D summarizes the fit of the estimated 3-type learning and full-information models. We simulate tenancies according to each estimated model and compare statistics to those in the model estimation sample. Both the learning and full-information models are able to replicate key qualitative patterns in eviction rates, payment rates, and repayment rates, though the fits are not perfect. In particular, Appendix Figure 6 shows that these models replicate the non-monotone patterns in eviction rates with respect to tenant tenure, and the nonlinear patterns in eviction rates with respect to a tenant's current balance. The learning model does a slightly better job fitting the steep initial rise and then decline in eviction rates as tenure increases, as well as eviction rates for tenants with high balances. The full-information model underpredicts eviction rates at high balances for the same reason that estimated eviction costs are especially high in this model: landlords are slow to evict tenants whom they must have known for months were low types (revealed to the econometrician by subsequent defaults).

The richness of our preferred specification is needed to match key payment and eviction patterns in the data. Appendix Figures 7 and 8 compare the fit of the 3-type learning model to that of more restrictive models—the 2-type learning model reported in column (3) of Table 4, and a model allowing 3 *permanent* types. Panel (a) of Figure 7 shows that these more restrictive models do a poorer job matching eviction rates over the course of a tenancy. The 2-type model fails to match the non-monotone pattern in eviction rates over time, while the permanent type model predicts that eviction rates peak and begin falling much earlier. The permanent type model also does not match how eviction and subsequent payment rates vary with a tenant's balance. In Panel (b) of Appendix Figure 7, the permanent type model predicts that eviction rates rise much less steeply as a tenant's balance increases than is observed in the data. Appendix Figure 8 shows that this model also predicts much higher payment rates for tenants who have fallen 3 or more months behind. In the data, many tenants begin repeatedly defaulting after being good tenants for a long time; the permanent-type model cannot match the frequency with which this occurs.

Figure 3 – Distribution of Landlord's Posterior about Type L, Conditional on Default



Notes: The figure shows, for all tenants in default, the distribution of landlords' posterior beliefs that a tenant is currently Type L. The sample includes all tenant-months in the model estimation sample where the tenant is in default and has not been evicted in a past month. These distributions are plotted separately for tenants evicted (in red) and not evicted (in blue) in the current month. The posteriors are calculated using the estimates reported in column (1) of Table 4.

Landlord Learning. Figure 3 illustrates the role of landlord learning in the incomplete information model by showing, among tenants currently in default, the distribution of landlords' posterior probabilities that the tenant is currently the lowest ability-to-pay type. Landlords typically wait to evict tenants in default until it is likely that the tenant will have persistently low payment rates going forward. For tenants the landlord chooses to evict, these posterior probabilities are typically 70% or higher; for tenants the landlord chooses not to evict, these posteriors are instead typically below 35%. This fact helps us understand why we find limited scope for recovery among tenants who avoid eviction in our counterfactual exercises.

Although landlords face considerable uncertainty about their tenants' types in the incomplete information model, they learn quickly. Appendix Figure 9 plots the probability that a landlord's best guess of their tenant's type is correct in each month of tenancy. The landlord's best guess is only correct 60% of the time after one month, but after month 6, their best guess has an 80% chance of being correct. This relatively fast pace of learning echoes a similar result about employer learning in Lange (2007). It is also a lower bound on landlord learning if landlords have additional information other than tenants' payment histories.

Finally, we examine how the friction of imperfect information impacts which evictions do and do not occur. Appendix Figure 10 plots eviction rates for simulated tenancies in periods where a tenant's type has just improved or worsened. In each panel, the solid line plots eviction rates under the incomplete information model, in which the landlord's beliefs lag behind the type change. The dashed line plots the probability the landlord would evict the tenant if the tenant's type were revealed in that period only. Tenants who just transitioned from Type M to H or from L to M see lower eviction rates, while tenants who just transitioned from H to M or from M to L see much higher eviction rates, when the landlord is given perfect information in that month. This shows how some tenants benefit, while others are harmed, by incomplete information. On average, incomplete information leads to more forbearance and lower eviction rates than full-information because the landlord has option value from waiting to learn more about the tenant's type.

6. Counterfactuals

The estimated model allows us to quantify how landlord eviction decisions are likely to change in counterfactual policy environments. This section asks, first, what is the scope for recovery among evicted tenants? Second, are the types of policy interventions being proposed to reduce evictions likely to do so, and for which renters? Third, what are the potential costs and benefits of these policies to landlords, renters, and the government?

Section 6.1 introduces the counterfactual exercises, Section 6.2 presents the simulation results, and Section 6.3 assesses the robustness of our findings to alternative modeling assumptions.

6.1 Policy Space

This section introduces the three types of interventions we consider—taxes on filing an eviction, delays in the eviction process, and short-term rental assistance—and the economic and policy motivation for each. While each exercise is motivated by policies that U.S. cities and states have considered or implemented, our goal is not to model the exact effects of any specific policy, which will depend on market conditions and implementation details. Rather, the counterfactuals illustrate how different ways of regulating evictions might impact landlords and tenants. To make the three policy instruments comparable, our main results choose policy parameters that deliver the same reduction in eviction rates, and compare the impacts of the policies along other dimensions.

Eviction Tax. One approach to reducing eviction cases is simply to tax them. A tax could be motivated by costs from eviction that are not internalized by landlords or tenants.[31] The filing fee already charged to landlords by eviction courts varies across jurisdictions from near zero to several hundred dollars (Gomory et al., 2023). Our benchmark counterfactual policy adds a $250 tax to each eviction case, which is nearly equal to the lowest baseline fee ($287) in Cook County, IL, during our sample period. In the model, we simply increase the landlord's estimated filing cost by $250. The Delay and Short-term Rental Assistance policy parameters are chosen to match the eviction rate generated by this tax.

Delay, e.g. via Right-to-Counsel: Subsidized legal representation for tenants in eviction court is one of the most commonly proposed eviction protections in U.S. cities. Between 2017 and 2024, it was introduced in at least 17 cities, two counties, and 5 states (National Coalition for a Civil Right to Counsel, 2024). Studies of a recent roll-out of the program in New York City have found that one of its primary impacts is to lengthen court proceedings (Ellen et al., 2021; Cassidy and Currie, 2023; Collinson et al., 2024a), allowing the tenant more time in their unit before they have to leave.[32] We focus on this aspect of legal aid programs (abstracting away from the legal costs they also incur) and model an expected delay by adjusting δ_e, the rate at which tenants depart after an eviction is filed.

Short-term Rental Assistance (SRA). Both delays and taxes discourage evictions by increasing the effective cost of filing for the landlord. An alternative is to reward landlords for not evicting by paying tenants' owed rent. This is the idea behind rental assistance programs available in many U.S. cities.[33] While the implementation details vary across jurisdictions, these policies share a few common features: they are not always available due to limited funding; they pay up to a few months of back

[31] Recent research has demonstrated that eviction causally lowers incomes and increases homeless shelter and emergency room visits, all of which are costly for taxpayers (Collinson et al., 2024b). Thus, fiscal externalities are a straightforward rationale for intervention, and motivate a Pigouvian tax.

[32] New York City's right-to-counsel program also reduced the likelihood of a possession judgment and monetary judgment amounts, and may have generated additional legal costs due to longer and more involved court proceedings. Thus, in addition to delay, legal aid may impact the landlord's net costs of filing an eviction directly. Given that it is more difficult to estimate these costs, we focus on delay, which is unique relative to other policies we consider.

[33] For example, New York City offers "One Shot Deals," which are supposed to be available to tenants once if they are behind on rent. Chicago also offers short-term rental assistance.

rent; they are supposed to be one-time payments rather than repeated; and the owner retains the ability to evict the tenant if they default again.[34]

Implementing SRA requires us to choose several policy parameters. We do so in a way that reflects both common practice and the intent of these programs. In particular, we assume that SRA pays $A = 2$ months' owed rent directly to the landlord and that resources are limited, so that receipt of SRA upon application is uncertain (as in Evans et al., 2016). We also assume that all tenants are eligible while their balance exceeds 2 months, and that all eligible tenants apply. We model limited resources and other barriers to access through a monthly probability δ_a of receiving assistance while eligible, which we vary to adjust the program's effective generosity.[35] Tenants are eligible for the assistance once *per tenancy*. Finally, landlords cannot receive payments once they have filed an eviction, but there are also no restrictions on filing after SRA is received. Thus, SRA encourages forbearance through the promise of a future payment, but it does nothing to protect tenants after the payment has been received. This reflects the fact that in practice, landlords are usually able to evict tenants even if they have received SRA in the past.

Since each tenant can only receive SRA once, we add prior SRA receipt to the model as an additional state variable. This new state variable affects the landlord's eviction decision.

6.2 Results on Policy Counterfactuals

We re-solve the landlord's optimal stopping problem under each set of policy parameters and simulate outcomes for a large sample of tenants. See Appendix C.3 for implementation details. Unless otherwise specified, results are based on the model estimates from column (1) of Table 4.

Table 5 summarizes outcomes under the baseline policy in column (1) and the alternative policy regimes in the remaining three columns. At baseline, units are occupied 87% of months and collect just over 80% of owed rent, with the average tenant staying in the unit for 15.1 months. Most, but not all, evicted tenants would not consistently pay rent going forward—14.7% would miss fewer than 3 of the next 12 months of rent if they stayed and were not evicted during that time.

Introducing a \$250 tax reduces evictions by 5%. Thus, there is some scope for a moderately-sized tax to reduce evictions. The median tenant whose eviction is delayed or prevented enjoys 7 additional months in the unit. The tax is costly for landlords both directly due to the increased filing fee, and

[34] In some cases, these payments are given in exchange for landlords dropping eviction proceedings they have already initiated. This has raised concerns about gaming, which motivate us to focus on a policy that pays landlords before they file an eviction case.

[35] Abramson and van Nieuwerburgh (2024) study a closely related contract structure which they call "Rent Guarantee Insurance." Their focus is on whether such contracts would be provided by private markets in equilibrium, and whether a taxpayer-funded program could be revenue-neutral or -improving. We focus instead on how a taxpayer-funded program that is not universally available would impact landlords' incentives to evict.

Table 5 – Counterfactual Results

	Baseline	Tax	Delay	Short-Term Rental Assistance
	(1)	(2)	(3)	(4)
Eviction Rate (%)	2.27	2.15	2.15	2.15
Share of Rent Collected (%)	79.79	79.40	77.98	79.25
Tenure (months)	16.32	16.64	16.97	16.64
Occupancy Rate (%)	85.26	85.50	85.74	85.51
Gvt. Cost ($/unit-month)	–	-5.36	–	7.18
Landlord Cost ($/unit-month)	–	6.28	8.20	-4.10
Gvt + Landlord Cost ($/unit-month)	–	0.91	8.20	3.08
Compensating Rent Change ($)	–	9.01	11.90	-5.89
Tenure increase if > 0 (months)	–	7	4	7
Would have paid, evicted at baseline (%)	14.65	–	–	–
Would have paid, eviction delayed/averted (%)	–	21.57	11.77	16.92
Recovered, evicted at baseline (%)	–	1.37	0.53	1.18
Recovered, eviction delayed/averted (%)	–	10.09	4.95	7.50

Notes: Simulations use estimates reported in column (1) of Table 4. Statistics are means unless stated otherwise. The three policies reported yield the same eviction rates: a $250 eviction tax; an expected delay of 5 weeks; and a rate of rental assistance receipt of once every 45 months. Occupancy Rate is the fraction of months a unit is occupied, and eviction rate is per unit-month. Landlord Cost is the monthly transfer which equalizes the value of a vacancy under each counterfactual and Baseline. Compensating Rent Change is the equalizing change in contract rent if landlords evict optimally. Tenure increase if > 0 is the median change in tenure among tenants whose tenure increased relative to Baseline. A tenant Would have paid if, had they remained in the unit and not been evicted, they would have missed no more than 2 months' rent over the next 12. Recovered requires that, in the counterfactual scenario, the tenant stays in the unit, avoids eviction, and misses no more than 2 months' rent during the 12 months of the simulation following their baseline eviction month. A tenant is evicted at baseline if a case is filed while they are still in the unit; the tenant has their eviction delayed/averted if the filing date changes relative to the baseline scenario.

indirectly due to additional tolerated nonpayment. The expected costs for landlords are equivalent to \$6.28 per unit-month, while the tax would generate \$5.36 per unit-month in government revenue. Taking account of vacant months and landlord re-optimization, these landlord costs would lead to a \$9.01/month (about 1%) rent increase if fully passed through to tenants.

The tax also delays or prevents evictions for tenants relatively likely to pay in the future. We estimate that 21.6% of tenants with evictions delayed or prevented by the tax would resume paying in the sense defined previously (i.e., pay at least 10 of the next 12 months of rent), in contrast with 14.7% of all tenants evicted at baseline. Hence, evictions that are marginal under the tax are for better-paying tenants than the average evictee at baseline—a central difference between the tax and, as we show next, the Delay counterfactual.

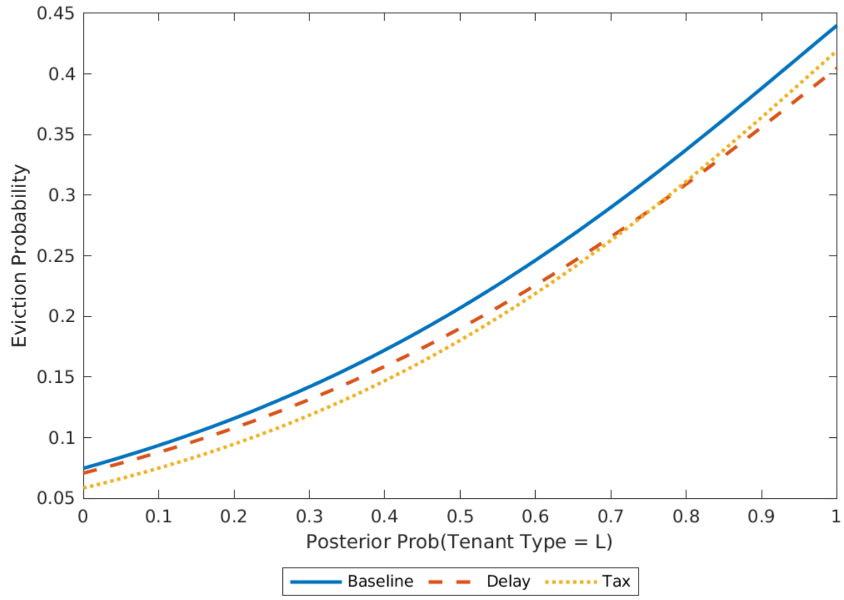
Compared to a tax, a delay that causes the same reduction in evictions generates higher costs for landlords and less tenant recovery. The delay equivalent to a \$250 tax is 5 weeks, which is large: New York City's right-to-counsel program increased the average case duration by about 2 weeks after adjusting for partial take-up (Cassidy and Currie, 2023; Collinson et al., 2024a).[36] Collected rent falls, and so the cost to the landlord is higher than under a tax (\$8.20/month compared to \$6.28). By delaying exit *after* an eviction is filed, a delay keeps the lowest-paying tenants in the unit for longer. As a result, delay discourages filing evictions on lower ability-to-pay tenants, and only 11.8% of tenants whose evictions are delayed or prevented would have resumed paying (compared to 21.6% under a tax and 14.7% of those evicted at baseline). The median tenant whose tenure increases spends 4 additional months in the unit.

Figure 4 illustrates how landlords differentially target evictions under our counterfactual policies. The figure shows how the probability of filing an eviction depends on a landlord's belief that a tenant is currently the lowest ability-to-pay type. Delay decreases eviction filings most for tenants whom the landlord believes are most likely to be the low type, as delay is most costly for these tenants. In contrast, a tax reduces the absolute eviction probability by a similar amount across posterior probabilities.

Short-term rental assistance produces outcomes between a tax and delay in terms of the costs to landlords and the types of evictions delayed or prevented. When the rate of SRA receipt is set to achieve the same reduction in evictions as a \$250 tax, about 10.5% of tenants receive assistance at some point. The program induces landlords to tolerate additional default, but unlike taxes or delays, SRA makes landlords better off. In terms of costs, SRA generates about \$7 per unit-month in government costs and about \$4 per unit-month in landlord benefits. The net cost of about \$3 per unit-month is higher than that of a tax (about \$1 per unit-month), but lower than that of delay (about \$8 per unit-month).

[36] The estimated effect of representation on case duration is 2 to 3 months, and the impact of the program on representation is 16 percentage points (Collinson et al., 2024a).

Figure 4 – Counterfactual Eviction Strategies



Notes: The figure plots landlord eviction probabilities against the posterior belief that the tenant is currently the lowest ability-to-pay type. For the eviction strategies shown in the plot, the landlord's posterior belief assigns all remaining probability mass to the medium type. We consider tenants renting at \$600-800/month in Chicago who are currently 2 months behind. The figure shows results for Baseline, Delay, and Tax counterfactuals reported in Tables 5 and 7.

The effects of all three counterfactual policies depend crucially on our estimates of model primitives. Table 6 shows that all three policies would achieve greater reductions in evictions at lower costs under an alternative set of model parameters where tenants' types change more frequently, and where the eviction filing cost C_e is changed to generate, given these more transitory types, the same eviction rate as at baseline.[37] Other estimated parameters are left unchanged. A \$250 tax on evictions is six times as effective at reducing evictions as it is under our actual parameter estimates; an expected five-week delay in the eviction process is twice as effective; and short-term rental assistance is 3.5 times as effective. Tenants whose evictions are prevented by policy also have greater scope for recovery in this alternative-parameter environment; for example, 22.5% of tenants whose evictions are delayed or prevented under the Delay counterfactual resume paying (i.e., would pay at least 10 of the next 12 months of rent) in this environment, as opposed to 11.8% under our actual parameter estimates. These greater recovery rates help illustrate why more evictions are elastic to policy when there is lower

[37] Specifically, we take a 2:1 convex combination of our estimated M and a uniform matrix $\tilde{M}$ with equal probability $1/K$ in each cell. We then solve for the C_e that generates the same eviction rate as under our baseline model estimates, which corresponds to a \$827.66 reduction in landlords' eviction costs. *Lower* eviction costs are needed to rationalize the baseline eviction rate under a less persistent tenant type process, because the (gross) expected benefit of eviction filing is lower when evicted tenants' types are more likely to evolve to be similar to other tenants' types in the future.

persistence in tenant types: the tenants with the highest default probability recover more quickly, so more of the tenants facing eviction are close to the margin of whether the landlord prefers to evict them or not.

Table 6 – Counterfactual Results with Less Persistent Tenant Types

	Baseline (1)	Tax (2)	Delay (3)	Short-Term Rental Assistance (4)
Eviction Rate (%)	2.27	1.52	2.02	1.85
Share of Rent Collected (%)	63.91	63.80	63.54	63.84
Tenure (months)	16.32	18.43	17.33	17.49
Occupancy Rate (%)	85.41	86.78	86.08	86.20
Gvt. Cost ($/unit-month)	–	-3.79	–	15.74
Landlord Cost ($/unit-month)	–	4.98	1.46	-13.70
Gvt + Landlord Cost ($/unit-month)	–	1.19	1.46	2.03
Compensating Rent Change ($)	–	8.76	2.56	-23.63
Tenure increase if >0 (months)	–	13	5	13
Would have paid, evicted at baseline (%)	25.41	–	–	–
Would have paid, eviction delayed/averted (%)	–	24.97	22.53	23.57
Recovered, evicted at baseline (%)	–	4.64	1.12	2.97
Recovered, eviction delayed/averted (%)	–	12.24	10.55	11.44

Notes: Counterfactual results under the alternative type process and eviction cost described in Section 6.2 of the text. Column (1) reports the Baseline case in the alternative-parameter environment; by construction, the baseline eviction rate is the same as the eviction rate under our actual estimated parameters. The remaining columns and all rows of the table are as described in Table 5.

Next, returning to our estimated model parameters, Table 7 describes the incidence of the three counterfactual policies across different types of tenants who face eviction at baseline. The first row of the table shows the type distribution of tenants evicted at baseline. Although roughly two-thirds (69.4%) of evicted tenants are the lowest ability-to-pay type in the month they are evicted, 27.7% are Type M and 2.9% are Type H. This reinforces our previous finding that while most evicted tenants would not recover if they remained in the unit, a significant minority of tenants would.

Beginning with the Tax in the first three columns of the table, the remaining rows describe how counterfactual eviction outcomes change for the high-, medium-, and low-type tenants who are evicted at baseline. An eviction tax leads to more delayed or prevented evictions for tenants with higher ability to pay—30% for Type H tenants, but only 10% of Type L tenants. Of these prevented or delayed evictions, 75% of Type H tenants avoid eviction entirely, and those who do not are evicted more than a year later. In contrast, 85% of Type L tenants whose eviction outcomes change are evicted later, by 3.3 months on average. These differences are also reflected in the share of tenants of each type who stay and resume paying (i.e., pay 10 of the next 12 months' rent after their baseline eviction date) under the policy. 16% of Type H tenants stay and resume paying, whereas almost no Type L tenants do. This is also reflected in the average increase in tenure for tenants of each type.

The next three columns of Table 7 repeat this analysis for Delay. In contrast to the Tax, Delay delays or prevents a larger share of evictions (12%) for lower ability-to-pay tenants, but far fewer for Type H (6%) and Type M (9%) tenants. Among tenants whose eviction outcomes change, the shares of each type recovering and avoiding eviction entirely are similar to those under a Tax. However, Delay increases tenure much more for Type L tenants because they benefit equally from the 1.2-month expected delay after the eviction is filed. Thus, Delay reduces eviction in part by delaying evicted tenants' exit, in addition to encouraging forbearance and recovery. This contributes to delay being an especially costly way to reduce evictions for landlords.

The last three columns of Table 7 show analogous results for short-term rental assistance, which has intermediate distributional impacts compared to a Tax or Delay. Compared to the other policies, large shares of Type M (20%) and L (13%) tenants have their evictions delayed or prevented, but relatively few Type H tenants do (14%). This is driven by the fact that landlords can receive SRA only when their tenant has missed at least two months' rent, which is much more likely for lower ability-to-pay tenants.

While the policies studied here do not change eviction outcomes for the majority of tenants, another implication of our results is there may be scope for targeted policies to prevent evictions at lower cost than universal policies. If tenants with favorable odds of recovery can be identified, a more significant share of evictions might be prevented. For example, we estimate that among the 3% of evicted tenants who are Type H, of those whose evictions are delayed or prevented, 40-50% would have stayed at least one year and paid at least 10 months' rent. The corresponding value for Type M tenants facing eviction is about 15%.[38] Whether policymakers can successfully identify and target policies towards these tenants is an empirical question that we leave for future work.

[38]In Table 7, this is calculated for each tenant type and policy by dividing the share of tenants who "Recovered (12 mo.)" by the sum of the shares of tenants Evicted Later On and Never Evicted.

Table 7 – Counterfactual Outcomes by Tenant Type

	Type at Filing, Baseline								
	Type H	Type M	Type L	Type H	Type M	Type L	Type H	Type M	Type L
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Share of Evicted Tenants, Baseline (%)	2.93	27.72	69.35	2.93	27.72	69.35	2.93	27.73	69.34
	Tax			Delay			SRA		
Evicted Same Month (%)	69.76	80.11	89.67	94.40	90.99	88.31	86.32	80.27	86.74
Evicted Later On (%)	6.98	12.65	8.82	1.40	6.02	9.95	3.37	12.69	11.17
Never Evicted (%)	23.27	7.24	1.52	4.20	2.99	1.74	10.23	6.98	1.92
Evicted Earlier (%)							0.08	0.06	0.17
Mean Months Later	16.70	8.22	3.30	14.96	7.59	3.34	14.97	8.24	3.37
Recovered (12 mo.)	15.88	3.00	0.10	2.86	1.33	0.12	6.54	2.99	0.14
Time in Unit	4.45	1.46	0.30	1.90	1.73	1.40	1.81	1.43	0.37

Notes: This table summarizes how outcomes change under each counterfactual policy for tenants evicted in the baseline simulation. Each column corresponds to the tenant's type in the month they are evicted. The first row records the proportion of tenants of each type. The remaining rows report mean outcomes for these tenants under an alternative policy relative to baseline. Tax, Delay, and SRA correspond to the counterfactual policies in Table 5. A tenant is evicted later on if they are evicted in a later month than at baseline. A tenant is never evicted if they move out prior to eviction. Mean months later is calculated for tenants evicted later on. A tenant Recovered if, under the alternative policy, they stay in the unit for at least 12 months after the eviction is filed at baseline, avoid eviction, and miss at most 2 months' rent during that time. Time in unit is the mean number of additional months each tenant occupies the unit.

6.3 Results from Alternative Specifications

This section assesses the robustness of our main findings to three sets of alternative modeling assumptions. First, we consider a full pass-through benchmark in which rents rise to compensate landlords for the costs of additional eviction protections. Second, we consider how our results would change if stronger eviction protections directly impacted payment rates. Finally, we present results from our "full-information" model in which the landlord observes the tenant's current type each month. Results tables are in Appendix Section D.

Full Cost Pass-Through. A natural concern with policies that make eviction more difficult is that some of the costs to landlords will be passed through to tenants through higher rents. To understand how such price responses would impact our main predictions, we consider a "full pass-through" benchmark in which landlords raise rents so that the value of a vacancy remains at its baseline value, given that landlords evict optimally under the new rents. Other primitives are held fixed, including the payment process, tenant departure rates, and vacancy lengths.[39] Appendix Table 9 shows that landlord eviction decisions are nearly unchanged if rents adjust to compensate them for each policy—eviction rates and other outcomes (other than landlord costs, which are zero) are almost identical to those in Table 5. The is in part because both the costs and the benefits to evicting a tenant in many cases scale with the contract rent, such as losses from nonpayment and the cost of a vacancy.

Moral hazard. Our main results hold the payment process fixed under alternative policies, ruling out strategic default. We consider how moderate changes in payment rates induced by our counterfactual policies would affect our conclusions. Because we estimate that Type M tenants' payment behavior has substantially more variability than other types' payment, we consider a model in which Type M default probabilities are the most responsive to policy.[40] Specifically, we simulate a reduction in the middle type's payment rate equal to half the reduction (7%) that we estimate occurs post-filing at baseline, and re-solve for the landlord's optimal eviction behavior under the alternative policy and payment process.[41] Appendix Table 10 presents the results. Evictions only fall by about 1%. Unsurprisingly, if

[39]This rules out price-sensitivity of default because tenants are more likely to face liquidity constraints at higher rents. This is more plausible when compensating rent changes are small, as they are under our counterfactuals. Our full pass-through scenario also abstracts from the effects of strategic interactions between landlords, for example as studied in Calder-Wang and Kim (2023).

[40]One endogenous default model consistent with this assumption is found in Chatterjee et al. (2023), where the magnitude of the derivative of payment rates θ with respect to a "price" of default is increasing in $\theta(1-\theta)$.

[41]Strategic default could also induce complex strategic interactions between the landlord and the tenant. The alternative payment process is a "reduced form" for the possibility that tenants' payment behavior responds to a market-wide change in eviction rates rather than a tenant's own landlord's behavior.

tenants are less likely to pay in response to the policy, these behavioral responses will partially offset the policy's disincentive for landlords to file.

Full-Information Model. Our baseline model assumes landlords only observe a tenant's payment history, but do not have other information about the tenant's future ability to pay. To assess how this informational assumption impacts our findings, we simulate counterfactuals under the full-information model assuming the landlord perfectly observes the tenant's current type each month, using the parameter estimates reported in column (3) of Table 4. Appendix Tables 11 and 12 repeat the iso-eviction exercise under this model, finding delay and rental assistance policies that match eviction rates under a \$250 eviction tax.

Qualitatively, the main takeaways are similar in the full-information and baseline models, but there are quantitative differences. Eviction rates fall by 3% in response to a \$250 eviction tax—less than under the baseline model—and fewer tenants would be able to pay if allowed to stay in the unit. Only a 0.6-month delay (instead of 1.2 months) is needed to produce this smaller reduction in eviction rates, while a slightly less generous rental assistance policy (eligible tenants have a 2.0% probability of receipt each month, instead of 2.2%). These differences arise because the full-information model interprets evictions as reflecting the tenant's true underlying type. Almost all (93.7% of) evicted tenants have the lowest ability-to-pay, meaning that the landlord is rarely close to being indifferent between evicting and not evicting. A tax or rental assistance policy is therefore less likely to change their optimal decision. In contrast, a delay is especially costly if all evicted tenants are Type L, since the landlord expects to receive very little rent until the tenant moves out.

7. Conclusion

We collect novel, lease-level ledger data—records not previously available to researchers—to study what drives landlords' eviction decisions and how those decisions respond to eviction prevention policies. The data yield several insights about the relationship between nonpayment and the landlord's eviction decision. First, rent nonpayment rates can be high for landlords operating in low-income rental markets, with half of tenants in our sample falling at least 30 days behind at some point in their lease. Second, delinquency is often temporary, with most delinquencies not resulting in eviction, implying that landlords tolerate substantial arrears. Third, observed filing patterns are consistent with landlords updating beliefs about a tenant's evolving risk of non-payment. Taken together these patterns suggest that landlords face a trade-off between initiating a costly eviction or waiting to learn whether a tenant can continue paying.

Guided by this evidence, we propose and estimate a dynamic discrete choice model of the landlord eviction decision in which landlords weigh the costs of eviction against the uncertain benefit from removing a tenant who may recover. We estimate that landlords' eviction costs are equivalent to 2-3 months of rent. These costs, combined with our estimates of tenant payment dynamics and their implications for landlord learning, lead landlords to often wait to evict until the prospects of recovery are low.

Our results suggest that uniformly applied policies can generate additional forbearance for tenants, but they do not prevent most evictions. At the same time, we find that about 15% of those who are evicted would have resumed paying rent, indicating that more targeted interventions could prevent evictions at a lower cost than the broad-based interventions we study. Lastly, we find a clear trade-off between specific policy instruments. Procedural delays generated by interventions in the eviction process itself provide more protection to tenants who are least likely to resume regular payments (and may be in the greatest financial distress), while direct financial incentives for landlords in the form of taxes or subsidies have lower costs per eviction prevented.

Our analysis suggests several directions for future work. One is determining the full welfare and distributional implications of tenant-protection policies and their optimal design. Another is understanding the underlying income, employment, and other financial drivers of rental default. Third, an important open question is the extent to which tenant-support interventions can be targeted towards tenants who will resume payment in the future. Finally, our results pertain to a specific context and sample of rental units. While the market we study is similar to urban areas representing a large portion of national eviction filings, and we argue it is not an outlier on market conditions or the regulatory environment, there is substantial heterogeneity in these local conditions. Additional work on the drivers of eviction in other rental markets, perhaps using similar data and methods as those developed in this paper, would be valuable.

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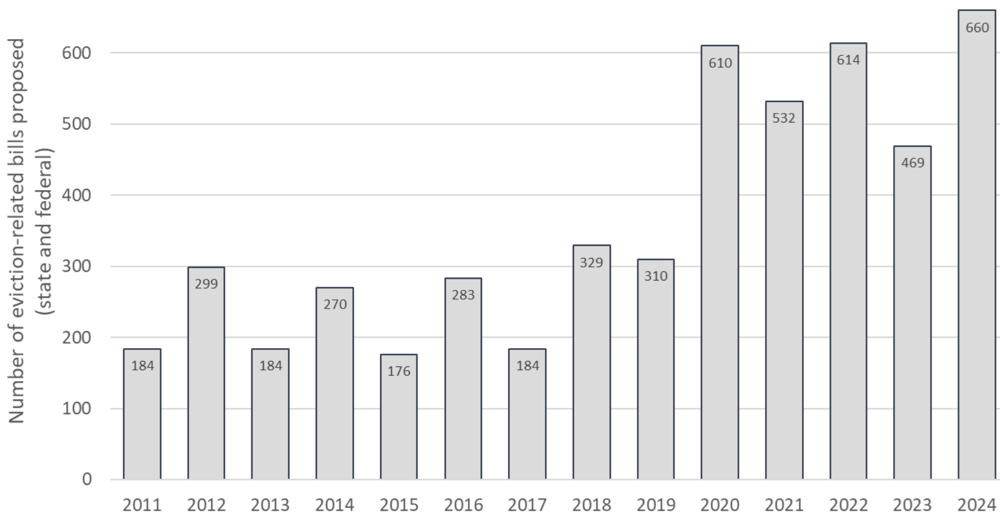
A. Additional Descriptive Evidence

Appendix Figure 1 – Number of Eviction-Related Bills Proposed (Federal and State).



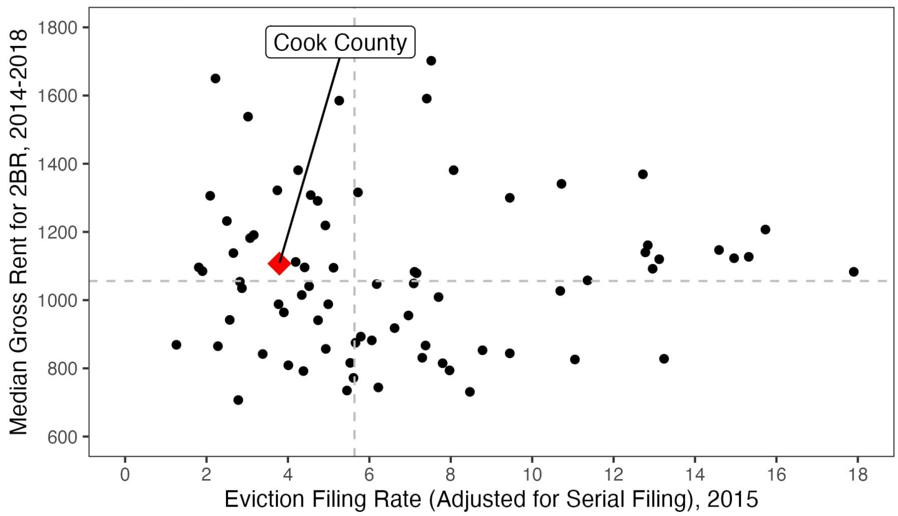
Notes: This figure shows the number of bills proposed at the federal and state level each year that include the word "eviction." The sample was constructed by using "eviction" as a search term on the website Billtrack50.com, which collects and makes searchable the full text of federal and state bills.

Appendix Table 1 – Cook County Sample Comparison

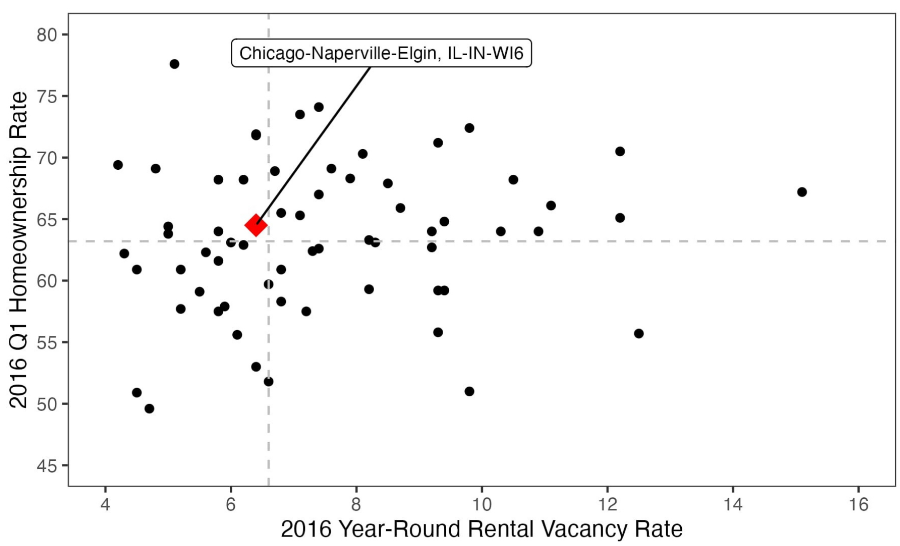
	Analysis Sample ZIPs	Estimation Sample ZIPs	Cook County ZIPs
Median Rent ($)	954	952	1,176
Median Household Income ($)	2,800	2,544	4,304
Annualized Rent Growth (%)	1.70	1.61	2.42
Unemployed (%)	12.7	13.5	7.5
Poverty (%)	24.3	24.0	14.5
Annualized Eviction Rate (%)		5.64	3.39
White (%)	18.5	13.5	42.0
Black (%)	58.3	61.2	24.7
Asian (%)	3.0	3.8	8.3
Hispanic (%)	20.5	21.9	25.3
Lease Counts	5,809	1,814	

Notes: This table compares ZIP code characteristics for tenancies in our analysis sample and model estimation sample to Cook County averages. Sample construction is described in section 2. The means in columns (1) and (2) are weighted by the number of sample tenancies in each ZIP code. The means in column (3) are weighted by ZIP population. For eviction rates, we divide eviction counts from 2015 to 2019, calculated through a similar process to that in Collinson et al. (2024b), by the ACS 5-year estimate of total renter-occupied housing units (Manson et al., 2023b). All other data are from the 2015-2019 5-year ACS.

Appendix Figure 2 – Comparing Rental Housing Markets.



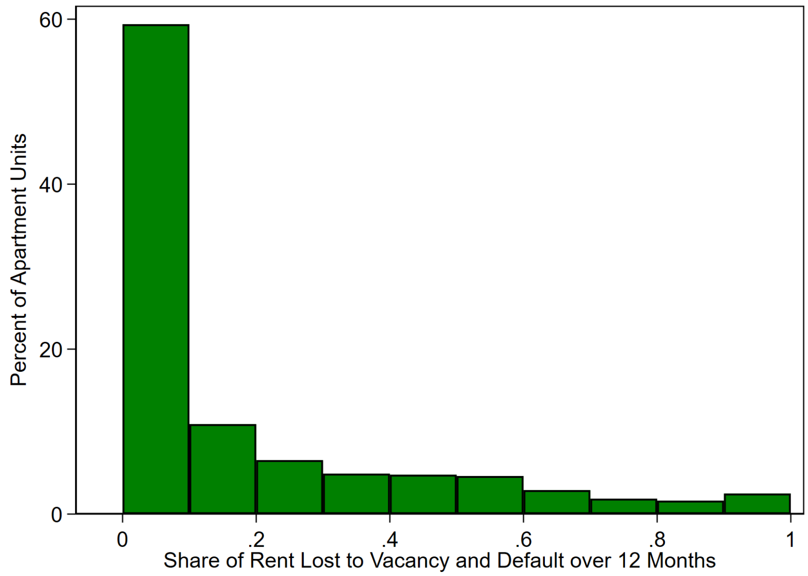
(a) County Level Eviction Filing Rates and Gross Rent



(b) MSA Level Vacancy and Homeownership Rates

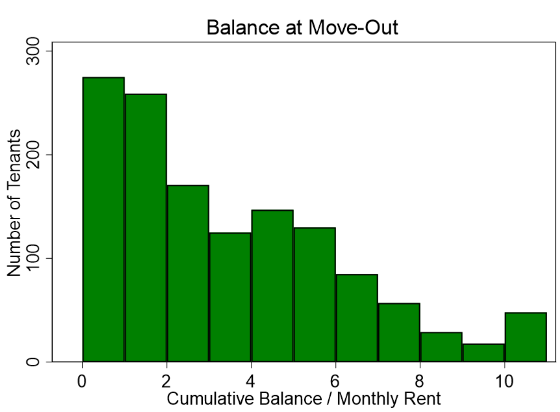
Notes: The top figure shows county-level eviction filing rates and county-level median gross rents for two-bedroom apartments. Eviction filing rates are computed for the year 2015, using public data from the Eviction Lab, which relies on court case filings and adjusts them for serial filing on the same household to compute what they refer to as "households threatened with eviction" (The Eviction Lab, 2019). For clarity, we refer to this as the "eviction filing rate (adjusted for serial filing)." County-level median gross rents are obtained from the 2014-2018 ACS (Manson et al., 2023a). The sample is restricted to the 76 counties with population above 300,000 and for which data on the eviction filing rate was available from Eviction Lab. The bottom figure shows a scatterplot of MSA-level homeownership rates and MSA-level vacancy rates. Both variables are obtained from the CPS Housing and Vacancy Survey for 2016, which is published for the 75 largest MSAs, all of which are included in the sample (U.S. Census Bureau, 2024a,b). Dashed lines represent medians computed across all locations included in the sample.

Appendix Figure 3 – Rent Revenue Lost to Vacancies and Defaults

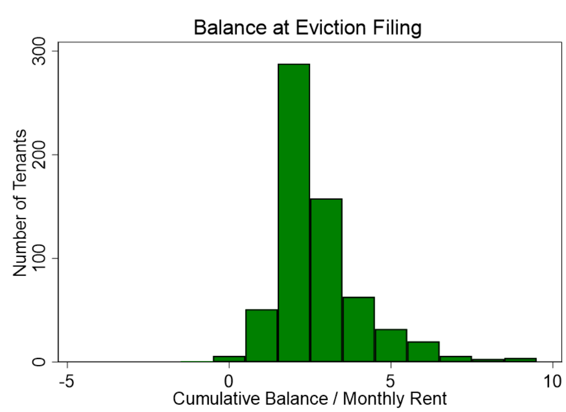


Notes: The figure plots the distribution of the share of rent that goes unpaid due to either vacancy or nonpayment, across all unit-years in our data (i.e., all units in all possible 12-month windows). For example, a share lost of 0% corresponds to a unit occupied for all twelve months by tenants who accumulated zero balance at end of the 12-month window; a share lost of 100% corresponds to a unit that received zero rent payment over an entire 12-month period. Approximately 30.6% of all unit-years have a share lost of 0%. The sample is restricted to units that appear for all 12 months of a given 12-month rolling window, excluding units that are censored in the middle of the window due to, for example, a landlord's purchase or sale of a building in the middle of the window.

Appendix Figure 4 – Nonpayment Risk, Model Estimation Sample



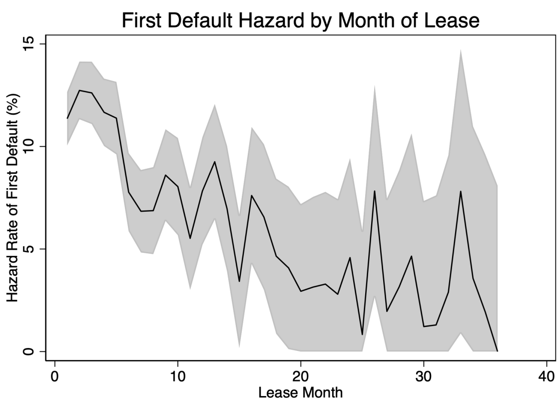
(a) Months Behind at Move-Out



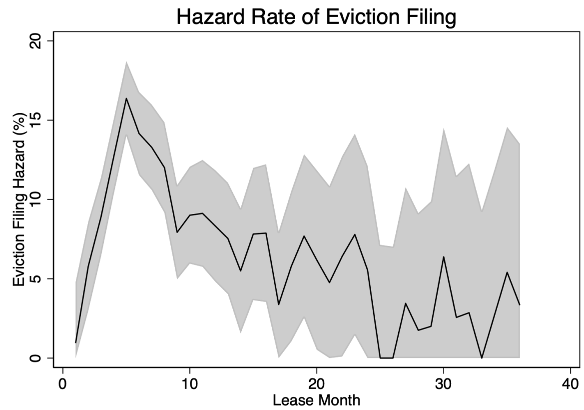
(b) Months Behind at Eviction Filing

Notes: This figure repeats the analysis in Figure 1, here restricting to the model estimation sample described in Section 2.

Appendix Figure 5 – Default and Eviction over Time, Model Estimation Sample



(a) First Default Hazard by Month of Lease



(b) Eviction Filing Hazard for Delinquent Tenants

Notes: This figure repeats the analysis in Figure 2, here restricting to the model estimation sample described in Section 2.

Appendix Table 2 – Descriptive Statistics on Tenancies and Nonpayment, Model Estimation Sample

	All (1)	Not Evicted (2)	Evicted (3)
Rent ($)	804	807	799
Share of Rent Collected (%)	82.6	92.5	60.3
Balance at Moveout ($)	1,952	894	3,931
Evicted (%)	34.8	0.0	100.0
Months Tenure	14.3	15.2	12.5
Months Vacant after Moveout	2.2	1.8	2.6
Tenancies	1,814	1,182	632
Units	1,261	938	542

Notes: This figure repeats the analysis in Table 1, here restricting to the model estimation sample described in Section 2. Unlike in Table 1, results are not presented separately by voucher status because the model estimation sample is limited to tenants without a voucher.

Appendix Table 3 – Scope for Recovery, Model Estimation Sample

Statistic	Leases (1)	Ever Recovered (%) (2)	Share Paid (%) (3)	Tenancy (4)	Stayed 12mo Paid 10mo (%) (5)
New Tenants	1814	–	81.2	13.9	33.8
1 month behind	1,195	34.3	70.0	8.7	16.1
2 months behind	817	9.5	50.7	5.4	6.2
3 months behind	585	2.2	31.5	3.7	2.2
4 months behind	392	0.8	21.3	3.2	1.5
5 months behind	242	0.4	21.9	3.2	2.5
6 months behind	157	0.6	24.4	3.2	2.5

Notes: This table repeats the analysis in Table 2, here restricting to the model estimation sample described in Section 2.

B. Evidence on Tenant Screening

For a subset of our data in 2019, we observe detailed tenant-screening reports that landlords use to decide which applicants to approve or deny for a new lease. These reports include past evictions, credit histories, income, and criminal backgrounds. These reports are available for 477 applications that were approved and converted into leases in our ledger data, of which 332 are for non-voucher tenants, as well as 1,263 other non-converted applications for the same units. For 802 of the non-converted applications, we also observe whether the applicant was rejected, or was approved but did not take the apartment.

We first establish that the data in these reports are relevant for screening decisions. We regress an indicator for whether the applicant was approved and moved in on covariates drawn from the screening reports, together with indicators for missing data in the reports and fixed effects for landlord and time.[42] We present results in Table 4. Higher (i.e., better) FICO scores, higher income, lower debt service, and the absence of prior eviction records all significantly predict greater odds of approval and move-in. While these variables do not fully predict the outcome—the R-squared is 12.9%—these results suggest the data in these reports are relevant for landlords' screening decisions.

We next explore whether these screening variables predict (non)payment among the set of tenants who are accepted and move in. If these variables predict payment among approved tenants, it suggests landlords have scope to make their screening criteria stricter in order to decrease realized nonpayment, if (for example) policy were to make evictions a more costly or less effective tool for managing default risk ex-post. On the other hand, if these variables are uninformative about risk among tenants whom we observe moving in, it suggests landlords would have difficulty tightening their screening criteria,

[42]We focus on the "moved in" outcome because it is observable for a greater number of applications in the data, assuming all tenants who moved in were approved. Results are similar if we instead use an "approved" outcome for the subset of applications where we see the landlord's actual approval or rejection decision.

Appendix Table 4 – Tenant Screening

	Baseline Mean	*Dependent Variable:* Application Accepted, Tenant Moved In			
	(1)	(2)	(3)	(4)	(5)
Fico Score (100 points)	5.80	0.0217	(0.0164)	0.0275	(0.0165)
Fico Score Missing	28.16%	0.166	(0.0982)	0.189	(0.0987)
Income ($1000/mo)	1.92	0.0277***	(0.00717)	0.0248***	(0.00720)
Income Missing	4.25%	-0.0395	(0.0537)	-0.0491	(0.0538)
Debt Payments ($1000/mo)	0.91	-0.0193**	(0.00742)	-0.0183*	(0.00742)
Debt Payments Missing	0.06%	-0.287	(0.424)	-0.279	(0.424)
Any Housing Collections	19.25%	-0.235***	(0.0261)	-0.242***	(0.0261)
Any Felony Record	5.40%	-0.0351	(0.0490)	-0.0355	(0.0491)
Any Misdemeanor Record	9.31%	-0.0614	(0.0421)	-0.0603	(0.0421)
Firm FEs		Yes		Yes	
Month FEs				Yes	
Observations	1,740	1,740		1,740	
Adjusted R-squared		0.122		0.129	

Notes: Estimates from OLS regressions predicting whether rental applicants are approved and move in as a function of applicant characteristics. The estimation sample is all observed rental applications between April 2019 and February 2020, when variables drawn from landlords' tenant screening reports are available. The first column reports the baseline mean value of each covariate. The next two columns report coefficient estimates for those covariates controlling for unit fixed effects. The last two columns add month fixed effects. Housing collections indicate prior housing court records that involve a money judgment against the tenant, usually generated by an eviction case. Debt payments are monthly and are aggregated from consumer credit reports. Standard errors are in parentheses. *p<0.05; **p<0.01; ***p<0.001

even in a counterfactual policy environment that could plausibly incentivize tighter screening.

Because our sample with both screening data and ledger data is small relative to the number of variables in the screening data, we summarize the screening variables using an approval score $\hat{a}$, defined as the fitted values from the screening regressions in Table 4. We then regress measures of ex-post performance in the ledger data on $\hat{a}$. The estimation sample is restricted to non-voucher tenants, given how voucher tenants' payment performance is in part ensured by their voucher subsidy.

We present estimation results in Table 5. Because $\hat{a}$ is a generated regressor, we report bootstrapped standard errors. Across various measures of payment, including an indicator for any default, a count of number of months defaulted, and a share of total rent paid, over both a 3- and 6-month horizon after move-in, we find no significant evidence that the screening variables summarized in $\hat{a}$ are predictive of performance. We reach similar conclusions when testing the joint significance of all screening variables included in Appendix Table 4, though one of the six joint tests is marginally significant ($p = 0.093$).

Appendix Table 5 – Payment Performance and Tenant Screening Data

	First 3 Months			First 6 Months		
	Share Paid (1)	Any Default (2)	Number of Defaults (3)	Share Paid (4)	Any Default (5)	Number of Defaults (6)
Acceptance Probability	0.099	0.003	-0.296	0.088	0.137	-0.227
	(0.151)	(0.271)	(0.441)	(0.18)	(0.341)	(0.986)
Observations	332	332	332	332	332	332
Adjusted R-squared	0.017	0.029	0.029	0.043	0.018	0.039
Joint Test of All Screening Variables (p-val)	0.286	0.551	0.205	0.093	0.127	0.249

Notes: Estimates from OLS regressions predicting delinquency measures as a function of a tenant's Acceptance Probability. Acceptance probabilities are fitted values from the screening regressions in Table 4. Regressions control for landlord fixed effects, time fixed effects, move-in month fixed effects, and the rent level (to capture unit characteristics). The estimation sample is all tenants in the ledger data for whom we observe a detailed tenant-screening report at the time of lease application. Standard errors, in parentheses, are calculated by bootstrap to account for estimation error in the acceptance probability. The joint test p-values are from a separate regression that includes all screening variables from Appendix Table 4, not including the Acceptance Probability itself.

Given the small sample size and limited time frame, these results are only suggestive. However, evidence from other contexts is also consistent with the conclusion that landlords have limited scope to tighten their screening criteria in response to policy interventions that make evictions more difficult. Collinson et al. (2024a) study the roll-out of New York City's right-to-counsel universal legal aid program and find that, while it delayed evicitons proceedings by roughly 2 months on average, it did not change landlord screening based on income, credit score, or other credit report variables.

Related to screening, another potential landlord response to eviction-prevention policy is to increase security deposits. While we cannot rule this out as a response in some markets, data from the low-income markets we study suggest that landlords in these markets also have limited scope to require security deposits. Only 15% of leases in our analysis sample have security deposits, and fewer than 1% of leases in our model estimation sample do. We also sometimes observe instances where a landlord requests a security deposit, does not receive it, and allows the tenant to begin their lease anyway. This absence of security deposits is especially striking given the considerable nonpayment risk in our sample. These patterns, together with other evidence on severe liquidity constraints among lower-income and lower-credit score populations in the U.S. (Morduch and Schneider, 2017; Keys et al., 2023; Lloro et al., 2024), suggest landlords may have limited scope to require security deposits among their pool of applicants.

C. Model, Estimation, and Counterfactual Details

C.1 Full-Information Model

The baseline model assumes the landlord only observes the tenant's payments, but has no additional information about their underlying type. To assess robustness of our findings to this assumption, we consider an alternative model in which the landlord perfectly observes the tenant's type each month. This simplifies the state space for their dynamic problem dramatically; it is simply (ω_t, y_t, b_t). The econometrician's posterior about the tenant's type (or equivalently, the landlord's type) is more complicated, however. This posterior depends on both the payment history and the timing of landlord eviction decisions, taking into account both the possible histories of ω_t and the fact that the landlord chose not to evict the tenant knowing ω_t.

Fortunately, the fact that the owner's posterior has only a few support points simplifies the econometrician's problem. We can recursively update the posterior distribution over ω_t given the payment history and the fact that the owner has not evicted in the past. Specifically,

$$\pi_t(k) \equiv Pr(\omega_t = \omega_k \mid y_t, e_{t-1} = 0; \pi_{t-1})$$

We can think of belief updating as having three steps between π_{t-1} and π_t:

1. The landlord chose not to evict ($e_{t-1} = 0$):

$$\tilde{\pi}_{t-1}(k) \equiv Pr(\omega_{t-1} = \omega_k \mid \pi_{t-1}, e_{t-1} = 0) = \frac{Pr(e_{t-1} = 0 \mid \omega_t = \omega_k)\pi_{t-1}(k)}{\sum_{l=1}^{K} Pr(e_{t-1} = 0 \mid \omega_t = \omega_l)\pi_{t-1}(l)}$$

2. Markov transition:

$$\hat{\pi}_t = \tilde{\pi}_{t-1} M$$

3. Payment is realized, $y_t = 1$ (similar for $y_t = 0$):

$$\pi_t(k) \equiv Pr(\omega_t = \omega_k \mid y_t = 1; \hat{\pi}_t) = \frac{Pr(y_t = 1 \mid \omega_t = \omega_k)\hat{\pi}_t(k)}{\sum_{l=1}^{K} Pr(y_t = 1 \mid \omega_t = \omega_l)\hat{\pi}_t(l)} = \frac{\omega_k \hat{\pi}_t(k)}{\sum_{l=1}^{K} \omega_l \hat{\pi}_t(l)}$$

The likelihood that the econometrician observes an eviction is then

$$\sum_{k=1}^{K} \pi_t(k) Pr(e_t = 1 \mid \omega_t = \omega_k).$$

C.2 Estimation

This section provides further details on the computation of the value function. We drop i and t subscripts and instead use π and π' to refer to current and next period values of each variable, respectively.

Learning model. We discretize the state space (π, b) and solve for the value function in the occupied state at each grid point. Conditional on (π, b), the current period's payment realization $y \in \{0, 1, 2\}$ does not affect the choice-specific conditional value functions $\bar{v}^{e=1}, \bar{v}^{e=0}$ or, therefore, choice probabilities. Beliefs are evaluated at 0.01-spaced grid points in each dimension on Δ^{K-1}. Further reducing the grid spacing had little impact on our estimates. We allow tenant balances to take values $b \in \{0, 1, 2, 3, 4\}$ using the accounting conventions in Appendix F. A tenant's balance is bounded below at $b = 0$ due to the payment model and our accounting conventions. We assume the balance remains capped at $b = 4$ if there is additional default beyond 4 months' rent, but that any repayment when $b = 4$ reduces the balance to $b' = 3$. Repayment is extremely rare for tenants with balances above 3 months. Partial payment is also rare in the data, as are payments above twice the monthly rent, so this discretization is a good approximation to actual payment patterns.

For a guess of the parameters Γ, we solve for the choice-specific conditional value function that includes the rent payment $\bar{v}^{e=0}(\pi_g, b_g; s = o, y = 0)$ evaluated at every grid point using the bellman operator:

$$\bar{v}^{e=0}(\pi_g, b_g; o, 0) = -c+$$
$$\beta\left[\delta_d V_v + (1-\delta_d)\left(\sum_{y'\in\{0,1,2\}} p(y' \mid \pi_g, b_g)\log\left(\exp\left(\sum_{g'} w_{g'}(\pi')\bar{v}^{e=0}(\pi_{g'}, b'; o, y')\right) + \exp\left(\sum_{g'} w_{g'}(\pi')\bar{v}^{e=1}(\pi_g, b'; o, y')\right)\right) + \gamma\right)\right]$$

In the above, the next-period payment probabilities $p(y' \mid \pi_g, b_g)$ are given by equation 12; the next-period beliefs are given the payment realization by equation 6; next-period balance b' depends on the current balance and next-period payment realization; and the weights $w_{g'}(\pi')$ approximate the value function at next-period beliefs by linear interpolation of values at nearby grid points g'. The choice-specific value from evicting next period is given by equation 10. The value of a vacancy is calculated analogously, integrating over the landlord's posterior after the first month's payment realization.

Full-Information model. In the full-information model, when a unit is not vacant the relevant state is simply (ω, b), the tenant's current type (which is observed by the landlord) and balance. This state space has a small (finite) number of elements, so the value function is straightforward to compute. Instead, the primary challenge is forming the likelihood accounting for the landlord's information about the tenant's type, which the econometrician lacks. We use the updating rules from Appendix Section C.1 to integrate over the tenant's (equivalently, landlord's) type. The likelihood is then based on the econometrician's posterior belief about the tenant's type, calculated recursively from the payment history and whether the landlord has filed an eviction, which determines that period's payment and eviction probabilities.

For all specifications, we search over the parameter space to maximize the likelihood defined in equations 11 and 12 using fmincon in MATLAB, which uses an interior point algorithm. The value function is solved using value function iteration on the Bellman operator, with a supnorm tolerance of 10^{-12}.

C.3 Counterfactuals

This subsection further describes how we solve for counterfactuals in Table 5, Table 7, and related exhibits.

We simulate 100,000 tenancies. The observable type of each tenancy is drawn in proportion to the number of units of each observable type in the data (i.e., in each category of rent level $\times$ landlord as described in Section 5.1). For the main counterfactuals, we draw initial tenant payment types, type changes, rent payments, tenant exits, and landlord taste shocks based on the baseline model estimates in column (1) of Table 4, and on the exit and vacancy fill rates in Appendix Table 6. We simulate each tenancy for 72 months, by which time more than 99% of tenants have exited either through eviction or otherwise. Random draws are held fixed across all counterfactuals.[43] The robustness exercises modify the relevant distributions as described in the main text.

We then re-solve the landlord's eviction problem in each counterfactual environment, and compute outcomes for each tenancy. We begin with a \$250 eviction tax as described in Section 6.1, modeled as an increase in the landlord's eviction filing cost C_e. We recompute the landlord value function and counterfactual eviction choices. Many of the outcomes described in our counterfactual tables—the share of rent collected, occupancy rate, changes in tenure—can then be calculated directly by aggregating across simulated leases under these counterfactual eviction choices. Other counterfactual outcomes require additional steps to calculate, and are described below.

In many cases, we report results from Delay and SRA policies that yield the same reduction in evictions as the \$250 tax. For the Delay counterfactual, we solve for the value of δ_e that leads to the same change in the eviction rate as under the \$250 tax. To do so, for a candidate value of δ_e, we compute counterfactual outcomes just as for a tax. We then solve for the iso-eviction value of δ_e in an outer loop. For the SRA counterfactual, we similarly solve for the monthly probability of SRA receipt among eligible tenants that leads to the same change in the eviction rate as we found under the \$250 tax.[44] With regard to timing, we model SRA receipt as occurring in the middle of each period, after payments are realized but prior to landlord eviction decisions. Because tenants can only receive SRA once, we expand the state space to include an indicator for prior SRA receipt and recompute landlord value functions, which now depend on whether the tenant has already received assistance.

For all counterfactual outcomes, we aggregate across observable unit types by weighting the simulated tenancies by the average duration of one lease and one subsequent vacancy in each observable unit type, normalized by the durations in the baseline simulation. This weighting means that our reported statistics represent long-run averages across a fixed set of units in different counterfactuals environments. This weighting also assumes that the pool of incoming tenant types is invariant to the counterfactual environment; this is likely to be conservative vis-à-vis our findings that moderately

[43] Any draws from the "latent future" after tenant exit in the baseline (no counterfactual) case are used to determine lease paths in counterfactuals that lead to delayed exit.

[44] An alternative calibration would solve for Delay and SRA parameters that lead to the same total cost as the \$250 tax. Under this approach, the Delay and SRA policies lead to much smaller changes in eviction rates than the Tax, as reported in Appendix Table 8.

scoped policies do not prevent most evictions, as it implies low types who move out on their own after their evictions are counterfactually prevented do not worsen the pool of new tenants.

To calculate the costs of counterfactual policies reported in Table 5 (and in robustness versions of that table), we first compute the total change in government expenditure or revenue per unit-month. In the case of an eviction tax, for example, this is the expected monthly tax receipt per unit. This is reported as "government cost." We next solve for a monthly unconditional transfer to landlords that would equalize the value of a *vacancy* with the landlord's value at baseline. This is reported as "landlord cost." Given landlord preferences, this unconditional transfer does not change the landlord's eviction decision. As an alternative measure of landlord cost, we solve for what change in rent would equalize the value of a vacancy with the landlord's value at baseline. For this exercise, we allow landlord eviction decisions to reoptimize, since the rent determines the opportunity cost of a vacancy. For purposes of this exercise, we assume the rent change (which we estimate to be somewhat modest in all counterfactuals) does not affect tenant behavior. This is reported as "compensating rent change."

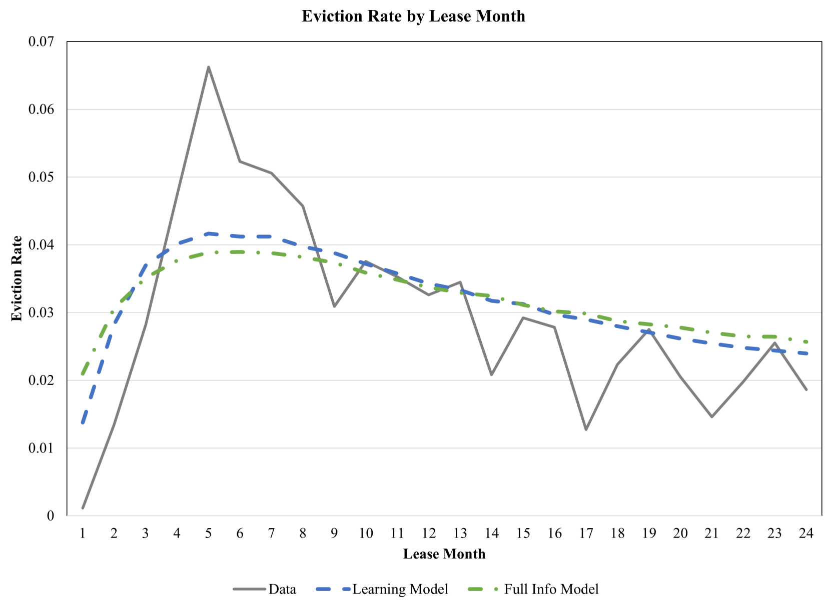
D. Additional Model Estimation Results

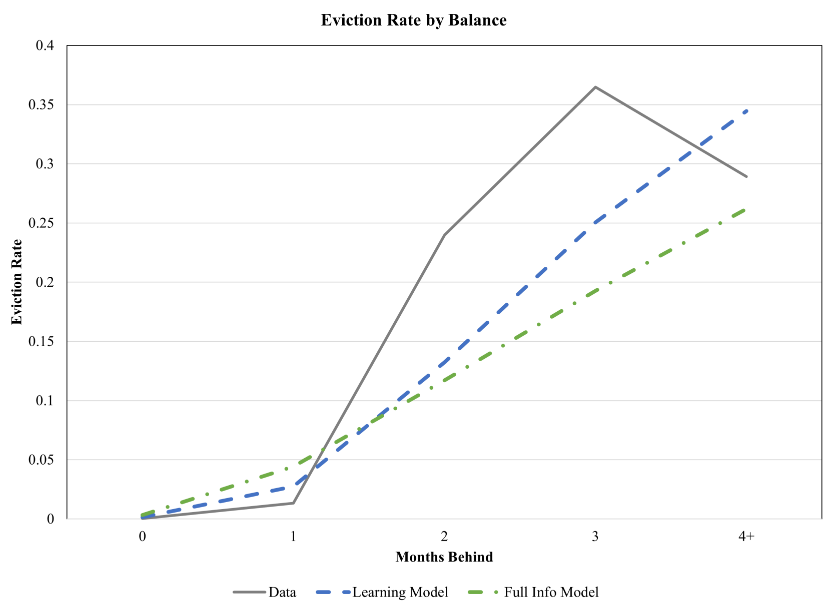
Appendix Table 6 – Tenant Departure and Vacancy Filling Rates

Statistic		**Estimate**
Prob(Exit | Not yet Evicted)	δ_d	0.037
Prob(Exit | Evicted)	δ_e	0.234
Prob(Vacancy Filled)	δ_v	0.436

Notes: The first and second rows report the estimated probabilities of exit for not-yet-evicted and evicted tenants, respectively. These statistics are computed as mean departure rates across tenant-months in the model estimation sample, excluding leases belonging to buildings that were sold to new owners. The third row reports the vacancy filling hazard, estimated as the reciprocal of the mean vacancy duration between two tenancies.

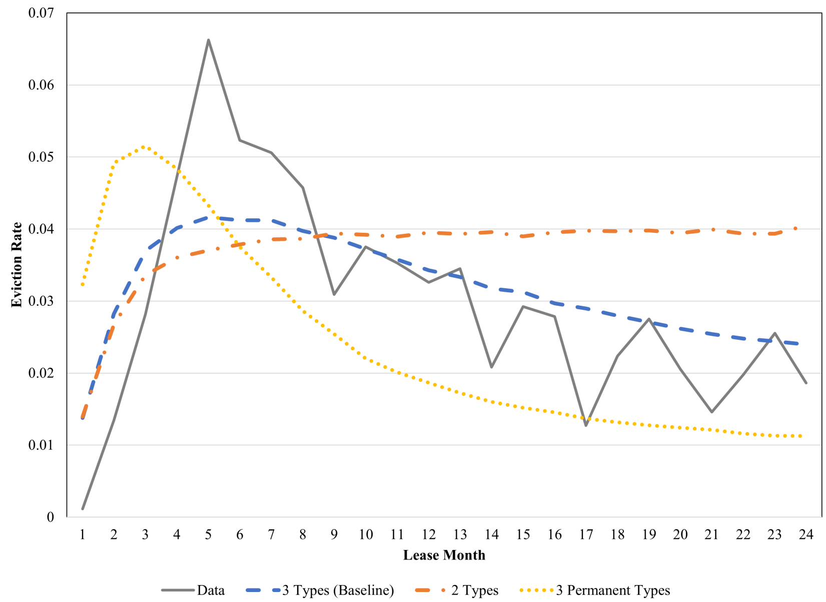
Appendix Figure 6 – Evictions Model Fit

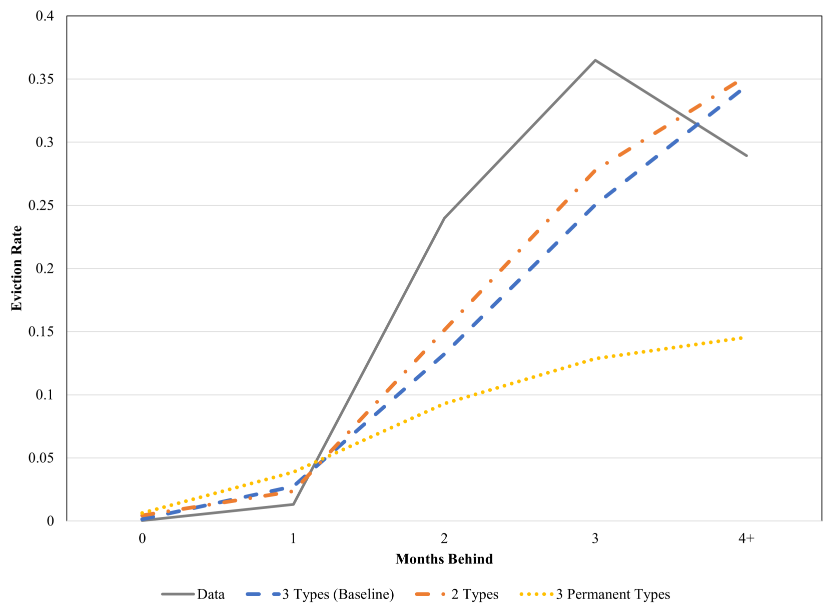




Notes: Top figure shows eviction rates by month of lease; bottom figure plots eviction rates by the tenant's cumulative balance. Simulated eviction rates are based on estimates from the 3-type models reported in columns (1) and (3) of Table 4, which assume that the landlord's information set is either payments (learning) or types (full information). Observed eviction rates are computed from the model estimation sample. Eviction refers to an eviction court filing or landlord's notice to attorney to file eviction, regardless of whether the tenant moved out after. Months behind is computed as the sum of monthly differences between rent charges and payments from move-in through current month, normalized by current rent.

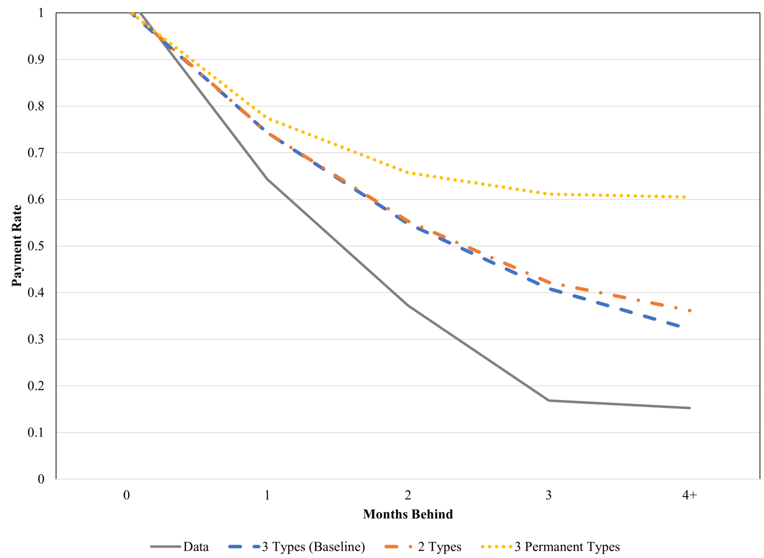
Appendix Figure 7 – Evictions Model Fit Comparison





Notes: Top figure shows eviction rates by month of lease; bottom figure plots eviction rates by the tenant's cumulative balance.. Simulated eviction rates are based on estimates from the 3-type (baseline), 2-type, and 3-permanent type learning models, which assume that the landlord's information set is only payments. These estimates as well as observed eviction rates are based on the 2015-2019 model estimation sample. Eviction refers to an eviction court filing or landlord's notice to attorney to file eviction, regardless of whether the tenant moved out after. Months behind is computed as the sum of monthly differences between rent charges and payments from move-in through current month, normalized by current rent.

Appendix Figure 8 – Payments Model Fit Comparison



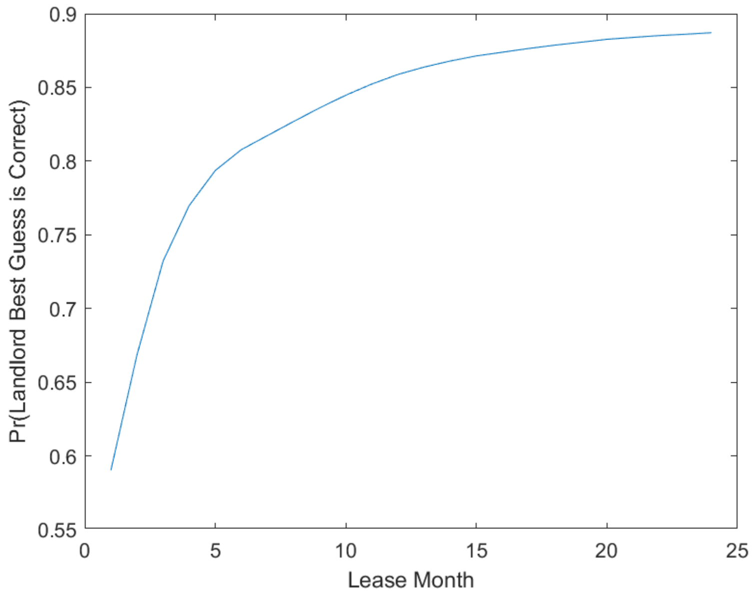
Notes: The figure shows payment rates by the tenant's cumulative balance. Simulated payment rates are based on estimates from the 3-type (baseline), 2-type, and 3-permanent type learning models, which assume that the landlord's information set is only payments. Observed payments are computed from the model estimation sample. Share of rent paid is computed using equation (13). Months behind is computed as the sum of monthly differences between rent charges and payments from move-in through current month, normalized by current rent.

Appendix Table 7 – Model Fit

Statistic	**Data**	**Learning Model**	**Full Info Model**
	(1)	**(2)**	**(3)**
Eviction Rate	0.2811	0.2894	0.2864
Payment Rate	0.8607	0.8425	0.8453
Repayment Rate	0.0215	0.0119	0.0137

Notes: The table reports average eviction, payment, and repayment rates in the first 12 lease months using the 2015-2019 model estimation sample in column (1), simulated data based on parameter estimates from the 3-type learning model in column (2), and simulated data based on parameter estimates from the 3-type full-information model in column (3). Share of rent paid is computed using equation (13).

Appendix Figure 9 – Landlord Learning

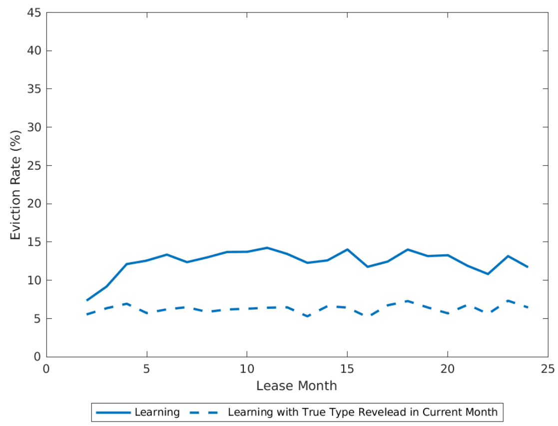


Notes: Model simulations from the 3-type baseline model. The figure shows, by lease month, the probability that a landlord's best guess of a tenant's type (i.e., the mode of the landlord's posterior belief distribution) is correct at the end of the month. Beliefs are shown for simulated tenants who have not yet been evicted.

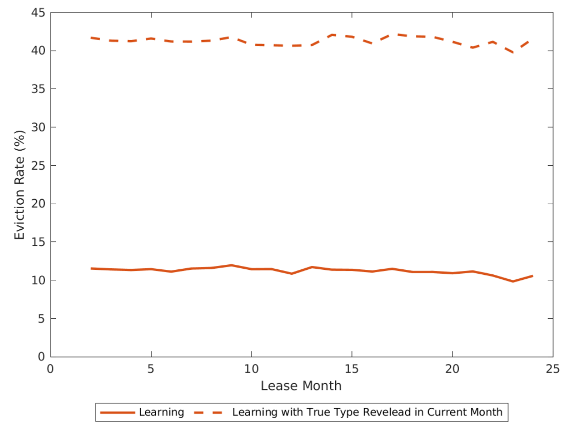
E. Additional Counterfactual Results

Appendix Figure 10 – Counterfactual Eviction Rates when True Tenant Type is Revealed to the Landlord in Current Month

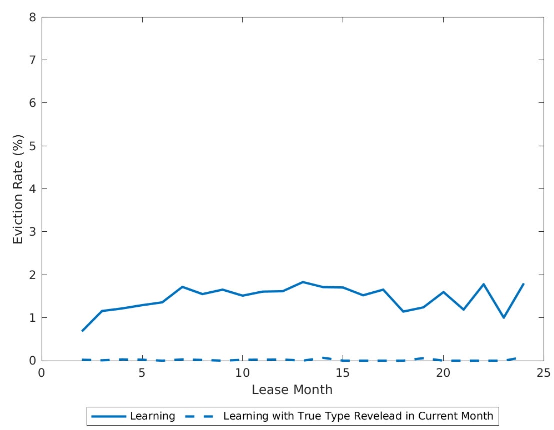
(a) Type L (previous month) → Type M (current)



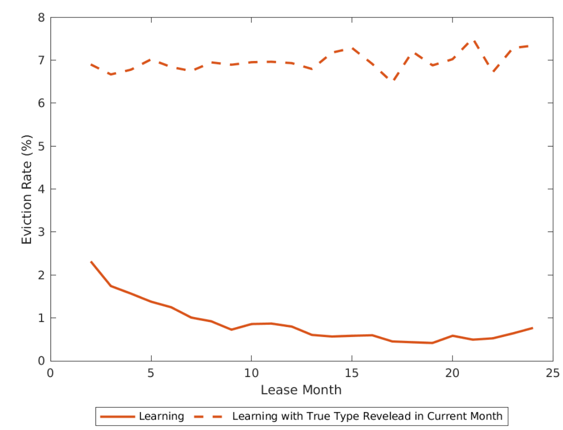
(b) Type M (previous month) → Type L (current)



(c) Type M (previous month) → Type H (current)



(d) Type H (previous month) → Type M (current)



Notes: Panel (a) shows eviction rates in each lease month for current Type M tenants who were Type L in the previous month. Panels (b)-(d) show the corresponding eviction rates for tenant type transitions M to L, M to H, and H to M, respectively. Solid lines correspond to eviction rates under the baseline 3-type learning model, which assumes that the landlord's information set is only payments. Dashed lines display eviction rates under the 3-type learning model in which tenant true types are revealed to the landlord in current month. Eviction refers to an eviction court filing or landlord's notice to attorney to file eviction, regardless of whether the tenant moved out after.

Appendix Table 8 – Counterfactual Results under Iso-Cost Policies

	Baseline (1)	Tax (2)	Delay (3)	Short-Term Rental Assistance (4)
Eviction Rate (%)	2.27	2.15	2.25	2.23
Share of Rent Collected (%)	79.79	79.40	79.60	79.63
Tenure (months)	16.32	16.64	16.38	16.42
Occupancy Rate (%)	85.26	85.50	85.31	85.34
Gvt. Cost ($/unit-month)	–	-5.36	–	2.11
Landlord Cost ($/unit-month)	–	6.28	0.91	-1.20
Gvt + Landlord Cost ($/unit-month)	–	0.91	0.91	0.91
Compensating Rent Change ($)	–	9.01	1.30	-1.73
Tenure increase if >0 (months)	–	7.00	3.00	7.00
Would have paid, evicted at baseline (%)	14.65	–	–	–
Would have paid, eviction delayed/averted (%)	–	21.57	11.11	17.42
Recovered, evicted at baseline (%)	–	1.37	0.04	0.40
Recovered, eviction delayed/averted (%)	–	10.09	3.75	7.75

Notes: The three policies reported are calibrated to yield the same total costs to landlords and the government if rents do not adjust: a delay of 4 days, a $250 tax, and a rate of rental assistance receipt of once every 13 years. All statistics are as described in Table 5.

Appendix Table 9 – Counterfactual Results under Compensating Rent Changes

	Baseline (1)	Tax (2)	Delay (3)	Short-Term Rental Assistance (4)
Eviction Rate (%)	2.27	2.15	2.15	2.14
Share of Rent Collected (%)	79.79	79.43	78.02	79.23
Tenure (months)	16.32	16.62	16.95	16.65
Occupancy Rate (%)	85.26	85.49	85.73	85.51
Gvt. Cost ($/unit-month)	–	-5.38	–	7.13
Landlord Cost ($/unit-month)	–	0.00	0.00	0.00
Gvt + Landlord Cost ($/unit-month)	–	-5.38	0.00	7.13
Compensating Rent Change ($)	–	9.01	11.90	-5.89
Tenure increase if > 0 (months)	–	7.00	4.00	7.00
Would have paid, evicted at baseline (%)	14.65	–	–	–
Would have paid, eviction delayed/averted (%)	–	22.61	12.10	16.61
Recovered, evicted at baseline (%)	–	1.33	0.49	1.18
Recovered, eviction delayed/averted (%)	–	10.58	5.15	7.30

Notes: Results from counterfactual simulations assuming rents adjust to equalize landlords' value of a vacant unit in each counterfactual to the value baseline. Landlords re-optimize their eviction decisions in response. Simulations are based on estimates reported in column (1) of Table 4. The policy parameters are the same as those in Table 5, and all statistics are defined analogously.

Appendix Table 10 – Counterfactual Robustness to Moral Hazard

	Baseline (1)	Tax (2)	Delay (3)	Short-Term Rental Assistance (4)
Eviction Rate (%)	2.27	2.25	2.25	2.25
Share of Rent Collected (%)	79.79	78.19	76.65	78.09
Tenure (months)	16.32	16.37	16.74	16.37
Occupancy Rate (%)	85.26	85.31	85.58	85.31
Gvt. Cost ($/unit-month)	–	-5.62	–	8.62
Landlord Cost ($/unit-month)	–	15.30	9.06	-4.64
Gvt + Landlord Cost ($/unit-month)	–	9.68	9.06	3.98
Compensating Rent Change ($)	–	25.99	15.32	-7.71
Tenure increase if > 0 (months)	–	7.00	4.00	7.00
Would have paid, evicted at baseline (%)	13.59	–	–	–
Would have paid, eviction delayed/averted (%)	–	19.57	11.24	16.26
Recovered, evicted at baseline (%)	–	1.25	0.49	1.20
Recovered, eviction delayed/averted (%)	–	8.82	4.14	6.83

Notes: Simulations use estimates from the 3-type learning model reported in column (1) of Table 4. Moral hazard is introduced under counterfactual policies through a reduction in the middle-type's payment rates equal to half of the reduction observed after eviction at baseline. The Delay and SRA parameters are calibrated to have the same effect on eviction rates a $250 Tax under moral hazard. All statistics are as described in Table 5.

Appendix Table 11 – Counterfactual Results under Full-Information Model

	Baseline (1)	Tax (2)	Delay (3)	Short-Term Rental Assistance (4)
Eviction Rate (%)	2.33	2.26	2.26	2.26
Share of Rent Collected (%)	77.31	76.90	75.94	76.81
Tenure (months)	16.21	16.37	16.54	16.37
Occupancy Rate (%)	85.17	85.29	85.43	85.29
Gvt. Cost ($/unit-month)	–	-5.65	–	6.67
Landlord Cost ($/unit-month)	–	6.04	5.50	-4.16
Gvt + Landlord Cost ($/unit-month)	–	0.39	5.50	2.52
Compensating Rent Change ($)	–	10.25	9.40	-7.05
Tenure increase if > 0 (months)	–	6.00	4.00	6.00
Would have paid, evicted at baseline (%)	2.27	–	–	–
Would have paid, eviction delayed/averted (%)	–	6.56	1.15	4.17
Recovered, evicted at baseline (%)	–	0.36	0.05	0.26
Recovered, eviction delayed/averted (%)	–	3.75	0.66	2.40

Notes: Simulations use estimates from the 3-type full-information model reported in column (3) of Table 4. The simulated policies yield the same reduction in evictions under the full-information model and estimates: a 2.5-week delay, a $250 tax, and rate of a rental assistance receipt of once every 50 months. All statistics are calculated as in Table 5.

Appendix Table 12 – Counterfactual Outcomes by Tenant Type under Full-Information Model

	Type at Filing, Baseline								
	Type H	Type M	Type L	Type H	Type M	Type L	Type H	Type M	Type L
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Share of Evicted Tenants, Baseline (%)	0.00	6.27	93.73	0.00	6.27	93.73	0.00	6.27	93.73
	Tax			Delay			SRA		
Evicted Same Month (%)	0.00	70.38	91.82	100.00	97.03	92.81	100.00	80.26	89.45
Evicted Later On (%)	0.00	18.03	6.84	0.00	1.86	6.10	0.00	12.30	8.78
Never Evicted (%)	100.00	11.59	1.34	0.00	1.11	1.09	0.00	7.44	1.64
Evicted Earlier (%)							0.00	0.00	0.14
Mean Months Later	0.00	9.24	3.53	0.00	8.22	3.41	0.00	9.77	3.45
Recovered (12 mo.)	100.00	5.52	0.01	0.00	0.59	0.01	0.00	0.00	0.00
Time in Unit	14.00	2.41	0.23	0.00	0.80	0.76	0.00	1.69	0.28

Notes: This table summarizes how outcomes change under each counterfactual policy for tenants evicted in the baseline simulation. The 3-type full-information model and estimates reported in column (3) of Table 4 are used. All statistics are calculated as in Table 7.

F. Data Sample Construction

To arrive at our analysis sample, we apply several restrictions to the raw data. First, we remove duplicate observations and filter out records with missing tenant or unit information. Second, we exclude any leases associated with non-residential units (e.g., commercial, store front, or storage units). Third, we remove leases without an actual move-in. We identify those as not having any rent charge or payment. Fourth, we exclude leases that have been terminated, but continue to have an auto-charge in the data. Fifth, we remove transferred leases with missing initial ledger records. These left-censored leases are identified as: (i) having an opening balance at the start of the available ledger record or an initial charge that is a multiple of average monthly charge; or (ii) belonging to new buildings that entered the data during the sample period. Sixth, we further remove leases that we suspect did not have an actual move-in by excluding non-evicted tenants who never paid rent. Finally, we restrict the data period by (i) keeping only leases with move-in on or after 2015 and (ii) filtering out ledger records on or after January 2020. Appendix Table 13 summarizes the number of leases (and share of raw data leases, %) remaining after these restrictions.

Appendix Table 13 – Analysis Sample Restrictions

#	Sample restriction	# Leases (% Raw)
0	–	12,341 (100.0)
1	Remove duplicates; exclude missing leases	12,339 (100.0)
2	#1 + Exclude non-living units	12,043 (97.6)
3	#2 + Limit to leases with at least one rent charge or payment	11,328 (91.8)
4	#3 + Exclude terminated leases with auto-charge after move-out	11,301 (91.6)
5	#4 + Remove left-censored transfer leases	8,345 (67.6)
6	#5 + Exclude leases of non-evicted tenants who never paid rent	8,052 (65.2)
7	#6 + Remove leases with move-in before 2015	7,211 (58.4)
8	#7 + Exclude lease-month records after December 2019	5,809 (47.1)

Notes: Lease count (and share of raw data leases, %) following sample restrictions used to construct the 2015-2019 analysis sample for descriptive analyses. A lease refers to a specific tenant in a specific unit. Move-in (-out) date is measured based on the first (last) date of rent charge or payment, with appropriate adjustments for pre-move-in charges and payments (as detailed in Appendix F). Non-living units include commercial, store front, and storage units.

To move from the analysis sample to the model estimation sample, we make three additional adjustments. First, we exclude leases associated with voucher holders. Second, we restrict our attention to leases in units located in Cook County, IL, that comprise Chicago and some of its closest suburbs (Des Plaines, Northlake, Oak Lawn, and Maywood). Last, to have a relatively comparable set of tenants, we keep leases with monthly rent between \$600 and \$1,000. Appendix Table 14 documents the count of leases (and share of raw data leases, %) following each subsequent restriction.

Appendix Table 14 – Model Estimation Sample Restrictions

#	Sample restriction	# Leases (% Raw)
8	Analysis sample limitations #1-8	5,809 (47.1)
9	#8 + Exclude voucher holder leases	3,847 (31.2)
10	#9 + Limit to Cook County, IL subsample	2,194 (17.8)
11	#10 + Restrict to leases with median rent \$600-1000	1,814 (14.7)

Notes: Lease count (and share of raw data leases, %) following restrictions used to construct the 2015-2019 model estimation sample for structural analysis. The Cook County, IL subsample is defined in section 2. A lease refers to a specific tenant in a specific unit. Analysis sample restrictions #1-8 are detailed in Appendix Table 13. We treat tenants as voucher holders if they have at least one rental assistance charge or payment. Cities in Cook County, IL, include Chicago, Des Plaines, Maywood, Northlake, and Oak Lawn. Median rent is computed over the full tenure of a given lease.

In addition to sample restrictions, we further collapse our samples to the monthly level and take the following steps in order to make them usable for analyses.

- **Move-in, Move-out Dates, and Tenure.** Move-in (-out) date is identified as the first (last) date of rent charge or payment reported in the ledger. To account for pre-payments and/or -charges, we make the following adjustments when measuring move-in date. For leases with first payment appearing before first charge (pre-payment), we push forward the first payment date to the date of first charge and treat the corresponding date as move-in. And for leases with first charge before first payment and no late fee on or before first payment (pre-charge), we move up first charge date to the date of first payment and treat the corresponding date as move-in. Tenure is computed as the number of months between move-in and move-out dates (assuming that the tenant moves out at the end of the move-out month).

- **Eviction Timing.** To identify the timing of evictions, we use the reported eviction filing month, if available; otherwise, we use the date the landlord notified an attorney to file an eviction. If there are multiple eviction dates recorded, we only consider the first date.

- **Rent Charge, Payment, Cumulative Balance, and Share Paid.** We only use primary rent charges and payments (e.g., we do not consider in our calculations security deposit, late fee, parking, or utility charges/payments). To take into account arrears as well as pre- and re-payment, we compute share paid as follows:[45]

$$\text{SharePaid}_t = \frac{\text{SyntheticRent}_t - (\max\{B_t, 0\} - \max\{B_{t-1}, 0\})}{\text{SyntheticRent}_t} \times 100\%, \tag{13}$$

where SyntheticRent_t is the mode of the closest (in time) 7 months of rent charge, designed to capture the tenant's current rent obligation; and cumulative balance, B_t, is computed as the sum of monthly differences between synthetic rent and payments from the beginning of the lease through month t. We sometimes refer to synthetic rent just as "current rent."

- **Active, Occupied, and Vacant Units.** We define a unit as *occupied* in period t if there is a lease assigned to that unit in period t with move-in date on or before t and move-out date after t. A unit is *vacant* in period t if it: (i) is not occupied in period t, (ii) was occupied in some period preceding t, and (iii) will be occupied in some period following t. A unit is *active* in period t if it is either occupied or vacant in period t.

- **Voucher Holders.** We flag tenants as voucher holders if they have at least one rental assistance charge or payment. We treat subsidy payments as deterministic: for each observed subsidy charge, we assume that an equivalent rental assistance payment has been realized in the same month of the corresponding charge. Share paid for voucher holders include the subsidy as well as the tenant's portion of the rent.

[45]To illustrate how the share paid formula takes into account pre-payment, for example, consider a lease with monthly rent of $800 and a payment stream of $800, $1,000, and $600 in the initial three months. The resulting payment rates using equation (13) are 100% each period. This approach reflects that the tenant is current on payments each period (because of pre-payment in the second month). Notice that a simpler calculation of contemporaneous share paid (payment/charge) would yield a payment rate of 75% in month 3. This alternative method would miss the fact that the tenant was ahead on payments at the beginning of that month.