Eviction as Bargaining Failure: Hostility and Misperceptions in the Rental Housing Market*

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Abstract

Court evictions from rental housing are common but could be avoided if landlords and tenants bargained instead. Such evictions are inefficient if they are costlier than bargaining. We test for two potential causes of inefficient eviction — hostile social preferences and misperceptions — by conducting lab-in-the-field experiments in Memphis, Tennessee with 1,808 tenants at risk of eviction and 371 landlords of at-risk tenants. We detect heterogeneous social preferences: 24% of tenants and 15% of landlords exhibit hostility, giving up money to hurt the other in real-stakes Dictator Games, yet more than 50% of both are highly altruistic. Both parties misperceive court or bargaining payoffs in ways that undermine bargaining. Motivated by the possibility of inefficient eviction, we evaluate the Emergency Rental Assistance Program, a prominent policy intervention, and find small impacts on eviction in an event-study design. To quantify the share of evictions that are inefficient, we estimate a bargaining model using the lab-in-the-field and event-study evidence. Due to hostile social preferences and misperceptions, one in four evictions results from inefficient bargaining failure. More than half would be inefficient without altruism. Social preferences weaken policy: participation in emergency rental assistance is selected on social preferences, which attenuates the program's impacts despite the presence of inefficiency.

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Evictions are costly and common. Tenants are often traumatized and lose possessions when evicted (Desmond, 2016), and landlords face costs from vacancies and property damage. Formal evictions — i.e., those involving a court order — increase homelessness, induce financial distress, and impose court costs on both parties (Collinson et al., 2024). Despite these costs, courts in the United States give eviction orders to around 2% of renters each year (Gromis et al., 2022; Graetz et al., 2023).[1] During the pandemic, evictions' costs motivated the $40-billion Emergency Rental Assistance Program (ERAP), which nearly doubled the $50 billion spent annually on low-income rental assistance. Advocates have proposed a permanent version of ERAP, describing the program as "the most important eviction prevention policy in American history."[2]

In a Coasean world with frictionless landlord–tenant bargaining, all formal evictions would be privately efficient. Then, since landlords and tenants could otherwise bargain to avoid court, formal evictions would only occur when their benefits to the parties exceed their costs. According to this view, evictions may be costly but still better than bargained alternatives. Eviction's private costs have attracted substantial scholarly and policy attention. But if evictions are efficient, the case for policy intervention to stop evictions hinges on other arguments like externalities or redistribution, rather than eviction's private costs.[3]

Still, two forces suggest that landlord–tenant bargaining is not always frictionless, so some evictions could be undesirable because of private costs alone. First, hostility (i.e., anti-altruistic social preferences) in landlord–tenant relationships could impede efficient bargaining. In that case, formal evictions might even be "repugnant" — that is, normatively undesirable if the social planner does not respect hostile preferences. Second, misperceptions about eviction or bargaining costs, perhaps arising from eviction's complicated legal environment, may cause inefficient evictions. Whether formal evictions are driven by bargaining costs, hostility, or misperceptions affects the desirability and efficacy of anti-eviction policy versus other ways of assisting the poor.

This paper seeks to answer three main questions. How prevalent are hostility and misperceptions among tenants facing eviction and their landlords? Do hostility and misperceptions cause a meaningful share of formal evictions? And what are the consequences of emergency rental assistance for formal evictions — efficient, inefficient, and repugnant — and welfare?

We study these questions in four parts. First, we outline a simple model that shows how formal evictions may emerge from bargaining costs, hostility, and misperceptions. Second, we test for the presence and quantitative importance of hostile social preferences and misperceptions by conducting lab-in-the-field experiments with landlords and tenants facing eviction, linking their

[1]Surveys suggest formal evictions constitute perhaps 15% (Gromis and Desmond, 2021) to 33% (Desmond and Shollenberger, 2015) of all forced moves.

[2]For example, the Eviction Crisis Act introduced in the Senate would allocate $3 billion to permanent emergency rental assistance for renters facing eviction. The quotation is from "Fact Sheet: White House Summit on Building Lasting Eviction Prevention Reform," The White House, August 2022.

[3]For instance, Collinson et al. (2024) find fiscal externalities via, e.g., stays in homeless shelters.

behaviors in the experiment to realized formal evictions. Third, we study ERAP's causal effects with event studies, and use data from the experiment to explain its effectiveness. Fourth, we combine the experimental and observational moments with the model to quantitatively estimate the importance of each force for evictions, conduct welfare analysis, and analyze counterfactuals.

Section 1 presents a simple bargaining model that illustrates when evictions occur, whether eviction is efficient or inefficient, and how policy affects eviction and welfare. We define eviction as formal eviction and view informal evictions as implicitly bargained outcomes that avoid court costs.[4] Absent misperceptions and social preferences, this framework yields a classical benchmark: eviction occurs if and only if efficient, that is, if and only if net bargaining costs exceed net court costs. By contrast, misperceptions (e.g., if landlords overestimate court payoffs) and hostile social preferences can cause inefficient or repugnant evictions. We then introduce a government program modeled after ERAP in which landlords receive a payment for the tenant's back rents but pay a private take-up cost. The program's effect on evictions depends on court costs, misperceptions, and social preferences among enrollees. For instance, those who enroll could be altruists who would rarely evict, attenuating ERAP's effect. Moreover, if ERAP does stop evictions, these forces also govern whether ERAP stops efficient, inefficient, or repugnant ones. The nature of evictions and the impact of anti-eviction policy are thus empirical questions and the focus of our analysis.

Our setting of Memphis, Tennessee is well-suited for studying evictions (Section 2). Memphis is a large city with high rates of poverty and housing insecurity. Bargaining to avoid formal eviction is common in Memphis. For instance, 57% of surveyed tenants have formed repayment plans to repay back rents over time to landlords, and about 40% of court processes do not conclude in evictions because of bargaining. We partner with the housing agencies that administer Memphis's ERAP to recruit 1,808 tenant applicants and 371 landlords of tenant applicants for lab-in-the-field experiments. ERAP applicants and experiment participants have rental debts exceeding $3,000, three–four times their monthly household incomes, and are at high risk for eviction.

We examine social preferences by conducting more than 4,000 real-stakes Dictator Games (DGs) with landlords and tenants (Section 3). We modify the standard DG to allow players to exhibit hostility as well as altruism. We call players "hostile" if they choose a dominated bundle in which they forgo money to lower another player's payment. Players are "altruistic" if they forgo money to raise the other's payment.

Both hostility and altruism are prevalent. About 15% of landlords and 24% of tenants are hostile toward their own tenants and own landlords respectively. Suggestive evidence on matching suggests that about one in three landlord–tenant pairs has at least one hostile party. Yet most

[4] Although causal evidence is scarce, formal evictions may be costlier than informal evictions. For tenants, formal evictions are publicly observable by credit agencies and future landlords. For landlords, formal evictions cost money to file and time to manage. In Appendix E, we present evidence from tenant experiment participants that court eviction is particularly costly. Finally, the negative causal effects of eviction in Collinson et al. (2024) identify the effect of formal evictions in a sample where the not-formally-evicted comparison group is likely to experience informal eviction.

landlord–tenant relationships are altruistic: 69% of landlords halve their payment to ensure their own tenant receives an equal payment. 53% of tenants do the same for their own landlord.[5]

Next, we test for misperceptions that affect the perceived payoffs from eviction or bargaining and therefore suggest the potential for inefficiency (Section 4). With landlords, we elicit beliefs about back rents that they can attempt to recoup in court. With tenants, we elicit beliefs about landlords' hostility and bargaining rates after initiating the court process. We test for misperceptions by: (i) comparing prior beliefs to known ground truth; (ii) testing whether information treatments induce revisions of related beliefs; and (iii) testing whether information treatments affect revealed-preference bargaining behaviors, like whether tenants propose repayment plans for back rents to landlords.

Rates of misperceptions are high. Our most conservative tests imply that 25% of tenants and 17% of landlords have misperceptions exceeding 20 pp. Other tests give misperception rates of 41–79%. As further evidence that beliefs affect behaviors, and of independent interest, we find that correcting misperceptions affects some bargaining and ERAP take-up decisions, with larger impacts for landlords than tenants.

We then ask whether these experimental elicitations can help us understand ERAP's impacts and eviction behavior beyond the experiment (Section 5). Given high rates of misperceptions and hostility, many evictions may be inefficient or repugnant. A natural question is then whether policy intervention stops evictions. To study the Memphis ERAP's causal effects, we use administrative data on around 4,700 enrollees in a cash-only arm of the program. Conducting event studies around payment, we find statistically significant, but sometimes short-lived effects on eviction filings (i.e., the orders that start the court eviction process), and null or small effects of ERAP on eviction judgments (i.e., ultimate formal evictions). These estimates suggest that ERAP was not cost effective. The 95% confidence intervals in our best-case specification suggest that it cost more than $70,000, distributed across tenants, to avert one judgment for 6 months. These results cast doubt on advocates' claims that ERAP meaningfully reduced eviction.

To shed light on ERAP's small effects on eviction, we link the experiment data to administrative records on ERAP take-up. The model indicates that perverse selection on altruism could reduce ERAP's treatment effects: program enrollees who find ERAP desirable, despite its take-up costs, could be altruists with low counterfactual eviction propensities. Indeed, we find that altruists are more likely to be paid and are paid faster. Social preferences may thus influence policy effectiveness in the Memphis ERAP.

[5]To rule out elicitation error, we randomize if participants play against their own or a random landlord or tenant. We find, for instance, that landlords are significantly more hostile to their own tenant than random tenants. We find higher rates of hostility and altruism in the landlord and tenant samples than in nationally representative samples we recruit to play the same game against anonymous landlords and tenants. The rates of altruism exceed the 30% of subjects who give half or more in a classical DG (Engel, 2011). As a comparison to hostility, less than 10% typically reject even offers in ultimatum games (Camerer, 2003). We also randomize stakes among tenants and find that altruism and hostility persist in DGs with $1,000 stakes.

We additionally confirm that hostility, misperceptions, and court costs correlate with evictions in the direction that the model predicts. We link the experiment to court eviction records. An index which combines experimental proxies for the three forces predicts eviction filings. Of the three forces, an indicator for hostility is the strongest predictor, itself correlating with a 10 pp increase in filings among tenants and 15 pp among landlords (off base rates of 33% and 23%). Hostility also correlates with tenant bargaining offers and reported housing-code violations.

Using our estimates and the model, we conduct welfare analysis of eviction and ERAP (Section 6). An advantage of the lab-in-the-field approach is that we directly measure economic primitives — social preferences and beliefs — which other research must calibrate or estimate. However, we still do not observe costs and a few other primitives. We estimate the remaining primitives by matching model-implied behavior among experiment participants to moments from the experiment and ERAP program evaluation, using the Method of Simulated Moments.

Our main result from the model is that about one in four evictions is inefficient, and two-thirds of the inefficient evictions are repugnant. Different model assumptions yield that between 15–41% of evictions are inefficient. Given most evictions are efficient, we find that stopping the average eviction would reduce private surplus by at least $870 in our primary specification. The frictions we document are quantitatively meaningful, since if all evictions were efficient, stopping the average eviction would reduce surplus by at least $2,000. Even so, eviction's private costs alone cannot rationalize eviction policy. Naturally, implications could differ for policies that stop particularly inefficient evictions, or if there are other rationales for eviction policy like externalities or high social marginal welfare weights on tenants facing eviction (Saez and Stantcheva, 2016).[6]

The interaction of nonclassical forces partly explains why inefficient evictions are rare despite high rates of misperceptions and hostility: altruism offsets misperceptions. Absent altruism, the rate of inefficient evictions would rise to 56%. Intuitively, many landlords misperceive eviction payoffs but are altruistic and so do not pursue inefficient eviction. Since the joint distribution of altruism and misperceptions proves key, this mechanism highlights the value of collecting both misperceptions and social preferences in one experiment.

We draw several other conclusions. The model confirms that perverse selection on altruism depresses ERAP's Treatment Effect on the Treated (TOT): absent landlord altruism, ERAP would stop about half of evictions. We also find that a quarter of the evictions ERAP does stop are inefficient, meaning the program is no better-targeted than if it stopped evictions at random. Policy counterfactuals of (i) instating fines if landlords take ERAP and then evict, or (ii) targeting the program based on demographics can raise its TOT substantially without sacrificing targeting.

Related Literature. First, we add to research on housing insecurity and evictions. In examining the causes and efficiency of evictions, we study different questions than research that considers

[6] The last argument may have particular weight, as we estimate that tenants' eviction costs exceed $4,000. In this case, advocates could view the objective of anti-eviction policy as purely to redistribute rather than to stop evictions.

the effects of housing insecurity.[7] Although the sociology literature emphasizes the impact of landlord–tenant relationships on eviction (see Section 1), economists have largely neglected this mechanism. We contribute an economic framework that captures the role of social preferences, as well as data confirming their quantitative relevance among the housing-insecure. Along with Collinson et al. (2023), we conduct among the first empirical evaluations of ERAP. Prior analyses report summary statistics, rather than estimate ERAP's causal effects (e.g., Aiken et al., 2022).

Second, we contribute to the literature in behavioral public finance (Bernheim and Taubinsky, 2018) by studying how social preferences and information frictions affect poverty and welfare policy. Although a mature literature quantifies social preferences in various lab settings (e.g., Levitt and List, 2007), empirical evidence on how social preferences affect policy design is much rarer.[8] Our finding that social preferences can affect program take-up and targeting adds to the literature on benefit program design (Currie, 2004; Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019), which considers other nonclassical forces, like information frictions, rather than social preferences. The importance of altruism for program design could apply elsewhere if, for example, program participant–social worker relationships affect take-up. Meanwhile, misperceptions that cause inefficient bargaining failure yield a different rationale for policy intervention than in previous public-finance research on corrective taxation or nudges to address misperceptions (e.g., Allcott et al., 2022; List et al., 2023). To study misperceptions, we use laboratory techniques (Fuster and Zafar, 2022; Haaland et al., 2023) in a high-stakes field setting.[9],[10]

As a third contribution, we use experiments motivated by bargaining theory to conduct empirical tests of the bargaining literature. Bargaining theory emphasizes that biased beliefs can generate bargaining inefficiencies (Yildiz, 2011; Vasserman and Yildiz, 2019). How social preferences affect bargaining has attracted less theoretical attention (see Pollak, 2022, for a recent exception). Our finding that altruism offsets information frictions is consonant with Friedberg and Stern (2014), who find the same in the context of divorce. Empirical research in other high-stakes settings finds that social preferences affect behaviors (Hjort, 2014; Ashraf and Bandiera, 2018; Lowes, 2021; Blouin, 2022; Ramos-Toro, 2022), but does not link these forces to bargaining.[11]

[7] One antecedent to our modeling approach is Hoy and Jimenez (1991), who consider squatter evictions and bargaining. Recent empirical contributions to the literature on housing insecurity include Palmer et al. (2019), Cohen (2020), Fetzer et al. (2020), Abramson (2021), Geddes and Holz (2022), and Collinson et al. (2024).

[8] Public economics has long considered how altruism affects charitable donations (Andreoni, 1990, 1993; Andreoni and Miller, 2002; DellaVigna et al., 2012), bequests (Becker, 1974), and contributions to public goods (reviewed in Ledyard, 1995). The social preferences we consider are distinct from signaling motives or social norms (e.g., Allcott, 2011; Bursztyn and Jensen, 2017), and echo research on altruism and tax morale (Luttmer and Singhal, 2014).

[9] Our welfare analysis of eviction and ERAP applies a "structural behavioral economics" approach (DellaVigna, 2018), similar to recent work in housing (e.g., Andersen et al., 2022) and beyond.

[10] A recent literature has examined misperceptions in housing markets in particular, usually focusing on spending or investment choices (Armona et al., 2019; Bottan and Perez-Truglia, 2021; Chopra et al., 2023; Fairweather et al., 2023).

[11] Empirically, Freyberger and Larsen (2021) and Larsen (2021) find departures from efficient bargaining, and that these inefficiencies mostly do not reflect incomplete information as in Myerson and Satterthwaite (1983). Bargaining behavior consistent with fairness norms or heuristics is common (Backus et al., 2020; Keniston et al., 2022), and recent work finds that information interventions can improve bargaining outcomes in court (Sadka et al., 2020). Other work

1 A Model of Bargaining in the Shadow of Eviction

This section keeps the model as simple as possible. Extensions and proofs are in Appendix B.

1.1 Setup

Environment. Our model features two types of agents, Landlords and Tenants. Landlords and tenants have complete information about the other's preferences and beliefs. We index landlord–tenant relationships by i.

The tenant makes a take-it-or-leave-it bargaining offer $o_i \in \mathbb{R}$ to her landlord to repay exogenous rental debt d_i. The offer o_i can be positive or negative; for instance, the landlord may pay the tenant in a "cash for keys" arrangement ($o_i < 0$). This section considers eviction to be formal eviction. We view agreements to stay or leave but which avoid court costs as a form of bargaining. If bargaining occurs, the tenant pays o_i and cannot abscond.[12]

Formal eviction occurs when at least one party perceives it as profitable relative to bargaining. A fraction of tenants does not pay an eviction judgment, either because they abscond or win the case. If the landlord evicts, she recoups $p_i d_i$ in back rents from the tenant, where p_i represents the probability of receiving a full repayment from the tenant.[13]

We normalize bargaining costs to zero. Landlords and tenants respectively face net court costs of $k_{Li}, k_{Ti} \in \mathbb{R}$, where $k_{ji} > 0$ denotes that eviction is costlier to party j than bargaining. This model captures credit constraints by changing the relative costs between court and bargaining. If tenants can only bargain at high cost, because borrowing to obtain cash and transfer to landlords is expensive, that reduces k_{Ti} or makes it negative.

Payoffs. Utility is linear in money for both landlords and tenants. Payoffs V_{Li} and V_{Ti} satisfy:

$$V_{Li} = \begin{cases} p_i d_i - k_{Li}, \text{ if evicts} \\ o_i, \text{ if bargains} \end{cases} \quad \text{and} \quad V_{Ti} = \begin{cases} -p_i d_i - k_{Ti}, \text{ if evicted} \\ -o_i, \text{ if bargains.} \end{cases} \tag{1}$$

1.2 Classical Benchmark

When do landlords and tenants bargain to resolve debts, versus evict?

estimates relationship quality or misperceptions to rationalize observed bargaining (e.g., Merlo and Tang, 2019).

[12] Equivalently, one can permit the tenant to abscond with some probability and view o_i as the expected payment net of the tenant's absconding risk.

[13] While we informally describe p_i as a probability, one can recast p_i as any scalar that affects the expected value of the transfer the tenant makes to the landlord. For instance, p_i need not be limited to the interval $[0, 1]$. The real value of the court transfer may be negative, if the tenant does not pay rent during the court process.

Proposition 1. *Eviction occurs if and only if*

$$k_{Li} + k_{Ti} < 0. \tag{2}$$

This result affirms that, in our basic environment stripped of nonclassical features, a Coase theorem holds. Consider offer $o_i \in [p_i d_i - k_{Li}, p_i d_i + k_{Ti}]$. Offers within this interval are accepted, as both parties are weakly better off than going to court. The interval is well-defined as long as joint bargaining surplus is positive: $k_{Li} + k_{Ti} \geq 0$. Equivalently, eviction occurs if and only if it is efficient, where "efficient" means that joint surplus from eviction is positive.

The benchmark model does not imply that bargaining eliminates all evictions. To the contrary, evictions are possible as long as landlords or tenants have strong reasons to prefer going to court, such that joint surplus is negative. For example, landlords' costs of appearing "soft" to other tenants may exceed their legal fees: $k_{Li} < 0$. Alternatively, if landlords only accept positive cash offers, but tenants face high borrowing costs to obtain liquidity and bargain, then going to court may be cheaper than bargaining for tenants: $k_{Ti} < 0$.

1.3 Misperceptions

Motivation for Incorporating Misperceptions. Misperceptions are natural to consider. First, economic theory emphasizes beliefs as a cause of bargaining breakdown (Yildiz, 2011). Second, the sociology and law literatures emphasize how tenants lack information about the complicated eviction process (e.g., Bezdek, 1992; Chisholm et al., 2020). Relatedly, landlords could lack information about tenants' ability to pay.

Setup. Landlords and tenants may now disagree about the probability that the tenant pays an eviction judgment. The terms p_{Li} and p_{Ti} denote the landlord's and the tenant's beliefs about the probability that the tenant pays if evicted. We say there are "misperceptions" if beliefs do not coincide, $p_{Li} \neq p_{Ti}$. We write the difference in beliefs, or "misperception wedge," as $\Delta p_i := p_{Li} - p_{Ti}$.[14] We often consider $\Delta p_i > 0$, as this condition implies that landlords see formal eviction as more favorable for the landlord's payoffs than tenants do.

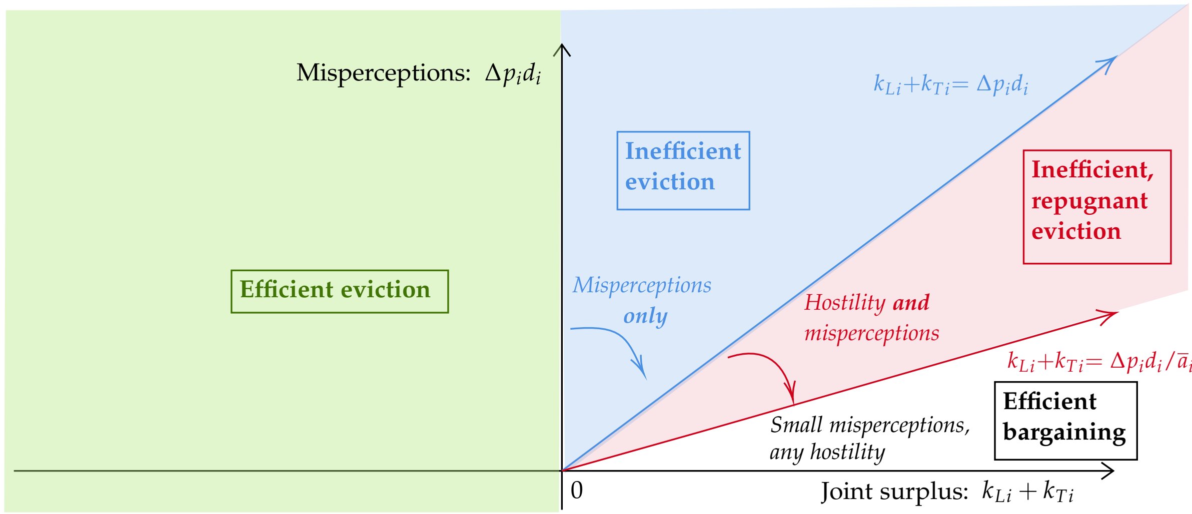
Eviction with Misperceptions. Misperceptions change the eviction condition as follows:

Proposition 2. *With misperceptions, eviction occurs if and only if*

$$k_{Li} + k_{Ti} < \Delta p_i d_i. \tag{3}$$

[14] We use the term "misperceptions" as shorthand for "non-coincident beliefs." With equal misperceptions, the misperception wedge is zero. We model a complete-information game with non-coincident beliefs. But inefficiency can also obtain in an incomplete-information game with uncertainty about the opponent's valuation (Myerson and Satterthwaite, 1983; Ausubel et al., 2002).

Diagram 1: Comparative Statics



We illustrate Proposition 2 in Diagram 1. Without misperceptions, evictions occur only if joint surplus $k_{Li} + k_{Ti}$ is less than zero (green shaded region). If $\Delta p_i > 0$, then misperceptions generate more evictions (blue shaded region), as eviction now occurs for $k_{Ti} + k_{Li} \in (0, \Delta p_i d_i)$ and would not occur with coincident beliefs. The additional evictions that misperceptions cause are inefficient, as bargaining would yield joint surplus.

Symmetrically, misperceptions reduce scope for eviction if $\Delta p_i d_i < 0$ — that is, if landlords are more pessimistic than tenants. Then misperceptions generate inefficient bargaining. We do not stress this case because our empirical evidence suggests a positive misperception wedge.

1.4 Social Preferences

Motivation for Incorporating Social Preferences. Social preferences between landlords and tenants are a second natural way to extend the model. The popular media regularly highlights examples of extremely deteriorated landlord–tenant relationships.[15] The vast literature on social preferences, including in other high-stakes settings (e.g., Hjort, 2014), suggests they might matter here. Yet economists have not stressed social preferences among landlords and tenants.[16]

Meanwhile, law and sociology literatures emphasize the importance of landlord–tenant relationships (e.g., Desmond, 2016; Garboden and Rosen, 2019; Balzarini and Boyd, 2021). In a law review, Bell (1984) describes evictions due to landlords' "coercive, punitive, or malicious reasons" as warranting special tenant protections, as they have "no social value" and "only satisf[y] the landlord's vindictiveness" (p. 537). In sociology, Vaughn (1964), Akers and Seymour (2018), and

[15]For instance, *The New York Times* describes a landlord whose tenants "curse and spit at her and owe more than $23,000 in rent" (Haag, 2021).

[16]An exception is the literature on racial discrimination in housing markets (e.g., Ewens et al., 2014).

Chisholm et al. (2020) describe eviction as emerging from coercive power dynamics between landlords and tenants. Conversely, Gilderbloom (1985) observes that inexperienced landlords sacrifice profits out of "concerns for tenants' welfare" (p. 159). Indeed, interviews suggest that altruism motivates landlord participation in housing assistance programs (Aubry et al., 2015) and Moving to Opportunity (Cossyleon et al., 2020). A recent sociology review summarizes that "social relationships," "resource constraints," and "supply-side actors and policy" all contribute to housing insecurity (DeLuca and Rosen, 2022, p. 344).

Set-up. Becker (1974)-type altruism parameters $a_{Li}, a_{Ti} \in (-1, 1)$ now enter parties' utilities. The value $a_{ji} = 0$ implies that party j's utility is unaffected by the other party's utility; $a_{ji} > 0$ implies altruism; and $a_{ji} < 0$ implies hostility (anti-altruism or "spite" in Levine, 1998). Utility remains linear. Payoffs are:

$$V_{Li} = \begin{cases} p_{Li}(1 - a_{Li})d_i - k_{Li} - a_{Li}k_{Ti}, \text{ if evicts} \\ (1 - a_{Li})o_i, \text{ if bargains} \end{cases} \quad \text{and} \quad V_{Ti} = \begin{cases} -p_{Ti}(1 - a_{Ti})d_i - k_{Ti} - a_{Ti}k_{Li}, \text{ if evicted} \\ -(1 - a_{Ti})o_i, \text{ if bargains.} \end{cases} \tag{4}$$

The altruism parameter scales the other's non-altruistic utility. For instance, if the landlord gets 20 utils of non-altruistic utility and the tenant has $a_{Ti} = 0.5$, then the tenant gets 10 utils plus her own payoff. Alternatively, $a_{Ti} = -0.5$ implies that the tenant gets –10 utils plus her own payoff: she is made worse off when the landlord gets positive utility.[17]

Hostility is distinct from signaling. Signaling implies that a party derives an *instrumental* benefit from appearing harsh and affects the net court cost k_{ji}. Hostility captures *non-instrumental* reasons why one party may get utility from harming the other.

Eviction with Social Preferences. The next proposition provides a condition for eviction with social preferences:

Proposition 3. *With social preferences and misperceptions, eviction occurs if and only if*

$$k_{Li} + k_{Ti} < \frac{\Delta p_i d_i}{\bar{a}_i}, \tag{5}$$

for "compound altruism" $\bar{a}_i := (1 - a_{Li}a_{Ti})/((1 - a_{Li})(1 - a_{Ti}))$.

To interpret Equation (5), note that compound altruism $\bar{a}_i$ is strictly positive and strictly increasing in either party j's altruism: $\frac{\partial \bar{a}_i}{\partial a_{ji}} > 0$. We collect comparative statics across propositions:

[17] Individuals do not internalize the others' social payoffs. That is, the tenant ignores that the landlord may get altruistic utility from giving the tenant money, and vice-versa. Our simple way of modeling social preferences is agnostic about whether they emerge from impure altruism (Andreoni, 1990), moral obligations (Rabin, 1995), inequity aversion (Fehr and Schmidt, 1999), or reciprocity (Charness and Rabin, 2002), among other models. A material share of the population having genuinely hostile preferences could rationalize how posturing as hostile (Abreu and Gul, 2000) might persist.

Corollary 1 (Comparative Statics)**.** Let $k_c^* := 0$, $k_m^* := \Delta p_i d_i$, and $k_a^* := \Delta p_i / \overline{a}_i$ be the "eviction thresholds" in the setups with classical preferences, misperceptions, and altruism and misperceptions. Then a positive misperception wedge raises the eviction threshold relative to the classical setup, and hostility raises the eviction threshold relative to misperceptions alone: (i) $\frac{\partial(k_m^* - k_c^*)}{\partial(\Delta p_i)} > 0$ and (ii) if $\Delta p_i > 0$, $\frac{\partial(k_a^* - k_{\mathrm{m}}^*)}{\partial(-\overline{a}_i)} > 0$.

Equation (5) gives three insights. First, hostility can cause evictions. More hostility ($\overline{a}_i \downarrow$) flattens the slope of Diagram 1's red ray and increases inefficient evictions (red shaded region), assuming positive joint surplus ($k_{Li} + k_{Ti} > 0$) and misperception wedge ($\Delta p_i d_i > 0$). We call evictions that occur only because of hostile preferences "repugnant" — that is, "repugnant" evictions are those which would not occur if a_{ji} were 0 for each j where $a_{ji} < 0$. Such repugnant evictions will always be inefficient.[18]

Second, altruism also affects eviction and efficiency. The symmetric comparative static is that if eviction is inefficient, more altruism ($\overline{a}_i \uparrow$) raises the chance of efficient bargaining because it steepens the slope of Diagram 1's red ray. Altruism can therefore sustain bargaining that would otherwise fail due to misperceptions. However, with opposite-signed misperceptions, altruism can cause efficient eviction, in the case where $\Delta p_i d_i < k_{Li} + k_{Ti} < \Delta p_i d_i / \overline{a}_i < 0$.

Third, with zero misperception wedge, social preferences play no role. In that case, Equation (5) reduces to the benchmark of Equation (2).[19] However, hostility can cause eviction from even modest misperceptions, since enough hostility can make $\overline{a}_i$ arbitrarily small.

1.5 The Role of Emergency Rental Assistance

Setup. We now consider an emergency rental program, intended as a stylized version of ERAP. Diagram 2 shows the full game with the policy intervention. The landlord decides whether to take up rental assistance, which pays the full back rent d_i but requires paying a take-up cost k_{Li}^P. If the landlord takes up the program, then eviction is impossible. If the landlord declines the program, then the tenant can make a bargaining offer as in the prior subsection.[20]

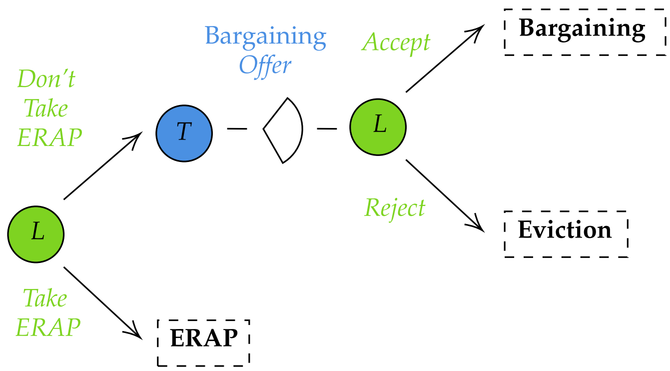
The payoffs are identical to the prior subsection if landlord does not take up. The landlord gets $d_i - k_{Li}^P$ if she takes up. The tenant gets 0 if the landlord takes up.

[18] Efficiency concepts with altruism are philosophically challenging (Friedman, 1988). If $a_T \neq a_L$, then utility is not transferrable and the natural efficiency concept has the more altruistic party transfer infinite wealth to the other. To sidestep this issue, we say that the efficient outcome is the natural definition for *equal* altruists ($a_T = a_L$): the outcome is efficient when eviction occurs if and only if $(1 + a_T)k_L + (1 + a_L)k_T < 0$. That is, eviction occurs if and only if joint surplus, adjusted for equal altruism, is negative. This definition happens to coincide with the definition of an efficient outcome when parties are both classical.

[19] In our model, with coincident beliefs, parties with social preferences reach a bargained solution if there is joint surplus from bargaining. For any altruism parameters, a higher altruism-adjusted surplus can always be achieved outside court. Social preferences only change how surplus is divided.

[20] We ignore the tenant's application choice. One way of thinking about this game is that it is played among the sample of tenants who have already applied for the program, and who need the landlord to accept it.

Diagram 2: Timing and Game Tree



Note: The arc for the tenant's decision indicates a continuous choice problem.

Take-Up Decision. This environment yields the following threshold condition for program take-up by the landlord:

Proposition 4. *Emergency rental assistance take-up occurs if and only if*

$$k_{Li} + k_{Ti} + w_i \geq \frac{\Delta p_i d_i}{\bar{a}_i}, \tag{6}$$

where the "program wedge" w_i is defined as:

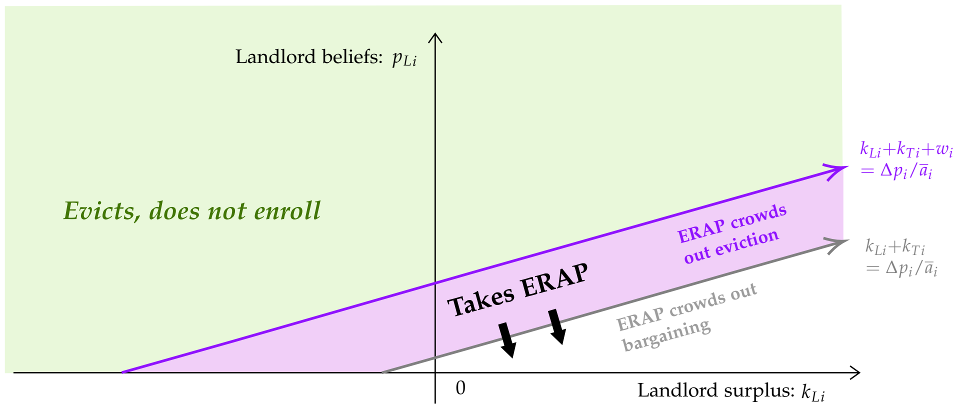
$$w_i = \frac{1}{\bar{a}_i}\left(\underbrace{\frac{d_i - k_{Li}^P}{1 - a_{Li}}}_{\substack{\textit{Altruism-adjusted} \\ \textit{net ERAP payment}}} - \underbrace{\left(p_{Ti}d_i + \frac{k_{Ti} + a_{Ti}k_{Li}}{1 - a_{Ti}}\right)}_{\textit{Outside option adjustment}}\right). \tag{7}$$

Proposition 4 indicates that the program can "crowd out" (i.e., cause not to occur) bargaining, or efficient eviction, or inefficient eviction. Notice that Equation (7) reverses the sign of the inequality of Equation (5), but is augmented with the program wedge w_i. If $w_i < 0$ and take-up occurs, then an eviction would not have occurred without the program: ERAP entirely crowds out bargaining. If $w_i > 0$ and take-up occurs, then it may also crowd out some eviction.

To develop intuition for the program wedge, observe that the first term within parentheses is an altruism-adjusted ERAP net payment (benefit d_i less take-up cost k_{Li}^P). As this term rises, ERAP becomes more valuable. The second term accounts for the fact that ERAP forecloses bargaining. As the landlord's outside option of bargaining improves (e.g., the tenant's court cost rises), the second term rises, which shrinks the wedge overall.

Diagram 3 shows take-up by eviction versus bargaining for a fixed $w_i > 0$, now in p_{Li}/k_{Li} space (i.e., fixing other parameters, so translating the comparative statics in Diagram 1). For this configuration of parameters, landlords enroll for all values to the right of the gray line. ERAP

Diagram 3: Effects of ERAP



Note: The diagram considers a case with $a_{Ti}=0$, such that the wedge is constant for all k_{Li}.

crowds out all bargaining (white region) as well as some evictions (purple region). If $w_i < 0$, which corresponds to shifting the purple line to the right of the gray line, then ERAP exclusively crowds out bargaining.

Even absent nonclassical forces, ERAP is not perfectly targeted. The landlord's net surplus from enrolling in ERAP is:

$$S_{Li} := \underbrace{d_i - k^P_{Li}}_{\text{ERAP payoff}} - \underbrace{\left(p_{Li}(1-a_{Li})d_i - k_{Li} - a_{Li}k_{Ti}\right)}_{\text{Outside option}}. \tag{8}$$

Enrollment occurs if and only if landlord net surplus S_{Li} is positive, as the condition $S_{Li} \geq 0$ is equivalent to (6). Consider the classical benchmark with zero altruism. The landlord's net surplus S_{Li} is increasing in the landlord's court costs k_{Li}. Simultaneously, higher court costs also raise the value of bargaining. Thus, even in the benchmark, ERAP risks enrollment among pairs who otherwise bargain.

Still, altruism amplifies the risk of inframarginality. Observe that surplus S_{Li} is increasing in a_{Li}. Intuitively, enrolling in ERAP costs landlords a fixed k^P_{Li}. Landlords' outside option of evicting tenants or bargaining with them is decreasing in altruism. When enrolling, altruists gain more utility per dollar of take-up cost. Perversely, altruism also makes bargaining more likely (Diagram 1). We call this force, in which the pairs who enroll are altruists who seldom evict in the first place, "perverse selection on altruism."

If ERAP does stop evictions, the evictions that it prevents could be efficient, inefficient or repugnant. The values of costs, misperceptions, and social preferences govern whether households at the margin of inefficient eviction enroll.

Appendix B extends the framework to include a two-period dynamic setting with endogenous rental debts, concave tenant utility, and Nash Bargaining. Sufficiently low poverty or enough impatience can recover the same forces as in the main text.

2 Institutional Details and Experiment Sample

2.1 Background

Setting. Our setting is the City of Memphis, Tennessee and its surrounding county of Shelby County. Memphis has a population of 620,000, is 65% Black, and has a poverty rate of 24%, according to U.S. Census Bureau estimates for 2022. Shelby County has a population of 920,000. In 2016, according to estimates from Eviction Lab (Gromis and Desmond, 2021), there were 48 eviction judgments per 1,000 renter households in Shelby County. Shelby County was in the top quartile of eviction filings in 2018 among counties with at least 100,000 renter households.

Eviction Process. Most evictions in Shelby County are caused by nonpayment of rent, though property damage or other violations can also prompt eviction. Once a tenant fails to pay rent for a single month, landlords typically gain the legal right to evict. To formally evict a tenant, landlords must first serve an eviction notice that gives the tenant 14 days to pay. If the tenant fails to pay, the landlord files for an eviction warrant (a *filing*), which begins the court process and includes a hearing date. If the judge rules in favor of the landlord at the hearing (a *judgment*), the tenant must vacate the property and, if they do not do so within ten days, the landlord may obtain a writ of possession from the county sheriff. A writ authorizes them to remove the tenant and their belongings against their will from the property.

Landlords can either seek judgments for possession only or judgments for money (owed back rents or damages) as well as possession. Of the roughly 26,600 evictions filings in 2019, 71% yielded judgments, of which 54% included money. Money judgments require the tenant pay the landlord overdue back rents, and give the landlord the legal right to garnish tenants' wages. But they require that landlords give "personal service" of the eviction notice, which can be costly as tenants can intentionally evade landlords.

Figure A1 shows a time series of the share of filings that result in judgments within 150 days of filing. Pandemic-related court delays and the eviction moratorium reduced judgment rates below 20% until early 2021. After the eviction moratorium expired in Shelby County in spring 2021, only 40% of filings yielded judgments. The lower level of judgments persists throughout our study period.

ERAP. Under the CARES Act and Consolidated Appropriations Act of 2021 (CARES II), state and local governments received funding to form Emergency Rental and Utilities Assistance Programs (ERAPs). The programs paid overdue debts of rent and utilities that accrued during the coronavirus pandemic. We partner with the Memphis and Shelby County ERAP, which operated as an integrated program. Through December 2022, the Shelby County ERAP distributed at least $100 million in assistance to around 20,000 households.[21] By comparison, Section 8 supports about 35,000 households in all of Tennessee.

Memphis ERAP shared application and payment data with the landlord and tenant contact information that we use to solicit participation in survey experiments. See additional ERAP program details in Section 5.2.

2.2 Experiment Overview and Recruitment

We conduct separate experiments with landlords and tenants. Figure 1 presents a visualization of the survey flow for each. There are two main sections to each experiment: a part that uses Dictator Games to elicit social preferences (Section 3) and a part that elicits misperceptions (Section 4). Additional details are in Appendix C.

Experiment Sample Recruitment. We recruited experiment participants from Memphis ERAP application records. We used completed and partial applications, so not all participants ultimately received ERAP payment.

We conducted all surveys online. Recruitment materials informed participants that they would receive a $20 gift card for completing the survey and could earn other rewards. Participants were informed about the purpose of the study and provided consent before beginning. Responses are identified and linked to ERAP records.

We conducted the landlord survey experiment from August 29 to October 28, 2021, sending several reminders over this time period by email and text message. We contacted 3,966 unique email addresses associated to landlords and property managers listed on tenant ERAP applications. We received 371 valid responses, for a response rate of about 9% (12% conditioning on a valid landlord email address).

We contacted 16,861 unique tenants via email over 14 survey waves from February 14 to May 30, 2022. We sent one email reminder, along with text-message invitations and one reminder to tenants with valid phone numbers. We obtained 1,808 valid responses in total, for a response rate of 11% (12% conditioning on a valid email address).

Ethics. Given the vulnerability of our study population, we took care to ensure our experiment created minimal risk of harm. We designed the surveys in partnership with ERAP personnel and eviction defense attorneys in Memphis. We vetted all information provided, experimental elicitations, and outcome modules to ensure they would help both parties. We only provided information that we believed would reduce risk of eviction. Elicitations that had any potential

[21] More precise estimates are difficult to obtain because we only have complete data on some parts of the program. Section 5 studies an arm of the program that paid more than $25 million to about 5,000 unique households.

for adverse outcomes (e.g., choices in Dictator Games) were strictly anonymous. Landlords and tenants benefited from survey participation, as they were offered payment, information about eviction and ERAP, and opportunities to participate in ERAP or negotiate to avoid eviction.

2.3 Demographics

Tenant Characteristics. Study participants are low-income and housing-insecure (Table 1, Column 1). The experimental sample is 81% female and 88% Black (Panel A). They have incomes of less than $1,000 per month and owed a median of around $3,500 when they applied for ERAP. About a third have ever been evicted, and almost 90% have had overdue rents (Panel B).[22]

Landlord Characteristics. 58% are Black, and 62% of landlord participants are female (Table 2, Column 1). 62% are landlords, and 29% identify as property managers. We refer to both groups as "landlords." 48% report owning or managing 10 or more units, with the remainder about evenly divided between managing 1 or 2 units and 3 to 10 units. Because large landlords may have multiple property managers or employees, we have many observations from the larger Memphis landlords and property management companies.

Selection. We use administrative data on tenant applications to examine whether experiment participants differ from the overall ERAP pool (Table 1, Columns 2–4). Overall differences are relatively small, with the exception of the share female (9 pp higher among participants) and monthly incomes ($160 higher among participants). We do not have demographic data on non-participating landlords, but we see information about their tenants. Participating landlords' tenants owe more in back rents and less in utilities. The tenants of participating landlords are also slightly more likely to be white.

Balance and Attrition. We test balance for our survey experiments by examining characteristics of people who complete the study (Appendix C shows attrition funnels and balance tables). One of five treatments across experiments, the landlord information experiment, is unbalanced on observables ($p = 0.01$). Results are unchanged when we include controls or use post-double-selection Lasso to select controls (Belloni et al., 2014).

Conditional on completing a short demographic questionnaire that starts the survey, 77% of tenants and 65% of landlords complete the surveys. Our sample consists of participants who complete the whole survey.[23]

[22]We survey tenants at different times in the ERAP process. Back rents at the time of the survey are lower than at application because some tenants are evicted, move, or are paid before the survey.

[23]Attrition rates are constant across treatments (joint p-value > 0.2 for both surveys, Table A6). 96% of tenants and 91% of landlords complete the DGs. Appendix C shows results for the Dictator Games among this larger sample, including those who attrit later.

3 Altruism and Hostility: Measurement and Results

3.1 Measurement

Background on Dictator Games. We adapt the laboratory experiment called the "Dictator Game" (DG). In a standard DG, the Dictator is given an endowment. The experimenter elicits how much of the endowment the Dictator wants to give to another player, the "Opponent." Classical economic models predict that the Dictator gives \$0. A meta-analysis finds that 65% of Dictators give a positive value, and the conditional average given is around 40% (Engel, 2011).

Modified Dictator Game. We modify the DG so that it can measure both altruism and hostility. Consider a Dictator who gives \$0 in the standard DG. It is ambiguous whether she has classical preferences and wishes to maximize her own payment, or if she has hostile preferences and derives utility from reducing her opponent's payment.

In our modification, the Dictator chooses between two bundles: (\$$s$ to Dictator, \$0 to Opponent) and (\$$x$ to Dictator, \$$x$ to Opponent). In the first bundle, the Dictator receives \$$s$ and the Opponent gets nothing. In the second bundle, both players receive \$$x$. We vary s within-subject until we identify (bounds on) the participant's "indifference point" $S(x)$ — i.e., the value that renders her indifferent between $(s, 0)$ and (x, x).

With classical preferences, $S(x) = x$: the Dictator would prefer whichever bundle gives a larger private payoff. With altruism, $S(x) > x$: the Dictator would forgo some private payoff so that the Opponent receives a payoff. With hostility, $S(x) < x$: the Dictator would forgo some private payoff so that the Opponent receives \$0.

The game exactly delivers the Becker (1974) altruism parameter. For individual i, granting linear preferences, their altruism parameter $a_i \equiv (S_i(x) - x)/x$, as $S_i(x)$ must satisfy

$$\underbrace{S_i(x) + a_i \cdot 0}_{\text{Payoff from } (S(x),0) \text{ bundle}} = \underbrace{x + a_i \cdot x}_{\text{Payoff from } (x,x) \text{ bundle}} . \tag{9}$$

Relative to the large literature employing the standard DG, this modification is relatively novel and has the benefit of eliciting both altruism and hostility at once.[24]

Elicitation and Opponents. Bundles are Amazon gift cards. We elicit bounds on indifference points using a multiple price list: we repeatedly ask whether a participant prefers $(s, 0)$ or (x, x), varying s until the participant switches her response. We implement a small share of choices to ensure incentive-compatibility (see Appendix C). We provide plain-English instructions (see Figure A2A for an example elicitation). We inform participants that it is in their best interest to

[24]The modification takes inspiration from other DG adjustments (e.g., Charness and Rabin, 2002; Kranton et al., 2016) and the joy-of-destruction game (Abbink and Sadrieh, 2009; Abbink and Herrmann, 2011). Contemporaneous work on religious conflict in Nigeria implements a similar DG modification (Ortiz, 2023).

respond truthfully (Danz et al., 2022).

Landlords play the game twice against three potential opponents (Figure 1): their own, named tenant (with probability 2/3); a random, unnamed tenant (with probability 1/3); and a random, unnamed landlord (all landlord participants). Tenants play the game twice against a symmetric group of three potential opponents: their own, named landlord (with probability 2/3); a random, unnamed landlord (with probability 1/3); and a random, unnamed tenant (all tenant participants). Landlords' DG behaviors against own versus random tenants indicate whether preferences reflect the specific relationship versus attitudes toward the entire class of tenants. Similar logic holds for interpreting tenants' behaviors toward own versus random landlords.

Instrumental reasons for helping or harming the opponent, such as signaling motives, are unlikely to explain DG behavior. We inform all participants when they provide consent that their responses will be anonymous. We also remind tenants in the DG that "the gift card will not be associated with your name and won't count as rent" and include an extra confirmation check. Even if someone receives a gift card, they cannot deduce that it came from their own landlord or tenant, as they could receive a gift card from a random landlord or tenant.[25] Some free responses for both parties do mention instrumental reasons for their choice, so we cannot rule out the concern entirely.

Stakes. Landlords play the game where the alternative $\$x$ is a \$10 gift-card given to each party and $\$s$ can vary between \$1 and \$20. Among tenants, we randomly vary $x \in \{10, 100, 1000\}$ across participants and correspondingly vary s between $x/10$ and $2x$. For instance, we ask some tenants whether they prefer (\$900 self, \$0 opponent) or (\$1,000 self, \$1,000 opponent), and others whether they prefer (\$9, \$0) or (\$10, \$10). This subexperiment lets us test whether tenant altruism and hostility are purely low-stakes phenomena.

Benefits of DGs. DGs have several strengths. First, there should be a high burden of proof to argue that participants derive utility from reducing each other's utility. Observing participants actually burn money to prevent the opponent from getting a payoff arguably provides stronger evidence than free-response reflections or similar. Second, the game delivers the Becker (1974) measure of social preferences, facilitating quantitative analysis. Third, experimental manipulation lets us compare affect across opponents. To raise confidence in the DG results, we show that qualitative measures and simpler quantitative elicitations correlate with DG behavior.

[25]88% of tenants correctly answer a comprehension check about anonymity. With landlords, we discuss anonymity only at the point of consent. Related real-stakes measures of hostility with no room for instrumental motives have similar levels and are highly correlated for both landlords and tenants (Appendix C).

3.2 Results

Landlords and tenants exhibit substantial hostility and altruism (Figure 2).[26] 15% of landlords are hostile toward their own tenant (Panel A), meaning they forgo money so that their own tenant receives nothing. 7% of landlords are hostile toward a random tenant, implying a 9-pp difference in hostility between own versus random tenants (p-value of difference $= 0.006$). Meanwhile, landlords are similarly hostile toward own tenants as random landlords. Taken together, these differences suggest landlords' acrimony toward their own tenants is relationship-specific and not distaste for renters as a class.

Tenants are more hostile than landlords (Panel B). 24% of tenants are hostile toward their own landlord, and 22% are hostile toward a random landlord, both of which are significantly higher than rates of landlord hostility toward own tenants ($p < 0.01$) and random tenants ($p < 0.001$). Tenants are 10 pp more hostile toward their own landlords versus random tenants ($p < 0.001$), while the difference in hostility between their own landlord and random landlords is statistically insignificant ($p = 0.279$). The fact that tenants are hostile toward random landlords suggests that tenants' acrimony is generalized and not only relationship-specific.

As further evidence that hostility is intense, and that minor elicitation noise does not drive the results, more than half of landlords and tenants who express any hostility are "highly hostile," meaning they prefer the bundle $(x/10, 0)$ to (x, x). Differences in high hostility are also significant (Table A15). For instance, 9.0% of landlords are highly hostile to their own tenants, versus 3.6% who are highly hostile to random tenants (p-value of difference: 0.031).[27]

Altogether, 24–39% of landlords and tenants have at least one party exhibiting hostility toward their own tenant/landlord, depending on matching between hostile landlords and tenants. Suggestive evidence about assortative matching between landlords and tenants suggests that about one third of landlord–tenant pairs have at least one hostile party (Appendix E). If we conservatively posit that only own-tenant/own-landlord minus random-tenant differences give true hostility, rather than elicitation errors, we still obtain estimates of 9–19%. Even if just 9% of parties were hostile, not all will receive evictions (we match parties to evictions in Section 5). Thus, these rates of hostility could cause a meaningful share of ultimate evictions.

Nevertheless, more than two in three landlords are "highly" altruistic toward their own tenant, meaning they forgo doubling their own private payoff to ensure an even split. Landlords are

[26] Table A15 summarizes results and presents statistical tests. Figure A4 shows histograms of indifference points.

[27] The relatively high rates of hostility toward random tenants and landlords are partially driven by anchoring. Recall that each participant does the DG twice, in random order. When we consider behavior in only the *first* DG each participant plays, 3% of landlords and 8% of tenants are hostile to random tenants (Figure A6 and Table A16), similar to rejection rates of even offers in ultimatum games with generic opponents (Camerer, 2003). Reassuringly, in this non-anchored sample, the levels of hostility toward own tenants and own landlords are similar to our primary estimates. They are statistically different from the benchmarks despite cutting the sample in half. Residual hostility among tenants toward other tenants could reflect negative affect toward other low-income households, which sociology research documents is prevalent among the poor (Shildrick and MacDonald, 2013).

6.5 pp more likely to be highly altruistic to their own tenant than a random landlord ($p = 0.03$). Tenants are more likely to be highly altruistic toward another tenant rather than own or random landlords. Yet we still see substantial tenant altruism toward landlords, which is notable given our sample's precarious housing situations.

These social preferences are stronger than those in a benchmark. We test whether hostility and altruism are more common in the landlord and tenant samples than among random participants recruited from online survey panels. We conduct the same DG with random participants in Memphis ($N = 275$) and a nationally representative sample ($N = 623$) (see Appendix E for recruitment details). Landlords are 14 pp more likely to be highly altruistic toward their own tenant than the pooled random sample is to a random tenant (Figure A10, $p < 0.001$). Tenants' hostility toward own landlords exceeds the pooled random sample's hostility toward random tenants by 11 pp ($p < 0.001$) and toward random landlords by 4.4 pp ($p = 0.014$).[28]

Turning to heterogeneity by demographics, we find that large landlords and landlords with eviction experience are significantly more hostile; tenants with high levels of back rents are significantly less hostile; and female and Black tenants are significantly more hostile (Figure A5).

Free-Response Reflections. Free-response reflections confirm extreme social preferences. One tenant wrote:

> "He also harassed me for rent even though I had applied to ERA. Even sent his property manager to my door with a gun."

This tenant had an indifference point of $S(1{,}000) \in [300, 400]$: she was willing to give up at least \$600 so that her landlord would receive \$0. On the other hand, another tenant wrote that her landlord "is a very good person and is willing to help the very best she can." Some participants also appealed to religion. One landlord wrote, "I am a Christian and I try to live out my faith."

Validation and Experimenter Demand. One might worry that the high levels of altruism and hostility reflect inattention, confusion, or other artifacts of the lab setting. In Appendix C, we describe our attention checks and show that dropping inattentive participants does not affect results. Appendix C also shows that behavior in the DG is highly correlated with: (i) additional measures of landlord–tenant relationships that we included in the surveys, (ii) tenant sentiment about landlords extracted from simple Likert scales and natural-language processing analysis of free-response questions, and (iii) externally validated measures of preferences that extend beyond the laboratory from Falk et al. (2018). Another benefit of these exercises is that several of them are especially unlikely to have instrumental value, suggesting that behaviors in the DG indeed reflect social preferences and not other motives.

[28] The hostility exceeds the shares who reject even offers in ultimatum games (Camerer, 2003). However, the rates of hostility are similar to those collected by Kranton et al. (2016) among college students when allocating to members of a different political party (20%), or the shares who choose to destroy in joy-of-destruction games where subjects can pay to destroy others' bundles (26% in the hidden treatment in Abbink and Herrmann 2011).

In a particularly informative check, we ask participants whether they want to enroll their own landlord or own tenant in a lottery to win a gift card (Figure A14). Enrolling the opponent is costless (see details in Appendix C). The shares of participants who reject enrolling the opponent are similar to the shares who are hostile in the DG. Moreover, behavior in this exercise and the DG is highly correlated.

Differences by opponent further allay elicitation concerns. For instance, tenants are unlikely to be more attentive to DGs played against a random tenant than against their own landlord.

Experimenter demand is unlikely to explain this pattern of results. First, demand effects would push participants to appear more generous than they might behave in private. But in fact, the rates of hostility we find are high relative to similar DGs. Second, some of the above validations are especially insulated from demand concerns. For instance, we conduct tenants' free-response reflections before lab games. Tenants seemed to share honest (often negative) perspectives.

A related concern is that landlords give tenants money because they expect to recoup it as rent. This motive would imply that we overestimate altruism and underestimate hostility. First, observe that this concern cannot explain behavior of landlords who are highly altruistic (nearly two-thirds). Absent altruism, these landlords are weakly better off just taking $20 directly. Second, we use landlords' elicited beliefs about the amount of money they can recoup from tenants in an eviction (Section 4) to scale down their implied altruism. Naturally, landlords become less altruistic and more hostile. But preferences remain extreme (Figure A15).

Selection Tests. An important concern is that people who complete the study if invited are more likely to be altruistic than people who do not. In general, we expect this concern to attenuate our estimates of hostility. In support of this hypothesis, participants who complete the DGs but attrit mid-study have slightly higher levels of hostility and, among tenants, lower rates of high altruism (Table A15 vs. Table A18), suggesting that our main results on hostility are conservative. Next, we use entropy balancing (Hainmueller, 2012) to reweight participants on observables to match non-participants. Our estimates of social preferences are essentially unchanged (Table A17).

Robustness to Stakes and Controls. Using experimental variation from our stakes sub-experiment, we find no evidence that stakes affect behavior (Figure A9).[29] Landlords' differences between own tenants and random tenants are robust to controls or limiting the sample to attentive participants. This robustness also applies to tenants' differences between own landlords and random tenants (Tables A10–A11).

[29]This result contributes to a conflicted literature on the importance of stakes in social-preference experiments. For instance, stakes affect behaviors in ultimatum games in India (Andersen et al., 2011).

4 Misperceptions: Measurement and Results

4.1 Measurement

Structure. For both tenants and landlords, we start the misperceptions module by eliciting prior beliefs (i.e., beliefs before being possibly treated with information), after conducting the DGs (Figure 1). We then randomly expose half to information, before measuring posterior beliefs.

At a high level, we structure both misperceptions elicitations as follows. We elicit participants' prior beliefs about an observed statistic that we can benchmark to ground truth. We also elicit participants' beliefs about a closely related statistic that we cannot observe. Finally, we elicit posterior beliefs about the unobserved statistic among the treated group only.

Landlords: Recoupment Rates. In the landlord survey, we focus on beliefs about whether tenants repay eviction judgment balances after a court eviction. We use public court records to link eviction balances to evictions. Less than 10% of money judgments in Shelby County eviction court result in any repayment to the landlord within a year. Informing landlords that money judgments are rarely recouped could reduce their perceived return to formal eviction.

Why did we think judgment balances are important to landlords' cost–benefit analysis of formal eviction? First, in part due to the pandemic context, the sample contains many small landlords who lack prior eviction experience. Second, as Section 2 notes, roughly half of landlords obtain evictions for *money* and not just *possession*, even though money judgments are costlier to obtain. Money judgments' value relative to judgments for possession is determined by the rental debts recouped from money judgments.

Among landlords, we elicit prior beliefs about the recoupment rate of judgment balances for the *average* tenant in Shelby County court (the observed statistic above). We ask landlords the share of tenants who fully repaid balances between January 2020 and August 2021. We also elicit landlords' subjective probability of recouping judgment balances if they filed an eviction against their *own*, named tenant (the unobserved statistic above). When eliciting beliefs, we included several confirmation questions and visual aids (Figure A2B shows an example).

Tenants: Two Beliefs. Tenant misperceptions about landlords' social preferences could affect tenant bargaining propensity. To examine this hypothesis, we elicit tenants' beliefs about the share in the landlord experiment who preferred to split a $20 gift card evenly with their own tenant, rather take the gift card for themselves (i.e., their indifference point was more than $20). 69% of landlords preferred the bundle ($10, $10) to the bundle ($20, $0) when their own tenant was the opponent.[30] We also elicit the subjective probability that the tenant's *own* landlord would evenly split the gift card.

[30] We elicit this fact soon after tenants play the DG, so participants are familiar with the game.

Misperceptions about landlords' behavior in court could also affect tenant bargaining propensity. Tenants could believe that eviction filings are empty threats unlikely to cause eviction, or, conversely, that bargaining after court filings is impossible. We elicit tenants' beliefs about the share of landlords who bargain during the formal eviction process to avoid eviction judgments. 31% of Shelby County landlords that initiated the court eviction process (i.e., filed) in 2019 had explicitly withdrawn or settled (i.e., not obtained judgments) by August 2021. We elicit tenant beliefs about this statistic on average, and also their subjective probability that their *own* landlord would drop an eviction if filed.

Treatment and Posteriors. We randomly treat half of participants with information intended to shift their beliefs. The purpose of shifting beliefs is to study whether correcting beliefs can change behaviors, which has direct policy implications and also sheds light on the relationship between misperceptions and evictions.

For each beliefs module, the information that we provide precisely corresponds to the observed statistic in each elicitation. For instance, we inform landlords that 6 out of 100 cases on average fully repaid judgment balances. The tenant experiment cross-randomizes the two treatments.

After the information, we collect posterior beliefs for treated participants. These posteriors update the prior beliefs about one's own tenant or landlord (the unobserved statistic). For instance, we ask landlords to report posterior beliefs about whether their own tenant would repay debts in a money judgment. The treatment is informative for posterior beliefs if the unobserved statistic is correlated with the observed statistic.

To ensure that participants intend to update, we first ask participants whether their prior belief is "too high," "too low," or "still correct." Then we ask them to report a posterior consistent with the direction of the reported belief update. We impose that the control group's posteriors are equal to their priors since they did not receive information.[31]

Outcomes. We consider three groups of belief outcomes. First, we test whether participants' prior beliefs about the observed statistic are accurate. Second, we test whether participants revise beliefs about the unobserved statistic after receiving information. If prior beliefs are unbiased, they should not respond on average to truthful information. Third, we test whether providing information leads participants to change revealed behaviors.

Our main revealed landlord outcome is whether they choose to receive informational materials about applying for the ERAP. This is a real choice: the program sent materials to landlords who requested them.[32] We see this measure as revealing landlord interest in ERAP participation.

[31]This procedure assumes that eliciting beliefs does not itself cause beliefs to move in one direction on average. To test this assumption, we collected a pre-registered placebo belief for landlords. Landlords do not update on average about the placebo after receiving information (Appendix E).

[32]Because of concerns about power, we pre-registered this and other revealed belief outcomes as secondary. We show other outcomes in Section 4.2 and Appendix C and conduct tests for multiple hypotheses.

Our main revealed tenant outcome concerns how tenants treat a real opportunity to propose a payment plan to their landlords. A payment plan is an agreement in which the tenant agrees to repay none, some, or all of their rental debts over time. Tenants could always negotiate outside the experiment or decline to propose a plan. However, forming a payment plan in the experiment may reduce transaction costs, confer legitimacy, or provide useful structure to tenant proposals. We see this measure as revealing tenant interest in bargaining.

In the survey, we ask tenants with back rents if they are interested in a payment plan. If they are, then we ask how much they propose to repay and the payment period. We confirm with tenants several times that we will actually email their payment plan, and we do so after the survey concludes. Payment-plan proposals in the experiment are nonbinding offers. We restrict proposals to involve tenant repayments between zero and the total back rents owed.[33]

Connection to the Model. Landlord's beliefs about tenant repayment exactly correspond to p_{Li}. Tenants' beliefs about landlord behavior in the DG map to beliefs about a_{Li}. Beliefs about landlord propensity to drop eviction filings map to beliefs about k_{Li}. If tenants make bargaining offers based on their beliefs about whether Equation (5) is satisfied, then believing either of a_{Li} or k_{Li} is low directly reduces bargaining propensity.

Unlike in the model, the experiments compare landlords and tenants to ground truth, rather than each other's beliefs. We view this as a conservative test about the importance of noncoincident beliefs, although it is possible that both parties' misperceptions exactly cancel.[34]

4.2 Results

Landlords are highly optimistic about the probabilities of recouping back rents (Figure 3A). Their incentivized beliefs about average tenants are 18 percentage points higher than the true value of 6% (s.e.: 1.4), with substantial mass above 50%.[35]

Landlords who have ever evicted their tenant have 7 pp more accurate beliefs on average (Figure A16B). Nevertheless, more than 25% of landlords who have ever evicted believe there is at least a 35-percent chance of recouping back rents from the average tenant.

Tenants, meanwhile, exhibit two beliefs that may suppress bargaining. They underestimate landlord altruism (Figure 4A). The average tenant reports that 47% (s.e.: 0.84) of landlords would

[33] We do not allow negative or large payments to avoid the possibility that such proposals could cause retaliation from landlords or otherwise harm tenants.

[34] We did not elicit beliefs about the court eviction process symmetrically because doing so could introduce ethical concerns. For instance, we did not want to provide tenants with information that tenants rarely repay eviction judgments, as that could harm landlords.

[35] Landlords' beliefs about their own tenants are even more optimistic: landlords believe they have a 43-percent chance of recouping back rents from their own tenant (s.e.: 1.8). Landlords could in principle be unbiased about their own tenant if this sample of landlords would indeed have a 43-percent chance of recouping back rents from their own tenants. That seems unlikely, since it would render these landlords extraordinarily effective relative to the average landlord's recoupment rate of 6%. Beliefs about own and average tenants are highly correlated (Figure A16A), which further suggests that landlords are biased about their own tenant.

choose ($10 self, $10 own tenant) to ($20, $0), when in fact 69% do (vertical red line). Similarly, we find that tenant beliefs about landlords' propensity to drop eviction filings in the future are overly optimistic (Panel B).

Presenting information about the average tenant causes 43% of landlords to update beliefs about their own tenant (s.e.: 3.8 pp). The average unconditional belief update is -12 pp (s.e.: 1.6) (Figure A8A). Thus, the difference between landlords who have and have not evicted — a measure of eviction experience — is associated with belief updates that are 60% as large as receiving information directly.

Information corrects both tenant beliefs, although less than in the landlord experiment. If given information, 32% and 37% of tenants update beliefs about altruism and dropping filings, respectively. Information about landlord altruism increases beliefs on average by 5.9% (Figure A19). There is heterogeneity in the belief update, since the information is central in the beliefs distribution. Among people whose beliefs about their own landlord lie above 69%, beliefs shift down by 8 percentage points. For bargaining, the treatment reduces beliefs on average by 5.9 percentage points (Panel D).[36]

We obtain lower bounds on the share with misperceptions by calculating the percent of participants whose prior beliefs are at least 20 pp from ground truth (Figure A17).[37] 33% of landlords and 79% of tenants have misperceptions by this measure. 17% of landlords and 41% of tenants both have incorrect beliefs and choose to update when told the truth.[38]

Revealed-Preference Outcomes: Landlords. The treatment increases the probability that landlords request informational materials about the ERAP by 11 pp, or 17% (Figure 3B, $p = 0.02$). These are large effects for a light-touch information intervention.

These intent-to-treatment estimates are stable or rise if we change how we choose controls or restrict the sample to attentive landlords. As this treatment does exhibit imbalance (Section 2.2), including controls is an important check.

Treatment effects are concentrated among landlords whose prior beliefs were especially incorrect and thus who, on average, updated more in response to the information treatment. We estimate an instrumental-variables specification for the effect of landlord beliefs about recoupment on requests for ERAP materials, using the prior beliefs, in Appendix E. We also conduct placebo tests which examine whether unrelated beliefs move due to the information treatment, using additional data we collected on landlord beliefs about time to process an eviction.

[36]Tenants are more uncertain than landlords: they are more likely to report 50%, which may suggest cognitive uncertainty (Enke and Graeber, 2019). Dropping tenants reporting 50% has little effect on average misperceptions. Using a Likert scale, we ask tenants to report how certain they are about their prior beliefs. Uncertain tenants report lower beliefs about whether their landlord would split the gift card and whether their landlord would bargain.

[37]We choose 20 pp so that tenants who report 50 are conservatively treated as having correct beliefs.

[38]The only way to obtain small shares of misperceptions using this approach is to consider the share of tenants who have incorrect priors about *both* beliefs, and choose to update. 5% of tenants have misperceptions according to this measure. However, as having one mistaken belief suffices impede bargaining, this test may be overly conservative.

We test the effects of information on several other revealed landlord outcomes and find no significant effects of the treatment among any outcome except requesting ERAP materials (Table A12; see table notes for outcomes). Muted or wrong-signed effects on other outcomes (e.g., tenant referrals) suggests that information may be insufficient to change other behaviors.

A related concern is multiple-hypothesis testing. A stacked test rejects the null that the treatment does not affect any measured outcome ($p = 0.01$). Meanwhile, multiple-hypothesis corrected p-values (Romano and Wolf, 2005) attenuate the effect for requesting materials ($p = 0.10$). Altogether, we recommend caution when interpreting this evidence.

Revealed-Preference Outcomes: Tenants. 74% of tenant survey participants are eligible for payment plans because they owe their landlord money for rent at the time of our survey. Neither information treatment had a significant intent-to-treat effect on whether the tenant requested a payment plan. The absence of these overall effects is unsurprising because the true information is more central in the prior-beliefs distribution. Some tenants update up and others update down, which attenuates the average effect.

As a result, we split tenants by whether their prior beliefs about their own landlord lie above or below the information. We find a moderate effect for the altruism correction (Figure 4C) and no detectable effect for the bargaining correction (Panel D). In particular, tenants with optimistic beliefs about their landlord's altruism become 9 pp less likely to request a payment plan ($p = 0.08$). This result suggests that correcting tenant misperceptions can affect real bargaining behaviors, but we acknowledge the moderate effect size and multiple hypotheses.

Survey responses from landlords about payment plans suggest the presence of inefficient bargaining failure. We surveyed landlords to whom we sent payment plans from tenants. Out of 691 payment plans sent (including emails that bounced), we received 96 responses indicating whether the landlord would accept the payment plan (response rate: 14%). 11% of respondents said they would accept the payment plan outright, and 45% said they would accept or discuss the payment plan with their tenant. Even if every non-respondent would reject the proposed payment plan, these results imply that a minimum of 6% of landlords would accept or discuss the payment plan with their tenant. The payment plans were nearly costless to propose to landlords: we simply emailed tenants' survey responses to landlords.

5 Connecting the Model and Experiment to Evictions and Policy

5.1 The Model and Experiment Predict Evictions

5.1.1 Adjusted Surplus Index

We now study whether the forces in the experiment predict evictions and other real-world outcomes, as the model suggests. The model implies a notion of "adjusted surplus" in the landlord–tenant relationship. For each individual, we wish to construct:

$$\theta_i := k_{Li} + k_{Ti} - \frac{\Delta p_i d_i}{\bar{a}_i}, \tag{10}$$

for adjusted surplus θ, which comes from Equation (5). As adjusted surplus increases, the model predicts eviction to be less likely.

We test whether a proxy for θ_i negatively correlates with eviction risk. The advantage of testing θ_i's correlation with outcomes is that θ_i combines the three forces in a model-consistent way. Moreover, as it is similar to an index formed from the three forces, it can add power. But since it implies strong parametric restrictions, we also show the effects from each force separately.

It is infeasible to construct θ_i directly. First, we do not observe k_{Ti} or k_{Li}. Second, as the landlord and tenant experiments were conducted separately, for a given tenant, we do not know their landlord's a_{Li}, p_{Li}, or k_{Li}, and vice-versa.

We form proxies of landlord and tenant costs as follows (see Appendix D for details). We form proxy $\tilde{k}_{Ti}$ using questions where we ask tenants about their private cost of having an eviction. We form proxy $\tilde{k}_{Li}$ using questions where we ask whether the landlord would accept less than full rent for the given tenant. Appendix D also details how we prepare the misperceptions and altruism data from the experiment to be suitable for this test.

We then form adjusted-surplus proxy $\tilde{\theta}_i$ by plugging in the individual's own misperception, own altruism parameter, and own cost proxy into Equation (10). We set their opponent's misperceptions, altruism, and cost at 0.

We link the data from the experiment to eviction filings (Appendix D). We consider any eviction filing among the same individual at the same address after January 2018, so the filing itself could affect the index. We restrict to tenants who have back rents at the time of the tenant survey. For power, we pool the landlord and tenant experiments.

Specification. We estimate:

$$Y_i = \beta_0 + \beta X_i + \gamma \text{Controls}_i + \varepsilon_i \tag{11}$$

where Y_i is an indicator for having an eviction filing and X_i is a covariate. Controls_i contains an indicator for whether the observation comes from the tenant survey versus the landlord survey, and an indicator for randomized variation in how the landlord's costs were elicited. When covariate X_i is an indicator for having positive adjusted surplus $1(\tilde{\theta}_i > 0)$, the parameter of interest, β, shows the relationship between surplus and eviction filing propensity. We also test whether $\tilde{\theta}_i$'s constituent elements (real costs, hostility, misperceptions) are correlated with evictions.

5.1.2 Results

Eviction Filings. Adjusted surplus negatively predicts eviction filings (Figure 5, Panel A). Pooling both samples, having positive adjusted surplus is associated with a 10 pp reduction in eviction filings. These effects are economically large relative to the mean of 37%. They are also statistically significant and consistent across landlords and tenants.

Court costs are predictive among landlords, but not tenants. On the other hand, we find that hostility among both parties has a large quantitative association with evictions. Among tenants, hostility toward own landlords is associated with a 10 pp additional propensity to have an eviction filing (off a base rate of 33%). Among landlords, hostility toward own tenants is associated with a 15 pp additional propensity (65% of their base rate of 23%). Finally, misperceptions predict evictions in the pooled sample, with results driven by tenants.

These patterns survive controls (Figure A20A). Moreover, hostility and costs predict ultimate eviction *judgments* (Figure A20B), which are rarer and thus worse-powered. The parametric combination given in the adjusted surplus index is suggestive. The patterns also persist if X_i is an indicator for being in the bottom-quartile of surplus, rather than an indicator for being positive (Figures A20C and D).[39]

These correlations let us conduct back-of-the-envelope exercises to quantify the shares inefficient and repugnant. First, 48% of the parties with eviction filings have above-median misperceptions and 26% are hostile. These figures are upper bounds: if all these parties' filings were caused by misperceptions or hostility, that implies that just under half of evictions are inefficient and 26% are repugnant.[40]

As a second exercise, we consider the marginal predictive effect of above-median misperceptions or hostility from Equation (11). As above-median misperceptions are not themselves correlated with judgments, we cannot reject zero inefficiency from this back-of-the-envelope alone. Hostility is associated with a 7.1 pp increase in judgments, off a base rate of 21.0%. As 20.9% in this sample are hostility, that implies that 7% of evictions are repugnant ($\approx 0.071 \times 0.210 \div 0.209$).

Misperceptions and Hostility Are Complements. We find support for the model prediction that hostility and misperceptions are complements (Figure 5). Among participants with below-median misperceptions, hostility is not associated with any increase in eviction filing risk. However, hostility has substantial bite among parties with above-median misperceptions, in which case hostile participants have 13 pp larger filing risk than non-hostile participants.

Code Violations. As additional evidence that hostility correlates with behavior outside the lab, we show that highly hostile tenants live in units that have significantly higher rates of 311 calls to

[39] We observe a discernible increase in hostility in tenants who do the DG in the 100 days after a filing, though this increase also manifests for hostility toward random tenants (Figure A11).

[40] The 48% is not mechanical, as above-median misperceptions could have been negatively correlated with judgments.

Memphis's code enforcement department (Appendix E). On the other hand, tenant bargaining offers for the payment plan are not correlated with adjusted surplus (Figure A21).

5.2 Policy Evaluation

Given the magnitudes of hostility and misperceptions, we cannot rule out the possibility of inefficient evictions. A natural next question is whether rental assistance stops evictions, and whether it stops inefficient ones. We present graphical evidence here. In Appendix E, we provide a formal event study, as well as details about data construction, identification, balance and robustness checks.

Data and Sample. We use administrative data on ERAP program receipt to form panels of households before and after ERAP payment. Our main sample consists of about 4,700 households who applied between September 1, 2021 and December 31, 2022, and is comparable to those in the experiment and the full sample of ERAP payees (Table 1). ERAP payment involved full repayment of rental debts and utility bills, as well as payment of between 1–3 months of future rent payments. Payment was made directly to landlords in most cases.

Graphical Evidence. Figure 6 shows, among tenants who receive an ERAP payment, the cumulative shares with filings (blue line) and judgments (orange line) relative to the payment week. These shares rise linearly in the weeks before payment. After payment, the filing share stops rising for about eight weeks and then returns to trend. Judgments *increase* after payment. Extrapolating linear trends fit from the period between two and 16 weeks before payment (dashed lines) suggests filings fell for about eight weeks versus pre-payment trend, while judgments were above trend.

Event Studies. We conduct formal event studies that leverage two sources of variation. First, we use variation in payment timing among households who apply to ERAP around the same time and are paid. Second, we employ comparisons between paid and non-paid households who apply to ERAP, in a differences-in-differences type design.

The event studies confirm the graphical evidence and yield modest results on judgments (Appendix E). In the best-case specification for ERAP (Table A23), we reject that ERAP reduced judgments by more than 7.6 pp, and 95% confidence intervals include 0 ($p = 0.150$). As households receive more than \$5,000 on average, it is perhaps surprising that the policy has a small effect. Assuming homogeneity and dividing the average payment amount by the average treatment effect, the best-case fiscal cost to stop a judgment for six months exceeds \$70,000. Other specifications suggest that the cost exceeds \$100,000.

Meanwhile, we detect effects on eviction filings at 1–2 months, but mixed evidence on filings at 6 months. The best-case fiscal cost to stop a filing for six months exceeds \$35,000.

We highlight several limitations. First, this high fiscal cost is partially driven by the fact that judgments are rare in this sample and period (Figure 6), which both reduces power and the best-case impact of ERAP. In the best-case specification, despite still implying a large fiscal cost to stop an eviction, the point estimate is around 38% of the maximal effect if ERAP stopped all judgments among payees. Still, other specifications give weaker effects, and the result still implies that the remaining 62% simply take ERAP and evict within six months anyway.[41] The second limitation is that Memphis's ERAP had a distinct arm that provided legal assistance to the most at-risk tenants. We focus on the cash-only arm for external validity, as most ERAPs did not have such a legal services arm, and because the legal arm may complicate identification (see discussion in Appendix E). However, this sample restriction reduces power and could understate the overall effects of the Memphis ERAP on eviction, as the legal sample may have had different effects.

5.3 Explaining ERAP's Effects

The model and experiment data can potentially explain why so many landlords are inframarginal to ERAP. The model raises the possibility of perverse selection on altruism, wherein ERAP is more desirable for altruists than hostile parties. Relatedly, ultimate ERAP receipt involves cooperation between landlords and tenants. Applicants for ERAP must upload a lease and overdue rent ledger, documents that typically require landlord cooperation to obtain. After application, their landlord also must approve it, although if the landlord declines, the tenant may be able to obtain a check for the overdue rent directly.

Among landlords, we focus on whether hostility measured in the experiment predicts demand for ERAP materials. We also link some tenant experiment participants to program receipt using administrative ERAP data (Appendix D). We cannot do the corresponding exercise for landlords because we have a small number of landlord survey participants linked to payment status.

Results. Model forces predict landlord interest in ERAP (Figure A21). Altruism-adjusted surplus is associated with more than a 10 pp increase in landlord interest. These results are driven by hostility, which is associated with more than a 20 pp reduction.

Tenants who are hostile to their own landlord are less likely to receive funds quickly (Figure 7A, black series). We focus on the survival function of open applications as measured by days from initial submission to payment, comparing hostile and non-hostile tenants. In particular, hostile tenants are 15 pp less likely to receive ERAP funds in less than 50 days. Adding controls for prior beliefs, tenant demographics, and tenants' economic conditions changes little (blue series). We also regress ERAP receipt in the administrative data on hostility. Hostile tenants are 15.6 pp less likely to receive payment at all ($p = 0.056$). Kaplan-Meier failure curves, which aggregate these tests, show similar effects (Panel B, Wilcoxon p-value < 0.05).

[41] On the other hand, these cost estimates may be lower bounds because they only include the cost of the first payment made to a household at a given address.

6 Quantitative Analysis of Eviction Model

6.1 Empirical Model

We augment the model in Section 1 to be suitable for empirical estimation. Appendix B contains model and estimation details.

Payoffs. We propose landlord payoffs from four separate end states. We model the landlord's decision to take ERAP payment, beginning from once the tenant has applied. If the landlord declines to take ERAP, she can either get an eviction judgment ("eviction") or bargain not to get a judgment. If the landlord takes ERAP, she receives a payment of the back rents less an eviction cost. She can either get a judgment or bargain, but the judgment and bargaining payoff are less valuable as the tenant has no back rents.

Building on the environment in Section 1, the landlord's payoffs V_{Li}^{k} are:

$$V_{Li}^{\text{NoERAP,Evict}} = (1-a_{Li})p_{Li}d_i - a_{Li}k_T - k_L + \varepsilon_{i1} \tag{12}$$
$$V_{Li}^{\text{NoERAP,Bargain}} = (1-a_{Li})n_i^*(d_i) + \varepsilon_{i2} \tag{13}$$
$$V_{Li}^{\text{ERAP,Evict}} = d_i - k_L^P - a_{Li}k_T - k_L + \varepsilon_{i3} \tag{14}$$
$$V_{Li}^{\text{ERAP,Bargain}} = d_i - k_L^P + (1-a_{Li})n_i^*(0) + \varepsilon_{i4}. \tag{15}$$

Terms without subscripts i are constant across households and are parameters to be estimated or calibrated, whereas terms with subscripts i are data or unobserved idiosyncratic shocks.

The terms $\varepsilon_{i1}, \ldots, \varepsilon_{i4}$ represent idiosyncratic payoff-specific shocks from each state. For this reason, we can interpret $-(\varepsilon_{i1} - \varepsilon_{i2})$ as an extra idiosyncratic eviction cost relative to bargaining, if ERAP were not available.

The landlord's payoffs when she does not take ERAP (Equations 12 and 13) are the same as Section 1, except $n_i^*(\cdot)$ represents the Nash Bargaining payoff (described below). Whenever the landlord takes ERAP, she receives the mechanical ERAP payoff of back rents owed d_i, less the additive take-up cost k_L^P. In addition to the mechanical ERAP payoff, after taking ERAP, the landlord can choose to evict and get only the court costs (Equation 14), as there is no rental debt that she can recover. Alternatively, she can bargain with the tenant to avoid eviction and additionally get the Nash Bargaining payoff (Equation 15).

Bargaining. We posit asymmetric Nash Bargaining over surplus from avoiding court, which yields standard closed-form solutions for $n_i^*(d_i)$ and $n_i^*(0)$ (see expressions in Appendix B). These Nash payoffs naturally depend on tenant bargaining power $\beta \in [0, 1]$. When $\beta = 1$, payoffs correspond to a take-it-or-leave-it offer by the tenant, as in Section 1.

Nash Bargaining can occur in both the take-up and non-take-up case. In the non-take-up case, landlords and tenants bargain over tenant rental debts and court costs, as in Section 1. In the take-up case, they bargain over court costs only, as rental debts are fully paid by ERAP and not recoverable in court. Even absent the possibility of recouping rental debts, landlords still wish to evict the tenant if the sum of court costs and the shock is negative. For instance, court can be net profitable for landlords if formal eviction sends a message to other tenants or allows them to turn over the unit faster.

Nash Bargaining occurs if and only if bargaining is weakly profitable for both parties. In the case where the landlord does not take ERAP, eviction occurs if and only if:

$$k_L + k_T + \varepsilon_{i2} - \varepsilon_{i1} \leq \frac{(p_{Li} - p_{Ti})d_i}{\bar{a}_i}, \tag{16}$$

whereas when the landlord does take ERAP, eviction occurs if and only if:

$$k_L + k_T + \varepsilon_{i4} - \varepsilon_{i3} \leq 0. \tag{17}$$

Observe that these equations are lightly augmented versions of Equation (5). The expression in Equation (17) comes from substituting $d_i = 0$ into the eviction condition in Equation (16) and exchanging the shocks.

Timing and Landlord Maximization Problem. Landlords draw shocks ε_{ik}. These values and landlord payoffs are publicly observed by the tenant. These shocks define whether either of Equations (16) and (17) are satisfied. There are four distinct combinations of whether eviction or bargaining is possible when the landlord does and does not take up ERAP, yielding four possible maximization problems depending on the realization of the shocks. The landlord then chooses the actions ERAP/No ERAP and Evict/Bargain which maximize her payoffs, depending on which maximization problem she faces (see full problem in Appendix B).

Discussion. Despite its simplicity, the model captures many relevant economic forces.

Valuations of the physical property. There may be a portion of the landlord payoff (owing to possession of the physical property) that she automatically recoups with an eviction judgment, regardless of tenant behavior. We can think of this component as entering the landlord payoff from an eviction judgment via k_{Li}, as well as the landlord–tenant transfer.

Credit constraints. As in Section 1, tenant credit constraints enter the model through the bargaining costs and subjective beliefs that the tenant will repay if a court eviction occurs.

Money judgments versus judgments for possession. To reduce the number of landlord actions, the model assumes all judgments are for money. In reality, about half are exclusively for possession of the property (Section 2). Note that, in principle, one can think of $p_i d_i$ as representing the total expected value of the landlord–tenant transfer in court, which could include possession of the property only. However, in our empirical implementation, we treat p_i as beliefs about recoupment, and d_i as back rents. We explore the consequences of this simplification in robustness.

6.2 Estimation

We let ε_{ik} be type-I extreme value with scale parameter σ.[42] We seek to estimate the vector of parameters $\Theta := \{k_L, k_T, \beta, \sigma\}$. The values $X_i := \{a_{Li}, a_{Ti}, p_{Li}, p_{Ti}, d_i, k_L^P\}$ are either observed in the data from the landlord and tenant experiments or calibrated using that data.

The random shocks ε_{ik}, together with latent heterogeneity in the data, induce model-implied shares of landlords in each of the end states. We use the experiment to obtain the distribution of beliefs, altruism, and other data among the landlords and tenants (X_i). Given this distribution of inputs into model-implied landlord choices, we solve for the parameters that match a set of moments using the Method of Simulated Moments (MSM). Simulation-based estimation is useful because the shocks change the choice-set restrictions (Equations 16 and 17) and then give non-standard take-up probabilities.

We estimate four parameters from 24 moment conditions. We target the following moments: ERAP's treatment effects on judgments; the mean judgment rates among landlords who take up ERAP; the take-up rate among landlords; the bargaining offer made by tenants in the payment plans; and interactions between experimental variables and predicted or observed eviction judgments, landlord take-up, and the tenant bargaining offer. Intuitively, the estimator uses the functional form above to match the event study's "well-identified moment" and the correlations in the experiment. We form standard errors by block bootstrapping both the treatment effect moment and the experiment data.[43]

Data and Calibration. A key advantage is that most of X_i can be read directly from the experiment data. For instance, we plug in a_{Li}, a_{Ti}, and p_{Li} from the experiment. Thus, we directly observe key primitives that are typically estimated or calibrated in other research. Still, we do not observe take-up costs. We calibrate $k_L^P = \$500$ and show sensitivity to this assumption.[44]

We make several additional decisions to facilitate estimating the model (Appendix B). First, we use the tenant proposals about payment plans that we collected in the tenant survey to proxy for the Nash solution $n_i^*(d_i)$. The data we collected on tenant proposals constitute an unusual and rich trove of information about informal landlord–tenant negotiations. Second, we simulate assortative matching on altruism at the midpoint of our suggestive estimates (Appendix E). Standard errors take into account the random matching process. Third, as we do not exactly elicit tenant beliefs

[42] The type-I extreme value shocks impose an Independence of Irrelevant Alternatives (IIA) assumption, which forecloses richer substitution patterns between judgments and bargaining in our model.

[43] Our estimator may be biased because the shocks enter both the maximand and the choice restrictions. In particular, we do not account for potential bias induced by minimizing the distance between the data and a nonlinear function of the shocks. This problem is a cousin to the familiar bias in Maximum Simulated Likelihood (Train, 2009).

[44] This parameter includes all costs associated with interacting with ERAP and payment delays. Our calibration may exceed a strict accounting. For instance, suppose: landlords discount future payments at 8% per year; it takes two months to receive payment; it takes three hours to interact with ERAP and produce relevant materials; and landlords value their time at \$50 per hour. Under these assumptions, ERAP costs are lower than \$500. However, landlords also expressed significant frustration at ERAP which seemed to exceed the strict accounting costs. We can explain this frustration if interacting with ERAP and managing applications impose additional hassle costs.

about repayment probabilities, we assume that their beliefs that we do elicit scale one-for-one with beliefs about repayment and present how different choices affect results.

What role does selection into survey participation play? Our experiment sampling frame was all tenants, or landlords of tenants, who applied or began applying. The model begins once a tenant applies. If the experiment data are representative of the sampling frame, then they need no adjustment. Our diagnostics in Section 3 do not suggest important reasons to be concerned.

We permit correlated altruism and misperceptions within landlords/tenants by using the empirical distributions captured in the experiment. However, an important assumption, explored in robustness checks, is that landlord and tenant eviction costs are orthogonal to altruism and misperceptions. We also assume that tenants have homogenous costs, whereas landlords have heterogeneity via ε_{ki}.

6.3 Results

Parameter Estimates and Model Fit. The model delivers reasonable parameter estimates (Table 3). Total unconditional eviction costs (i.e., the sum of k_{Li} and k_T) are positive ($3,400 on average), indicating that court is costlier than bargaining. However, landlords' mean eviction cost is negative, at –$641, while tenants' is highly positive (exceeding $4,000). The standard deviation of landlord eviction costs (given by σ) is about $3,500, suggesting that eviction could be efficient for many parties. Tenants have high bargaining power ($\beta > 0.8$). To assess model fit, we compare several model-simulated moments with their targets and find they are broadly similar (Panel B and Table A25).[45]

Efficient and Inefficient Evictions. We use the model to quantify the shares of evictions and bargaining that are efficient versus inefficient. Figure 8A shows an empirical version of the model comparative statics in Diagram 1 (see Table A24 for levels). We consider a state of the world without ERAP, shutting down those payoffs and simulating landlord behavior if the two non-ERAP payoffs are the only ones available. Altogether, 19.1 pp of the sample obtains evictions. This low percentage reflects that eviction judgments are extreme events, even among our sample.

Of those who get evictions, 23.9% are inefficient (s.e.: 4.7%). We obtain this share by counting the number who obtain evictions despite positive surplus $k_L + k_T + \varepsilon_{i2} - \varepsilon_{i1}$. The remaining three quarters are efficient.[46] Hostility causes about two thirds of inefficient evictions, meaning about one in six evictions is repugnant. We attribute an eviction to hostility if it would not occur if hostile parties all had classical preferences (i.e., if we set $a_{Li} = a_{Ti} = 0$ when less than 0).

[45] Standard errors are large for decomposing landlord versus tenant costs. Nevertheless, the results below show that the main model conclusions about inefficient evictions are estimated precisely.

[46] Note that even if the means of k_{Li} plus k_T are larger than 0, the large scale parameter for landlord eviction costs means a substantial share can have a negative sum.

These results lie between the back-of-the-envelope figures for inefficient/repugnant evictions that we computed in Section 5. After all, we target the correlations between misperceptions/social preferences and judgments in the model directly. The other moments that we target do not greatly alter the conclusions from the simple back-of-the-envelope.

On the other hand, bargaining is far more common than eviction, and nearly all bargaining is efficient (Table A24B).

Given most evictions are efficient, policy that stops evictions destroys joint surplus on average (Panel B). In principle, as one in four evictions is inefficient, the average eviction cost could be positive if inefficient evictions are four times as costly as efficient evictions are beneficial. However, stopping the average eviction costs \$871 of private surplus if we do not normatively respect social preferences (i.e., the average $k_{Li} + k_T = -871$) and \$2,033 if we do normatively respect social preferences (i.e., the average $(1 + a_{Ti})k_{Li} + (1 + a_{Li})k_T = -2\,033$).[47]

Even though the average eviction has positive private surplus, misperceptions and social preferences are still key to the economics of eviction. First, Panel B shows that the average efficient eviction yields more than \$2,000 in surplus without respecting social preferences. Thus, non-classical forces reduce the average surplus from eviction by half.

Second, a partial explanation for why few evictions are inefficient is that altruism offsets misperceptions. We show this point by conducting an exercise in which we shut down altruism — replacing social preferences as being classical if altruistic — and resimulate behavior assuming the same value of other primitives (Panel C). In this exercise, 56% of evictions would be inefficient, and the average eviction has a positive court cost. Thus, high rates of altruism stop many pairs from inefficiently evicting. The underlying feature of the data that is responsible for this result is the positive correlation in the experiment between landlord altruism and the belief that their own tenant would repay in an eviction (Figure A24). Consequently, this result highlights the value of collecting the joint distribution of preferences and beliefs.

Our finding that the average eviction has positive private surplus rejects the view that evictions' private costs exceed their benefits on average. A given anti-eviction policy can still be justified if, among other arguments: (i) the policy targets inefficient evictions better than average, so that the average eviction averted has positive costs; (ii) evictions' externalities exceed their net private benefits; or (iii) the policy induces self-targeting. The potential presence of bargaining failure is a less compelling policy rationale in isolation. If the objective is to assist low-income households undergoing a costly and traumatic event, direct transfers to tenants may be more efficient than stopping the eviction per se. That said, we do find sufficiently prevalent bargaining failure to meaningfully reduce the magnitude of externalities that would justify eviction policy.

[47] Since we shut down the ERAP payoffs in this exercise, $k_{Li} := k_L - (\varepsilon_{i1} - \varepsilon_{i2})$. Intuitively, as tenants are altruistic on average and many landlords who evict have negative court costs, social preferences only amplify the efficiency costs of stopping the average eviction.

Perverse Selection on Altruism. Table 4 varies nonclassical forces and court costs (Panel A) and considers policy counterfactuals (Panel B).

First, we conduct a positive analysis of ERAP's low treatment effects, confirming that altruism reduces ERAP's effectiveness. In our primary simulation (Row 1), ERAP has a small TOT on evictions (Column 3). A natural way of scaling the TOT is to divide Column 3 by Column 5, the baseline rate of counterfactual judgments. When we shut off both social preferences and misperceptions entirely, the TOT becomes *positive* (Row 2). But underscoring the results in Section 5, shutting off social preferences (Row 3) or particularly landlord altruism (Row 4) would cause ERAP to have a –15.7 pp effect on judgments. The rate of evictions also rises, as eviction is more desirable without altruism, but the TOT as a percent of the counterfactual judgment rate is almost 60%.[48] Adjusting misperceptions (Rows 7–8) makes a smaller difference for ERAP's TOT. Doubling costs for both parties would lead to a large ERAP TOT (–3.5 pp out of 6.3 pp), but counterfactual evictions then become very rare (Row 10).

As foreshadowed in Section 1, even absent non-classical forces, ERAP is not perfectly targeted. A classical force pushes toward enrollment among inframarginal "never-evictors," since ERAP is more desirable for landlords with high court costs. Altruism amplifies this force, raising the value of ERAP and reducing the value of eviction.

However, we augment the model in Section 1 to allow pairs to evict even with ERAP. Such "always-evictors" take ERAP and still pursue eviction (Equation 14). The model then captures a countervailing mechanism, as court costs and altruism both *reduce* inframarginality from always-eviction. They both make eviction less desirable, including when in ERAP. Still, the never-evict force is more quantitatively important than the always-evict force. Eliminating altruism amplifies ERAP's TOT (Rows 4–5), whereas eliminating hostility attenuates ERAP's TOT (Row 6).

ERAP's Targeting and Welfare Impacts. Now, we conduct a normative analysis of ERAP. Even if most evictions are efficient, a given eviction intervention could still improve welfare if well-targeted. However, in the primary estimate, 23% of the evictions that ERAP stops are inefficient (Row 1, Column 4), meaning it is about as well-targeted as stopping the average eviction.

We next present a suggestive analysis of ERAP's welfare impacts, when the planner does and does not normatively respect altruism (Columns 6 and 7). We compute the average gain from ERAP as the landlord's net idiosyncratic payoff from enrollment, plus the effect of ERAP on eviction times the net eviction cost, less the landlord's take-up cost (see Appendix B for details). ERAP's other impacts run through the mechanical ERAP transfer or intrahousehold bargaining payments, both of which we assume have zero social value or cost. In this sense, we assume that the government can finance ERAP without distortionary taxation and employ a marginal cost

[48] The difference between Rows 4 and 5 is explained by tenant altruism, which generates favorable Nash bargaining offers for landlords. Shutting down tenant altruism makes ERAP less effective because tenant altruism has a larger proportionate effect on the Nash bargaining outcome when landlords take up (Equation 22 vs. 23).

of public funds of 1. We ignore other administrative costs or fiscal externalities. Given these limitations, we treat the exercise cautiously, noting that we probably produce an upper bound on welfare generated from ERAP.

ERAP increases welfare in the baseline specification. Intuitively, ERAP yields gains to inframarginals, as landlords enroll when they receive a large draw of the idiosyncratic shock. ERAP has a small effect on stopping evictions. If the effect were larger, that would reduce surplus on average. Echoing our finding of perverse selection on altruism, landlord altruism attenuates ERAP's welfare impacts (Row 4 vs. Row 1).

ERAP gives $211 (Row 1, Column 6) or $83 (Column 7) in net welfare gains, if society does not or does respect altruism. We scale payoffs by landlord and tenant altruism in Column 7 but always count transfers as having zero social value. These gains involve transferring at least $2,300 on average (the average back rents at the time of the tenant survey) and administrative costs (which we ignore). The transfer is not waste, especially as landlords in our study are often lower income themselves. Still, the increase in welfare from ERAP only comes from aspects of the program that are unrelated to stopping evictions.[49]

Counterfactuals. Finally, the model lets us study how counterfactual policies affect ERAP's TOT and targeting (Rows 11–13). As one example, we simulate fining landlords $2,000 if they take ERAP and then evict a tenant (Row 11). This intervention raises ERAP's TOT to about one third of total counterfactual evictions. We also simulate restricting eligibility among those in the top 10% most predicted to have a large treatment effect from an OLS regression.[50] Doing so would generate a TOT that is about half of the counterfactual judgment rate. It would also raise the share of evictions that are inefficient to about 40% of evictions prevented, though it would reduce ERAP's welfare impacts. While it may be infeasible to completely restrict eligibility to those households, the program could certainly expand outreach.

Robustness. Changing model moments or assumptions does not greatly affect the main conclusions above: that a material but minority share of evictions are inefficient, that hostility drives the inefficiency, and that eliminating landlord altruism increases ERAP's TOT (Table A26). Across different checks or assumptions, 15–41% of evictions are inefficient.

We discuss several especially important tests. The main conclusions are relatively robust to

[49]Relatedly, ERAP's Marginal Value of Public Funds (Hendren and Sprung-Keyser, 2020), respecting altruism, is $\frac{T+83}{T+\text{FE}}$, for ERAP transfer $T \approx \$2{,}300$ and fiscal externality FE. FE includes administration costs and fiscal externalities that run through ERAP's effects on evictions. This calculation places equal weight on landlord and tenant WTP, so intrahousehold transfers do not enter the MVPF numerator. Given administration costs of around 10%, ERAP has a similar MVPF to providing a non-distortionary cash transfer.

[50]The demographics exclude variables like beliefs or altruism that are not available on the application. The landlord variables are: age, gender, race, education, landlord report of tenant's tenure in landlord experiment, whether the participant reports being a landlord (versus property manager or other), rent in the unit, number of units, years of experience. The tenant variables are: race, gender, age, education, whether they have formed a payment plan, whether they have overdue rent, back rent, monthly rent, monthly income, and an employment dummy. We also include interactions between all demographics.

our assumption about landlords' take-up cost k_L^P. If we double k_L^P to \$1,000 (Table A26, Row 8), we obtain that about a third of evictions are inefficient. While we see that as an implausibly high take-up cost, future research that quantifies take-up costs would be valuable.

We also explore the importance of the assumption that landlords' eviction cost k_{Li} is uncorrelated with p_{Li} and $\overline{a}_i$. We replace landlords with above-median misperceptions as having 0.2 s.d. higher eviction costs. This value would approximately generate the observed correlation between misperceptions and cost proxies in the experiment. Allowing such correlation has modest effects (Table A26, Row 10), raising the share inefficient to about 30%.

While measurement error could affect the magnitude of our results, measurement error would need to be severe to overturn our conclusions. We focus on non-classical measurement error that would cause systematic bias in our elicitations due to, e.g., lack of numeracy in the study population. First, we simulate the share of efficient/inefficient evictions that would obtain if we rescale social preferences as $\hat{a}_{ji} := sa_{ji}$ for different $s < 1$ (Figure A25A) and posit the same value of estimated and calibrated parameters. We do not study $s > 1$ because the model requires $a_{ji} < 1$. If social preferences are overstated by half ($s = 0.5$), then about one in three evictions would be inefficient. The reason measurement error would generate more inefficiency is because we find more altruism overall, so reducing social preferences on net generates more inefficient evictions from misperceptions. Second, we scale misperceptions for various $s \in \mathbb{R}^+$ (Panel B) and find our conclusions are also reasonably insensitive. One reason that these results are not highly sensitive to measurement error is that we find a large scale factor for eviction costs, which generates many efficient evictions among those who obtain a high cost draw.[51]

Our results are most sensitive to our assumptions about assortative matching (Figure A25C). Our primary specification simulates assortative matching at the midpoint of suggestive estimates. That is, we assume that 27.5% of tenants and landlords are assortatively matched on altruism or hostility, and the remainder are matched at random (Appendix E.2). If all pairs are matched at random, then the share of evictions that are inefficient falls to 15%, holding other parameters constant. If all pairs are matched on social preferences, then the share inefficient rises to 41%. More matching on social preferences generates more inefficiency because compound altruism $\overline{a}_i$ becomes large as a_{ji} goes to 1 for either party j, regardless of the other party $-j$'s hostility. Sufficient altruism thus dampens the impact of even intense hostility.

[51] A related concern is that we assume all judgments are for money, when half of evictions are for possession. We simulate Δp_i from the landlord data as being $0.5(p_{Li} - 0.06)$, holding all parameters fixed. This exercise moves inefficiency to about 21%. Intuitively, with any misperception wedge, the wedge is greatly amplified by hostility.

7 Conclusion

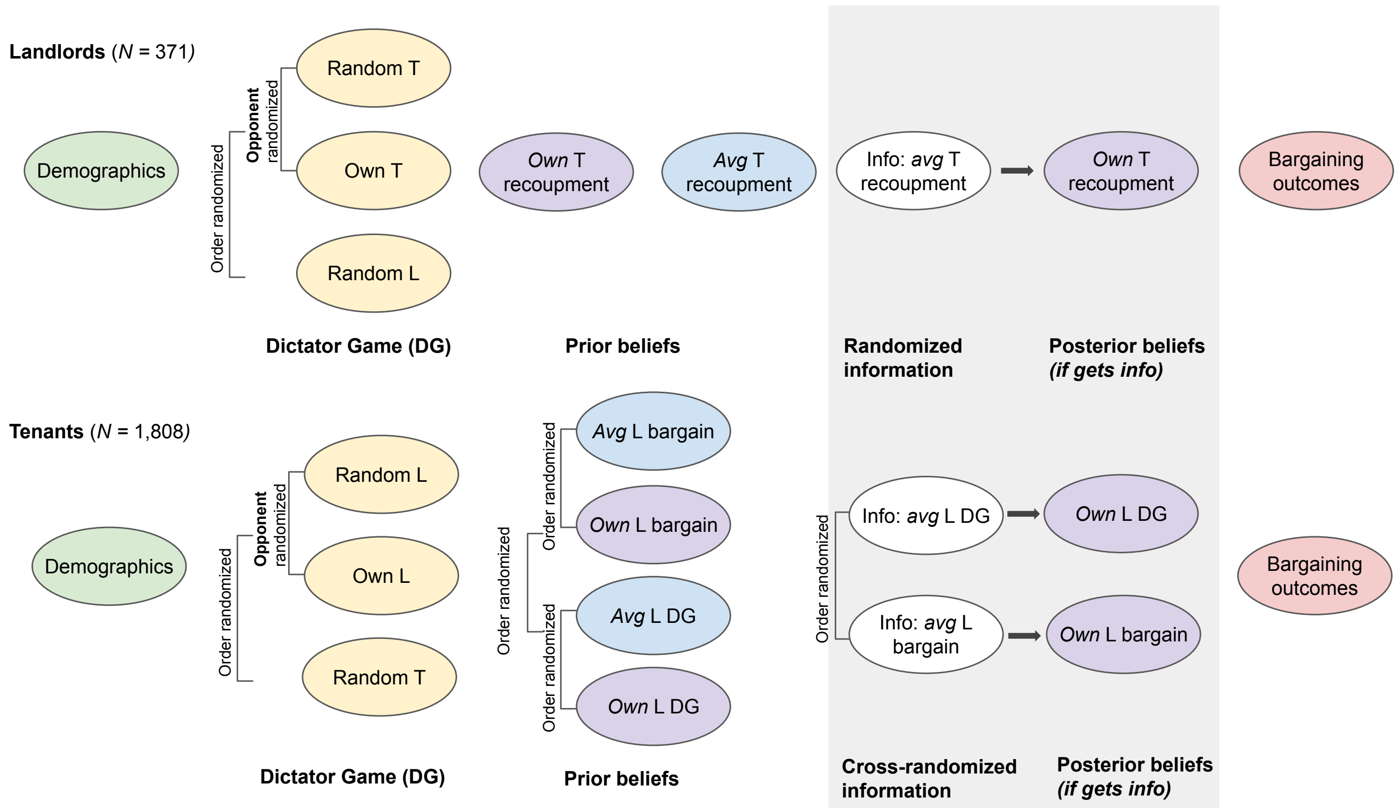
Formal evictions represent the culmination of an unsuccessful bargaining process. If misperceptions or hostility cause bargaining failure, some evictions could be inefficient or repugnant. Belief elicitations and Dictator Games with landlords and tenants facing eviction in Memphis, Tennessee suggest substantial misperceptions and strong social preferences. These nonclassical forces predict eviction, render material shares of evictions inefficient and repugnant, and sustain bargaining that would otherwise not occur.

Even so, just one in four evictions is efficient, and stopping the average eviction destroys private surplus. Thus, concerns that evictions are privately inefficient cannot rationalize eviction policy, unless the policy targets inefficient evictions in particular. Other arguments, such as externalities or welfare weights on the evicted, can rationalize eviction intervention. Credible empirical research suggests that evictions indeed cause fiscal externalities via, for instance, stays in homeless shelters and hospitalizations (Collinson et al., 2024). We find enough inefficiency to meaningfully reduce the magnitude of externalities that make eviction intervention socially desirable. Future work that builds on Collinson et al. (2024)'s estimates would be valuable.

The presence of misperceptions and hostility has important implications for eviction policy and beyond. An illuminating literature suggests that misperceptions and mistakes are key to the economics of poverty and welfare programs (Mullainathan and Shafir, 2013; Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019). Intense social preferences have received less attention. Our evaluation of Memphis's Emergency Rental and Utilities Assistance Program suggests these forces reduce the policy's cost-effectiveness and affect its targeting. Yet social preferences are likely important for housing policies other than ERAP, as Section 8 vouchers and other low-income housing policies also rely on landlord–tenant cooperation. Indeed, misperceptions and hostility may affect efficient bargaining in settings outside housing — including, for instance, marriage/divorce or labor strikes. When these forces are present, they have the potential to influence the desirability and effectiveness of policy intervention.

8 Figures

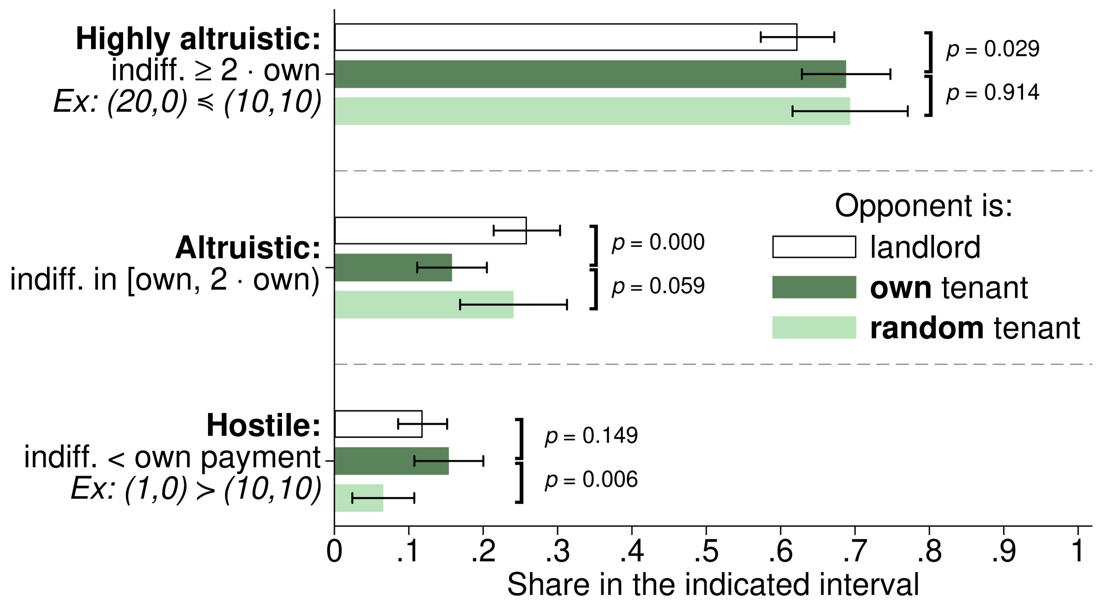
Figure 1: Survey Flow



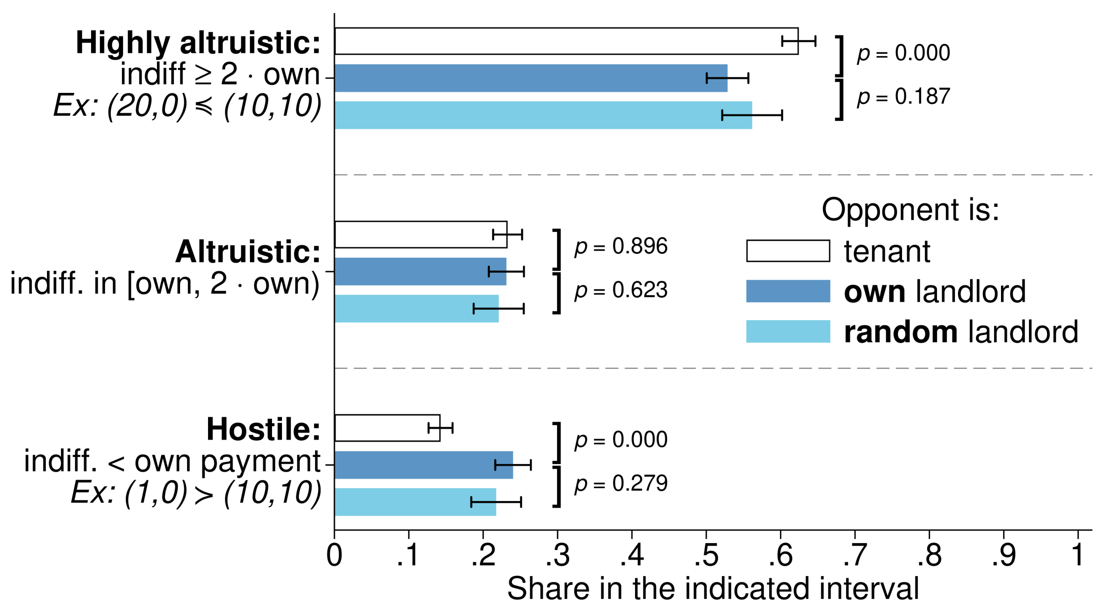
Note: This figure depicts the survey flow for the landlord and tenant experiments. The yellow ovals show the opponent for the dictator game; each participant played the game twice. We elicited all depicted prior beliefs for each participant. We randomize the order of prior beliefs for tenants only. We provided information only to randomly selected participants, with $p = 0.5$ for each treatment. We elicited posteriors only for participants to whom we provided information. We provided information about the average landlord or tenant, but we elicited posterior information only about the participant's own landlord or tenant. Several secondary elicitations are omitted for legibility.

Figure 2: Behavior in Modified Dictator Game

(a) Landlord participants ($N = 371$)



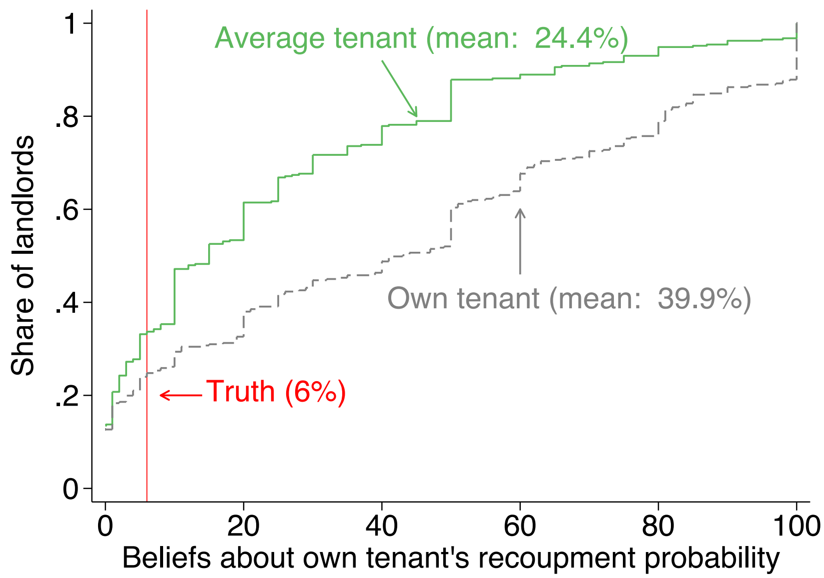
(b) Tenant participants ($N = 1{,}808$)



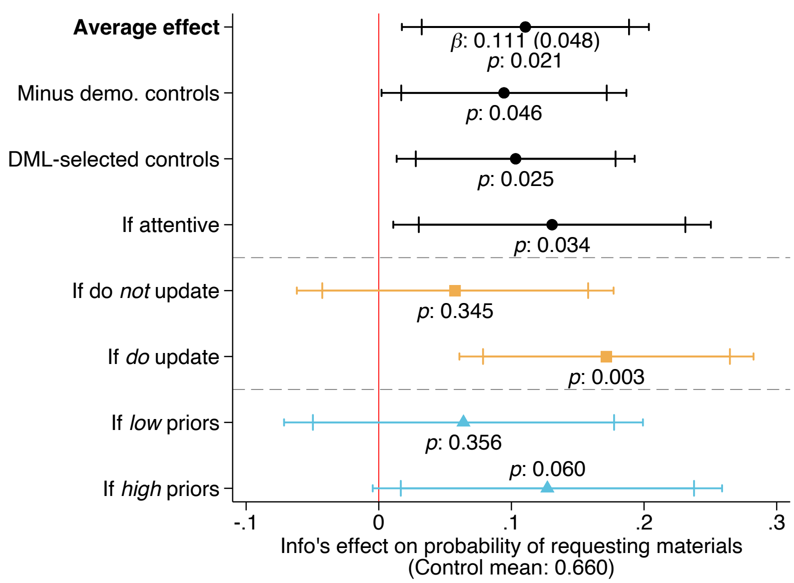
Note: This figure summarizes the results from the Dictator Games among landlord participants (Panel A) and tenant participants (Panel B). Landlords were randomized to play the game against their own tenants (named) or against random tenants (unnamed). All landlords additionally played the game against random landlords (unnamed). Tenants were randomized to play the game against their own landlord or a random landlord. All tenants played the game against random tenants. We elicit bounds on the the point $S(x)$ at which a participant is indifferent between the bundle (\$$s$ self, \$0 other) and (\$$x$ self, \$$x$ other). If $S(x)/x < 1$, then the player is hostile; if $S(x)/x > 1$, then the player is altruistic. We show the share of people who are "highly altruistic" ($S(x)/x > 2$), "altruistic" ($S(x)/x \in (1, 2)$), and "hostile" ($S(x)/x < 1$). We elicit bounds on $S(x)$ using a multiple price list. Our elicitation gives bounds on the indifference point, explaining why no one is exactly classical.

Figure 3: Landlord Misperceptions

(a) Priors about recoupment

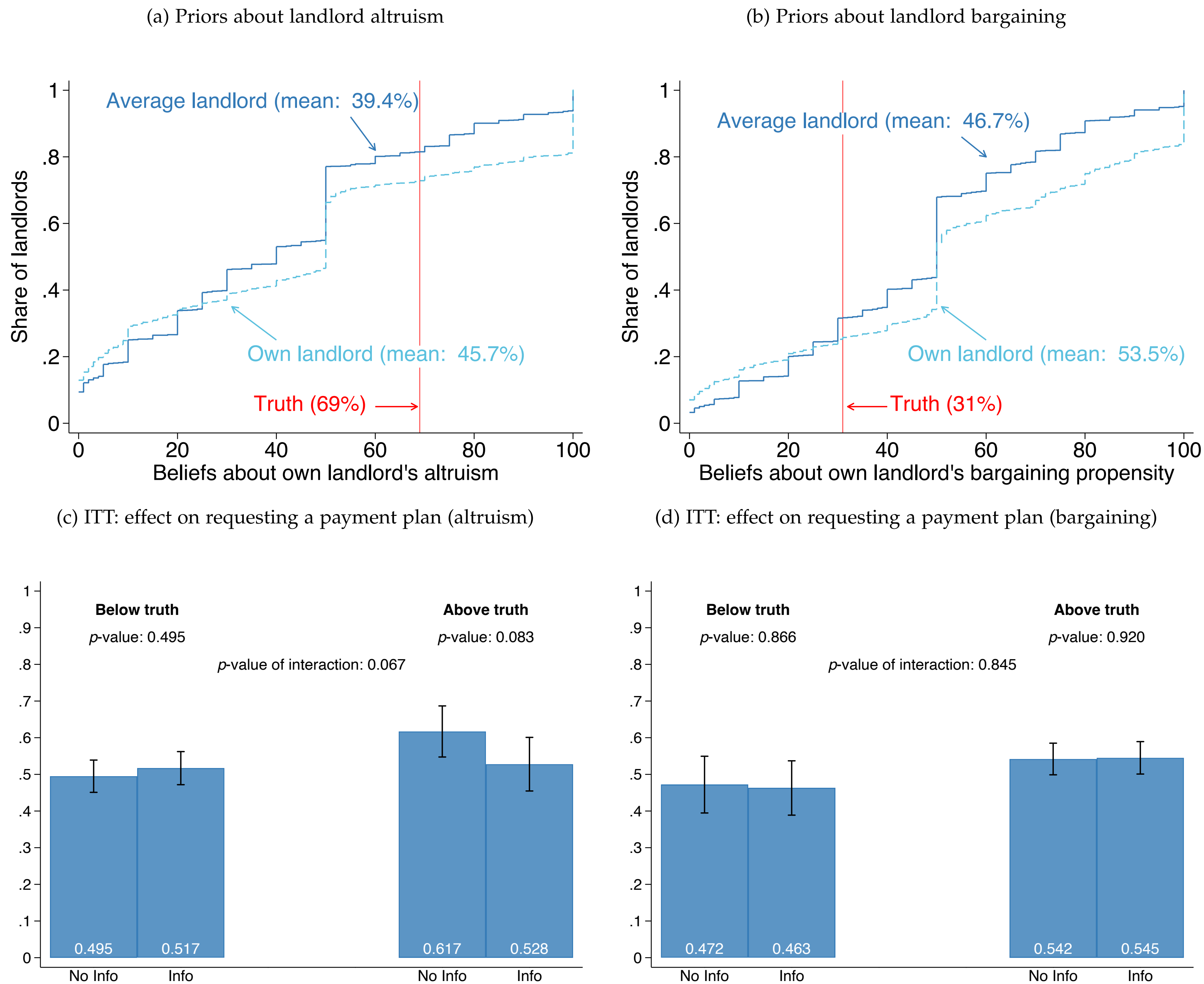


(b) ITT: requests materials



Note: This figure shows data from the landlord sample ($N = 371$). Panel A shows the cumulative distribution function of landlord priors about their own tenant and the average tenant. Landlords recouped 6 percent of back rents during the time period that we asked about (red line). Panel B shows the ITT of providing information on whether the landlord requests materials about ERAP.

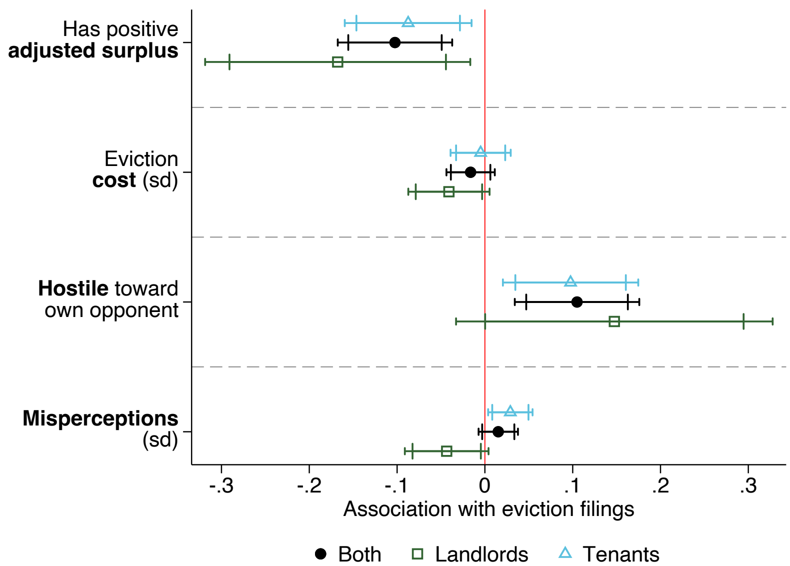
Figure 4: Tenant Misperceptions (Altruism)



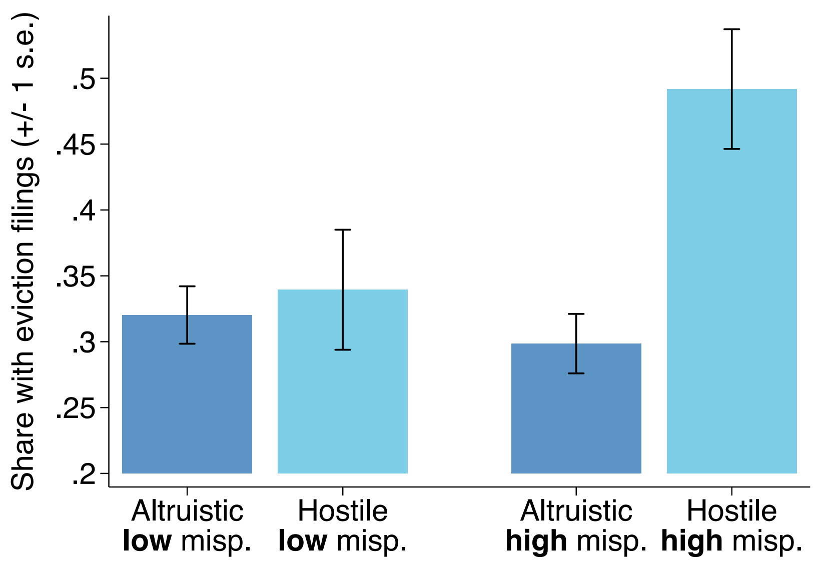
Note: Panels A and B show cumulative distribution functions of tenant priors for beliefs about landlord altruism (i.e., the share of landlords who prefer (x self, x own tenant) to ($2x$ self, 0 own tenant)) and bargaining in court conditional on filing an eviction. The truth is indicated in a vertical red line. We cross-randomized information treatments. Panels C and D shows intent-to-treat effects of providing information on whether the participant requested a payment plan, splitting the sample by whether they are *above* or *below* the information provided. The sample is the tenants who were eligible to request a payment plan.

Figure 5: Model Validations

(a) Adjusted Surplus Predicts Eviction Filings

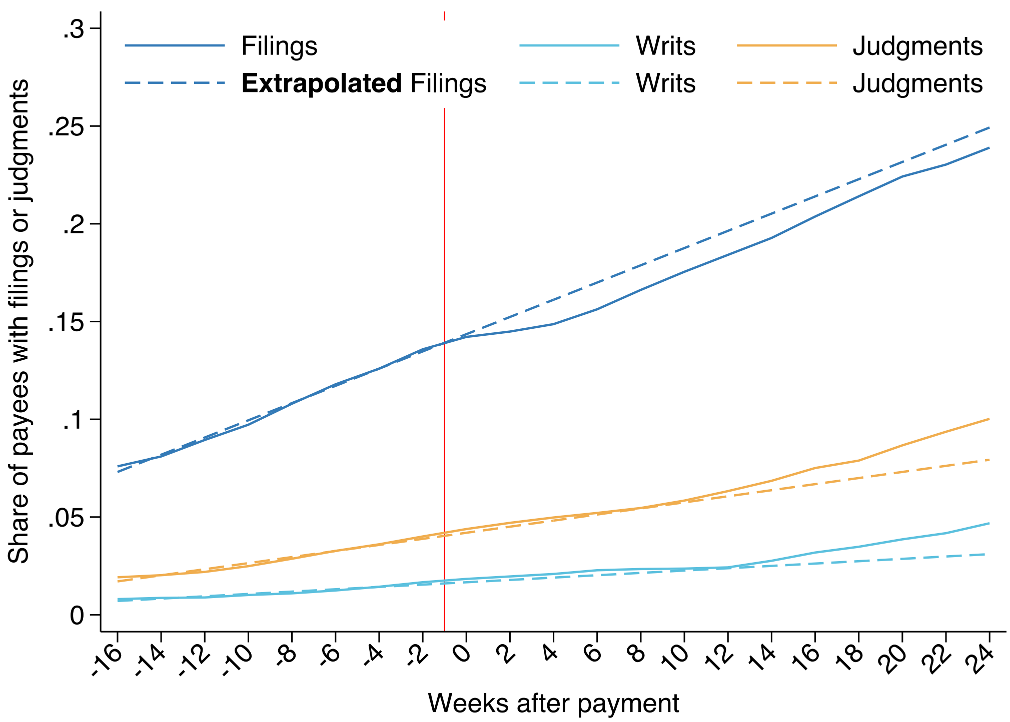


(b) Social Preferences Predict Evictions Only With Misperceptions



Note: Panel A presents estimates of $\hat{\beta}$ from Equation (10), where the covariate X_i is either positive adjusted surplus, a proxy of eviction cost, hostility, or misperceptions. Appendix E gives details on how these are formed. Panel B splits the data by having above- or below-median misperceptions and hostility. It presents mean eviction filings. The outcome in both panels is whether the participant (either tenant of landlord in landlord survey, or tenant in tenant survey) gets an eviction filing at the address in the experiment between January 2019 and June 2023. In the surplus and hostility regressions, we keep only the landlords or tenants who played the DG against their own tenant. The tenant sample contains only tenants with positive back rents at the time of the survey. The specifications that involve landlord costs control for experimental variation in how the landlord costs were elicited. All pooled specifications include an experiment fixed effect.

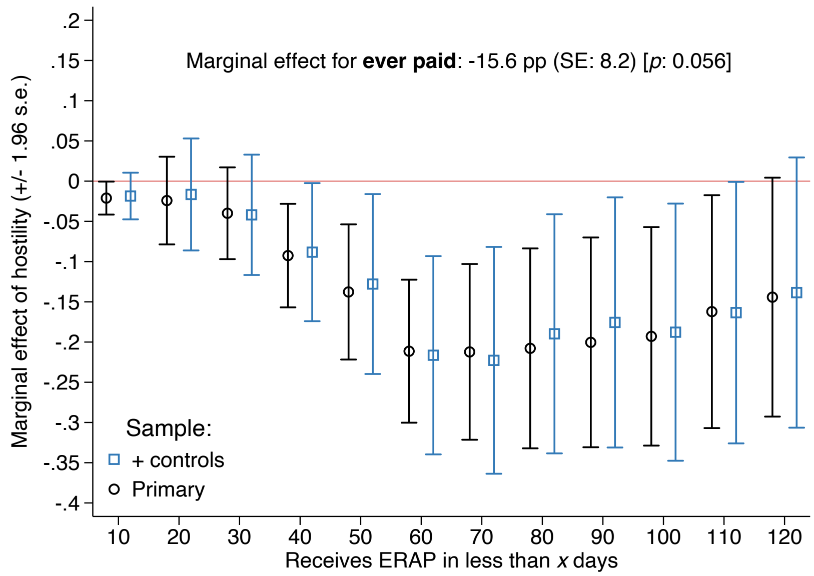
Figure 6: Rental Assistance Filings and Judgments by ERAP Payment



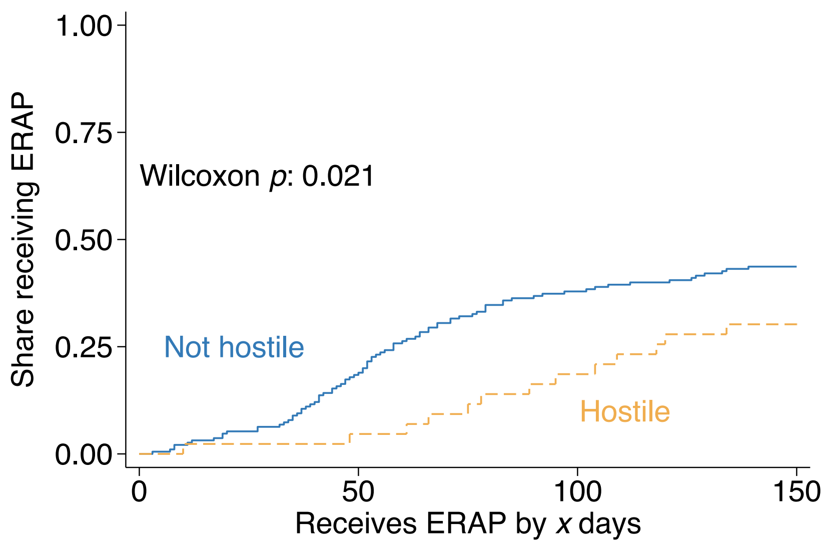
Note: This figure shows filing and judgment rates for tenants who receive payments from the Memphis-Shelby County Emergency Rental and Utilities Assistance Program. The red line indicates the date of payment for that tenant. The dashed lines show linear extrapolations from 16 to 2 weeks before payment.

Figure 7: Tenant Hostility and Speed of ERAP Receipt

(a) Receives ERAP Quickly



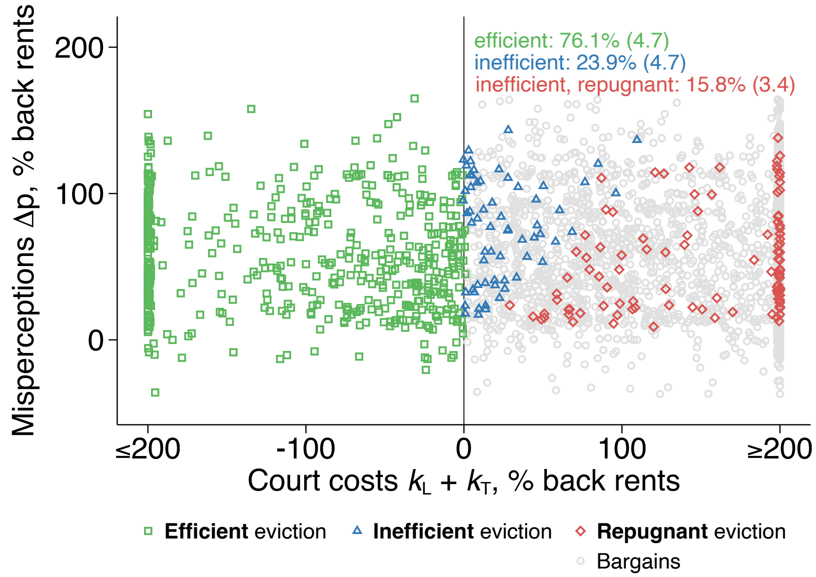
(b) Kaplan-Meier Failure Curves



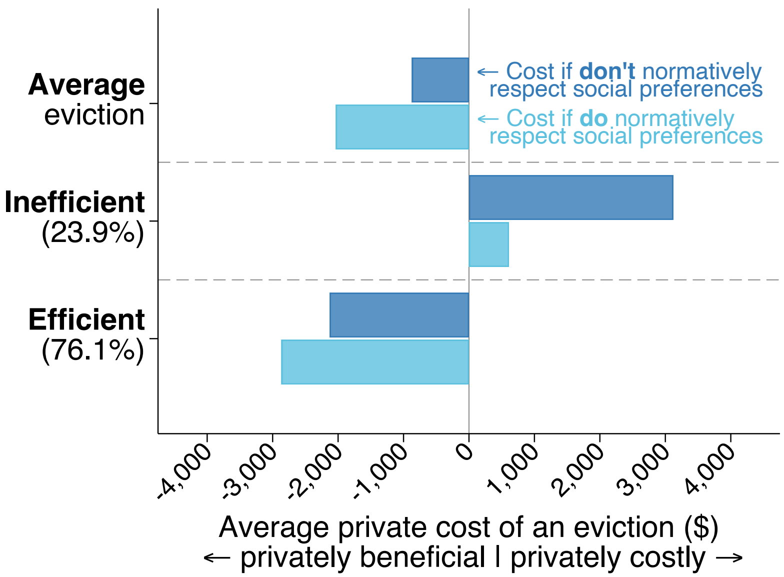
Note: This figure presents tests of whether hostile tenants (defined as in Section 3) receive ERAP funds within the number of days listed on the horizontal axis. Panel A shows regressions of receiving ERAP withn the indicated number of days on tenant hostility. Controls for beliefs and demographics are: controls for prior beliefs about own/average landlord bargaining behavior or behavior in the DG; controls for demographic variables: race, gender, age, and education; controls for economic variables: log back rent owed, log monthly rent, log monthly income, and employment status. Panel B presents Kaplan-Meier curves and a Wilcoxon test for differences. This figure shows the sample of tenants who: play the DG against their own landlord; had not moved between applying for ERAP and participating in our study; did not enter the separate ERAP eviction representation process; and applied after September 1, 2021, since that is when data on ERAP reciept is available.

Figure 8: Decomposing Eviction: Efficient and Inefficient Evictions

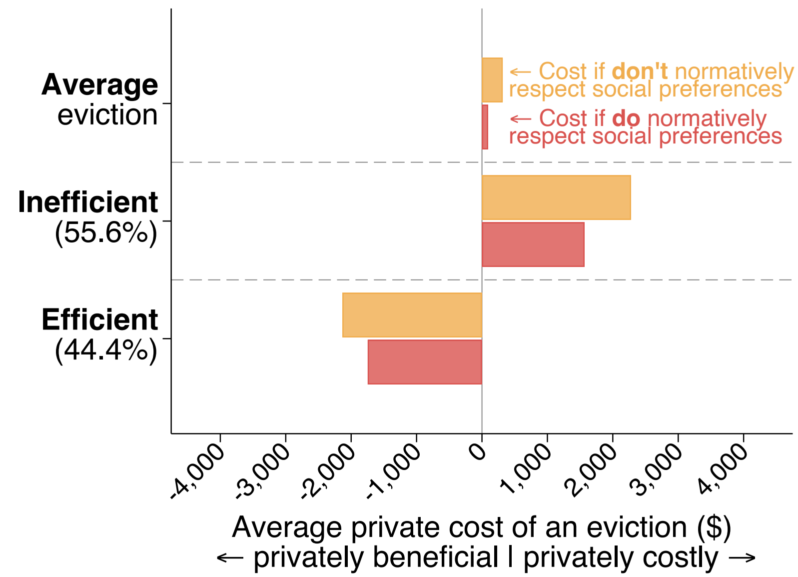
(a) Empirical Analog of Diagram 1



(b) Magnitudes of Eviction Costs



(c) Magnitudes of Eviction Costs Absent Altruism



Note: Estimates from 200 bootstraps. Panel A presents the simulated estimates of the share of evictions in each region of Diagram 1. We shut off ERAP payoffs and simulate using the procedures in Section 6. Parentheses in Panel A display standard errors. Panel A plots a 2% sample but percentages are from the full sample. Panel B shows the average cost of an eviction. The bars that do not normatively respect social preferences report the average $k_{Li} + k_T$ among those who evict, where $k_{Li} := k_L - (\varepsilon_{i1} - \varepsilon_{i2})$. The bars that do normatively respect social preferences report the average $(1 + a_{Ti})k_{Li} + (1 + a_{Li})k_{Ti}$. Panel C replaces altruists as having classical social preferences and resimulates their behavior and costs, holding all other parameters and primitives constant.

9 Tables

Table 1: Tenant Demographics

Sample:	Experiment Participant (1)	Experiment Non-Participant (2)	Difference (1 – 2) (3)	SE Difference (4)	Memphis ERAP (Paid) (5)	Memphis ERAP (Paid & Nonlegal) (6)	Shelby County (7)
A. Observed for Experiment Participants and Non-Participants							
Age	34	35	-1.1	0.27	37	39	45
Female	0.81	0.72	0.09	0.010	0.77	0.76	0.53
Black	0.88	0.89	-0.01	0.008	0.92	0.91	0.53
Disabled	0.14	0.14	-0.00	0.009	0.10	0.11	0.14
Household size	2.1	1.9	0.2	0.04	2.3	2.3	2.2
Employed	0.53	0.51	0.01	0.012	0.43	0.40	0.64
Household monthly income	891	733	157	29	1,185	1,172	5,408†
Monthly rent	872	900	-28	12	949	978	834
Back rent owed at application†	3,585	3,900	-315	173	4,000	4,155	
B. Observed for Experiment Participants Only							
Some college +	0.56				0.55		
Never married	0.84						
Ever evicted	0.33						
Ever overdue rents	0.86						
Ever formed a payment plan	0.57						
Back rent owed at survey†	1,014						
Paid by ERAP at survey	0.55						
N	1,808	15,062			10,111	4,742	5,586

Note: This table shows demographic means for tenants in our survey experiments (Columns 1 and 2), ERAP samples (Columns 5 and 6), and in the 2019 ACS for Shelby County (Column 7). The observation count includes people who appear in any row but each row excludes missing values. Columns 3 and 4 show the difference and standard error on the difference between experiment participants and non-participants. Panel B shows additional demographic data collected in the tenant experiment. Monthly income is conditional on having non-zero income. †: displays median.

Table 2: Landlord Demographics

Variable	(1) Participant	(2) Participant's Tenant	(3) Non-Participant's Tenant	(4) Difference (2 – 3)	(5) SE Difference
A. Observed for Landlords and Tenants					
Age	49	39	37	2.1	0.68
Female	.62	0.66	0.69	-0.03	0.026
Black	.58	0.77	0.84	-0.07	0.023
White	.32	0.09	0.06	0.03	0.015
B. Observed for Tenants Only					
Veteran		0.04	0.02	0.01	0.010
Back rent owed†		2,700	2,400	300	210
Utilities owed		387	535	-148	81
C. Observed for Landlords Only					
Years of experience	13				
Some college +	.85				
Landlord	.62				
Property manager	.29				
Small landlord (1–2 units)	.26				
Medium landlord (3–10 units)	.24				
Large landlord (10+ units)	.48				
Ever evicted	.71				
N	371	371	3,595		

Note: This table presents demographic characteristics of the landlord sample. Cells present means. Column 1 shows demographic characteristics collected in the survey experiment. Columns 2 and 3 use administrative data from the Emergency Rental and Utilities Assistance Program to obtain demographic information about the tenants of the landlord sample participants, as well as the landlords who were invited to participate but did not. In the landlord survey, landlords are asked about a randomly selected reference tenant (the person against whom they play the Dictator Games below). We compare the reference tenant of the landlords who were invited to the study but did not participate to the tenants of landlords who did participate. Columns 4 and 5 show difference in means between Columns 2 and 3 and standard errors on the difference. Some demographic information was collected only in the survey and is presented in Panel C. †: displays median. Back rents are reported owed at application.

Table 3: Empirical Bargaining Model: Parameter Estimates and Model Fit

A. Parameter Estimates

Explanation	**Parameter**	**Estimate**
Total unconditional eviction cost ($)	—	3,401
		(1,087)
		{3,507}
Landlord cost ($)	k_L	-641
		(1,806)
Tenant cost ($)	k_T	4,042
		(1,654)
Tenant bargaining power	β	0.84
		(0.12)

B. Model Fit

Selected Moments	**Estimated Value**	**Targeted Value**
1. ERAP mean judgment rate (unconditional)	0.158	0.091
2. Treatment effect on judgments (unconditional)	-0.016	-0.009
3. Landlord take-up rate	0.668	0.648

Note: Estimates from 200 bootstraps. Panel A shows the estimated parameters from the empirical model presented in Section 6. The number in braces shows the standard deviation of unconditional eviction costs, which is the standard deviation of estimated $-(\varepsilon_{i1} - \varepsilon_{i2})$. Panel B shows the model fit to several targeted moments. For the full set of moments, see Table A25.

Table 4: Empirical Bargaining Model: Mechanisms and Counterfactuals

	Descriptives		Targeting			ERAP Welfare Impact	
	Judgment % (absent ERAP)	Takeup (%)	TOT: judgments (pp)	Inefficient judgment (%) (if prevented)	Counterfactual judgment (%) (enrollees)	Raw ($)	Altruism-adjusted ($)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Mechanisms							
1. Primary	19.1	66.8	-1.6	22.7	17.5	211	83
2. No social prefs., misperceptions	14.6	68.3	5.7	0.0	11.9	-124	-124
3. No social preferences	28.1	61.0	-5.4	55.4	23.7	206	206
4. No landlord altruism	23.0	56.9	-15.7	31.3	26.7	802	834
5. No altruism	32.6	60.1	-9.1	62.5	27.7	372	245
6. No hostility	16.1	67.7	0.8	10.4	15.0	96	110
7. No misperceptions	14.6	68.4	2.2	0.0	13.6	54	31
8. High misperceptions	20.3	65.0	-2.0	24.4	17.8	236	121
9. Mean T and L court costs are 0	53.7	66.6	8.8	8.4	48.1	326	398
10. Double T and L court costs	6.7	66.8	-3.5	50.8	6.3	330	290
B. Counterfactual Policies							
11. ↑ eviction penalty	19.1	63.2	-6.3	22.7	17.2	356	327
12. ↓ take-up costs	19.1	72.4	-2.2	22.8	17.9	135	-31
13. Targeted on demographics	26.2	85.7	-10.6	41.2	25.2	119	-307

Note: Estimates from 200 bootstraps. We simulate behavior using data from the experiment and ERAP, using the model in Section 6. Row 1 is our primary specification, using parameters estimated via the Method of Simulated Moments procedure displayed in Table 3. Row 2–10 change social preferences, misperceptions, or costs. Rows 11 and 12 raise ERAP's eviction judgment cost (adding a cost of \$2,000 to Equation 14) or eliminate take-up costs ($k_L^P \to 0$). Row 13 conducts a targeting counterfactual by restricting ERAP eligibility to households whom an OLS procedure determines are in the top 10% most likely to bargain without ERAP. Column 1 shows the share of households that would pursue eviction judgments if ERAP did not exist. Column 2 shows the share of landlord–tenant pairs who enroll. Columns 3 and 4 show the treatment effect on judgments, and the share of judgments averted that are inefficient. Even if the estimate in Column 3 is positive, the estimate in Column 4 is non-missing because we consider any individual whose eviction is stopped. Column 5 shows the total percentage rate of judgments. Thus, Column 3 divided by Column 5 ($\times$100) shows a treatment effect in percentage terms. Columns 6 and 7 show the per-household welfare impact of ERAP among enrollees, depending on if the planner does not versus does normatively respect altruism. Column 7 does not scale governmental or intrahousehold transfers by altruism.

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Appendix Materials

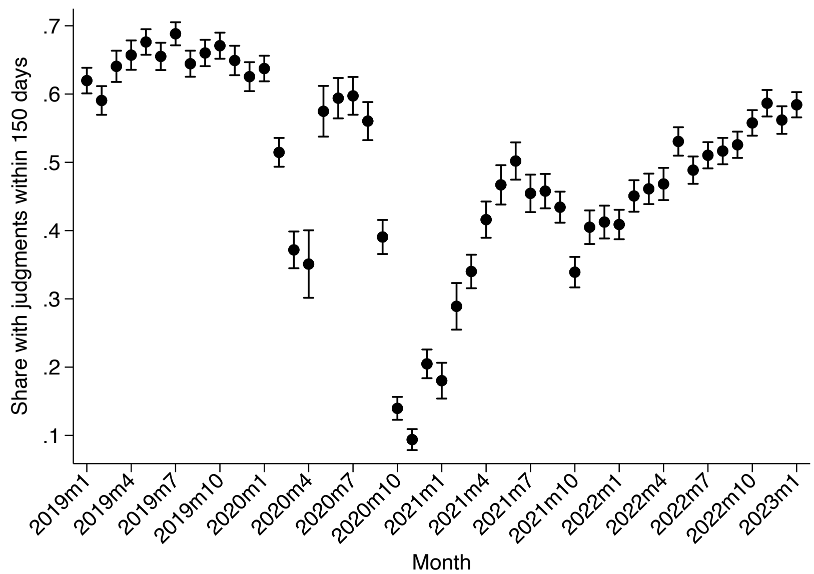
Please see Rafkin's website for a Supplementary Appendix containing extra exhibits (prefixed with "S") and survey instruments.

A Additional Figures and Tables

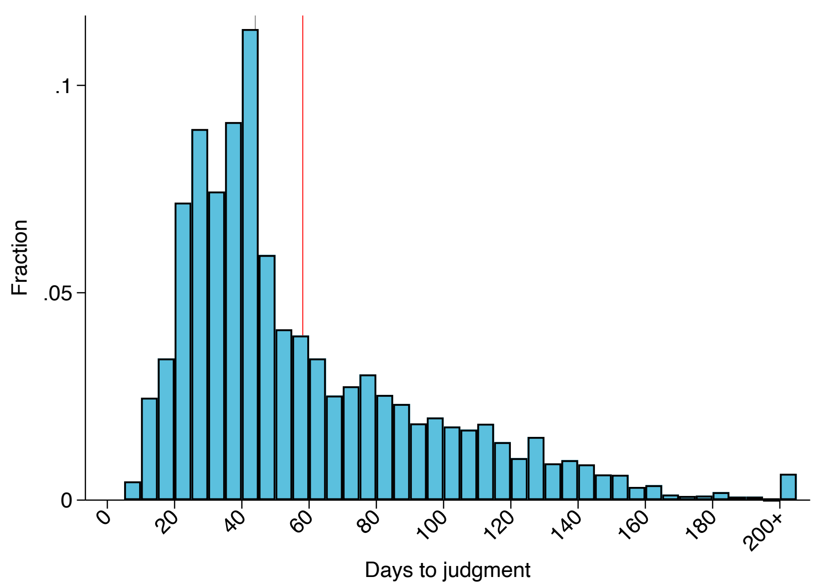
A.1 Figures

Figure A1: Eviction Prevalence: Descriptive Statistics

(a) Share with Judgments over Time



(b) Time between Filing and Judgment



Note: Panel A shows the share of filings that result in judgments within 150 days over time. Panel B shows the distribution of time elapsed between filings and judgments, if a judgment occurs.

Figure A2: Experiment Screenshots

(a) Example Elicitation: Dictator Game (Tenants)

Would you prefer to get **$1000** and [LandlordFirst] [LandlordLast] also gets **$1000**, or you get **$900** and [LandlordFirst] [LandlordLast] gets **$0**?

	I get $1000 and they get $1000	I get $900 and they get $0
Which would you prefer?	○	○

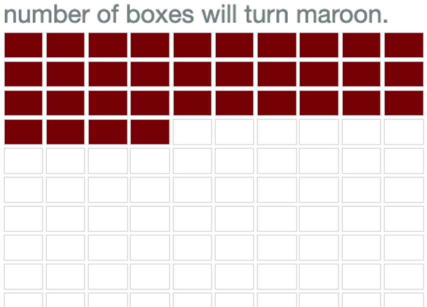
(b) Example Elicitation: Belief Elicitation (Landlords)

Consider monetary evictions judgments given in **January 2020** in Shelby County courts. January 2020 was before the coronavirus pandemic in the U.S.

Out of every 100 monetary judgments in **January 2020**, how many tenants had fully repaid the balances they owed by the beginning of **August 2021**?

Remember that, in the case of monetary judgments, not all landlords will necessarily succeed in collecting all the money they are owed.

To help you visualize your answer, there are 100 boxes below. Each represents a tenant given a monetary eviction judgment. When you type in an answer, the corresponding number of boxes will turn maroon.

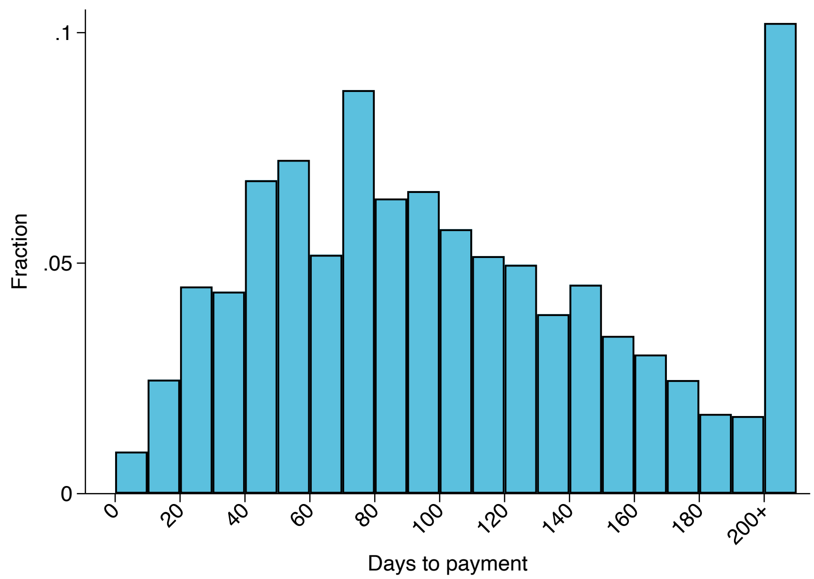


34 tenants had fully repaid

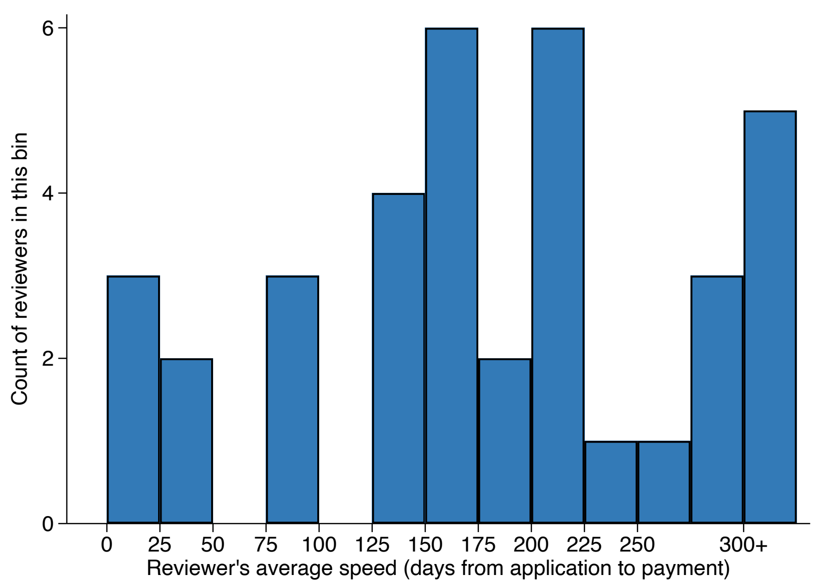
Note: Panel A shows a screenshot of one question asked in a multiple price list in the DG played among tenants. Stakes are $1,000. The elicitation iterates between questions of this type until we find the participant's indifference point. Panel B shows a screenshot of how we elicit average beliefs among landlords. The subsequent screen is a confirmation check.

Figure A3: ERAP Sample Statistics

Panel A: Filings, Judgments, and Applications



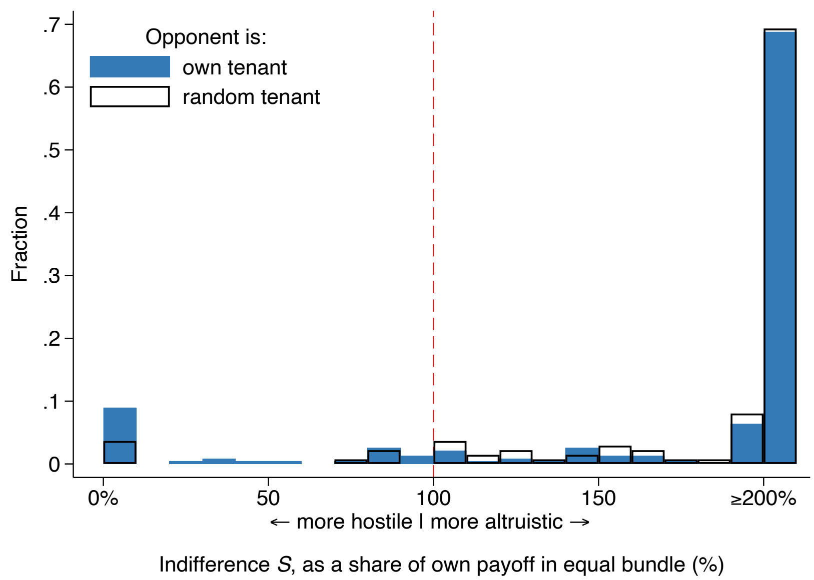
Panel B: Average Time from Application to Payment, by Reviewer



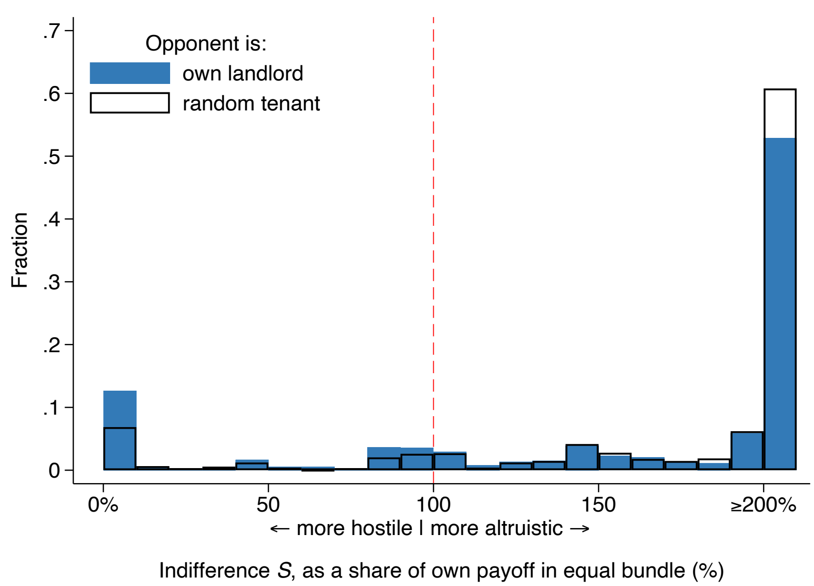
Note: Panel A shows the distribution of time until payment, conditional on application. Panel Bok shows the number of reviewers who take the indicated number of days to review cases, on average.

Figure A4: Behavior in Modified Dictator Game: Histograms

(a) Landlord participants ($N = 371$)



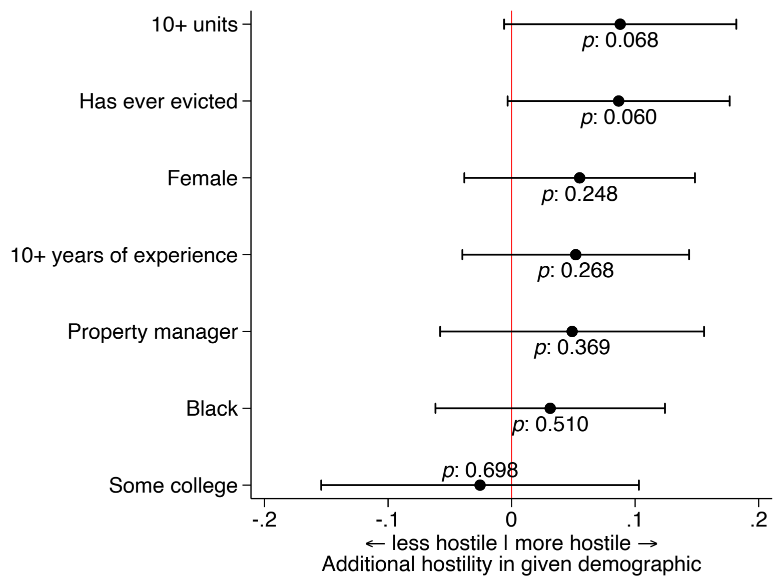
(b) Tenant participants ($N = 1{,}808$)



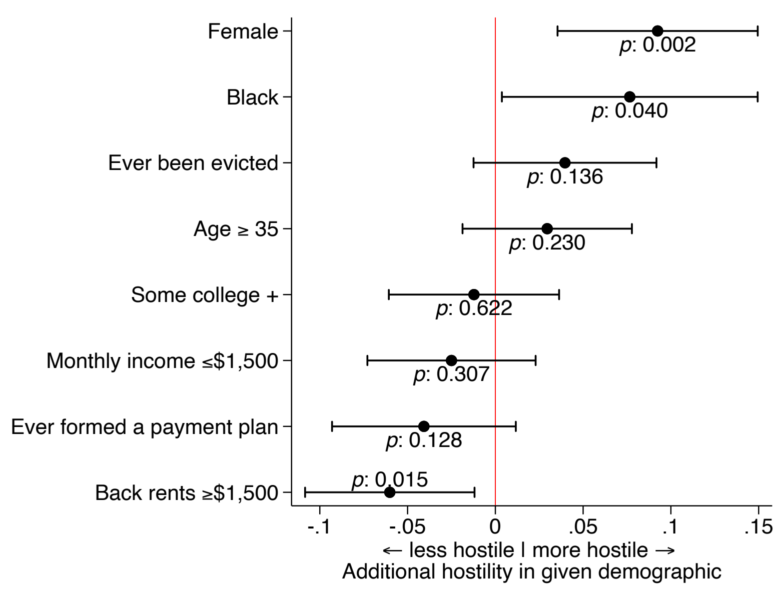
Note: This figure presents histograms of the distribution of the indifference point $S(x)$ in the dictator game for landlord (Panel A) and tenant (Panel B) participants, rescaled as a percentage of x. $S(x)$ represents the point at which a participant is indifferent between the bundle (\$$s$ self, \$0 other) and (\$$x$ self, \$$x$ other). If $S(x) < x$, then the player is hostile; if $S(x) > x$, then the player is altruistic. The horizontal axis presents $100 \times S(x)/x$.

Figure A5: Dictator Game: Heterogeneity

(a) Landlords



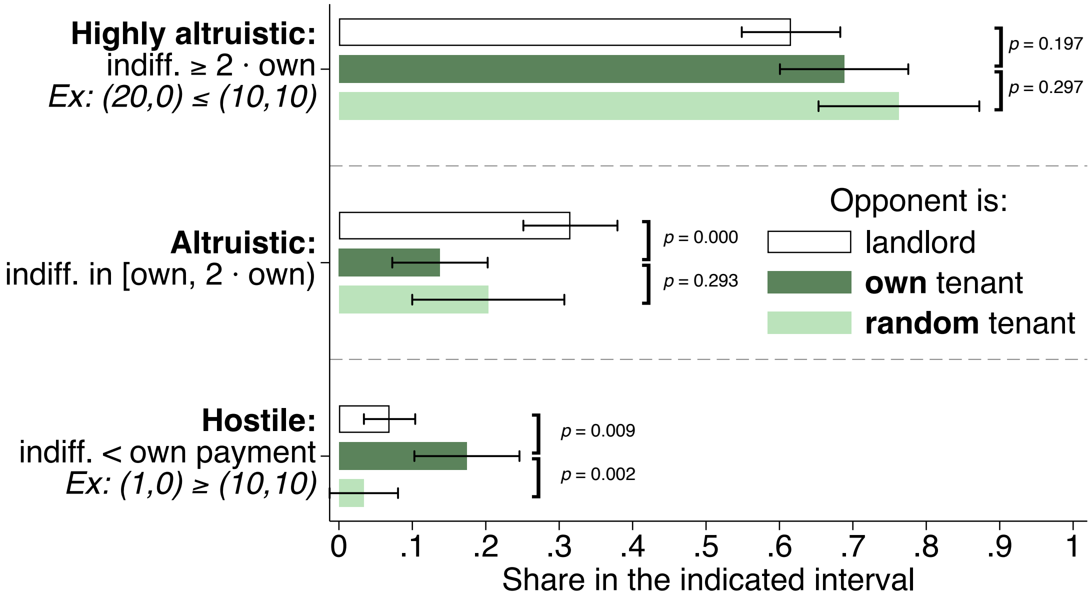
(b) Tenants



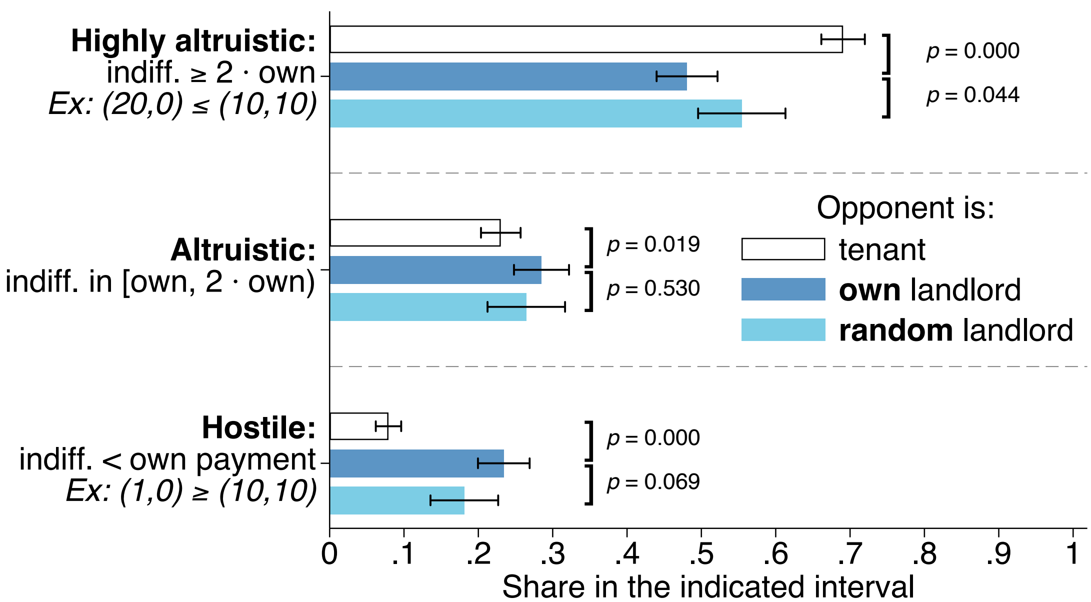
Note: This figure shows heterogeneity in our measures of hostility among landlords (Panel A) and tenants (Panel B). We elicit the point $S(x)$ at which a participant is indifferent between the bundle (\$$s$ self, \$0 other) and (\$$x$ self, \$$x$ other). If $S(x) < x$, then the player is hostile; if $S(x) > x$, then the player is altruistic. In Panel A, we interact our measure of hostility toward own tenants with the indicated demographic. In Panel B we interact our measure of hostility toward own landlords with the indicated demographic. Whiskers show 95-percent confidence intervals.

Figure A6: Behavior in Modified Dictator Game: First DG Only

(a) Landlord participants

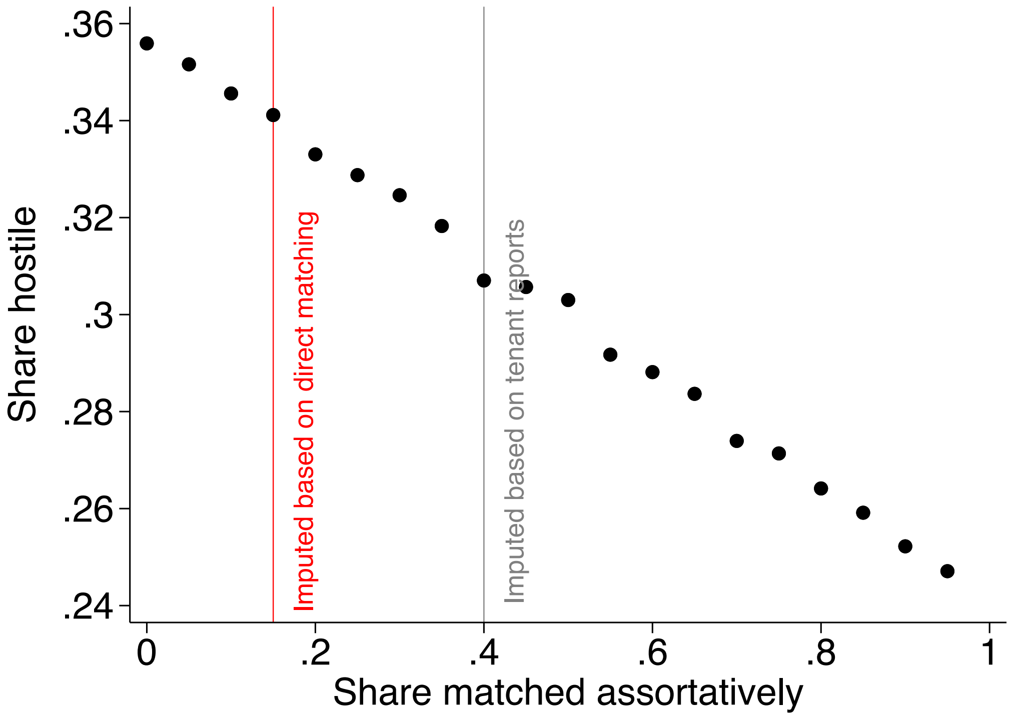


(b) Tenant participants



Note: This figure summarizes the results from the Dictator Games among landlord participants (Panel A) and tenant participants (Panel B). The figure is identical to Figure 2 except it uses only the first instance that each participant plays the DG. See Table A16 for sample sizes and additional details.

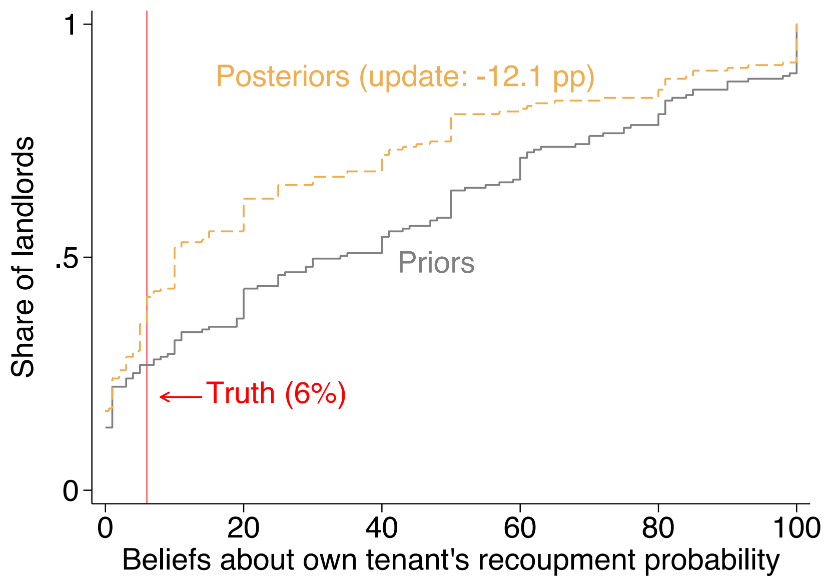
Figure A7: Assortative Matching: Simulations



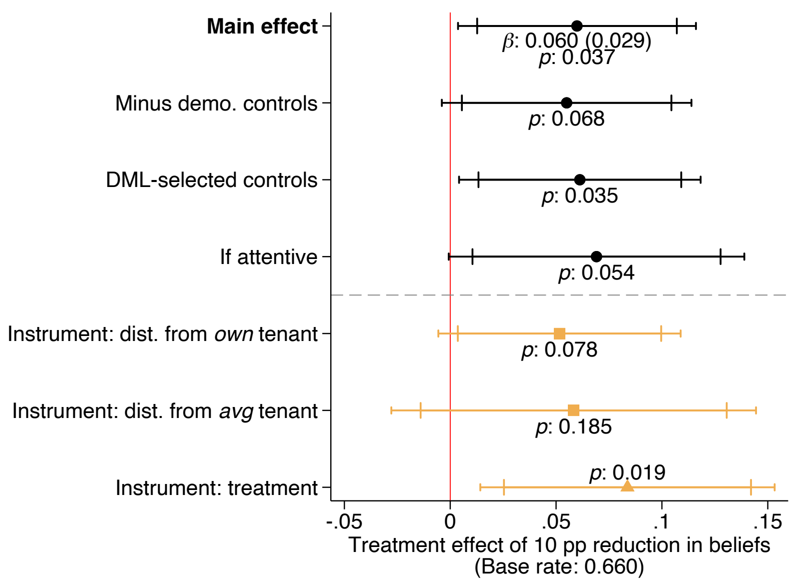
Note: We produce this figure by the process in Section E.2. Each point represents the mean of 25 simulations, where we block re-simulate the entire process. The vertical lines indicate the amount of assortative matching that generates the coefficients described in Section E.2 in the simulated sample.

Figure A8: Posteriors and IV: Landlord Information

(a) Posteriors about recoupment



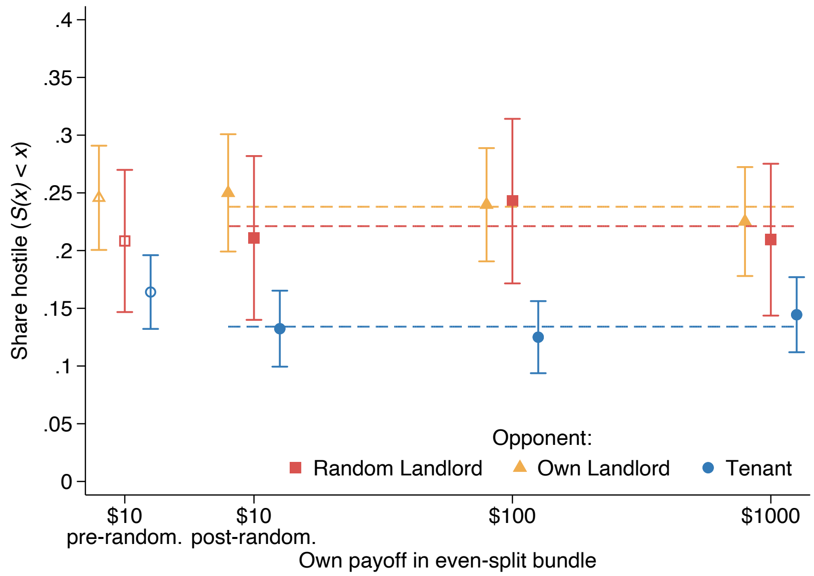
(b) IV: requests materials



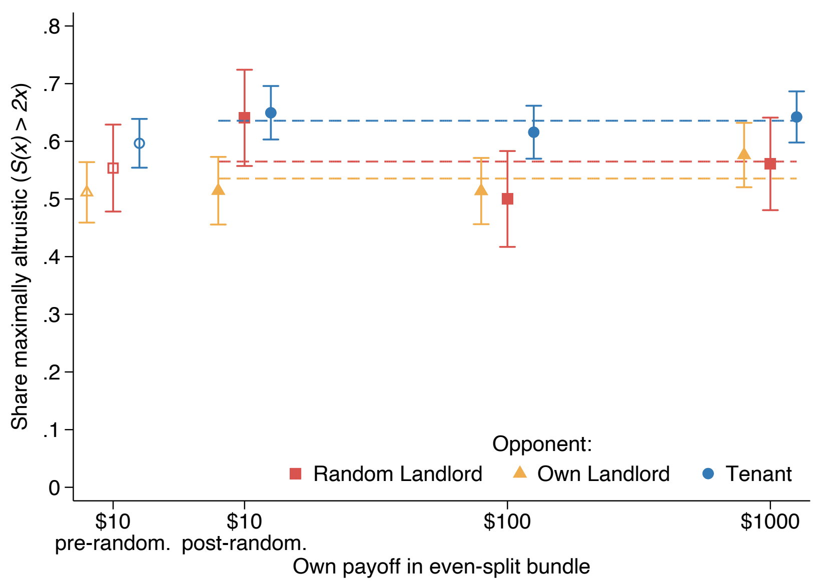
Note: Panel A includes only treated individuals and shows the prior and posterior beliefs about the treatment effect. Panel B shows the effect of beliefs on the outcome, instrumenting for the belief update.

Figure A9: The effect of stakes on tenant hostility and altruism

(a) Hostility



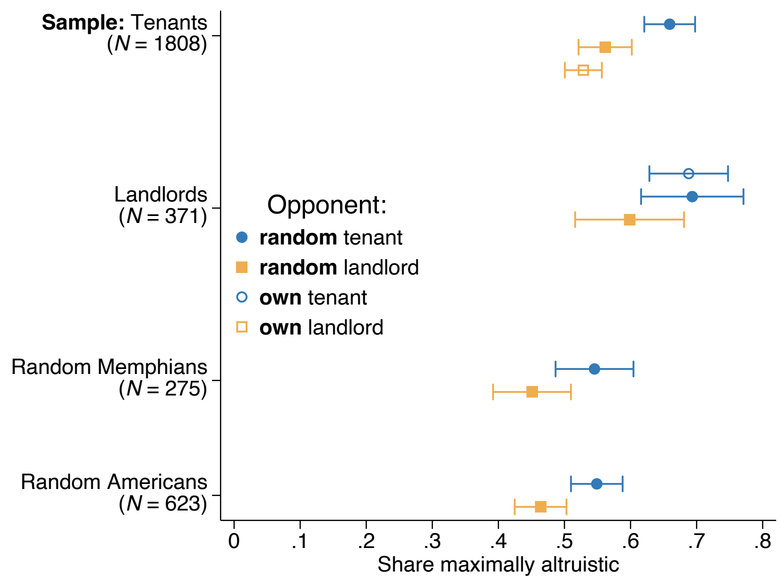
(b) High altruism



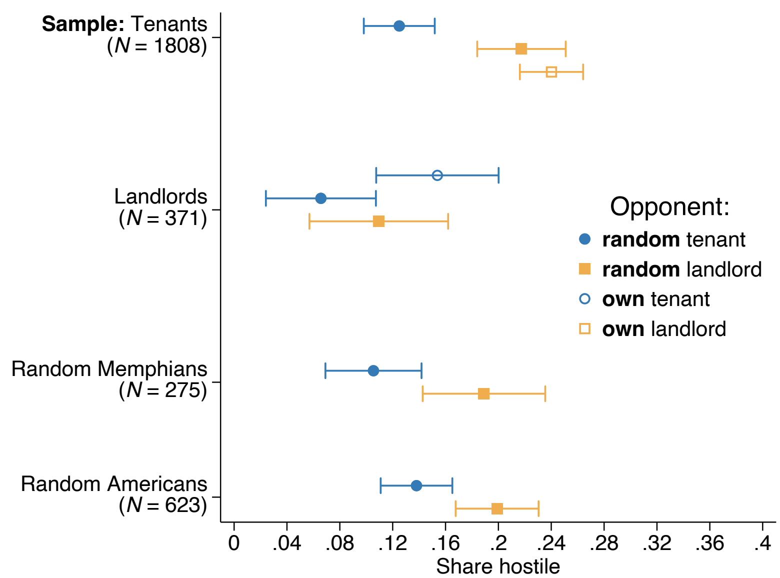
Note: This figure presents the effect of randomizing stakes on tenant behavior in the Dictator Game. We elicit bounds on the the point $S(x)$ at which a participant is indifferent between the bundle ($$s$ self, $0 other) and ($$x$ self, $$x$ other). If $S(x) < x$,then the player is hostile; if $S(x) > x$, then the player is altruistic; if $S(x) > 2x$, then the player is highly altruistic. We randomize $x \in \{\$10, \$100, \$1000\}$ for tenants starting March 27, 2022. Prior to March 27, $x = 10$ for all tenants. We show the share hostile when the opponent is the tenant's own landlord (orange series), random landlord (red series), and random tenant (blue series). Because the elicitation changed slightly once we randomize stakes (Appendix C), we disaggregate the data for $x = 10$ in the two leftmost points to show whether these subtle survey changes affected behavior when the stakes were the same.

Figure A10: Tenants, Landlords, Random Memphians, and Random Americans

(a) High altruism

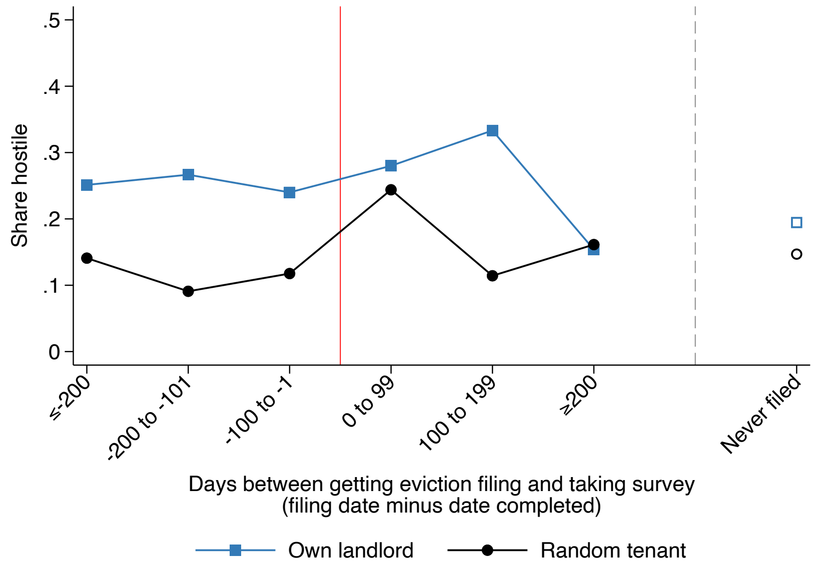


(b) Hostility



Note: This figure summarizes the share highly altruistic (Panel A) or hostile (Panel B) across four samples: the landlord sample and the tenant sample, as well as a sample of random Memphis residents and random Americans. We obtain the Memphis and American samples from survey provider Lucid. We elicit bounds on the the point $S(x)$ at which a participant is indifferent between the bundle (\$$s$ self, \$0 other) and (\$$x$ self, \$$x$ other). If $S(x) < x$,then the player is hostile; if $S(x) > x$, then the player is altruistic; if $S(x) > 2x$, the player is highly altruistic. Among the Memphis and American samples, we elicit $S(x)$ when the opponent is a random unnamed landlord or a random unnamed tenant only, to avoid identifying research subjects.

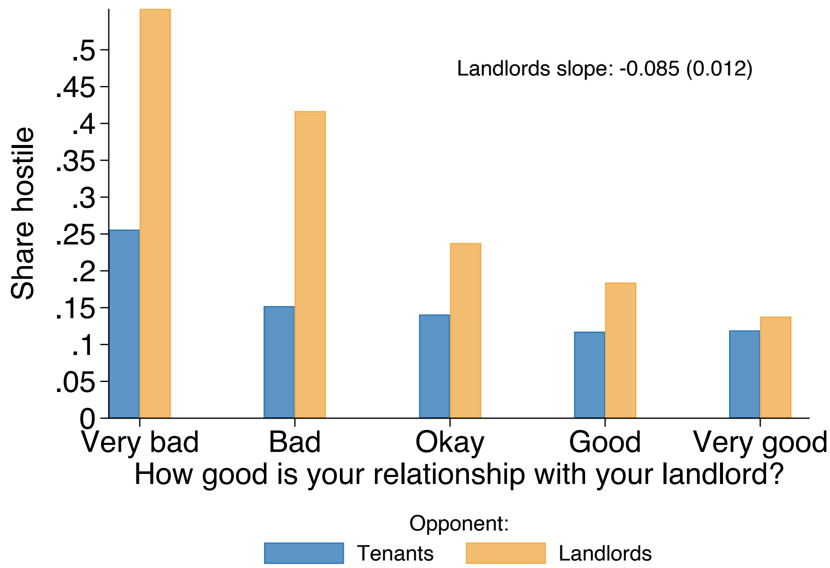
Figure A11: Tenant Hostility by Period between Filing and Survey



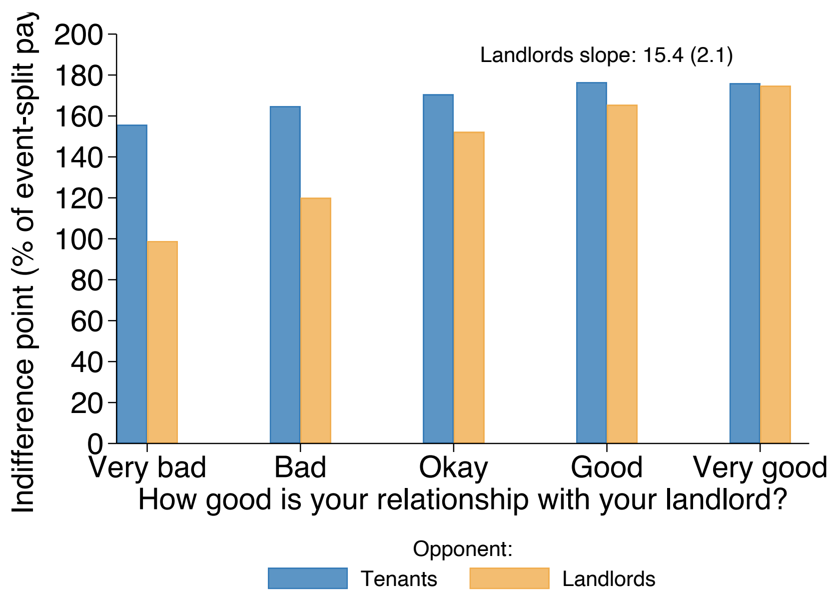
Note: The figure shows tenant hostility by the number of days until a filing, using the sample in Section 5.1.

Figure A12: Tenants' Self-reported Relationship with Landlord and Hostility and Indifference Points

(a) Hostility

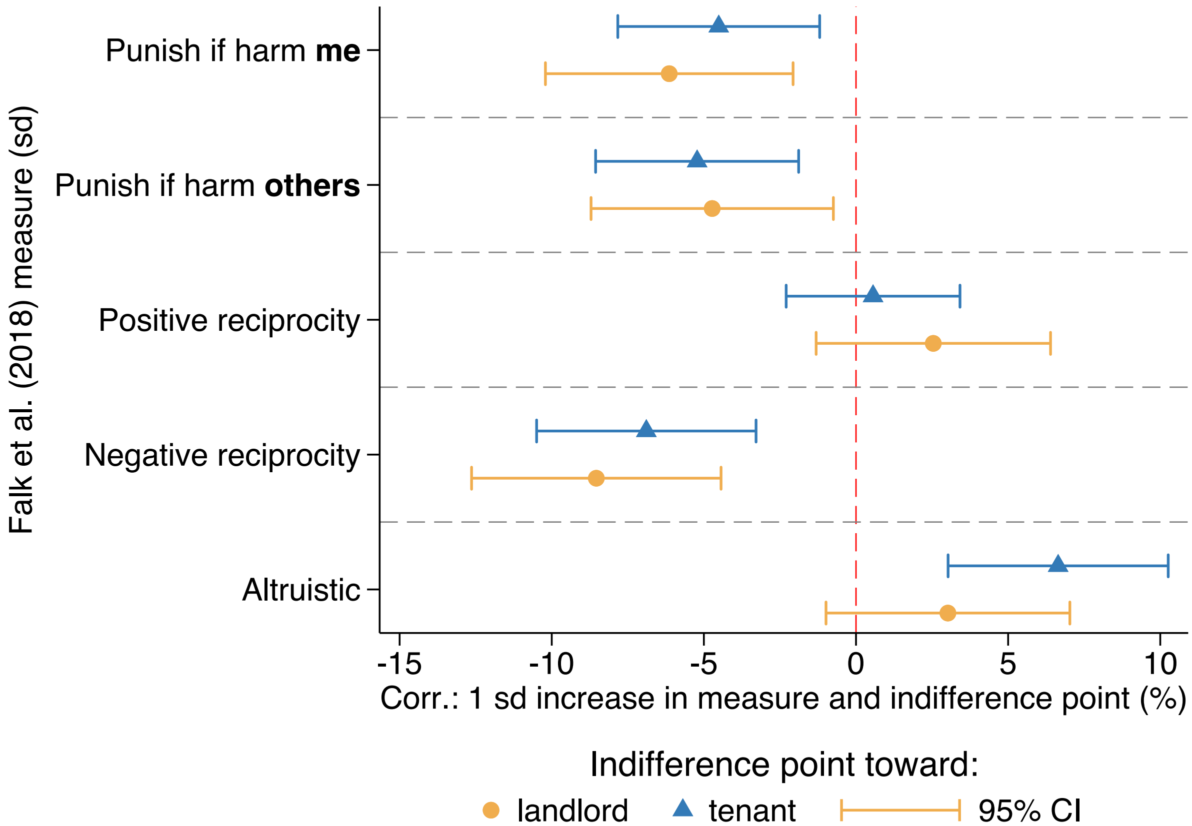


(b) Indifference point $S(x)$



Note: This figure shows the relationship between a Likert scale measuring relationship with the tenant's landlord and their behavior in the DG. The blue bars show the share of tenants who are hostile toward their own landlord, cut by their response on a question about their relationship with their landlord. The orange bars show the share of tenants who are hostile toward a random tenant, cut by their response to the question about their own landlord. $S(x)$ represents the point at which a participant is indifferent between the bundle ($\$s$ self, \$0 other) and ($\$x$ self, $\$x$ other). If $S(x) < x$, then the player is hostile; if $S(x) > x$, then the player is altruistic. Panel A shows the share hostile. Panel B shows the indifference point.

Figure A13: Correlation Between Falk et al. (2018) Questions and Modified Dictator Game

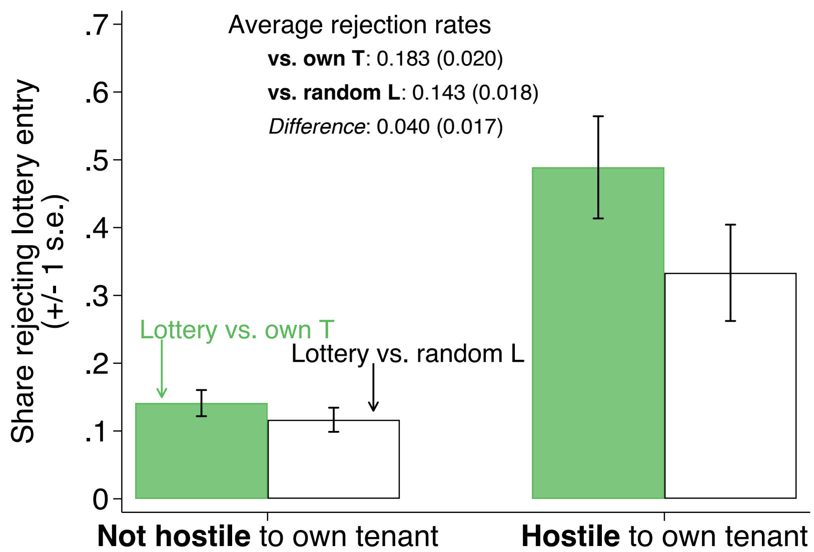


Note: This figure shows the correlation between the survey measures adapted from the Global Preferences Survey (Falk et al., 2018) and tenants' indifference points in the DG toward random tenants and landlords. We pool both own and random landlord opponent. The indifference point $S(x)$ corresponds to the value at which the tenant participant is indifferent between the bundle (\$$s$ self, \$0 other) and (\$$x$ self, \$$x$ other). If $S(x) < x$, then the player is hostile; if $S(x) > x$, then the player is altruistic. The questions from the Falk et al. (2018) survey are Likert scales (from 0 to 10) ask:

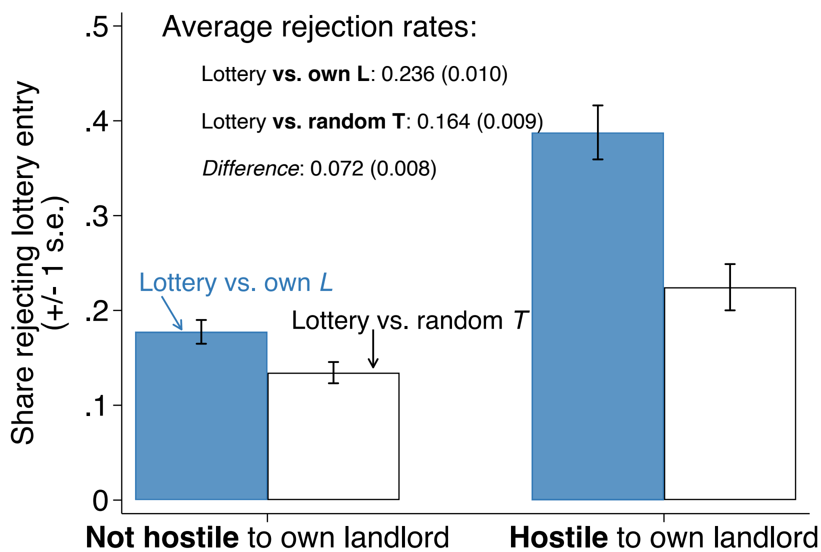
- *Punish if harm me:* "How willing are you to punish someone who treats **you** unfairly, even if there may be costs for you?"
- *Punish if harm others:* "How willing are you to punish someone who treats **others** unfairly, even if there may be costs for you?'
- *Positive reciprocity:* "When someone does me a favor, I am willing to return it."
- *Negative reciprocity:* "If I am treated very unjustly, I will take revenge at the first occasion, even if there is a cost to do so."
- *Altruistic:* "How willing are you to give to good causes without expecting anything in return?"

Figure A14: Hostility: Lottery outcome

(a) Landlords

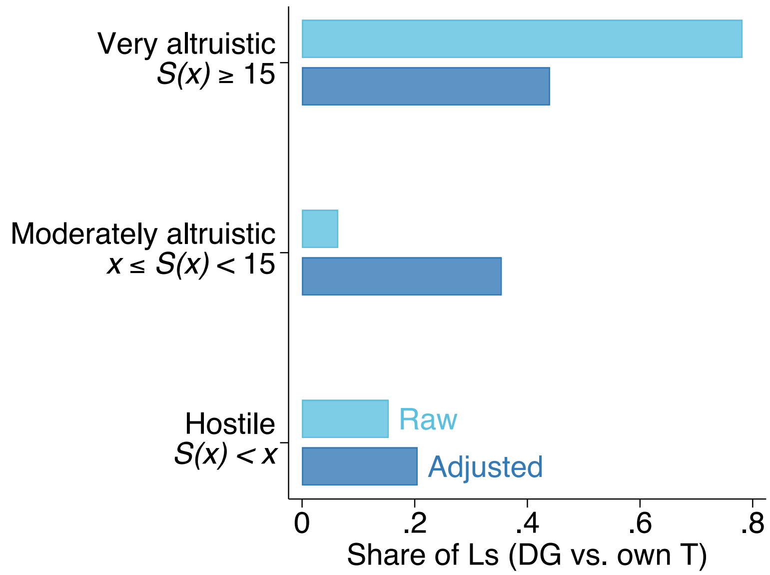


(b) Tenants



Note: This figure presents behaviors in the simple lottery task. The task asks landlords whether they would like to enroll their own tenant or a random landlord in a lottery to win a gift card. The task asks tenants whether they would like to enroll their own landlord or a random tenant in a lottery to win a gift card. The task mentions that the participant's response will be anonymous. It is costless for the participant to enroll the opponent. The text on the figures shows unconditional average rejection rates of enrolling the opponents among the entire sample. The bars show the correlation between behaviors in the lottery task and the DG. The bars limit the sample to the two-thirds that play the Dictator Game against their own tenant (among landlords) or own landlord (among tenants).

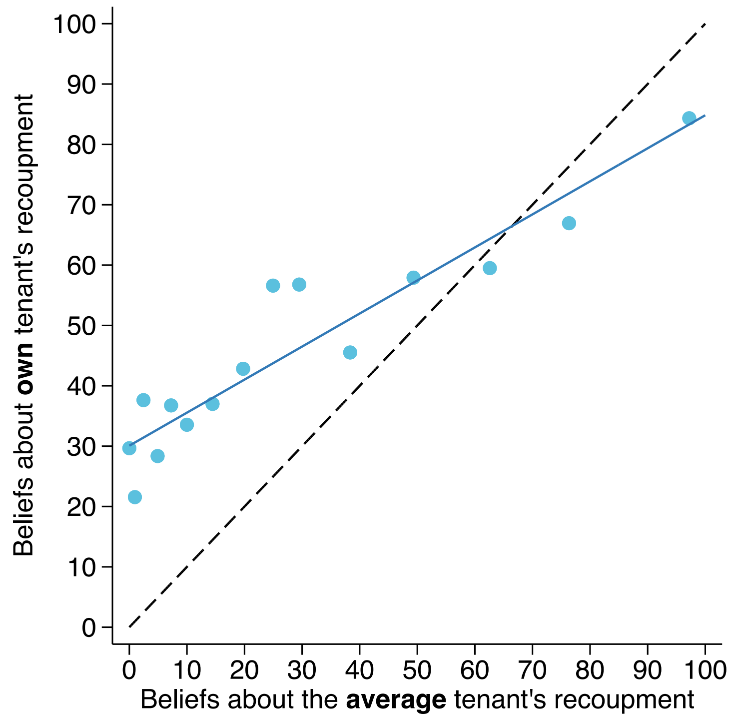
Figure A15: Social Preferences: Rescaling Landlords' Altruism



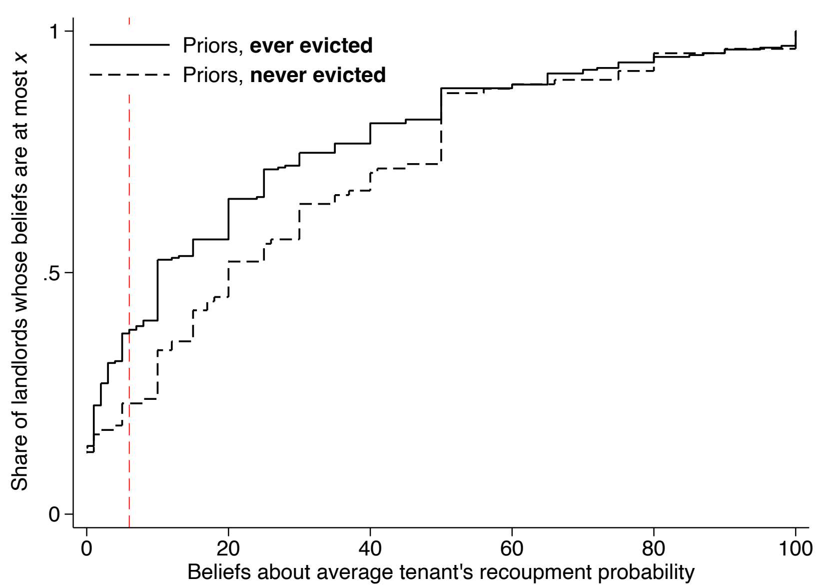
Note: This figure replaces the landlord's indifference point S_{Li}/x as $S_{Li}/x - p_{Li}$, where p_{Li} is the share of back rents they expect to recoup in an eviction. We see p_{Li} as an upper bound on how much money they can expect to recoup from a tenant if they endow them with money. This test therefore adjusts for potential beliefs landlords may have about the amount they could recoup in back rents. Mechanically, no $S_{Li}/x - p_{Li}$ can exceed 2 for $p_{Li} > 0.05$, since the maximally altruistic DG choice we elicit is $(20, 0)$ versus $(10, 10)$ for landlords. Thus we recategorize the most altruistic group into "very altruistic" ($S(x) > \$15$) rather than "highly" as in the main text.

Figure A16: Landlord beliefs: heterogeneity

(a) Correlation between Own and Average

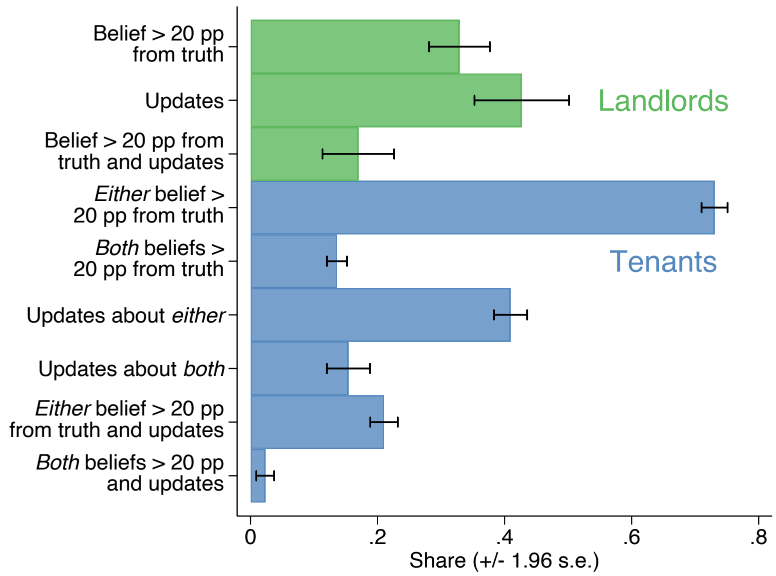


(b) Landlord Experience



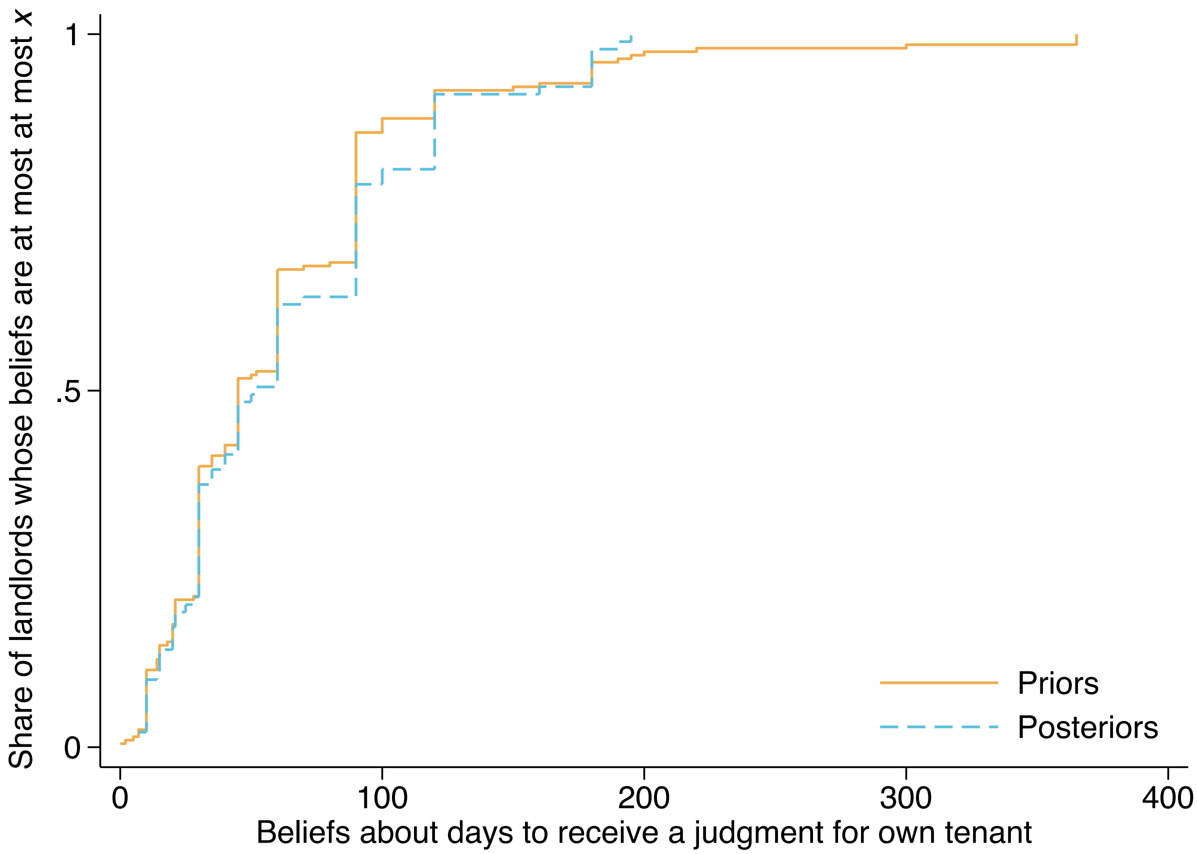
Note: Panel A shows a binned scatterplot of the correlation between a landlords' beliefs aobut their *own* tenant's recoupment probability and their beliefs about the *average* tenant. The dashed line is the 45 degree line. Panel B shows the cumulative distribution functions of landlord landlords' prior beliefs about tenants' recoupment probabilities, cut by whether the landlord reports having evicted a tenant before. The red dashed line shows the true value (6 percent).

Figure A17: Aggregating Tests for Misperceptions



Note: This figure shows the share of tenants and landlords who have misperceptions. We show misperceptions for any fact where we also provided an information correct. Bars show different samples, since we only compute the share who update among those who are exposed to information. For the tenant bars about *both* beliefs, we restrict to the sample of tenants who see both information treatments.

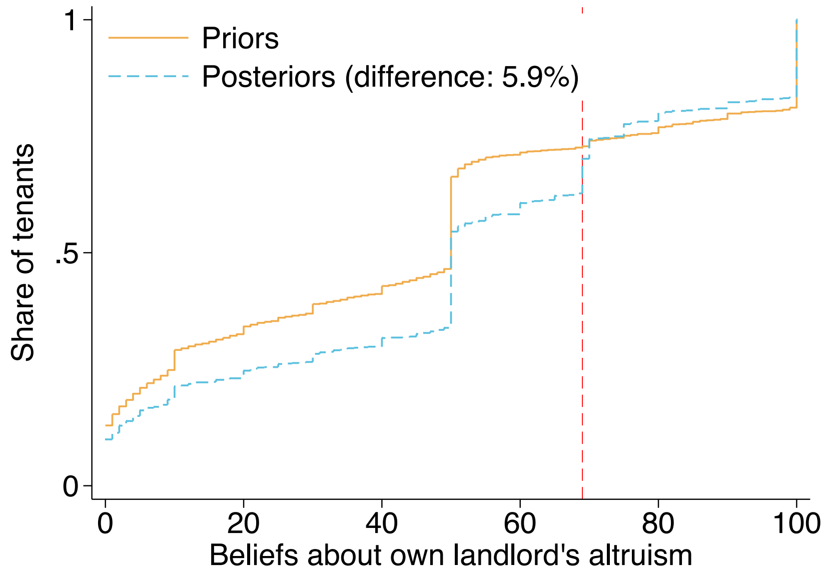
Figure A18: Placebo: information about recoupment and beliefs about days to receive judgment for own tenant



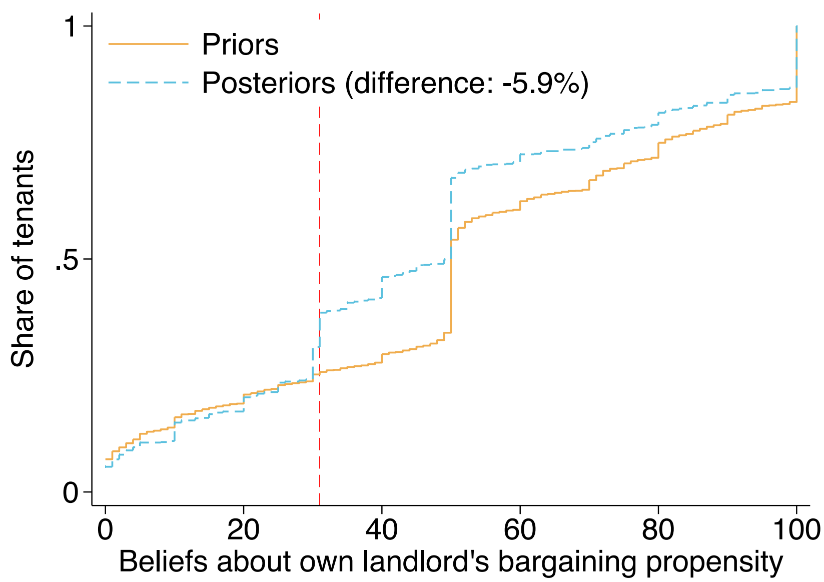
Note: This figure shows a placebo test of the cumulative distribution functions of landlord beliefs about court delays in receiving a judgment for their own tenant. The orange line shows prior beliefs, elicited before providing information; the blue dashed line shows posterior beliefs, elicited after providing information. This figure constitutes a placebo test for the exclusion restriction that providing information about average recoupment probabilities only affects beliefs about own recoupment probabilities.

Figure A19: Tenant Average Belief Updates

(a) Treatment effect: beliefs about landlord altruism



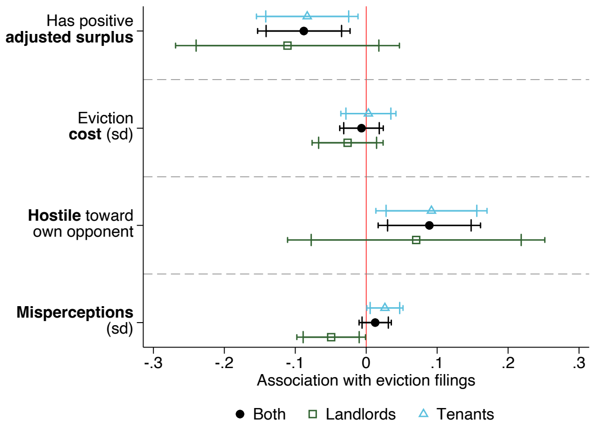
(b) Treatment effect: beliefs about landlord bargaining



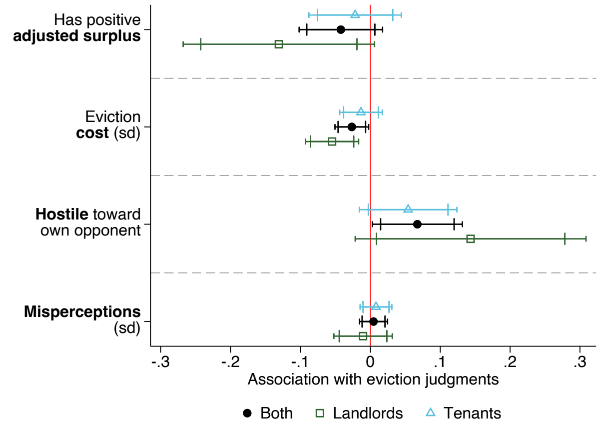
Note: This figure shows belief updates about landlord altruism and bargaining. We only show the sample of people who received each information. Treatments were cross-randomized.

Figure A20: Model Validations: Robustness

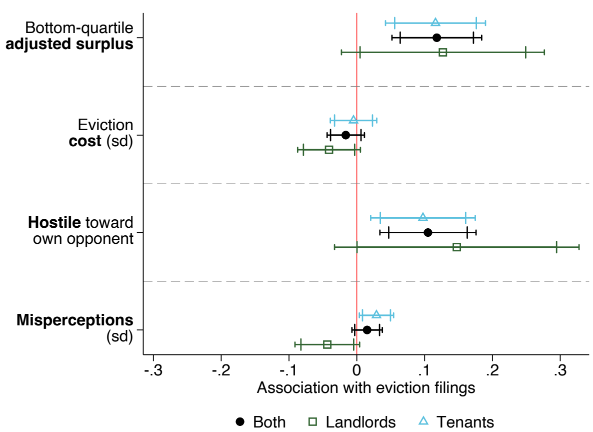
(a) Adjusted Surplus Predicts Eviction Filings: Controls



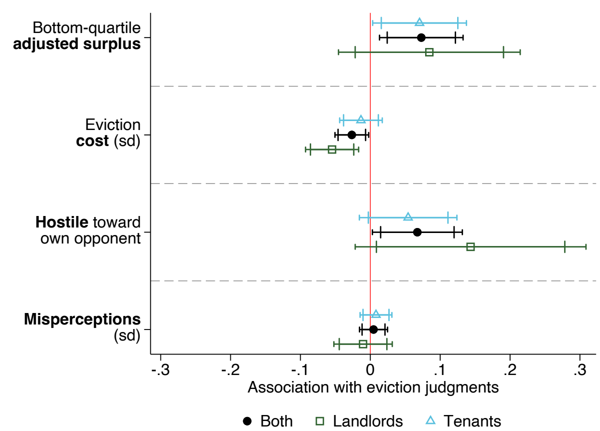
(b) Adjusted Surplus and Eviction Judgments



(c) Adjusted Surplus Predicts Eviction Filings: Bottom-Quartile Surplus



(d) Adjusted Surplus and Eviction Judgments: Bottom-Quartile Surplus

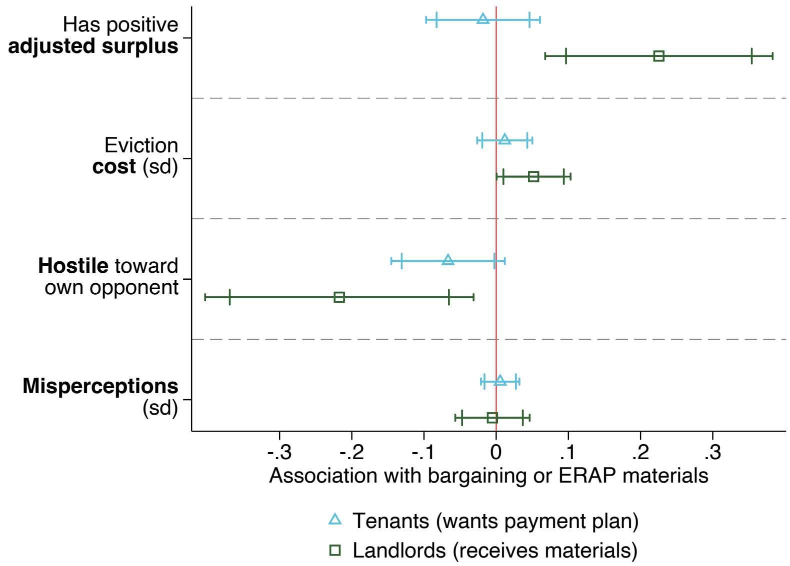


Note: Panel A presents estimates of $\hat{\beta}$ from Equation (10), where the covariate X_i is either positive adjusted surplus, a proxy of eviction cost, hostility, or misperceptions. Appendix E gives details on how these are formed. Relative to Figure 5A, it includes the following controls:

- Landlords. Gender, race (indicators for White or Black), education (indicators for less than high school or some college), occupation indicators (landlord or property manager), landlord size indicators (small (1–2) or medium (3–5)), an indicator for passing an attention check, and linear controls for: age, tenure, rent, and experience.
- Tenants. Indicators for being Black, female, having less than high school, being employed, indicators for passing each attention check, and linear controls for monthly rent and monthly income.

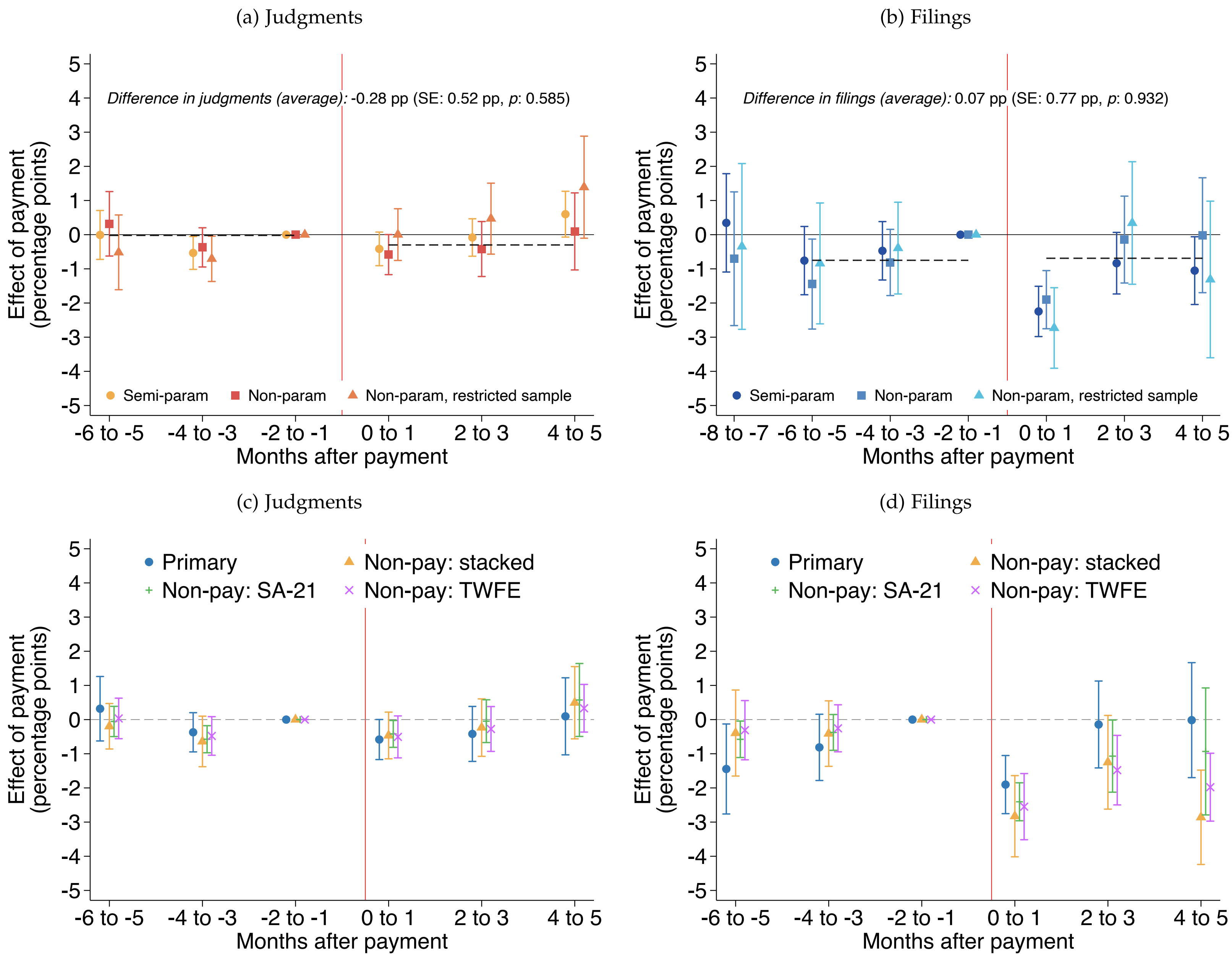
Panel B shows the effects on eviction judgments. Panels C is the same as Figure 5A, but the first coefficient presents $\hat{\beta}$ where the covariate X_i is an indicator for whether the individual has bottom-quartile surplus. Panel D shows the association with judgments using this notion of surplus.

Figure A21: Adjusted Surplus and Take-up/Bargaining



Note: Panel A presents estimates of $\hat{\beta}$ from Equation (10), where the covariate X_i is either positive adjusted surplus, a proxy of eviction cost, hostility, or misperceptions. Appendix E gives details on how these are formed. Panel B splits the data by having above- or below-median misperceptions and hostility. It presents mean eviction filings. The outcome for landlords is whether they wish to receive ERAP materials and for tenants is whether they wish to form a payment plan, as in the information experiment (Section 4).

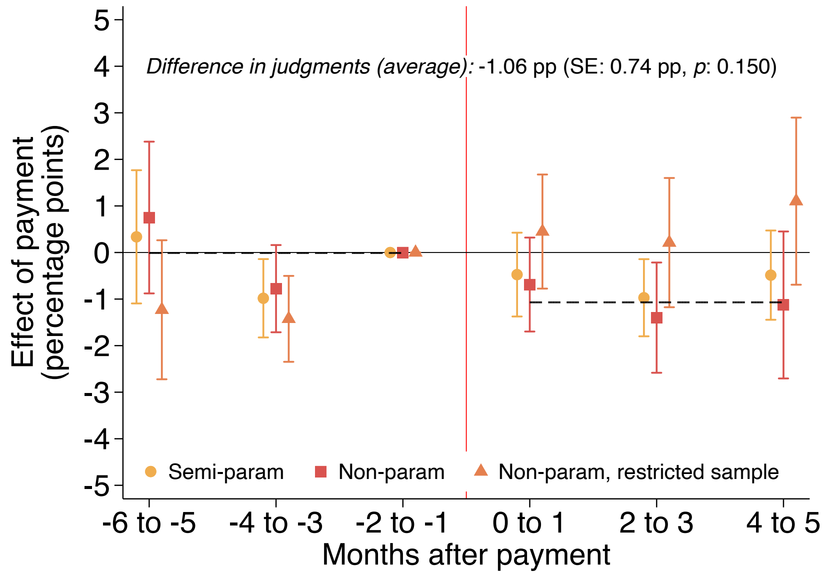
Figure A22: Effects of Rental Assistance on Eviction Judgments and Filings



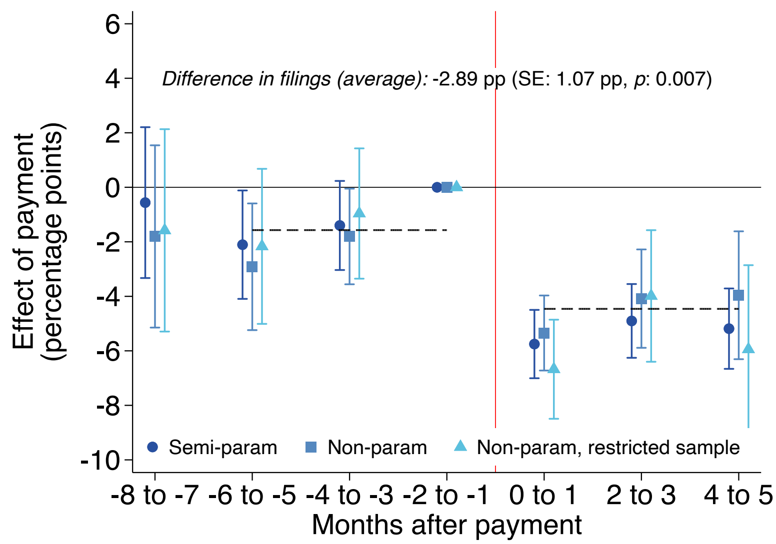
Note: Table A23 gives sample sizes. Panels A and B show the effect on judgments and filings from Equation (100). The semi-parametric specification sets $\sigma_s = 0$ for all s. The figure's point estimates come from the non-parametric specification on the full sample. The restricted sample drops households who apply with eviction notices or shutoffs, who were eligible to be expedited. Panels C and D show the alternative design which compares to non-paid households (Section E.6.3). The primary specification in Panels C and D refer to the estimates from Equation (100), nonparametric specification. The other estimates come from Equations (101) or (102), nonparametric specification. SA-21 refers to estimates from Sun and Abraham (2021), using non-paid households as a control group. TWFE refers to estimates from a Two-Way Fixed Effects specification. The primary estimates are estimated on the microdata. They are clustered at the household level. The non-pay coefficients are estimated on data collapsed to the payment-period by application-period by calendar-period level. They are weighted by the number of observations and clustered at the payment period (TWFE, Sun and Abraham (2021)) or payment period by dataset level (stacked).

Figure A23: Effects of Rental Assistance on Eviction Judgments and Filings (Reweighted)

(a) Judgments



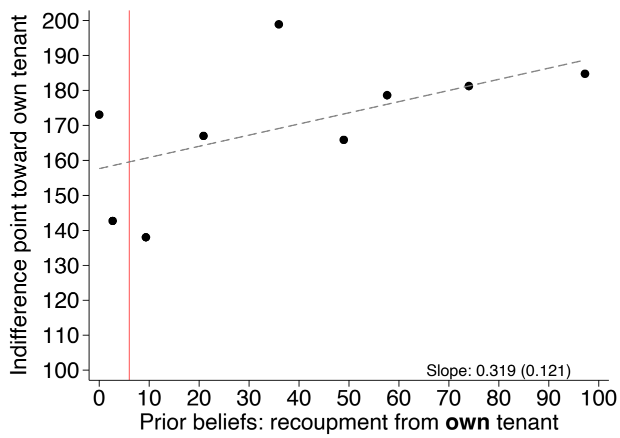
(b) Filings



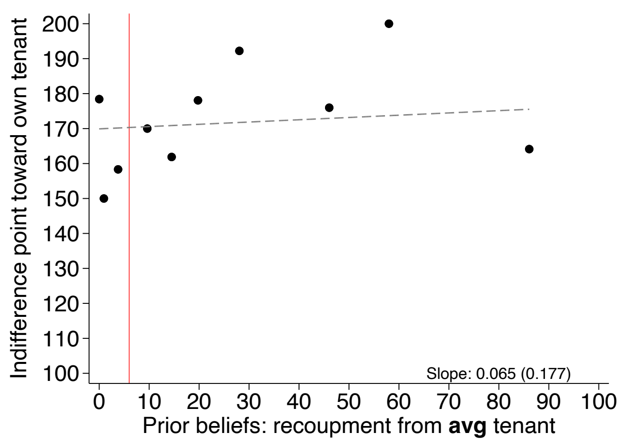
Note: This figure shows the treatment effect on judgments (Panel A) and filings (Panel B) from Equation (100). The semi-parametric specification sets $\sigma_s = 0$ for all s. The nonparametric specification is exactly as in Equation (100). The restricted sample drops households who apply with eviction notices or shutoffs, who were eligible to be expedited. Relative to Figure A22, we weight the estimates using the procedure in Appendix E. The figure's point estimates come from the non-parametric specification on the full sample. We cluster at the household level.

Figure A24: Correlation between Misperception and Altruism among Landlords

(a) Own tenant recoupment



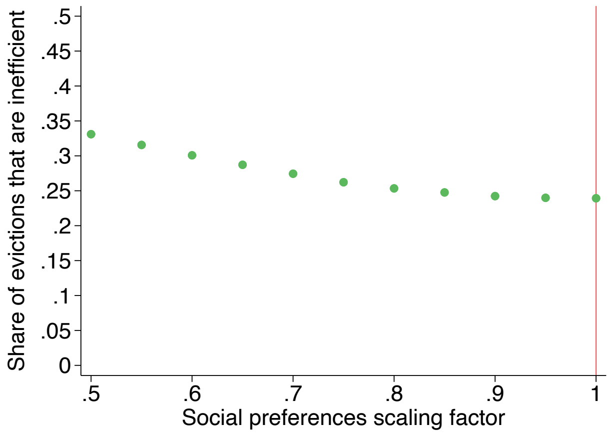
(b) Average tenant recoupment



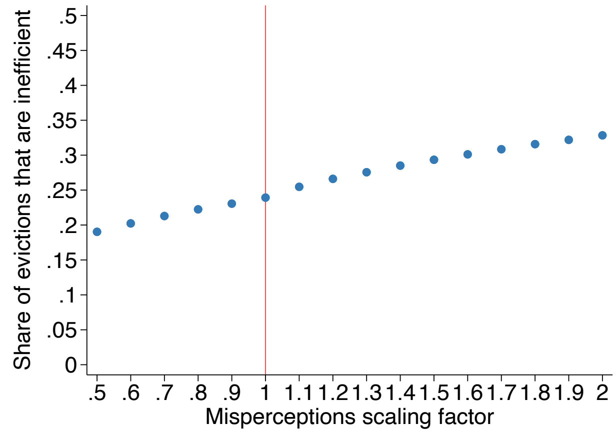
Note: This figure shows binned scatterplots of landlords' prior beliefs (horizontal axis) and landlord behavior toward their own tenant in the Dictator Game (vertical axis). Panels A and B show prior beliefs about the probability of recouping back rents from the landlord's own tenant or the average tenant.

Figure A25: Measurement Error: Simulations

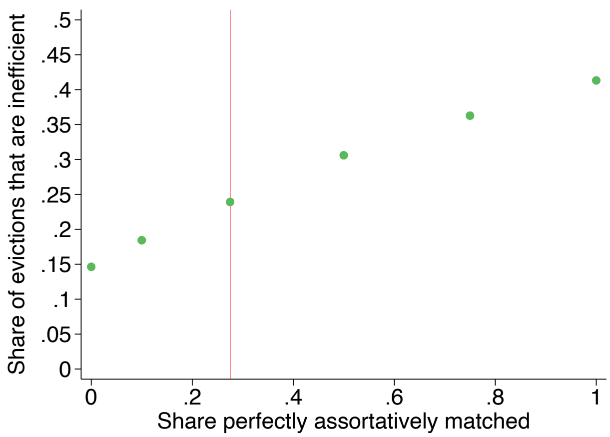
(a) Social Preferences



(b) Misperceptions



(c) Misperceptions



Note: This figure presents the share of evictions that we estimate are inefficient if we rescale social preferences (Panel A) or misperceptions (Panel B). In Panel A, we replace social preferences $\hat{a}_{ji} := sa_{ji}$ for $s < 1$. In Panel B, we replace tenant beliefs about bargaining and landlord beliefs as: $\hat{p}_{ji} := \max\{s, 1\} p_{ji} + (1-s) p_{\text{true}} + \mathbb{1}(s > 1)(s-1) p_{ji}$ for various $s \in \mathbb{R}$, where p_{true} is the true value (e.g., 0.06 for landlords' beliefs). For tenant beliefs about landlord altruism, we replace tenant beliefs as: $\hat{p}_{Ti} := \max\{s, 1\} s p_{Ti} + (1-s) p_{\text{true}} - \mathbb{1}(s > 1)(s-1) p_{Ti}$. This procedure raises misperceptions for $s > 1$ and explains the kink at 1 in Panel B. After applying this scaling factor for beliefs, we then apply the same procedure to obtain the misperceptions that we input into the estimation. In Panel C, we simulate different shares of households who are perfectly assortatively matched (see Appendix E.2). All panels use the estimated and calibrated values of other parameters as in our primary specification. The vertical red lines show the primary estimate.

A.2 Tables

Table A1: Landlord sample balance: random landlord treatment

	Own tenant	Random tenant	p-value
Age	49.3	47.8	0.390
Missing age	3.0	3.6	0.730
Female	67.1	54.0	0.012
White	32.9	29.9	0.553
Black	59.0	56.9	0.702
Has ever evicted	68.4	74.5	0.216
HS or less	17.1	10.2	0.070
Some college	27.4	27.7	0.936
Landlord	61.5	62.8	0.814
Property manager	29.5	27.0	0.611
Tenant tenure (months)	32.7	30.7	0.495
Tenant rent (monthly $)	829.0	778.4	0.198
Missing units	3.0	2.2	0.646
1–2 units	26.5	26.3	0.963
3–5 units	24.8	23.4	0.757
Experience (years)	13.0	13.2	0.866
Attentive	61.5	66.4	0.347
Information treatment	47.4	43.8	0.499
Joint F-test p-value			0.233
Observations	137	234	

Observations denote the total number of observations; some demographics are missing for a small number of observations. The joint p-value is from the joint test when all non-missing.

Table A2: Landlord sample balance: information treatment

	No information	Information	p-value
Age	49.9	47.4	0.117
Missing age	2.0	4.7	0.147
Female	59.0	66.1	0.162
White	28.5	35.7	0.140
Black	60.0	56.1	0.454
Has ever evicted	64.0	78.4	0.002
HS or less	18.0	10.5	0.042
Some college	23.5	32.2	0.063
Landlord	65.5	57.9	0.133
Property manager	23.5	34.5	0.019
Tenant tenure (months)	31.6	32.3	0.792
Tenant rent (monthly $)	805.3	816.2	0.776
Missing units	3.0	2.3	0.696
1–2 units	29.5	22.8	0.146
3–5 units	27.0	21.1	0.184
Experience (years)	13.1	13.0	0.927
Attentive	62.5	64.3	0.717
Random treatment	38.5	35.1	0.499
Hostile to T	0.1	0.1	0.934
Hostile to L	0.1	0.1	0.292
Tenant indifference (% of own payoff)	175.8	175.3	0.934
Landlord indifference (% of own payoff)	172.3	176.6	0.447
Priors (own recoupment)	46.4	39.9	0.079
Priors (avg recoupment)	26.2	22.2	0.155
Priors (own days)	6664.0	6043.3	0.299
Priors (avg days)	1621.7	1355.2	0.051
Uncertainty: average tenant	51.0	48.0	0.560
Uncertainty: own tenant	53.0	46.2	0.193
Joint F-test p-value			0.011
Observations	171	200	

Observations denote the total number of observations; some demographics are missing for a small number of observations. The joint p-value is from the joint test when all non-missing. We include experimental outcomes from the Dictator Games and prior beliefs that are elicited before the treatment.

Table A3: Tenant sample balance: random landlord treatment

	Own landlord	Random landlord	p-value
Black	90.4	89.7	0.633
Female	83.0	87.0	0.030
Age	35.5	35.5	0.948
HS or less	43.0	46.1	0.216
Ever payment plan	56.5	58.0	0.561
Ever overdue rent	85.4	86.5	0.533
Ever evicted	32.4	35.1	0.261
Back rent	2032.8	2010.4	0.904
Monthly rent	890.7	879.7	0.502
Monthly income	2653.6	2117.0	0.059
Employed	60.0	58.6	0.547
Paid by ERAP	54.8	55.1	0.899
Attentive	91.5	89.2	0.116
Attentive (alt. measure)	33.9	32.7	0.614
Treatment (altruism)	52.5	46.7	0.023
Treatment (bargaining)	48.9	49.1	0.935
Joint F-test p-value			0.285
Observations	584	1224	

Observations denote the total number of observations; some demographics are missing for a small number of observations. The joint p-value is from the joint test when all non-missing.

Table A4: Tenant sample balance: information treatment (altruism)

	No information (altruism)	Information (altruism)	p-value
Own L bargain	53.8	53.9	0.937
Avg. L bargain	46.8	46.6	0.869
Own L altruism	45.2	45.7	0.753
Avg. L altruism	39.3	39.5	0.884
Uncertain: own L bargain	0.4	0.4	0.911
Uncertain: own L altruism	0.4	0.4	0.601
Black	90.7	89.7	0.484
Female	84.4	84.2	0.869
Age	35.4	35.5	0.751
HS or less	44.2	43.7	0.825
Ever payment plan	56.7	57.2	0.832
Ever overdue rent	86.9	84.6	0.161
Ever evicted	32.5	34.1	0.464
Back rent	1994.9	2055.5	0.728
Monthly rent	898.4	876.2	0.149
Monthly income	2419.0	2540.1	0.649
Employed	59.8	59.3	0.844
Paid by ERAP	54.2	55.6	0.542
Hostile to T	13.0	15.5	0.124
Hostile to L	22.3	24.3	0.320
Indiff. for T ($S(x)$)	172.0	169.9	0.448
Indiff. for L ($S(x)$)	155.7	154.4	0.687
Attentive	89.8	91.7	0.167
Attentive (alt. measure)	31.8	35.2	0.127
Random landlord	34.8	29.8	0.023
Treatment (bargaining)	50.5	47.5	0.208
Joint F-test p-value			0.662
Observations	915	893	

Observations denote the total number of observations; some particular demographics are missing for a small number of observations. The joint p-value is from the joint test when all non-missing. We include experimental outcomes from the Dictator Games and prior beliefs that are elicited before the treatment.

Table A5: Tenant sample balance: information treatment (bargaining)

	No information (altruism)	Information (altruism)	p-value
Own L bargain	54.2	53.5	0.646
Avg. L bargain	46.2	47.3	0.356
Own L altruism	45.1	45.8	0.676
Avg. L altruism	39.1	39.8	0.605
Uncertain: own L bargain	0.4	0.4	0.233
Uncertain: own L altruism	0.4	0.4	0.969
Black	90.0	90.4	0.783
Female	85.5	83.1	0.162
Age	35.6	35.4	0.621
HS or less	43.6	44.4	0.746
Ever payment plan	55.9	58.0	0.401
Ever overdue rent	85.1	86.3	0.465
Ever evicted	33.8	32.7	0.617
Back rent	1868.6	2188.9	0.067
Monthly rent	881.4	893.1	0.444
Monthly income	2566.7	2390.3	0.508
Employed	59.9	59.3	0.790
Paid by ERAP	57.0	52.7	0.064
Hostile to T	14.0	14.6	0.730
Hostile to L	24.7	21.8	0.139
Indiff. for T ($S(x)$)	170.2	171.6	0.627
Indiff. for L ($S(x)$)	151.7	158.6	0.040
Attentive	90.8	90.7	0.979
Attentive (alt. measure)	32.9	34.2	0.548
Random landlord	32.2	32.4	0.935
Treatment (altruism)	52.1	49.1	0.208
Joint F-test p-value			0.704
Observations	886	922	

Observations denote the total number of observations; some particular demographics are missing for a small number of observations. The joint p-value is from the joint test when all variables are non-missing. We include experimental outcomes from the Dictator Games and prior beliefs that are elicited before the treatment.

Table A6: Experimental Attrition

Panel A: Tenant Survey Attrition

Attrition Funnel	N	% of total consenting	% of total completing demos
1. All who consent	4,440	100.0	·
2. Complete demographics	2,502	56.4	100.0
3. Complete altruism	2,402	54.1	96.0
4. Complete prior beliefs	2,175	49.0	86.9
5. Complete survey	1,929	43.4	77.1
Difference in attrition by treatment (conditional on finishing demographics)			
Random landlord in DG			-0.026
			(0.018)
			[0.147]
Altruism info treatment			0.020
			(0.017)
			[0.236]
Bargaining share info treatment			-0.013
			(0.017)
			[0.437]
Joint p across treatments			[0.252]

Panel B: Landlord Survey Attrition

Attrition Funnel	N	% of total consenting	% of total completing demos
1. All who consent	708	100.0	·
2. Complete demographics	620	87.6	100.0
3. Complete altruism	565	79.8	91.1
4. Complete prior beliefs	448	63.3	72.3
5. Complete survey	404	57.1	65.2
Difference in attrition by treatment (conditional on finishing demographics)			
Random landlord in DG			0.035
			(0.040)
			[0.380]
Info treatment			0.020
			(0.017)
			[0.236]
Joint p across treatments			[0.394]

Brackets indicate p-values. The total sample size is not the same as the text because it includes several drops, e.g. for tenants who complete the survey twice. The own versus random tenant treatment occurs between items 2 and 3. The information treatment occurs between items 4 and 5. The joint p value stacks the two treatments using seemingly unrelated regression.

Table A7: Landlord–tenant assortative matching (names)

(a) Landlord indifference point

	(1)	(2)	(3)	(4)
	Tenant: indifference point	Maximal altruism	Hostile	Maximal hostile
Landlord indifference	16.1	0.084	-0.058	-0.054
	(11.2)	(0.053)	(0.078)	(0.049)
	[0.158]	[0.121]	[0.458]	[0.283]
Observations	471	471	471	471

(b) Landlord hostility

	(1)	(2)	(3)	(4)
	Tenant: indifference point	Maximal altruism	Hostile	Maximal hostile
Landlord hostile	-26.4	-0.13	0.085	0.088
	(20.8)	(0.099)	(0.15)	(0.098)
	[0.212]	[0.204]	[0.577]	[0.377]
Observations	471	471	471	471

Note: This table shows the degree of assortative matching between landlords and tenants. Panel A regresses the tenant Dictator Game outcomes on the landlord's indifference point in the DG. Panel B regresses the tenant DG outcomes on the landlord's hostility in the DG. We use landlords–tenant pairs whom we could match (Appendix E.

Table A8: Tenant perceptions about assortative matching

	(1)
	Tenant: extreme altruism
Highly altruistic to own L	0.24***
	(0.019)
	[0.000]
Constant	0.32***
	(0.013)
	[0.000]
Observations	1224

Note: This table regresses tenant propensity to be highly altruistic (i.e., $S(x) > 2x$) in the DG on tenant beliefs about her own landlord's behavior in the DG.

Table A9: Tenant hostility and free response sentiment

	(1) Hostile: own landlord	(2) Random landlord	(3) Tenant
VADER score (sd)	-0.0811***	-0.0396	-0.00292
	(0.0217)	(0.0295)	(0.0135)
Observations	437	214	651

Note: This table presents the relationship between tenant hostility in the Dictator Game and their free response sentiment. The free response question was: Tenants play a Dictator Game (DG) against a landlord. We elicit $S(x)$, the value at which the tenant participant is indifferent between the bundle (\$$s$ self, \$0 other) and (\$$x$ self, \$$x$ other). If $S(x) < x$, then the player is hostile; if $S(x) > x$, then the player is altruistic. The VADER score represents a measure of sentiment from text responses, developed by Hutto and Gilbert (2014). A higher VADER score indicates more positive sentiment. The free response question was only elicited starting on March 27, 2021, and it was optional. The question was: "Do you have any thoughts about [landlord name] that you want to share?"

Table A10: Landlords Robustness

Panel A: Hostility

	(1) Raw	(2) If attentive	(3) Controls	(4) Full controls	(5) DS lasso
Random tenant	-0.0882***	-0.105***	-0.0847**	-0.0816**	-0.0763***
	(0.0318)	(0.0389)	(0.0339)	(0.0333)	(0.0287)
Observations	371	235	371	371	371
p	0.00582	0.00763	0.0131	0.0150	0.00777

Panel B: Indifference

	(1) Raw	(2) If attentive	(3) Controls	(4) Full controls	(5) DS lasso
Random tenant	11.02*	14.03*	10.27*	9.662*	9.705**
	(5.842)	(7.294)	(5.757)	(5.740)	(4.900)
Observations	371	235	371	371	371
p	0.0600	0.0556	0.0755	0.0933	0.0477

Note: This table presents robustness for the Dictator Game (DG) in the landlord experiment. Panel A focuses on the difference between hostility for own vs. random tenants (Row 4 of Table A15), whereas Panel B focuses on the difference between the indifference points $S(x)$. Column 1 corresponds to Row 4, Column 1 of Table A15. Column 2 limits to attentive landlords who correctly answer the "teal" attention check (Appendix C.6). Column 3 adds a vector of demographic controls for: behavior in the DG toward tenants, age, experience, landlord size, race, gender, education occupation, tenant's tenure in the appartment, rent, attentiveness, and date of completing the survey. Column 4 adds prior beliefs and landlord reports of tenant's tenure in the appartment/rent to the controls in Column 3. We separate them since they were collected after the DG randomization treatment and therefore could, in principle, be affected by it. Column 5 shows the effect using post-double-selection Lasso to select controls (Belloni et al., 2014). The observations is the total number of individuals, since each participant plays against one landlord.

Table A11: Tenants Robustness

Panel A: Hostility

	(1) Raw	(2) If attentive (one q)	(3) If attentive (both q's)	(4) Controls	(5) DS lasso
Random tenant	-0.0975***	-0.1000***	-0.0961***	-0.0990***	-0.0991***
	(0.0130)	(0.0134)	(0.0223)	(0.0130)	(0.0139)
Observations	3032	2780	1002	3032	3032
p	0.000	0.000	0.000	0.000	0.000

Panel B: Indifference

	(1) Raw	(2) If attentive (one q)	(3) If attentive (both q's)	(4) Controls	(5) DS lasso
Random tenant	17.40***	18.16***	18.82***	17.54***	17.51***
	(2.110)	(2.189)	(3.650)	(2.111)	(2.337)
Observations	3032	2780	1002	3032	3032
p	0.000	0.000	0.000	0.000	0.000

This table presents robustness for the Dictator Game (DG) in the tenant experiment. Panel A focuses on the difference between hostility for random landlords versus random tenants, whereas Panel B focuses on the difference between the indifference points $S(x)$. Column 1 corresponds to Row 11, Column 1 of Table A15. Column 2 limits to attentive tenants who correctly either attention check (Appendix C.6). Column 3 limits to tenants who pass both attention checks. Column 4 adds a vector of controls for: prior beliefs (about own and average, across both beliefs, as well as uncertainty), demographics: indicators for Black, female, less than HS, as well as linear controls for age; economic status: having ever formed a payment plan, ever having overdue rents, ever having been evicted, back rents, monthly rent, monthly income, an employment indicator, and self-reports about having been paid by ERAP; and indicators for passing either attention check. Column 5 uses post-double-selection Lasso to select controls (Belloni et al., 2014). The observations is the total number of DGs. As we have multiple observations per individual, standard errors cluster by individual.

Table A12: Landlords Information Treatment: Robustness

	(1) Request materials	(2) Notify	(3) Number referrals	(4) Wants offer	(5) Never agree	(6) Breakeven
Information	0.109**	-0.0444	-0.105	0.0225	-0.0293	3.306
	(0.0457)	(0.0426)	(0.168)	(0.0530)	(0.0240)	(2.122)
Joint F-test p-value	0.014					
Observations	371	371	371	202	371	371
Control Mean	0.660	0.800	0.605	0.790	0.0750	81.78
p-value	0.0168	0.297	0.533	0.671	0.222	0.119
MHC-adjusted p-value	0.099	0.624	0.762	0.762	0.624	0.465

Note: This table presents the effect of the information treatment on all outcomes in the landlord experiment. We use post-double-selection Lasso to select demographic controls from the set in Appendix E (Belloni et al., 2014). ERAP to resend the offer to settle back rents and is only available for landlords who received the offer in the first place (204/371 landlords). The joint p-value comes from a joint test of whether all outcomes are equal to zero, stacked using seemingly unrelated regression. The multiple-hypothesis corrected p-values come from the stepwise procedure in Romano and Wolf (2005).

Table A13: Validation of Dictator Game

	Hostile			Maximally Altruistic		
	(1)	(2)	(3)	(4)	(5)	(6)
Opponent:	Own L.	Random L.	Random T.	Own L.	Random L.	Random T.
Panel A. Index						
Index (SD)	-0.160***	-0.143***	-0.054***	0.144***	0.131***	0.089***
	(0.019)	(0.024)	(0.013)	(0.020)	(0.027)	(0.017)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
N	1224	584	1808	1224	584	1808
Panel B. Natural Language Processing						
Sentiment score (SD)	-0.132***	-0.069	-0.007	0.162***	0.112*	0.052
	(0.039)	(0.055)	(0.025)	(0.045)	(0.063)	(0.035)
	[0.00]	[0.21]	[0.79]	[0.00]	[0.07]	[0.13]
N	1000	487	1487	1000	487	1487
Panel C. Likert Scale						
Likert (SD)	-0.087***	-0.081***	-0.021**	0.070***	0.070***	0.032**
	(0.015)	(0.021)	(0.010)	(0.017)	(0.024)	(0.014)
	[0.00]	[0.00]	[0.05]	[0.00]	[0.00]	[0.02]
N	874	416	1290	874	416	1290
Panel D. Simple Lottery						
Enrolls own landlord in lottery	-0.218***	-0.261***	-0.111***	0.188***	0.227***	0.168***
	(0.032)	(0.044)	(0.022)	(0.033)	(0.046)	(0.027)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Constant	0.409	0.412	0.227	0.384	0.392	0.496
	(0.029)	(0.041)	(0.020)	(0.029)	(0.040)	(0.024)
Enrolls random tenant in lottery	-0.125***	-0.230***	-0.135***	0.130***	0.237***	0.199***
	(0.037)	(0.051)	(0.027)	(0.039)	(0.052)	(0.031)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Constant	0.346	0.406	0.256	0.419	0.368	0.458
	(0.034)	(0.048)	(0.025)	(0.036)	(0.047)	(0.029)
N	1224	584	1808	1224	584	1808

Note: This table presents validations of the Dictator Game. The index averages the outcomes in Panels B–D. The tasks are described in Appendix C.5. Brackets show p-values.

Table A14: Hostility and behavior in simple altruism task: landlords

	Hostile			Maximally Altruistic		
	(1)	(2)	(3)	(4)	(5)	(6)
Opponent:	Own T.	Random T.	Random L.	Own T.	Random T.	Random T.
Enrolls own tenant in lottery	-0.296***	-0.164**	-0.197***	0.168**	0.212*	0.330***
	(0.078)	(0.083)	(0.057)	(0.067)	(0.109)	(0.082)
	[0.00]	[0.05]	[0.00]	[0.01]	[0.05]	[0.00]
Constant	0.395	0.200	0.279	0.485	0.520	0.419
	(0.075)	(0.081)	(0.055)	(0.061)	(0.101)	(0.076)
Enrolls random landlord in lottery	-0.146*	-0.261**	-0.214***	0.198***	0.389***	0.157*
	(0.079)	(0.113)	(0.065)	(0.074)	(0.124)	(0.089)
	[0.06]	[0.02]	[0.00]	[0.01]	[0.00]	[0.08]
Constant	0.278	0.294	0.302	0.453	0.353	0.556
	(0.075)	(0.111)	(0.063)	(0.069)	(0.117)	(0.083)
N	234	137	371	371	137	234

Note: This table presents the analog to Table A13, Panel D, among landlords who completed the same task. Brackets show *p*-values.

Table A15: Behavior in Dictator Game

	(1) Hostile	(2) Indifference point	(3) Highly hostile	(4) Highly altruistic
A. Landlord sample (N = 371)				
1. Own Tenant	0.154***	171.5***	0.090***	0.688***
$N = 234$	(0.024)	(4.2)	(0.019)	(0.030)
	[0.000]	[0.000]	[0.000]	[0.000]
2. Random Tenant	0.066***	182.5***	0.036**	0.693***
$N = 137$	(0.021)	(4.0)	(0.016)	(0.040)
	[0.002]	[0.000]	[0.025]	[0.000]
3. Random Landlord	0.119***	174.3***	0.038***	0.623***
$N = 371$	(0.017)	(2.8)	(0.010)	(0.025)
	[0.000]	[0.000]	[0.000]	[0.000]
4. Own Tenant – Random Tenant	0.088***	-11.0*	0.053**	-0.005
(Row 1 – Row 2)	(0.032)	(5.8)	(0.025)	(0.050)
	[0.006]	[0.060]	[0.031]	[0.914]
5. Random Tenant – Random Landlord	-0.053**	8.2**	-0.001	0.071**
(Row 2 – Row 3)	(0.024)	(4.0)	(0.017)	(0.036)
	[0.028]	[0.042]	[0.941]	[0.048]
6. Own Tenant – Random Landlord	0.035	-2.8	0.052***	0.065**
(Row 1 – Row 3)	(0.024)	(4.0)	(0.017)	(0.030)
	[0.150]	[0.485]	[0.003]	[0.030]
B. Tenant sample (N = 1,808)				
7. Own Landlord	0.240***	153.5***	0.127***	0.529***
$N = 1{,}224$	(0.012)	(2.1)	(0.010)	(0.014)
	[0.000]	[0.000]	[0.000]	[0.000]
8. Random Landlord	0.217***	158.2***	0.103***	0.562***
$N = 584$	(0.017)	(2.9)	(0.013)	(0.021)
	[0.000]	[0.000]	[0.000]	[0.000]
9. Random Tenant	0.143***	170.9***	0.065***	0.624***
$N = 1{,}808$	(0.008)	(1.4)	(0.006)	(0.011)
	[0.000]	[0.000]	[0.000]	[0.000]
10. Own Landlord – Random Landlord	0.023	-4.7	0.024	-0.033
(Row 7 – Row 8)	(0.021)	(3.5)	(0.016)	(0.025)
	[0.279]	[0.183]	[0.130]	[0.187]
11. Random Landlord – Random Tenant	0.075***	-12.7***	0.038***	-0.063***
(Row 8 – Row 9)	(0.018)	(2.9)	(0.013)	(0.020)
	[0.000]	[0.000]	[0.003]	[0.002]
12. Own Landlord – Random Tenant	0.097***	-17.4***	0.062***	-0.096***
(Row 7 – Row 9)	(0.013)	(2.1)	(0.010)	(0.013)
	[0.000]	[0.000]	[0.000]	[0.000]

Note: For each participant, we elicit the point $S(x)$ at which participants are indifferent between the bundle (s self, 0 opponent) and (x self, x opponent). We randomly assign landlords (Panel A) to play against their own tenant (Row 1) or random tenant (Row 2), as well as a random landlord (Row 3). We randomly assign tenants (Panel B) to play against their own landlord (Row 7) or random landlord (Row 8), as well as a random tenant (Row 9). Columns 4–6 and 10–12 show differences between Dictator Game outcomes depending on the opponent. Column (1) shows the share of participants who are "hostile" — i.e., $S(x) < x$. Column (2) shows the normalized value $100 \times S(x)/x$, so that 100 represents that the participant is indifferent between (x, 0) and (x, x). Columns (3) and (4) show the share who are highly hostile or altruistic, respectively: the multiple price list permitted subjects to report $S(x) \in [0, x/10)$ (high hostility) or $S(x) > 2x$ (high altruism). Parentheses show robust standard errors. Brackets show p-values. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A16: Behavior in Dictator Game: First DG Only

	(1) Hostile	(2) Indifference point	(3) Highly hostile	(4) Highly altruistic
A. Landlord sample (N = 203)				
1. Own Tenant	0.174***	169.5***	0.092***	0.688***
$N = 109$	(0.037)	(6.4)	(0.028)	(0.045)
	[0.000]	[0.000]	[0.001]	[0.000]
2. Random Tenant	0.034	189.6***	0.034	0.763***
$N = 59$	(0.024)	(5.4)	(0.024)	(0.056)
	[0.159]	[0.000]	[0.159]	[0.000]
3. Random Landlord	0.069***	179.8***	0.025**	0.616***
$N = 203$	(0.018)	(3.2)	(0.011)	(0.034)
	[0.000]	[0.000]	[0.025]	[0.000]
4. Own Tenant – Random Tenant	0.140***	-20.1**	0.058	-0.075
(Row 1 – Row 2)	(0.043)	(8.3)	(0.036)	(0.071)
	[0.001]	[0.017]	[0.114]	[0.296]
5. Random Tenant – Random Landlord	-0.035	9.8	0.009	0.147**
(Row 2 – Row 3)	(0.030)	(6.2)	(0.026)	(0.065)
	[0.237]	[0.118]	[0.722]	[0.025]
6. Own Tenant – Random Landlord	0.105***	-10.3	0.067**	0.072
(Row 1 – Row 3)	(0.041)	(7.1)	(0.030)	(0.056)
	[0.010]	[0.150]	[0.025]	[0.198]
B. Tenant sample (N = 960)				
7. Own Landlord	0.234***	153.0***	0.112***	0.481***
$N = 572$	(0.018)	(2.9)	(0.013)	(0.021)
	[0.000]	[0.000]	[0.000]	[0.000]
8. Random Landlord	0.181***	163.4***	0.083***	0.554***
$N = 276$	(0.023)	(3.9)	(0.017)	(0.030)
	[0.000]	[0.000]	[0.000]	[0.000]
9. Random Tenant	0.079***	182.8***	0.026***	0.691***
$N = 960$	(0.009)	(1.5)	(0.005)	(0.015)
	[0.000]	[0.000]	[0.000]	[0.000]
10. Own Landlord – Random Landlord	0.053*	-10.3**	0.029	-0.074**
(Row 7 – Row 8)	(0.029)	(4.8)	(0.021)	(0.037)
	[0.069]	[0.033]	[0.179]	[0.044]
11. Random Landlord – Random Tenant	0.102***	-19.4***	0.057***	-0.136***
(Row 8 – Row 9)	(0.025)	(4.2)	(0.017)	(0.033)
	[0.000]	[0.000]	[0.001]	[0.000]
12. Own Landlord – Random Tenant	0.155***	-29.8***	0.086***	-0.210***
(Row 7 – Row 9)	(0.020)	(3.3)	(0.014)	(0.026)
	[0.000]	[0.000]	[0.000]	[0.000]

Note: See notes to Table A15 for description of our altruism and hostility measures. Parentheses show robust standard errors. Brackets show p-values. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table is identical to Table A15 except it only keeps the first instance that each participant plays the DG.

Table A17: Behavior in Dictator Game: Entropy-Weight Adjusted (Hainmueller, 2012)

	(1) Hostile	(2) Indifference point	(3) Highly hostile	(4) Highly altruistic
A. Landlord sample (N = 371)				
1. Own Tenant	0.149***	172.9***	0.085***	0.701***
$N = 234$	(0.024)	(4.2)	(0.019)	(0.031)
	[0.000]	[0.000]	[0.000]	[0.000]
2. Random Tenant	0.072***	183.0***	0.040**	0.712***
$N = 137$	(0.023)	(4.4)	(0.018)	(0.041)
	[0.003]	[0.000]	[0.025]	[0.000]
3. Random Landlord	0.125***	173.7***	0.040***	0.627***
$N = 371$	(0.018)	(3.0)	(0.011)	(0.027)
	[0.000]	[0.000]	[0.000]	[0.000]
4. Own Tenant – Random Tenant	0.077**	-10.1*	0.045*	-0.011
(Row 1 – Row 2)	(0.033)	(6.1)	(0.026)	(0.052)
	[0.021]	[0.099]	[0.083]	[0.831]
5. Random Tenant – Random Landlord	-0.053**	9.3**	0.000	0.085**
(Row 2 – Row 3)	(0.026)	(4.4)	(0.018)	(0.038)
	[0.043]	[0.033]	[0.982]	[0.026]
6. Own Tenant – Random Landlord	0.025	-0.7	0.045**	0.074**
(Row 1 – Row 3)	(0.025)	(4.1)	(0.018)	(0.032)
	[0.322]	[0.857]	[0.012]	[0.019]
B. Tenant sample (N = 1,808)				
7. Own Landlord	0.233***	154.4***	0.126***	0.535***
N = 1,224	(0.012)	(2.1)	(0.010)	(0.015)
	[0.000]	[0.000]	[0.000]	[0.000]
8. Random Landlord	0.226***	156.8***	0.102***	0.547***
$N = 584$	(0.018)	(3.1)	(0.013)	(0.022)
	[0.000]	[0.000]	[0.000]	[0.000]
9. Random Tenant	0.141***	171.4***	0.063***	0.628***
N = 1,808	(0.009)	(1.4)	(0.006)	(0.012)
	[0.000]	[0.000]	[0.000]	[0.000]
10. Own Landlord – Random Landlord	0.006	-2.4	0.025	-0.011
(Row 7 – Row 8)	(0.022)	(3.7)	(0.016)	(0.026)
	[0.780]	[0.521]	[0.135]	[0.666]
11. Random Landlord – Random Tenant	0.085***	-14.6***	0.039***	-0.081***
(Row 8 – Row 9)	(0.019)	(3.1)	(0.014)	(0.021)
	[0.000]	[0.000]	[0.004]	[0.000]
12. Own Landlord – Random Tenant	0.091***	-17.0***	0.064***	-0.092***
(Row 7 – Row 9)	(0.013)	(2.2)	(0.010)	(0.014)
	[0.000]	[0.000]	[0.000]	[0.000]

Note: See notes to Table A15 for description of our altruism and hostility measures. Parentheses show robust standard errors. Brackets show p-values. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. We compute weights by comparing tenant participants to non-participants based on observables at application. For landlords, we compare the tenant named on their survey to the randomly selected tenant of non-participants (as in Table 2).

Table A18: Behavior in Dictator Game: All Participants, Prior to Attrition

	(1) Hostile	(2) Indifference point	(3) Highly hostile	(4) Highly altruistic
A. Landlord sample ($N = 565$)				
1. Own Tenant	0.166***	169.8***	0.092***	0.671***
$N = 368$	(0.019)	(3.4)	(0.015)	(0.025)
	[0.000]	[0.000]	[0.000]	[0.000]
2. Random Tenant	0.061***	184.7***	0.030**	0.721***
$N = 197$	(0.017)	(3.2)	(0.012)	(0.032)
	[0.000]	[0.000]	[0.014]	[0.000]
3. Random Landlord	0.126***	173.9***	0.041***	0.628***
$N = 565$	(0.014)	(2.3)	(0.008)	(0.020)
	[0.000]	[0.000]	[0.000]	[0.000]
4. Own Tenant – Random Tenant	0.105***	-14.9***	0.062***	-0.050
(Row 1 – Row 2)	(0.026)	(4.7)	(0.019)	(0.040)
	[0.000]	[0.002]	[0.002]	[0.219]
5. Random Tenant – Random Landlord	-0.065***	10.8***	-0.010	0.092***
(Row 2 – Row 3)	(0.020)	(3.4)	(0.014)	(0.030)
	[0.002]	[0.002]	[0.453]	[0.002]
6. Own Tenant – Random Landlord	0.040*	-4.1	0.052***	0.043*
(Row 1 – Row 3)	(0.021)	(3.4)	(0.014)	(0.025)
	[0.054]	[0.233]	[0.000]	[0.081]
B. Tenant sample ($N = 2,402$)				
7. Own Landlord	0.260***	150.0***	0.141***	0.518***
$N = 1,621$	(0.011)	(1.9)	(0.009)	(0.012)
	[0.000]	[0.000]	[0.000]	[0.000]
8. Random Landlord	0.228***	156.4***	0.110***	0.552***
$N = 781$	(0.015)	(2.5)	(0.011)	(0.018)
	[0.000]	[0.000]	[0.000]	[0.000]
9. Random Tenant	0.157***	169.3***	0.073***	0.622***
$N = 2,402$	(0.007)	(1.3)	(0.005)	(0.010)
	[0.000]	[0.000]	[0.000]	[0.000]
10. Own Landlord – Random Landlord	0.032*	-6.4**	0.031**	-0.034
(Row 7 – Row 8)	(0.019)	(3.1)	(0.014)	(0.022)
	[0.087]	[0.040]	[0.028]	[0.114]
11. Random Landlord – Random Tenant	0.071***	-12.9***	0.037***	-0.070***
(Row 8 – Row 9)	(0.016)	(2.5)	(0.011)	(0.017)
	[0.000]	[0.000]	[0.001]	[0.000]
12. Own Landlord – Random Tenant	0.103***	-19.3***	0.068***	-0.104***
(Row 7 – Row 9)	(0.012)	(1.9)	(0.009)	(0.012)
	[0.000]	[0.000]	[0.000]	[0.000]

Note: This table is the same as Table A15, but includes all participants from either the landlord survey or tenant survey prior to attrition. The table includes all observations that complete the module, including potential duplicates who take the survey, attrit, and then re-take from another unique link.

Table A19: Belief Updating and Hostility

Panel A: Landlords

	(1)	(2)	(3)	(4)
	1(updates)	Update (signed)	1(updates)	Update (signed)
Information treatment	0.422***	-12.68**	0.431***	-15.02***
	(0.129)	(4.925)	(0.130)	(4.843)
Treat × indiff.	-0.0000958	0.0165	-0.000138	0.0266
	(0.000709)	(0.0269)	(0.000714)	(0.0259)
Indifference: tenant			-0.0000116	0.00278
			(0.0000280)	(0.00489)
Observations	234	234	234	234
Controls			✓	✓

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel B: Tenants

	(1)	(2)	(3)	(4)
	1(updates)	1(updates)	1(updates)	1(updates)
Info	0.374	0.340	0.374	0.340
	(0.017)	(0.017)	(0.017)	(0.017)
	[0.000]	[0.000]	[0.000]	[0.000]
Info × highly hostile	-0.068	-0.093	-0.068	-0.089
	(0.051)	(0.043)	(0.051)	(0.043)
	[0.181]	[0.033]	[0.181]	[0.041]
Observations	1808	1808	1808	1808
Outcome	Barg.	Alt.	Barg.	Alt.
Controls			✓	✓
p: joint test	.	0.0446	.	0.0535

Note: This table presents whether landlords and tenants update their beliefs and whether the magnitude of belief updates is related to hostility. For landlords in Panel A, the update is about tenants' recoupment probabilities, interacted by whether they are hostile toward their own tenants. The sample size is the 234 landlords who played the Dictator Game against their own tenant. For tenants in Panel B, the update is about landlords' bargaining probabilities and altruism, interacted by whether they are highly hostile toward landlords. We define high hostility in the notes to Table A15. Columns 1 and 3 show the effects on tenant beliefs about bargaining. Columns 2 and 4 show the effects on tenant beliefs about landlord altruism. The joint tests in Columns 2 and 4 show joint tests of the two interaction terms in Columns 1–2 and 3–4, respectively.

Table A20: Landlord heterogeneity: Information and Hostility

	(1) 1(receives materials)	(2) 1(receives materials)
Belief update	-0.0176***	-0.00108
	(0.00578)	(0.00418)
Update × hostile	0.0182**	
	(0.00769)	
Update × indifference point		-0.785*
		(0.422)
Observations	234	234

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: This table presents the differential effect of the information treatment on receiving materials. We interact the instrumental variables specification in Figure 3 with hostility toward own tenant (Column 1) or the landlord indifference point toward the tenant (Column 2). Rows 1 and 2 present separate specifications. The interaction term shows the additional effect of behavior in the dictator game on the relationship between beliefs and behaviors. There are 234 participants because 234 landlords out of 374 play the Dictator Game against their own tenant.

Table A21: Policy Evaluation Balance Table: Demographics

Demographic	Above Median Payment Time	Below Median Payment Time	Difference
Age	36.75	35.77	-0.98
			(0.45)
Female	0.76	0.75	-0.01
			(0.01)
Black	0.91	0.91	-0.00
			(0.01)
Disabled	0.11	0.11	-0.00
			(0.01)
Household size	2.4	2.3	-0.1
			(0.0)
Employed	0.40	0.42	0.03
			(0.02)
Household monthly income†	1478	1587	109
			(56)
Monthly rent†	850	871	21
			(10)
Back rent owed at application†	4500	3756	-744
			(182)
N	2,037	2,044	

Note: This table shows a regression of pre-payment eviction filing or judgment rates on an indicator for having below-median time between the date the case was created and payment. Columns 2 and 3 include controls for week of application payment and calendar time. Regressions restrict to the sample of people with all non-missing demographics, which is why the total N at the bottom is not the same as in Table 1. †: shows medians and differences from quantile regressions.

Table A22: Balance Table: Changes in Filing/Judgments Rates

	(1) Filing	(2) Judgment	(3) Filing	(4) Judgment
1(below med. time)	0.00320*	-0.000179	-0.00127	-0.00183
	(0.00166)	(0.00106)	(0.00222)	(0.00129)
Calendar week FE	Yes	Yes	Yes	Yes
Application and payment week FE	No	No	Yes	Yes
Case-period obs.	18968	18968	18968	18968
Cases	4742	4742	4742	4742

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: This table presents regressions of demographics on an indicator for having below-median time between the date the case was created and payment.

Table A23: ERAP Treatment Effects

	(1)	(2)	(3)
A. Treatment Effects (pp / 6 months)			
1. Average judgments	-0.85	-3.19	0.63
	(1.56)	(2.22)	(0.69)
	[0.585]	[0.150]	[0.362]
2. Average filings	0.20	-8.67	-6.13
	(2.32)	(3.21)	(1.57)
	[0.932]	[0.007]	[0.000]
3. 0–1 month filings	-5.71	-16.04	-8.48
	(1.30)	(2.11)	(1.82)
	[0.000]	[0.000]	[0.000]
B. Interpretation			
4. Maximum simulated effect: judgments	-4.71	-8.31	-4.77
5. Maximum simulated effect: filings	-11.13	-20.12	-16.44
6. Fiscal cost per judgment (point estimate)	644,310	172,412	.
7. Fiscal cost per filing (point estimate)	.	63,409	89,622
8. Fiscal cost per judgment (95% lower bound)	140,496	72,981	759,349
9. Fiscal cost per filing (95% lower bound)	126,527	36,750	59,621
N paid	4,742	4,742	4,742
N non-paid	.	.	4,905
Design:	Only-paid (non-parametric)	Only-paid, reweighted (non-parametric)	Non-paid (stacked)

Note: This table shows treatment effects, across empirical strategies, of ERAP payment on judgments and filings. Rows 1 and 2 show the cumulative effect over six months; that is, they multiply the average two-month effect by three. Row 3 shows the average effect in the first two months, multiplied by three; that is, it shows the effect in the same units as Rows 1–2. Rows 1 and 2 subtract the pre-period mean whereas Row 3 just shows the event-study coefficient for the first two-month period. The maximum simulated effect on judgments and filings (Rows 4–5) replace judgments or filings as 0 if in the treated group. To recover the treatment effects presented in the figures, divide estimates in Rows 1–2 by 3 (as those show per-period effects, and these show cumulative effects). Rows 6–9 present the fiscal cost of a deferred judgment or filing, based on the average payment made in the sample. Rows 6–9 are empty when the fiscal cost is infinite. Column 1 shows aggregates from the main specification (Equation 100). Column 2 shows reweighted estimates. Column 3 shows estimates from the alternative design with non-paid applicants (Equation 102). Parentheses show standard errors. Brackets show *p*-values.

Table A24: Efficient, Inefficient, and Repugnant Evictions

Panel A. Eviction	
Total eviction %	19.1
pp efficient	14.6
pp inefficient	4.6
pp from hostility	3.0
Panel B. Bargaining	
Total bargaining %	80.9
pp efficient	80.9
pp from altruism	13.5
pp inefficient	0.0

Note: This table shows the share of evictions that are efficient and inefficient. Inefficient evictions are those that would not occur without misperceptions. "Repugnant" evictions are those caused by hostility.

Table A25: Match to Moments

Moments	**Estimated Value**	**Targeted Value**
ERAP mean judgment rate (unconditional)	0.158	0.091
Treatment effect on judgments (unconditional)	-0.016	-0.009
Landlord take-up rate	0.668	0.648
Payment plan rate (proportion of back rent)	0.322	0.315
Interaction: a_{Li} and take-up	0.444	0.450
Interaction: p_{Li} and take-up	0.273	0.284
Interaction: $a_{Li} \times p_{Li}$ and take-up	0.214	0.219
Interaction: a_{Ti} and take-up	0.149	0.185
Interaction: $\tilde{p}_{Ti}$ and take-up	-0.057	-0.056
Interaction: $a_{Ti} \times \tilde{p}_{Ti}$ and take-up	-0.022	-0.031
Interaction: d_i and take-up	717	690
Mean: judgments (from tenants)	0.180	0.238
Interaction: a_{Ti} and judgment	0.058	0.110
Interaction: $\tilde{p}_{Ti}$ and judgment	-0.034	-0.045
Interaction: $a_{Ti} \times \tilde{p}_{Ti}$ and judgment	-0.008	-0.016
Interaction: d_i and judgment	404	526
Mean: judgments (from landlords)	0.180	0.161
Interaction: a_{Li} and judgment	0.096	0.064
Interaction: $\tilde{p}_{Li}$ and judgment	0.077	0.065
Interaction: $a_{Li} \times \tilde{p}_{Li}$ and judgment	0.045	0.040
Interaction: Takes_i and judgment	0.115	0.091
Interaction: $a_{Li} \times \text{Takes}_i$ and judgment	0.072	0.049
Interaction: $\tilde{p}_{Li} \times \text{Takes}_i$ and judgment	0.051	0.043
Interaction: $a_{Li} \times \tilde{p}_{Li} \times \text{Takes}_i$ and judgment	0.034	0.027

Notes: This table displays the match to the moments (see full list in Appendix B.1.2). The table displays unweighted values in their natural units. In estimation, all moments except the first two "macro moments" are scaled by the inverse standard deviation of residuals so units are comparable. Moments labeled with "Interaction" mean that we are displaying the mean value of the first variable interacted with the second variable.

Table A26: Section 6 Robustness

Change to quantitative model	(1) Efficient eviction rate	(2) Inefficient eviction rate	(3) Repugnant eviction rate	(4) % TOT if no L altruism
1. Primary estimate (no changes)	**14.6**	**4.6**	**3.0**	**-58.8**
2. ERAP TOT: 50% of judgments & filings	16.3	4.5	3.0	-66.0
3. Exclude landlord correlations	18.4	4.1	2.9	-58.1
4. Exclude tenant correlations	10.5	4.8	3.3	-51.6
5. More evictions: 25% judgment rate	16.3	4.6	3.0	-57.0
6. Different take-up proxy	13.6	6.2	3.2	-50.5
7. Different bargaining moment	14.3	4.6	3.1	-55.4
8. Costlier ERAP: k_L^P = \$1,000	13.1	6.5	3.3	-56.3
9. Cheaper ERAP: k_L^P = \$0	15.1	3.9	2.9	-55.9
10. Correlation: k_{Li} and p_{Li}	12.5	4.5	3.1	-61.1
11. Assume tenants have correct beliefs	16.0	2.6	1.6	-53.4
12. Use beliefs about the average	14.4	4.3	2.9	-55.3
13. Use identity weight for ERAP moments	16.8	4.6	3.0	-65.3

Notes: Rows 1 and 10 are estimated on the same 200 bootstraps as in the main analysis. Other rows are estimated on 51 bootstraps and involve re-estimating parameters. Columns 1–3 show the efficient, inefficient, and repugnant eviction rates ($\times$100), similar to Table A24. To compute the percent inefficient, divide Column 2 by Column 2 + Column 1. Column 4 shows the effect of ERAP on evictions if altruistic landlords are simulated not to be altruistic, similar to Row 4 of Table 4 (dividing Column 3 by Column 5 of that table). Row 2 of the Table A26 shows the effects if ERAP had a 50% TOT. Rows 3–4 show the effects if we exclude landlord or tenant correlations (Appendix B). Row 5 simulates the effects if evictions are more common, conditional on being in the sample. The purpose of this row is to examine the consequence of using depressed Covid-era judgment rates on our conclusions. Row 6 changes the take-up proxy to be 1 if and only if the landlord requested materials in the experiment. Row 7 changes the tenant bargaining moment to rely on an indicator for wanting a payment plan, rather than a continuous variable for the amount repaid (Appendix B). Rows 8–9 vary the ERAP cost parameter k_L^P. Row 10 permits correlation between k_{Li} and p_{Li} that roughly matches

Table A27: Associations between Hostility and 311 Calls

Panel A: Landlords

	(1) Has 311 call	(2) Has 311 call	(3) Has 311 call	(4) Has 311 call
Hostile	0.0522	0.160		
	(0.0864)	(0.0991)		
Highly hostile			-0.0131	0.0392
			(0.104)	(0.117)
Constant	0.353***	0.307***	0.361***	0.329***
	(0.0302)	(0.0379)	(0.0295)	(0.0372)
Observations	289	180	289	180
Sample	All	Own	All	Own

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel B: Tenants

	(1) Has 311 call	(2) Has 311 call	(3) Has 311 call	(4) Has 311 call
Hostile	0.0340	0.0285		
	(0.0303)	(0.0364)		
Highly hostile			0.113***	0.116**
			(0.0408)	(0.0484)
Constant	0.404***	0.407***	0.399***	0.400***
	(0.0142)	(0.0173)	(0.0132)	(0.0161)
Observations	1544	1049	1544	1049
Sample	All	Own	All	Own

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Panels A and B shows the relationships between hostility in the DG and matched 311 calls. The sample excludes landlords and tenants in Shelby County but not Memphis, since only Memphis provides public-access 311 data. “All” and “Own” refer to the samples against which the participant played the DG.

B Model Appendix

B.1 Quantitative Model Details (Appendix to Section 6)

B.1.1 Model Set-up

The landlord's payoffs V_{Li}^k are:

$$\begin{aligned} V_{Li}^{\text{NoERAP,Evict}} &= (1-a_{Li})p_{Li}d_i - a_{Li}k_T - k_L + \varepsilon_{i1} && (18)\\ V_{Li}^{\text{NoERAP,Bargain}} &= (1-a_{Li})n_i^*(d_i) + \varepsilon_{i2} && (19)\\ V_{Li}^{\text{ERAP,Evict}} &= d_i - k_L^P - a_{Li}k_T - k_L + \varepsilon_{i3} && (20)\\ V_{Li}^{\text{ERAP,Bargain}} &= d_i - k_L^P + (1-a_{Li})n_i^*(0) + \varepsilon_{i4}. && (21) \end{aligned}$$

Nash Bargaining yields the following solutions for bargaining payoffs (see proofs in Appendix B.1.4):

$$n_i^*(d_i, \varepsilon_{i2}, \varepsilon_{i1}; \beta) = \underbrace{\beta\left(p_{Li}d_i - \frac{k_L + k_{Ti}a_{Li} + \varepsilon_{i2} - \varepsilon_{i1}}{1-a_{Li}}\right)}_{\beta\,\times\,\text{altruism-adjusted outside option of landlord}} + \underbrace{(1-\beta)\left(p_{Ti}d_i + \frac{k_{Ti} + (k_L + \varepsilon_{i2} - \varepsilon_{i1})a_{Ti}}{1-a_{Ti}}\right)}_{-(1-\beta)\,\times\,\text{altruism-adjusted outside option of tenant}} \tag{22}$$

$$n_i^*(0, \varepsilon_{i4}, \varepsilon_{i3}; \beta) = \beta\left(0 - \frac{k_L + k_{Ti}a_{Li} + \varepsilon_{i4} - \varepsilon_{i3}}{1-a_{Li}}\right) + (1-\beta)\left(0 + \frac{k_{Ti} + (k_L + \varepsilon_{i4} - \varepsilon_{i3})a_{Ti}}{1-a_{Ti}}\right) \tag{23}$$

for tenant bargaining parameter β. The bargaining payoff $n_i^*(\cdot) \in \mathbb{R}$ represents a transfer from tenants to landlords when positive. We typically suppress dependence on the shocks and bargaining power for readability.

Let $\mathcal{E}^{\text{NoERAP}}$ be an indicator that is 1 if and only if

$$k_L + k_T + \varepsilon_{i2} - \varepsilon_{i1} \leq \frac{(p_{Li} - p_{Ti})d_i}{\bar{a}_i} \tag{24}$$

is satisfied. Let $\mathcal{E}^{\text{ERAP}}$ be an indicator that is 1 if and only if

$$k_L + k_T + \varepsilon_{i4} - \varepsilon_{i3} \leq 0 \tag{25}$$

is satisfied. Then the landlord's maximization problem is:

$$V_{Li} = \begin{cases} \max\left[V_{Li}^{\text{NoERAP,Evict}}, V_{Li}^{\text{ERAP,Evict}}\right] & \text{if } \mathcal{E}^{\text{NoERAP}} = 1 \text{ and } \mathcal{E}^{\text{ERAP}} = 1\\ \max\left[V_{Li}^{\text{NoERAP,Bargain}}, V_{Li}^{\text{ERAP,Evict}}\right] & \text{if } \mathcal{E}^{\text{NoERAP}} = 0 \text{ and } \mathcal{E}^{\text{ERAP}} = 1\\ \max\left[V_{Li}^{\text{NoERAP,Evict}}, V_{Li}^{\text{ERAP,Bargain}}\right] & \text{if } \mathcal{E}^{\text{NoERAP}} = 1 \text{ and } \mathcal{E}^{\text{ERAP}} = 0\\ \max\left[V_{Li}^{\text{NoERAP,Bargain}}, V_{Li}^{\text{ERAP,Bargain}}\right] & \text{if } \mathcal{E}^{\text{NoERAP}} = 0 \text{ and } \mathcal{E}^{\text{ERAP}} = 0. \end{cases} \tag{26}$$

Thus, based on the realization of the shocks, the landlord solves a maximization problem of whether to take up ERAP. She accounts for the fact that if she does or does not, the eviction decision is guaranteed.

Discussion of Model and the Memphis ERAP. The model abstracts from ERAP institutional details in several ways. First, if the landlord rejected ERAP, the tenant could still obtain a direct payment. The model assumes the landlord completely ignores this possibility. Second, the model assumes that ERAP pays just d_i but not additional months of rent. The months of rent were supposed to compensate the landlord for delays in receiving ERAP. Thus, we assume ERAP paid just d_i. Third, the model does not account for utilities payments or legal assistance that ERAP could provide.

B.1.2 Estimation

Method of Simulated Moments/Generalized Indirect Inference. We use Method of Simulated Moments (MSM). We draw a vector of shocks to form simulated moments, which we stack into $\hat{m}(X_i;\theta)$. We solve:

$$\hat{\theta} = \arg\min_{\theta\in\Theta} \left[\hat{m}(X_i;\theta)' W \hat{m}(X_i;\theta)\right] \tag{27}$$

for weight matrix W.

We use a Generalized Indirect Inference (GII) procedure to match moments (Bruins et al., 2018). MSM can often yield nonsmooth problems. Small changes in the parameter values can cause agents to choose a different bundle, and then simulating behavior given that choice will exhibit discrete differences. In our context, small changes in parameter values can generate "spiky" regions where slightly adjusting parameters induces large changes in conditional probabilities. A natural solution is to smooth the indicator into a probability so that the problem can be fed into standard optimization tools.[52] Bruins et al. (2018) provide a smoothing technique which is popular in problems of this form. We sketch our use of this algorithm below.

We use the following recipe:

1. Form data from the experiment Ξ_i.

2. Using the data from the experiment, form model-implied estimates of probabilities in each of the four end states.

3. Employ the Generalized Indirect Inference algorithm of Bruins et al. (2018):

   - Draw a value of type-I extreme value shocks ε_{ik} for landlord payoffs $k \in 1, \ldots, 4$.
   - Fix an initial smoothing value λ_0.
   - For each landlord payoff k, form smoothed simulated choice probabilities $\mathcal{T}_{ik}(\lambda_0) \in [0,1]$ as:
   $$\mathcal{T}_{ik}(\lambda_0) = \Phi\left(\lambda^{-1}\left(V_{ik}(X_i, \varepsilon_{ik}; \theta)\right)\right), \tag{28}$$
   for logistic CDF $\Phi(\cdot)$.
   - Stack simulated moments $\hat{m}(X_i;\theta)$ and solve:
   $$\hat{\theta}_0 = \arg\min_{\theta\in\Theta} \left\{\hat{m}(X_i;\theta)' W \hat{m}(X_i;\theta)\right\}. \tag{29}$$
   - Form $\lambda_j = \rho\lambda_{j-1}$ for $\rho \in (0,1)$. Repeat previous steps with λ_j until $\|\theta(\lambda_j) - \theta(\lambda_{j-1})\| <$ Tol for some norm $\|\cdot\|$.

[52] Global optimization tools did not converge reliably in our case, as the problem is sufficiently non-smooth.

The basic logic of GII in this setting is that, instead of simulating a discrete take-up choice based on model-implied utilities, let "as-if probabilities" $\mathcal{T}_i$ follow a logit structure over those utilities, and let the logit be characterized by scale parameter λ. These take-up probabilities are "as-if" because they are formed using the logit structure, but they do not have a direct model-based interpretation as probabilities since take-up is deterministic in the model. For a given $\lambda > 0$, the problem is now smooth. Then, iterate on λ until the model converges. As $\lambda \to 0$, $\mathcal{T}_i$ approaches an indicator.

In practice, to implement GII, we initialize at a coarse enough smoothing parameter such that probabilities are fairly uniform over the unit interval. We find an initial valuue using Matlab's `patternsearch` algorithm, which is designed for global optimization, to choose a candidate minimum value. We then iterate on the minimization using `patternsearch` and $\rho = 1/1.05$ until convergence.

We do not smooth the constraints (Equations 24–25) but tests indicate that doing so yields similar results.

Because all our parameter estimates use a matching procedure that involves randomness, we present the average value of the parameter across bootstraps in Table 3. Our counterfactuals estimate behavior using the value of θ obtained for the given bootstrap and then averages.

We form standard errors by bootstrapping the entire procedure, including matching the landlord–tenant pairs (below). We bootstrap the treatment effect moments and experimental data separately.[53] Standard errors represent the standard deviation of bootstraps.

Preparing the Experimental Data for Use in the Quantitative Model. The dataset includes the 903 tenants with positive back rents at the time of the tenant survey who played the DG against their own landlord, and the 199 landlords who played the DG against their own tenant.

We document how we prepare the variables that enter the estimation.

- Altruism a_{Li}, a_{Ti}. We read these directly off the DG experiments, putting $a_{ji} := (S(x) - x)$. To ensure that $\bar{a}_i$ is well-defined, we top-code $|a_{ji}|$ at 0.95.

- Landlord beliefs p_{Li}. We read this off the priors in the experiment questions about landlord recoupment. We use beliefs about own tenant, not average tenant.

- Tenant beliefs p_{Ti}. We do not observe tenant beliefs about paying back a judgment directly. We did not ask tenants whether they thought they could abscond in a court proceeding because we were concerned that this could harm landlords by making tenants more likely to abscond. We form a measure of tenant beliefs:

$$\tilde{p}_{Ti} := -\frac{1}{2}\left(\left(p'_{1i} - 0.31\right) + \left(0.69 - p'_{2i}\right)\right) + 0.06, \tag{30}$$

  where p'_{1i} and p'_{2i} are beliefs about own landlord's filing and altruism behaviors, respectively. The logic behind this measure is that if tenants have perfect beliefs about these forces, then we impute them as having perfect beliefs about repayment ($p_{Ti} = 0.06$). If tenants are pessimistic about landlords' altruism, we impute them as having optimistic beliefs (from their perspective) about whether they will pay back, and similarly if tenants are optimistic that landlords will drop a filing. We use beliefs about own landlords, not average landlords.

  Several assumptions are required for this imputation to be valid. First, we assume that beliefs scale linearly in percentage points across the measures. Second, our measure of $\tilde{p}_{Ti}$

[53] We do not bootstrap the mean judgment rates from the ERAP evaluation, so standard errors do not account for this (small) source of sampling variation.

may be negative. As noted in footnote 13, we can recast the p_{Li} and p_{Ti} values as scalar shifters of the expected value of reclaiming d_i if bargaining fails. For instance, if $p_{Ti} < 0$, tenants think they will get more money by going to court than by bargaining. Such a perspective is plausible if, for instance, they can reside in the unit while the court process is ongoing and they do not expect to pay a judgment.

As these are strong assumptions, we also conduct the exercise where we simulate $p_{Ti} = 0.06$ (Table A26, Row 11). This generally gives lower levels of inefficiency. We also conduct an exercise where we use beliefs about the average landlord or tenant, rather than own landlord or tenant and find similar results (Row 12).

- Back rent d_i. We use the tenant's value of back rents that they report in the experiment.

- Landlord take-up (ERAP_i). We estimate landlord take-up from the experiment as follows. Take-up is 1 if they indicate that they wish to receive a rental contract offer from ERAP. We then replace take-up equal to 0 if the landlord declines to receive materials about ERAP or if they decline to have ERAP notify their tenant when future opportunities to apply are available. The advantage of this measure is that it uses several of the take-up proxies. In a robustness check, we impute take-up as 1 if and only if the landlord chooses to receive ERAP materials (Table A26, Row 6). This moves the share of inefficient evictions to around 30% and share repugnant to around 50%.

- Judgments (Judgment_i). We use landlord and tenant judgments from links to court data (Section 5). As we randomly match landlords and tenants (see below), we have different values for whether the landlord links or tenant links to the court data are matched to a judgment (Judgment_{Li} and Judgment_{Ti}), but only one simulated value for a judgment based on the model.

- $n_i^*(d_i)$. We use the tenant's offer of how much back rent to pay back in a payment plan. Using these data requires the assumption that tenants' first offers exactly correspond to the Nash offer. We transform the data so that the variable n_i^*, used in moments below, is the fraction that the tenant offers to repay the landlord as a share of the total back rents that she owed. The variable n_i^* is censored between 0 and 1, since the payment plan activity did not let the tenant demand payment from the landlord or pay more than she owed. We accordingly also censor the model-implied bargaining offer.[54]

Other Data Preparation Details. For power, we do use experiment participants from both the legal and non-legal sides of the program and from people who moved after they applied to ERAP. We do not use participants who did DGs with random landlords or tenants.

We do not observe matched landlord–tenant pairs, since they participate in the experiments separately. As a benchmark, we draw landlord and tenant pairs to match at random. Given that we see moderate assortative matching on altruism, we simulate this assortative matching by replacing the landlord altruism as equal to the tenant altruism for 27.5% of pairs matched at random (Appendix E.2). Since there are more tenants than landlords, we resample landlords at random within the landlord sample.

Moments. Our moments are:

[54] There is slight abuse of notation when we write moments below: n_i^* and its predicted value correspond to the (censored) fraction offered to repay, not the continuous bargaining offer.

1. ERAP's treatment effect on judgments, where

$$\text{TOT} - \left(\mathbb{E}[\hat{\text{Judgment}}_{Li}|\text{ERAP}_{Li}] - \mathbb{E}[\hat{\text{Judgment}}_{Li}|\text{NoERAPExist}_{Li} \text{ and } \text{ERAP}_{Li}]\right) = 0. \quad (31)$$

Here TOT is the Treatment Effect on the Treated from Section 5.2, the first expectation is the mean judgment rate among landlords whom we simulate would take up, and the second expectation is the mean judgment rate if ERAP did not exist, among landlords whom we simulate would take up. We use the treatment effects on judgments under the nonparametric specification (Appendix E) and bootstrap the treatment effects.

2. The mean jugment rates among landlords who take up ERAP.

$$p - \mathbb{E}[\hat{\text{Judgment}}_{Li}|\text{ERAP}_{Li}] = 0, \quad (32)$$

where p comes from the ERAP data, and the second term come sfrom the experiment. We do not bootstrap the mean judgment rates.

3. The take-up rate among landlords, which we calibrate using data from the landlord survey.

$$\mathbb{E}\left[\text{ERAP}_{Li} - \hat{\text{ERAP}}_{Li}\right] = 0. \quad (33)$$

4. The bargaining offer made by tenants in the payment plans: outcome:

$$\mathbb{E}\left[n^*_{Li}(d_i) - \hat{N}^*_{Li}(d_i)\right] = 0. \quad (34)$$

5. Simulated moment conditions $E\left[X_i\left(Y_i - \hat{Y}_i\left(X_i, \varepsilon; \theta\right)\right)\right]$ where Y_i is an outcome and $\hat{Y}_i\left(X_i, \varepsilon; \theta\right)$ is a predicted outcome:

$$\mathbb{E}\left[a_{Li}\left(\text{ERAP}_{Li} - \hat{\text{ERAP}}_{Li}\right)\right] = 0 \quad (35)$$
$$\mathbb{E}\left[p_{Li}\left(\text{ERAP}_{Li} - \hat{\text{ERAP}}_{Li}\right)\right] = 0 \quad (36)$$
$$\mathbb{E}\left[a_{Li}p_{Li}\left(\text{ERAP}_{Li} - \hat{\text{ERAP}}_{Li}\right)\right] = 0 \quad (37)$$
$$\mathbb{E}\left[\text{Judgment}_{Li} - \hat{\text{Judgment}}_{i}\right] = 0 \quad (38)$$
$$\mathbb{E}\left[a_{Li}\left(\text{Judgment}_{Li} - \hat{\text{Judgment}}_{i}\right)\right] = 0 \quad (39)$$
$$\mathbb{E}\left[p_{Li}\left(\text{Judgment}_{Li} - \hat{\text{Judgment}}_{i}\right)\right] = 0 \quad (40)$$
$$\mathbb{E}\left[a_{Li}p_{Li}\left(\text{Judgment}_{Li} - \hat{\text{Judgment}}_{i}\right)\right] = 0 \quad (41)$$
$$\mathbb{E}\left[a_{Ti}\left(n^*_{Li}\left(d_i\right) - \hat{N}^*_i\left(d_i\right)\right)\right] = 0 \quad (42)$$
$$\mathbb{E}\left[\tilde{p}_{Ti}\left(n^*_{Li}\left(d_i\right) - \hat{N}^*_i\left(d_i\right)\right)\right] = 0 \quad (43)$$
$$\mathbb{E}\left[a_{Ti}\tilde{p}_{Ti}\left(n^*_{Li}\left(d_i\right) - \hat{N}^*_i\left(d_i\right)\right)\right] = 0 \quad (44)$$
$$\mathbb{E}\left[d_i\left(n^*_{Li}\left(d_i\right) - \hat{N}^*_i\left(d_i\right)\right)\right] = 0 \quad (45)$$
$$\mathbb{E}\left[\text{ERAP}_{Li}\left(\text{Judgment}_{Li} - \hat{\text{Judgment}}_{i}\right)\right] = 0 \quad (46)$$
$$\mathbb{E}\left[a_{Li}\text{ERAP}_{Li}\left(\text{Judgment}_{Li} - \hat{\text{Judgment}}_{i}\right)\right] = 0 \quad (47)$$
$$\mathbb{E}\left[p_{Li}\text{ERAP}_{Li}\left(\text{Judgment}_{Li} - \hat{\text{Judgment}}_{i}\right)\right] = 0 \quad (48)$$

$$\mathbb{E}\left[a_{Li}p_{Li}\text{ERAP}_{Li}\left(\text{Judgment}_{Li} - \hat{\text{Judgment}}_i\right)\right] = 0 \tag{49}$$

$$\mathbb{E}\left[\text{Judgment}_{Ti} - \hat{\text{Judgment}}_i\right] = 0 \tag{50}$$

$$\mathbb{E}\left[a_{Ti}\left(\text{Judgment}_{Ti} - \hat{\text{Judgment}}_i\right)\right] = 0 \tag{51}$$

$$\mathbb{E}\left[\tilde{p}_{Ti}\left(\text{Judgment}_{Ti} - \hat{\text{Judgment}}_i\right)\right] = 0 \tag{52}$$

$$\mathbb{E}\left[a_{Ti}\tilde{p}_{Ti}\left(\text{Judgment}_{Ti} - \hat{\text{Judgment}}_i\right)\right] = 0 \tag{53}$$

$$\mathbb{E}\left[d_i\left(\text{Judgment}_{Ti} - \hat{\text{Judgment}}_{Li}\right)\right] = 0. \tag{54}$$

We choose these moment conditions for the following reasons. We randomly match landlord and tenant pairs. As a result, we only include in X_i the variables that we observe in the experiment that provides the outcome Y_i. For instance, consider the take-up moments. We only include landlord values in X_i because the take-up moments come from the landlord take-up choice. As tenants are matched randomly, we should not use values for tenant variables in X_i. We also include the interactions of altruism and beliefs because Section 1 suggests this interaction is important.

For the judgment moments, notice that we have different observed judgments based on whether the landlord or the tenant receives a judgment in the court data. We use the landlord-side demographics with the landlord-side observed judgment, and vice-versa for tenants.[55]

We weight the "micro" moments by the inverse of their variance. In order to precisely match the "macro" moments (Equations 31 and 32), we weight the treatment effect by 10 and the mean by 2. We use these heuristic weights because the treatment effect is naturally estimated less precisely than the means from the experimental data, but it is valuable to match precisely. Results are highly similar if we weight these moments by the identity matrix (Table A26, Row 13).

Welfare Impact Calculation. The welfare impact is defined as follows:

$$W_i = \begin{cases} (1+a_{Ti})\left(\varepsilon_{i3} - \varepsilon_{i1} - k_L^P\right) & \text{if evicts with ERAP, evicts without ERAP} \\ (1+a_{Ti})\left(\varepsilon_{i4} - \varepsilon_{i2} - k_L^P\right) & \text{if bargains with ERAP, bargains without ERAP} \\ (1+a_{Ti})\left(\varepsilon_{i4} - \varepsilon_{i1} - k_L^P\right) + (1+a_{Ti})k_L + (1+a_{Li})k_T & \text{if bargains with ERAP, evicts without ERAP} \\ (1+a_{Ti})\left(\varepsilon_{i3} - \varepsilon_{i2} - k_L^P\right) - (1+a_{Ti})k_L - (1+a_{Li})k_T & \text{if evicts with ERAP, bargains without ERAP.} \end{cases} \tag{55}$$

The version that does not normatively respect altruism is identical but sets the altruism parameters equal to zero. Notice that the welfare impact excludes any intrahousehold transfer or the direct transfer (which comes at a fiscal cost of \$1 for each dollar transferred). We do not scale the fiscal transfer by tenant altruism in either calculation.

[55] Note that Equations (38) and (50) will have the solver choose to interpolate between the two intercepts, as we have two values in the data for whether a match gets a judgment.

B.1.3 Extension: Two-Period Model, Concave Utility, Nash Bargaining

We extend the model to add dynamic elements as in Dávila (2020) (in the setting of dischargeable bankruptcy), as well as more flexible utility and Nash Bargaining.

In period 0, the tenant decides whether or how much rent to pay of the balance d_0. In period 1, she either bargains over the remaining balance or is evicted. There is a random endowment shock in period 1 that is unknown to the tenant in period 0. Eviction takes place if and only if it is more desirable than bargaining for both landlords and tenants. The fundamental tradeoff is between consumption-smoothing motives and the fact that choosing to pay little rent today can make eviction or bargaining more likely tomorrow, depending on the realization of the state.

Tenants have well-behaved, strictly increasing and concave utility functions $u(\cdot)$. Landlords are risk-neutral. Tenants and landlords have altruism parameters $a_T, a_L \in (-1, 1)$ which scale payoffs and have the same interpretation as in the main text. We suppress the relationship-specific heterogeneity i.

Tenants' period-0 flow utility is:

$$u(c_0) := u(n_0 - d(1 - a_T)), \tag{56}$$

where c_0 is total consumption, n_0 is an exogenous period-0 endowment, and $d \in [0, d_0]$ is the amount of rent that the tenant chooses to pay of the total balance owed d_0. Notice that we put altruism as scaling consumption *within* the utility function.

In period 1, tenants get an exogenous and random period-1 endowment $s \sim F$, where the endowment s is weakly increasing in the state s and F has bounded or unbounded support $[\underline{s}, \bar{s}]$. If $s \geq d_0 - d$, tenants can then pay the full remaining balance and consume

$$c_1 = s - (d_0 - d)(1 - a_T). \tag{57}$$

Tenants are either evicted or bargain in period 1. Eviction or bargaining takes place over the residual amount that the tenant has not paid, $d_0 - d$.

In an eviction (i.e., an eviction judgment), tenants consume $j_T(x)$ and landlords consume $j_L(x)$, where we often explicitly notate these payoffs as depending on $x \in [0, d_0 - d]$:

$$u(s - j_T(x)) := u(s - \underbrace{(p_T x(1 - a_T) + k_T + (1 - a_T)k_L)}_{:= j_T(x)}) \tag{58}$$

$$j_L(x) := p_L x(1 - a_L) - k_L - (1 - a_L)k_T, \tag{59}$$

where p_T and p_L denote tenant and landlord beliefs as in the main text.

If landlords and tenants bargain, tenants pay landlords the asymmetric Nash solution $b^*(x)$. The solution arises from maximizing the following problem:

$$b^*(x) := \arg\max_{b(x)} \Big(u(s - (1 - a_T)b(x)) - u(s - j_T(x)) \Big)^{\beta} \Big(b(x)(1 - a_L) - j_L(x) \Big)^{1-\beta}, \tag{60}$$

for tenant bargaining power β. Taking the first-order condition and rearranging, the solution to this problem is implicitly defined by:

$$b^*(x) = \frac{j_L}{1 - a_L} + \left(\frac{1 - \beta}{\beta} \right) \left(\frac{u(s - (1 - a_T)b^*(x)) - u(s - j_T)}{u'(s - (1 - a_T)b^*(x))} \right), \tag{61}$$

which nests the solution in Equation (22) if tenant preferences are linear.

Bargaining is therefore possible if and only if:

$$(1-a_L)b^*(x) \geq j_L(x) \tag{62}$$
$$u(s-b^*(x)(1-a_T)) \geq u(s-j_T(x)), \tag{63}$$

which implies that bargaining occurs if and only if:

$$\frac{\Delta p x}{\bar{a}} \leq k_L + k_T, \tag{64}$$

where $\Delta p := p_L - p_T$ just as in the main text. This is exactly the same condition as the main text, therefore recovering Proposition 3. We write $\mathcal{B}(x) = 1$ if $\Delta p x/\bar{a} < k_L + k_T$ and $\mathcal{B}(x) = 0$ otherwise.

For discount factor δ, the tenant's ex-ante utility function is:

$$\begin{aligned} U(d) = u(n_0 - d(1-a_T)) + \delta\Bigg(&\int_{\underline{s}}^{d_0-d} u(s-b^*(s)(1-a_T))\mathcal{B}(s)dF + \int_{\underline{s}}^{d_0-d} u(s-j_T(s))(1-\mathcal{B}(s))dF + \\ &\int_{d_0-d}^{\bar{s}} u(s-b^*(d_0-d)(1-a_T))\mathcal{B}(d_0-d)dF + \int_{d_0-d}^{\bar{s}} u(s-j_T(d_0-d)))(1-\mathcal{B}(d_0-d))dF\Bigg). \end{aligned} \tag{65}$$

Solution. We now characterize the tenant's solution. Define $\hat{d} = d_0 - \frac{\bar{a}}{\Delta p}(k_L + k_T)$. Observe that a monotonicity condition holds, in that

$$\mathcal{B}(x) = 1 \implies \mathcal{B}(y) = 1 \text{ for all } y < x. \tag{66}$$

Thus, $\mathcal{B}(d_0 - d) = 1 \implies \mathcal{B}(s) = 1$ for all $s < d_0 - d$. This allows us to simplify the problem into only a few cases:

Case 1: The optimal $d^* \geq \hat{d}$. In this case, the tenant always bargains, no matter the draw of the state. Then, she solves the problem of maximizing the function:

$$U(d) = u(n_0 - d(1-a_T)) + \delta\left(\int_{\underline{s}}^{d_0-d} u(s-b^*(s)(1-a_T))dF + \int_{d_0-d}^{\bar{s}} u(s-b^*(d_0-d)(1-a_T))dF\right). \tag{67}$$

The solution d_1, if it exists, is implicitly characterized by the Euler Equation:

$$u'(n_0 - d_1(1-a_T)) = \delta\int_{d_0-d_1}^{\bar{s}} b'^*(d_0-d_1)u'(s-b^*(d_0-d_1)(1-a_T))dF, \tag{68}$$

noting that the $(1-a_T)$ terms cancel from both sides.

Case 2: The optimal $d^* < \hat{d}$. In this case, the tenant gets a judgment for a sufficiently high draw of the state. Then, we use the condition that

$$\mathcal{B}(s) = 1 \iff s \leq \frac{\bar{a}}{\Delta p}(k_L + k_T). \tag{69}$$

Therefore, the tenant solves the problem of maximizing the function:

$$U(d) = u(n_0 - d(1-a_T)) + \delta\Bigg(\int_{\underline{s}}^{\frac{\bar{a}}{\Delta p}(k_L+k_T)} u(s - b^*(s)(1-a_T))dF + \int_{\frac{\bar{a}}{\Delta p}(k_L+k_T)}^{d_0-d} u(s - j_T(s))dF + \int_{d_0-d}^{\bar{s}} u(s - j_T(d_0-d))dF \Bigg). \tag{70}$$

This solution d_2, if it is exists, is implicitly characterized by the Euler Equation:

$$u'(n_0 - d_2(1-a_T)) = p_T\delta \int_{d_0-d_2}^{\bar{s}} u'(s - j_T(d_0-d_2))dF, \tag{71}$$

noting that $j_T' = p_T(1-a_T)$.

There are also potential corner solutions at $d=0, d=d'$ or $d=d_0$. To solve the problem, the tenant checks which of $d^* \in \{0, d_0, d', d_1, d_2\}$ maximizes her utility $U(d)$.

Discussion. The tenant has several choices. First, she could pay all her rent ($d=d_0$). Second, she could pay less than all her rent but enough to guarantee she will not be evicted ($d=d_1$ or $d=d'$). Third, she could pay less than all her rent but pay enough that she has a chance of not being evicted if she does not have enough money in the next period that eviction is unprofitable for the landlord ($d=d_2$). Last, she could pay nothing ($d=0$).

The main set-up in the text is nested in the above problem when the tenant chooses $d^*=0$ and strictly prefers this choice to an interior d^*. Thus, we recover the model in the main text if $\delta \to 0$ or $n_0 \to -\infty$, as then the tenant consumes her full endowment in period 0 and whether she bargains is based only on the realization of the state s.

Notice that concave tenant utility does not change the fundamental bargaining solution in Equation (64). In fact, permitting concave landlord utility over both court costs and bargaining would also yield the same solution. Of course, the equation is not completely generic, as putting court costs *outside* the concave utility function can change it, but at a minimum the parametric expression for this threshold is not an artifact of linearity alone.

B.1.4 Proofs

Proof of Proposition 3. We prove Proposition 3, from which Propositions 1 and 2 follow as special cases in which $a_{Li} = a_{Ti} = 0$ and $\Delta p_i = 0$. For an offer to be made and accepted, it must satisfy both the tenant and landlord's participation constraints:

$$-(1-a_{Ti})o_i \geq -p_{Ti}d_i(1-a_{Ti}) - k_{Ti} - k_{Li}a_{Ti} \tag{Tenant constraint}$$
$$(1-a_{Li})o_i \geq p_{Li}d_i(1-a_{Li}) - k_{Li} - k_{Ti}a_{Li}. \tag{Landlord constraint}$$

Such an offer o_i exists if and only if:

$$p_{Li}d_i - \frac{k_{Li} - k_{Ti}a_{Li}}{1-a_{Li}} \leq p_{Ti}d_i + \frac{k_{Ti}+k_{Li}a_{Ti}}{1-a_{Ti}}. \tag{72}$$
$$\iff \frac{k_{Li}+k_{Ti}a_{Li}}{1-a_{Li}} + \frac{k_{Ti}+k_{Li}a_{Ti}}{1-a_{Ti}} \geq \Delta p_i d_i \tag{73}$$
$$\iff (k_{Li}+k_{Ti}a_{Li})(1-a_{Ti}) + (k_{Ti}+k_{Li}a_{Ti})(1-a_{Li}) \geq \Delta p_i d_i (1-a_{Ti})(1-a_{Li}) \tag{74}$$

$$\iff (k_{Li} + k_{Ti})(1 - a_{Li}a_{Ti}) \geq \Delta p_i d_i (1 - a_{Ti})(1 - a_{Li}) \tag{75}$$

$$\iff k_{Li} + k_{Ti} \geq \frac{\Delta p_i d_i}{\bar{a}_i} \tag{76}$$

meaning that eviction occurs if and only if

$$k_{Li} + k_{Ti} < \frac{\Delta p_i d_i}{\bar{a}_i}, \tag{77}$$

as desired.

Proof of Proposition 4. The landlord's ERAP participation constraint is

$$d_i - k_{Li}^P \geq \max\left\{(1 - a_{Li})o_i, V_{Li}^E\right\} \tag{78}$$

where o_i is the settlement offer they expect to receive from the tenant, which will leave them indifferent between informal settlement and formal eviction, and V_{Li}^E is the payoff from eviction. This is

$$d_i - k_{Li}^P \geq (1 - a_{Li}) \left[p_{Li}d_i - \frac{k_{Li} + a_{Li}k_{Ti}}{1 - a_{Li}}\right]. \tag{79}$$

Dividing through by $(1 - a_{Li})$ yields

$$p_{Li}d_i \leq \frac{k_{Li} + a_{Li}k_{Ti}}{1 - a_{Li}} + \frac{1}{1 - a_{Li}} \left[d_i - k_{Li}^P\right]. \tag{80}$$

We then subtract $p_{Ti}d_i$ from both sides:

$$(p_{Li} - p_{Ti})d_i \leq \frac{k_{Li} + a_{Li}k_{Ti}}{1 - a_{Li}} + \frac{1}{1 - a_{Li}} \left[(1 - p_{Ti}(1 - a_{Li}))d_i - k_{Li}^P\right]. \tag{81}$$

Then simplify terms, and also add and subtract the tenant-side cost expressions from the right-hand side:

$$\Delta p_i d_i \leq \bar{a}_i(k_{Li} + k_{Ti}) - \frac{k_{Ti} + a_{Ti}k_{Li}}{1 - a_{Ti}} + \frac{1}{1 - a_{Li}} \left[(1 - p_{Ti}(1 - a_{Li}))d_i - k_{Li}^P\right]. \tag{82}$$

Rearranging, we obtain:

$$\Delta p_i d_i \leq \bar{a}_i(k_{Li} + k_{Ti}) + \frac{1}{1 - a_{Li}} \left[(1 - p_{Ti}(1 - a_{Li}))d_i - \frac{1 - a_{Li}}{1 - a_{Ti}}(k_{Ti} + a_{Ti}k_{Li}) - k_{Li}^P\right]. \tag{83}$$

Divide through by $\bar{a}_i$ to get that

$$\frac{\Delta p_i d_i}{\bar{a}_i} \leq k_{Li} + k_{Ti} + \frac{1}{\bar{a}_i(1 - a_{Li})} \left[(1 - p_{Ti}(1 - a_{Li}))d_i - \frac{1 - a_{Li}}{(1 - a_{Ti})}(k_{Ti} + a_{Ti}k_{Li}) - k_{Li}^P\right]. \tag{84}$$

Then the landlord takes up if and only if:

$$k_{Li} + k_{Ti} + w_i \geq \frac{\Delta p_i d_i}{\bar{a}_i} \tag{85}$$

where

$$w_i = \frac{1}{\bar{a}_i}\left(\underbrace{\frac{d_i - k^P_{Li}}{1-a_{Li}}}_{\substack{\text{Altruism-adjusted} \\ \text{net ERAP payment}}} - \underbrace{\left(p_{Ti}d_i + \frac{k_{Ti} + a_{Ti}k_{Li}}{1-a_{Ti}}\right)}_{\text{Outside option adjustment}}\right). \tag{86}$$

Proof of Equations (22) and (23). Suppress dependence on i. Given altruism, Nash bargaining solves:

$$n^* = \arg\max_n \left(-\left(1-a_T\right)n - \mu_T\right)^{\beta}\left(\left(1-a_L\right)n - \mu_L\right)^{(1-\beta)} \tag{87}$$

where μ_L and μ_T represent the parties' outside options inclusive of their own altruism. Then taking logs, the first-order condition is:

$$-\frac{\beta(1-a_T)}{-(1-a_T)n^* - \mu_T} + \frac{(1-\beta)(1-a_L)}{(1-a_L)\,n^* - \mu_L} = 0. \tag{88}$$

From here, rearrangement gives:

$$n^* = \frac{\beta\mu_L}{1-a_L} - \frac{\mu_T(1-\beta)}{1-a_T}. \tag{89}$$

To recover the equations, note that if the landlord does not take up,

$$\mu_L \equiv p_L(1-a_L)d_i - k_L + k_{Ti}a_{Li} + \varepsilon_{i2} - \varepsilon_{i1}, \tag{90}$$

and

$$\mu_T \equiv -p_T(1-a_T)d_i - \left(k_{Ti} + (k_L + \varepsilon_{i2} - \varepsilon_{i1})a_{Ti}\right), \tag{91}$$

and make a similar substitution if the landlord does take up.

C Experiment Details

C.1 Recruitment Details

Landlord Survey. We limit the sample to landlords with valid own and tenant contact information. We asked each landlord participant questions about her tenant, whose name we link from the tenant application, selecting a reference tenant at random when a landlord is linked to multiple tenant applicants.

Tenant Survey. Because tenants may have moved between applying for ERAP and taking the survey, we ask tenants for information about their current landlord, which we use in the survey elicitations involving their landlord. We did not limit tenants to people who were paid. We also require valid own and landlord contact information. We include any tenants who applied or started applying before February 13, 2022.

Both Surveys. We contacted experiment participants with a Memphis/Shelby County ERAP email address and logo, conferring legitimacy to our outreach, in addition to an MIT logo and disclosure of our institutional affiliation.

C.2 Design Details

Multiple Price List for Dictator Game. We elicit indifference points S using a multiple price list-style elicitation and the strategy method. For each participant, we ask whether she prefers Bundle $A = (\$0.9x, \$0)$ to $B = (\$x, \$x)$. If she prefers B to A, we present another Bundle $A' = (\$1.5x, \$0)$ and ask her preferences over A' versus B. We vary the value of Bundle A to be as large as $(\$2x, \$0)$. We repeat a version of this elicitation until we obtain her switching point. We adopt this method because it is easier for subjects to understand than asking about their indifference point directly. We assume that if a participant prefers A to B for a given s, she will also prefer A to B for $s' > s$; our multiple price list elicitation uses a binary search-type technique to ask about progressively narrower choices between Bundles A and B.[56]

Multiple Survey Completions. Several individuals take the experiment multiple times, as indicated by the name they report in the experiment (and for tenants, other information such as phone number or address). We drop the second instance. There is no incentive to lie about one's name on the survey since participants were paid even if they took the survey twice.

Information About Own Tenant Or Own Landlord. For the landlord survey, we use information about the tenant applicant associated to ERAP to automatically populate throughout the survey. In the tenant survey, their current landlord may differ from the landlord they applied to ERAP with. For instance, they may have applied in April 2021 and be taking the survey in May 2022, so they may have moved in the interim. Early in the survey, we ask the tenant for their landlord's name and populate the response in the subsequent elicitations.

When landlords play against random, unnamed tenants, we indicate that the tenant recipient be a tenant of another landlord participant, and similarly for tenants playing against random, unnamed landlords.

[56]Because we only allow A to take whole-dollar values, we obtain upper and lower bounds on S_ℓ, and these bounds have a width of $\$0.1x$. Where appropriate, we assume that the indifference point lies halfway between the bounds. For instance, if a landlord participant prefers B to A if $s = 12$ but A to B if $s = 13$, we assume $S_\ell = 12.5$.

C.3 Text for Belief Elicitations

C.3.1 Landlords

The information provision was:

> *Of all monetary eviction judgments rendered in Shelby County Courts in January 2020, about 6 out of 100 cases had fully repaid their balances by the beginning of August 2021.*

To elicit beliefs about the average, we ask landlords:

> *Consider monetary evictions judgments given in January 2020 in Shelby County courts. January 2020 was before the coronavirus pandemic in the U.S.*
>
> *Out of every 100 monetary judgments in January 2020, how many tenants had fully repaid the balances they owed by the beginning of August 2021?*

To elicit beliefs about their own tenant, we ask landlords:

> *Imagine the courts gave you a monetary eviction judgment for [TenantName] today.*
>
> *We are asking you to make a prediction about what would happen in this scenario.*
>
> *What do you think is the percent chance that [TenantName] would repay the judgment to you, in full, by May 2023?*

We elicit prior beliefs after conducting the Dictator Game. We reject meaningful priming or order effects: we regress prior beliefs about the landlord's own and average tenant repayment on whether landlords play the DG against their own or a random tenant and find no association ($p = 0.91$ for own and $p = 0.93$ for average).

C.4 Incentives

C.4.1 Landlord survey

Fixed Payments. All payments were made in the form of Amazon gift cards send to participants' emails. Survey participants who complete the survey were paid \$20. One participant was randomized to win a bonus of \$500, which we advertised to increase participation.

Beliefs. We incentivized two belief elicitations:

1. Prior beliefs about recoupment probability of average tenants. We paid according to the quasi-quadratic function:

$$\text{belief bonus} = \max(0, 22 - 22 \times (\text{truth} - \text{response})/44)^2) + 3, \tag{92}$$

   rounded to the nearest dollar, where LSC data indicate the truth was 6.

2. Prior beliefs about court delays. We paid according to the quasi-quadratic function:

$$\text{belief bonus} = \max(0, 22 - 22 \times (\text{truth} - \text{response})/3500)^2) + 3, \tag{93}$$

   rounded to the nearest dollar, where LSC data indicate the truth was 54 cases.

We randomize 20% of participants to be paid for one belief. We choose which belief at random with probability 0.5.

We informed participants that they would maximize their payment if they reported beliefs that were closer to the truth, and that there was no incentive to distort their beliefs. Participants had the option to observe the formulas but did not see it directly unless they selected that they want to see them.

Dictator Game. We implement an incentivized multiple price list as follows. 5 landlords were chosen to have the Becker-Degroot-Marschak (BDM) mechanism implemented for their game when they played against a tenant. Whether the tenant opponent was the landlord's own tenant or random of another landlord who completed the study was randomized separately and displayed to the landlord. 5 landlords were chosen to have the BDM mechanism implemented for their game when they played against a landlord. We imposed monotonicity and used a multiple price list to elicit the participant's switching point between (\$10, \$10) and (\$x, \$0). The maximum value of \$x displayed was \$20. The minimum value was \$1. Participants could only record preferences in \$1 increments. Only one question was displayed at once. We then conducted a random draw across the choices {(\$10, \$10) versus (\$1, \$0); (\$10, \$10) versus (\$2, \$0); . . . , (\$10, \$10) versus (\$20, \$0)} with equal probability and implement the landlord's (implied) choice for that bundle. This mechanism preserves incentive compatability. We then send gift cards based on the choices to either the tenant or a random landlord in the survey.

Lottery Elicitation. In Section C.5, we describe a lottery elicitation. Landlords can choose to enroll another person in a lottery for \$10. If the landlord chooses to enroll their tenant in the lottery, we draw with probability 0.01 whether their tenant receives the gift card. If the landlord chooses to enroll a random other landlord in the lottery, we draw with probability 0.01 whether the random landlord receives the gift card.

C.4.2 Tenant survey

Payments were similar to the landlord survey.

Overview and Fixed Payments. Tenants were paid \$20 for completing the study. They choose either a Starbucks gift card or an Amazon gift card for all payments.

Beliefs. Belief payments were implemented with the same probabilities as the landlord survey and using a similar quasi-quadratic formula. The beliefs we incentivized were:

1. Prior beliefs about the percent chance of landlord settlement:

$$\text{belief bonus} = \max(0, 22 - 22 \times (\text{truth} - \text{response})/44)^2) + 3, \tag{94}$$

   rounded to the nearest dollar, where the truth was 31.

2. Prior beliefs about the percent chance of landlords' being highly altruistic:

$$\text{belief bonus} = \max(0, 22 - 22 \times (\text{truth} - \text{response})/44)^2) + 3, \tag{95}$$

   rounded to the nearest dollar, where the truth was 69.

Dictator Game. We employ the same structure as with landlords. The main departure is that some tenants were randomized into larger stakes. As with landlords, we obtain their switching point using a multiple price list and employ a BDM mechanism with equal probability across 20 possible questions.

Lottery Elicitation. Payments were exactly analogous to the landlord survey, but tenants could enroll their own landlord or a random landlord.

C.4.3 Additional Details about Outcomes

Tenants. We did not send payment plans to landlords whose tenants indicated that they did not know their landlords' email address. About 20 percent of the payment plan messages bounced.

C.5 Validation of Preference Measures

We included several additional tests to validate that behavior in Dictator Games reflects true preferences.

Attention and Confusion. We consider the following measures of tenant affect toward their own landlord:

1. *Free-Response Sentiment.* Before the DG, we ask tenants to share open-ended reflections about their landlord. We analyzed these free responses more systematically using VADER, a sentiment classifier from the natural-language processing literature (Hutto and Gilbert, 2014).[57] The classifier gives an aggregate sentiment score for each response.

2. *Likert Scale.* We asked tenants to subjectively rate their relationship with their landlord.

3. *Simple Lottery.* We conducted a simple real-stakes lottery elicitation as a secondary measure of altruism. We ask the landlord whether they wish to enroll a random other landlord and their own tenant in a lottery for $20 with probability 0.01. In the tenant experiment, we likewise ask whether to enroll her own landlord and a random tenant in the lottery. We tell participants that their choice will be kept private. We also tell participants that draws are made at random, so that enrolling others does not influence their own chances of winning or that of other entrants. Finally, we stress that choices will be anonymous.

   The idea behind this task is that it is free for the participant to enroll both the landlord and tenant in the lottery. It also shuts down instrumental preferences. They will do so if they have any altruism toward the other person.

   24% of tenants decline to enroll their landlord in the lottery. 18% of landlords decline to enroll their tenant. These rates are similar to the observed hostility in the DG.

   The benefit of this outcome is that it is a real-money choice that is perhaps simpler to understand than the DG.

Following Kling et al. (2007), we combine these measures via an index that is the average of the standardized measures. Panel A of Table A13 shows the index is highly correlated with tenant behavior in the DG toward their own landlord. A one-standard-deviation increase in the index is correlated with a 16-percentage-point decrease in hostility toward own landlords (column 1, $p = 0.00$). The correlation is similar with behavior towards random landlords (column 2) and

[57] VADER has been pre-trained on social media data and is designed to handle standard challenges with sentiment analysis like negation and slang.

markedly attenuated for behavior towards random tenants (column 3). The correlations are symmetric for high altruism (columns 3–6).

It is reassuring that this index of affect toward own landlords are most correlated with behavior toward own landlords and random landlords, but less so with random tenants. It implies that attitudes toward own landlords manifest most strongly in behaviors toward own landlords in the DG.

Panels B, C, and D of Table A13 respectively disaggregate the index into the free response, Likert scale, and simple lottery.

The lottery results are especially informative because these are additional revealed outcomes, where we stressed anonymity, and which were simple to understand. Enrolling one's own landlord in the simple lottery is correlated with a 21.8 pp reduction in hostility (more than 50% of the control mean). We also implemented the simple lottery with landlords to rule out their inattention or confusion and find symmetric results as with tenants (Table A14).

We further explore the lottery results and draw three conclusions (Figure A14). First, the rates of landlords and tenants who reject enrolling their own tenant or landlord in the lottery are reassuringly similar to the levels of hostility in the DGs. For instance, 24% of tenants reject entering their own landlord in a lottery to win money. Second, landlords' rates of rejecting enrolling their own tenant exceed the rates at which they enroll a random landlord. Meanwhile, tenants' rates of rejecting enrolling their own landlord exceed the rates at which they reject enrolling their own tenant. These differences by opponent are similar to the DG. Third, these behaviors are highly correlated with DG behaviors. That is, hostile participants are more likely not to enroll their own tenant or landlord. The increase in rejection among hostile participants is more concentrated against the participant's own landlord or tenant than against a random opponent.

Failing to enroll one's own landlord or tenant in the lottery is further evidence of hostility. It is costless to enroll one's landlord or tenant in the lottery. Participants with no social preferences would be indifferent between enrolling versus not enrolling. Yet landlords and tenants are not indifferent on average, as they exhibit different behaviors when enrolling tenants versus landlords. If they were indifferent, the opponent would not matter. Such behavior is more consistent with hostility that is directed toward particular opponents.

Global Preferences Survey. Another concern is that behavior in the DG will not extrapolate outside the lab. To rule this out, we show that behavior in the DG is correlated with externally validated survey measures of behavioral primitives. We use questions from the Global Preferences Survey (Falk et al., 2018, Forthcoming) to measure altruism, positive or negative reciprocity, and preferences for punishment. Figure A13 shows that negative reciprocity is correlated with a lower tenant point toward random landlords but not toward random tenants. Preferences for punishment are also correlated with a lower indifference point.

C.5.1 Anonymity and Instrumental Motives

An important concern is that behavior in the DG may reflect instrumental motives (e.g., reputation). On the one hand, if landlords or tenants burn surplus because of a reputation game, that is perhaps still interpretable as inefficient. On the other hand, it is not clear that such behavior would reflect a social preference.

We try to set up the DG so that parties would be anonymous. If the party is anonymous, there is no instrumental reason to make a choice in the DG. For both parties, we stress anonymity in the consent process, in the context of data storage and data sharing. For tenants, we stress anonymity with respect to landlords at the time of the game, and added an additional confirmation check.[58] 88% get the question right. For the 12% who get it wrong, we correct the answer. For the remaining 88%, we reiterate anonymity. We did not include these extra checks or reminders about anonymity for landlords.

As further evidence that these concerns are not decisive, the above lottery measure of preferences explicitly emphasizes anonymity in both surveys. We write: "Your response will be kept private from your tenant" or "Your response will be kept private from your landlord." The shares of landlords who do not enroll their own tenant, and tenants who do not enroll their own landlord, are very similar to the observed hostility. Moreover, behaviors in the DG are highly correlated with behaviors in the lottery (see discussion of Table A13, Table A14, and Figure A14 above). Even if one is skeptical of the DG evidence, the lottery data provide useful complementary evidence that cannot be explained with perceptions about anonymity.

We believe that the most relevant concern is that a share of landlords may express *altruism* for instrumental reasons. Recall that hostility simply means that the other party does not get a gift card. It does not take money away from them. Indeed, if the landlord wants to claim money from the tenant as rent, burning money means they cannot claim it. If true, this would mean that our main results about hostility are lower bounds.

C.6 Attention

General Attention Checks. In the landlord experiment, we included one general attention check: we ask participants what is their favorite color, but tell them to report that their favorite color is teal. We pre-registered in AEARCTR-0008053 that this attention check would not be dispositive because it could be too hard. 64% of landlords pass this check. In the tenant experiment, we also used a second attention check where we ask participants to report that their favorite number is 6. 92% of tenants pass the "6" attention check but 34% of tenants pass the "teal" check, giving further evidence that the "teal" check was too hard. Our primary findings do not condition on passing the general checks, following our pre-registrations. Because of well-known concerns about attention among Lucid participants, we do condition on the most stringent attention checks in the Memphis and National Samples.

We include numerous specific confirmation checks described in Section 3 that increase confidence that our participants are attentive for our key elicitations.

Specific Attention and Confirmation Checks.

- *Modified Dictator Game.* We randomize the order in which we elicit preferences toward tenants versus landlords. We randomize the order in which we ask about indifference in the multiple price lists (i.e., switching the order of Bundle A versus Bundle B). Because we randomize the order independently across elicitations, these randomizations reduce the likelihood that tenants simply click one button (to the left or right) in order to advance in the survey. To give further confidence that our results reflect actual preferences, we ask participants who report being very altruistic to explain why; we provide examples of their qualitative responses in the results. For tenants, we also included specific confirmation checks that ask whether the information would be shared with the landlord. Of the 1,175 tenants asked this question, 88% pass.[59] Finally, the fact that the DG responses are correlated

[58]We updated the anonymity language partway through the tenant study. See discussion in Appendix C.8. Notably, comparing tenant responses before and after we changed anonymity language suggests behavior is highly similar, which further implies that perceptions that responses would be identified do not drive results.

[59]The question was added partway through the experiment.

with simpler elicitations and free-text responses further increases our confidence that they are not driven by inattention.

- *Prior beliefs.* For beliefs about their own tenant or landlord, the participant must report a probability. After the elicitation, we convert the probability to an odds (which may be more intuitive), and participants must confirm the odds corresponds to the probability they have in mind. For beliefs about the average tenant or landlord, the participant must report a number out of 100 who would engage in the elicited behavior. We provide a visualization of 100 boxes that turn red as the number changes; we also ask participants to confirm that the number they report.

- *Posterior beliefs.* Participants must first report a *direction* in which they choose to update. After seeing the information, they are asked if their prior belief is "too high" or "too low." Then they must report a quantitative posterior that aligns with the direction they report.

C.7 Complete list of outcomes and pre-registration

We pre-registered a list of primary and secondary outcomes (AEA RCT Registry: AEARCTR-0008053 [Landlord], AEARCTR-0008436 [Random Sample], and AEARCTR-0008975 [Tenant]). Except where otherwise stated, the primary and secondary outcomes are all reported in the paper or appendix materials. Given the length of the paper, for brevity, we do not report heterogeneity listed in the preregistrations as secondary outcomes.

C.7.1 Landlord outcomes

As indicated in the pre-registration, the primary and secondary outcomes were as follows:

- Behavior in DG with landlords and tenants

- Choice of whether to enroll landlords or tenants in lottery (simple dictator game, Table A13).

- Accuracy of prior beliefs ("objective," by which we meant beliefs about the average tenant, and beliefs about the tenant).

The secondary outcomes were:

- Belief updating, as well as the belief updates about the placebo belief

- Interaction with the program, including the following five outcomes: choice of whether to receive informational materials, choice of whether to refer tenants to the program, choice of whether to decline to sign any legal agreement, choice of whether to receive new offer for back rent, and choice of whether to notify tenants. As pre-registered, we aggregate these into an index in Table A12.

- Heterogeneity.

C.7.2 Tenant outcomes

Primary Tenant Outcomes.

The categories of outcomes were:

- Behavior in DG.

- Beliefs (priors, posteriors, and belief update)
- Payment plan outcomes.

The specific outcomes for the DG and beliefs are reported in the text. The bargaining task outcomes were whether the tenant proposed a payment plan and the share of back rents that they proposed to repay. The extensive margin of payment is reported in the text. The intensive margin (share of back rents proposed to repay) is in Figure S6.

Secondary Tenant Outcomes.

- Willingness to pay for information. We conduct a task that is the willingness to pay for information about how the landlord behaves. This is incentivized using a BDM mechanism with probability 1%. We implemented the mechanism when implementing all payments. We report the treatment effect of information on the above WTP in Figure S5.
- Additional bargaining outcomes, described below.
- Validation of the altruism task. We report these in Appendix C and Table A13. We added these on March 27 as secondary outcomes (we did not collect them prior to March 27).
- Hypothetical indifference points for willingness to move and willingness to accept money in exchange for expunging an eviction record. These outcomes were elicited as multiple price lists. The latter outcome was only elicited among tenants that reported having an eviction filing. We pre-registered these as secondary because they are hypothetical. We also pre-registered that we would not examine the treatment effect on the second outcome. We report the treatment effect on the first outcome in Figure S7. We elicited an indifference point but just focus on the willingness to accept $1,000 versus the outcome so that the outcomes are comparable when we use them in Section 5.2. More details on this elicitation are in Section D.

C.7.3 Random sample surveys

Representativeness. The national sample was designed to be nationally representative. The company could only guarantee a sample of Memphians that was representative at the age $\times$ gender level.

Pre-registration. Our primary outcomes were: DG outcomes and beliefs about the eviction process (Memphis sample only). DG outcomes are reported in A10. Beliefs about the eviction process are in Figure S4.

C.7.4 Other Deviations from Pre-registration

We give a detailed list of minor deviations from pre-registration for complete transparency. None of these deviations are consequential for the paper.

- The tenant survey launched February 15, 2022. On March 27, we updated the tenant survey to include a subexperiment that varied the DG stakes. We also collected additional altruism validation outcomes, based on feedback from the first waves. We updated the pre-registration materials at the AEA website at this time. The full pre-registration history and dates are available there.

- In the tenant survey, we also made minor changes to the registration of specific DG outcomes or the information treatment before March 27. About 59% of the sample was collected on March 27 or later, and the main DG results do not change if we limit only to that sample (see Figure A9 or Table S1 for the disaggregation). On April 8, we changed an item pertaining to the sample size across the information treatments, which does not change what we report.
- As one of the outcomes under the DG behavior in both the landlord and tenant pre-registrations, we write that we would create "difference in indifferences" measures for the DG. For the DGs with landlords as dictators, this would correspond to: the preference for (Own tenant minus random landlord) minus (Own tenant minus random tenant). We realized after completing the study that these differences are not informative, as "own-tenant" would simply get subtracted off both sides. We therefore do not report them for brevity. These outcomes can be easily computed from the estimates reported in Table A15 by subtracting.

C.8 Survey Changes

C.8.1 Landlords

We made two minor small changes to the landlord survey while implementing it, based on feedback from participants or the Memphis ERAP officials. First, we reworded the question in a secondary outcome about declining to sign legal agreements (a secondary outcome for landlords) from a double negative that appeared to be confusing to participants. Second, we added a qualitative question about whether the participants truly wanted to split the DG bundle evenly.

C.8.2 Tenants

We made some minor changes partway through the tenant survey and one major change. The major change is that we added the stakes subexperiment about 40 days through the study, when about 40% of the sample had been collected. Changes are detailed below.

Elicitation Changes Associated with Stakes Randomization. The most important change we made to the tenant survey is that, after collecting roughly 450 observations, we paused the survey to begin randomizing stakes for the DG. Doing so required us to lightly change some of the wording. We also made an update to our RCT registration at the time of adding the stakes subexperiment to detail the changes to our experiment procedures.

Before March 27, all tenants who participated in the DG played the DG where we elicited the value $S(10)$ that made them indifferent between $(10, 0)$ and $(10, 10)$. Starting March 27, tenants were randomized into $x \in \{10, 100, 1000\}$. In order to newly randomize stakes for the survey, we needed to make the following minor changes to the DG elicitation: (i) we indicate to tenants that the funding for the DGs came from a separate research budget; (ii) we indicate that "at least one" tenant will have their choice implemented in each DG implementation; (iii) we emphasize that their responses will be kept private from their landlord.

For (i), we wanted to reassure tenants randomized into the $1,000 gift card condition that the funding for this activity was not coming from money that would otherwise be used for back rent repayments.

For (ii), we had previously told tenants that five tenants would be paid for their responses about landlords and five for their responses about tenants. Thus, this change reduced the probability of payment *only for people exposed to the new stakes*, though as noted above it does not appear to have affected behaviors. To keep the elicitation truthful for people who had previously enrolled, we paid five tenants exposed to the lowest stakes. Hence, more than one tenant was indeed paid. We needed to make this change because the budget precluded us from making ten payments in the $1,000 stakes condition.

For (iii), we newly emphasized to tenants that their choice would be kept private, to reduce separate concerns about anonymity and tenants' incentives.

Because we elicit the DG at for $(10, 10)$ both before and after March 27, we test whether behavior changes for participants at the same stakes ($10) but where preferences are elicited using different wordings. Figure A9 disaggregates pre-randomization and post-randomization (left two ticks) and finds no important difference in behavior, which justifies pooling all the data for power in our main analysis. In particular, comparing tenant behavior at the same $10 stakes before and after March 27 tests whether changes to the anonymity language affect results.[60]

Other Tenant Survey Changes. We made several other edits to the survey when fielding it:

- Based on our experience sending landlords payment plans, we added additional language when describing the payment plans, e.g. about the tenants' rights when talking with landlords.

- Based on tenants' free responses, we added additional language emphasizing to tenants that the gift cards in the DG would not be linked to them in particular and would not count as rent, and we added a confirmation question about whether the gift card is split to own versus random landlord.

C.9 Balance and Attrition

In each test, we include all available characteristics that we elicited before the interventions. For instance, because we conduct the DGs before the information treatments, we include the outcomes of the DGs in our sample balance tests for the information treatments. Among landlords, we find no evidence of important imbalances for the random tenant treatment (Table A1, joint $p = 0.284$). We find evidence of imbalances for the information treatment (Table A2, joint $p = 0.01$), driven by an imbalance in the share who has ever evicted. In robustness checks, we control for observable demographic characteristics and results are quite stable. Among tenants, we find no evidence of any imbalances across the random landlord treatment or the two information treatments (Tables A3, A4, A5).

D Data Appendix

D.1 Data Linkages

Fuzzy Merges to Eviction Data. We process first names, last names, addresses, and ZIP codes from the ERAP application or experiment. We retain only households whose applications had non-missing data for each of these. We merge only on street address and not unit number. To improve the match, we drop street suffixes, as they are not entered consistently (e.g., "road" versus "rd"). We merge these onto eviction records using Stata's `reclink`, with equal weights on first

[60]Because we had limited opportunities to pilot, we further changed anonymity language slightly between March 27 and March 30, which affected 10% of participants. Results are similar if we exclude this group or limit only to tenants who completed after March 30 (Table S1).

name, last name, address, and ZIP. We indicate a match if the threshold exceeds 0.9. In practice, few households have merges between 0.75 and 0.9.

We record several additional details about merging to the experiment details. We had sufficient information on about 330 of the landlord–tenant pairs to conduct a fuzzy match onto court records using tenant names and addresses. As with landlords, we match tenant behavior to eviction court records using fuzzy merges on name and address. Note that because we match on tenant's current address, the tenants with eviction filings still remained housed at the address at the time of taking survey.

Merges to ERAP Reciept. Merges to ERAP data are *not* fuzzy, and instead are based on an internal Case ID created when tenants initiate an application for ERAP. As tenant experiment participants were recruited using from ERAP application data, all of them are associated to a Case ID, which means we can track how they proceed through the ERAP receipt process.

We make several sample restrictions throughout Section 5.3. First, we only have accurate data on ERAP receipt time for tenants who applied after September 2021. Second, the experiment measures tenant hostility for their *current landlord* at that time, which may not be the same landlord as when they applied to the program. We limit to participants who have not moved between applying for ERAP and the survey.

D.2 Preparing the Data for Section 5.1

The tenant sample restricts to those with rental debts at the time of the survey.

Throughout, we let tildes ($\tilde{\cdot}$) represent proxies that we input into the construction of $\tilde{\theta}_i$, whereas primes ($'$) represent the raw data.

Misperceptions. For landlords, we construct $\Delta\tilde{p}_i := p_i' - 6$.

Tenant beliefs do not exactly map onto Δp_i in the model, as they do not represent beliefs about eviction repayment. However, they are beliefs about bargaining costs. This section posits that beliefs about bargaining can proxy for the inverse of beliefs about eviction, so if we find that tenants have a 1 pp misperception about bargaining relative to the truth, then they have a -1 pp misperception about judgments.[61]

For tenants, we have two measures of misperceptions. For tenants, we form

$$\Delta\tilde{p}_i := \frac{1}{2}\left(\left(p_{1i}' - 0.31\right) + \left(0.69 - \tilde{p}_{2i}'\right)\right) \tag{96}$$

where p_{1i}' and p_{2i}' represent beliefs about bargaining and altruism respectively. Notice that higher beliefs about landlord dropping the filing is associated with less bargaining, and vice-versa for beliefs about landlord altruism. Thus, as Δp_i grows, the chances of the tenant wanting to evict rise.

Altruism. We form $\tilde{a}_i := (a_i' - 100)/100$. We only take households whose $\tilde{a}_i$ is obtained for their own landlord or tenant. To accomodate the restrictions in Section 1 we winsorize $\tilde{a}_i$ at 0.95 if larger than 1.

Costs. For landlords, we ask whether they would accept an offer to repay future rent at a "discount." The landlord would have to agree not to file an eviction during the three months the future rent was paid. The discount would operate as a percent reduction below the price of rent.

[61] While tenants' misperceptions about recoupment are, in principle, bounded below by -6, p_i could also contain other probabilities like the chances of winning in court.

For instance, if the landlord agrees to an x% discount, then the government pays the landlord (1-x)% of the rent for 3 months.

We elicit the landlord's indifference point. For half the landlords, chosen randomly, we ask discount prices in: $\{0, 0.1, 0.3, 0.5\}$. For the other half, we ask prices in: $\{0.05, 0.2, 0.4, 0.6\}$. We generate their indifference point as the midpoint of the value that the landlord was willing to accept and control for which set the landlords were randomized into.

Adjusted Surplus. For both landlords and tenants, we read off their adjusted surplus by plugging in the misperceptions, altruism, and cost proxies from the above.

Sample Restrictions. In specifications where a_{ji} enters, we restrict only to landlords or tenants who play the game against their own tenant/landlord. Among tenants, we keep only those who report in the survey that they have positive rental debts. We did not elicit this question in the landlord survey, so we cannot make the restriction there.

E Supplementary Empirical Analysis

E.1 U.S. and Random Samples for Dictator Game

We conducted the same DG in November to December 2021 in samples of random residents of Memphis ($N = 282$) and random, nationally-representative Americans ($N = 632$) using the online survey company Luc.id.[62] Each participant plays the game twice, in random order, once against a random unnamed Memphis ERAP tenant applicant and once against a random unnamed landlord of a ERAP tenant applicant. We explain to participants that ERAP is designated for low-income households with rental debt.

Cross-sample comparisons suggest that the social preferences are stronger among the ERAP sample than among broader populations. ERAP applicants are more likely to be highly altruistic, consistent with positive selection based on relationships (Figure A10, Panel A). Pooling the random samples, landlords are 14 percentage points more likely to be highly altruistic to their own tenant than the pooled random sample is to a random tenant ($p = 0.000$). Tenants are 6 pp more likely to be highly altruistic to their own landlord than the random sample is to a random landlord ($p = 0.002$).

Study participants are also more likely to be in hostile relationships than the random samples (Panel B). Pooling the samples, tenants are 4 pp more hostile toward their own landlord than the pooled random sample is to random landlords ($p = 0.014$, Panel A) and 11 pp more hostile than the random sample is to random tenants. Landlords are 3 pp more hostile toward their own tenant than either random sample is to random tenants in the program, but results are not generally significant (Memphis sample: difference = 4.8 pp, $p = 0.108$; pooled sample: $p = 0.324$). As landlords themselves are less hostile to random tenants than the benchmark, we view the differences among landlords as more informative.

E.2 Assortative Matching

We have two objectives with assortative matching. First, we want to figure out the total share of landlord–tenant relationships that feature at least one hostile party. Since 15 percent of landlords or 24 percent of tenants are hostile, if hostility were perfectly negatively correlated,

[62] In the random samples, we limit the sample to participants who pass the attention check. Results are similar without this sample restriction. The Memphis sample is not strictly representative, because Luc.id could only provide a sample in this area that is representative on age and gender.

then 39 percent could be hostile. If pairs were randomly matched, then 35% would be hostile ($0.35 \approx 0.15 + 0.24 - 0.15 \times 0.24$). Second, we need a measure of assortative matching for Section 6.

We form statistics that are informative about assortative matching. We posit a data-generating process for assortative matching. Based on this DGP, we simulate the amount of assortative matching that generates these statistics.

Direct and Imputed Matching. Recall that even though most landlords have multiple tenants, we only conduct the DG for a single tenant–landlord pair. Because response rates are only about 10%, we only have 19 direct landlord-tenant links who both played the DG against each other. However, for larger property management companies, we observe many tenants linked to a given landlord. Therefore, leveraging these links, we observe 471 tenants linked to 49 unique landlords who participate (and where both play the DG against their own landlord tenant). We consider the mean of the landlord's behaviors toward her own tenant.

This measure is imperfect because it assumes that landlords who are (not) hostile to one tenant where we observe hostility are (not) hostile to all. For instance, if we observe landlord A play the DG against tenant B, and then we observe tenant C's DG behavior against A, we regress C's DG behavior on A's.[63]

We regress tenant behavior on the landlord's mean indifference point across observations for that landlord (Table A7, Panel A) and hostility (Panel B) among tenants and landlords we can match. Panel A, column 1 shows that raising the landlord's observed indifference point from 100% to 200% of the outside option is associated with a 16 cent increase in the tenant's indifference point, but it is not significant ($p = 0.16$). Moreover, raising the landlord's indifference point from 100% to 200% is associated with a 8 pp increase in the tenant being hostile toward her landlord, but is again insignificant.

Tenant Perceptions of Landlord Altruism. Section 4 documents that tenants are overly optimistic about landlord altruism. However, tenants may also have private information about landlords' altruism. We regress tenants' propensity to be highly altruistic toward own landlord on beliefs about the chance that their own landlord would be highly altruistic:

$$p_i = \beta_1 \mathbb{1}(\text{highly altruistic toward own } L)_i + \beta_0 + \varepsilon_i. \tag{97}$$

Our idea is that β_0 reflects average optimism. The coefficient β_1, meanwhile, represents private information as well as particular optimism among tenants who are themselves highly altruistic. If this optimism completely reflects private information, then it suggests an important role for assortative matching. Rates of high altruism among landlords, per tenants' perceptions, are 24 pp (75%) higher among tenants who are themselves highly altruistic. This measure yields a valid estimate of assortative matching if tenant beliefs are not *differentially* biased with respect to their own DG behavior.

Measuring Assortative Matching. We simulate the assortative matching in the data that would generate the above two observed statistics. In particular, within each of the landlords and tenant samples, we rank by altruism, breaking ties at random. We resample landlords (at random) so

[63] Unlike in the body, for this analysis, we all observations within a single large landlord or company. We conduct fuzzy matches on company name, landlord email domain, and phone number. Using tenant reports of their phones, emails, or landlord names, we form "connected sets" that link landlords. For instance, if one tenant reports landlord name A and phone number X, and another tenant reports landlord name B and phone number X, we infer that landlords A and B are the same. Thus, if we observe one landlord observation, we can potentially get a measure of hostility for many tenants.

that we have the same number of landlords as tenants. We choose randomly choose a share of tenants q to be matched with landlords perfectly assortatively. That is, among these tenants, we rank the ith most altruistic tenant with the ith most altruistic landlord. We then regress: (i) the tenant's indifference point on the landlord's indifference point; (ii) the landlord's high altruism on the tenant's high altruism. The idea of this exercise is to infer how much assortative matching would generate the two statistics above.

Figure A7 shows the share of matched relationships with at least one hostile party versus q, the share who are matched perfectly assortatively. We show the mean over 25 simulations for each q. Based on either measure, between 33–36% of relationships feature at least one hostile party and $q \in [0.175, 0.375]$.

In Section 6, we assume assortative matching by splitting the difference, positing that 27.5% of landlords and tenants are matched assortatively (i.e., $q = 0.275$).

E.3 IV Estimates: Beliefs

We use measures of landlord priors, interacted with their information-treatment assignment, as instruments for the magnitude of the landlord's belief update, similar to Haaland et al. (2023) and Bursztyn et al. (2020).[64] Our specification is

$$y_i = \beta \text{Update}_i + \boldsymbol{X}_i \delta + \varepsilon_i, \tag{98}$$

where we instrument for the landlord's belief update (Update_i) with the following instruments, all interacted with information-treatment assignment: (i) the wedge between the landlord's beliefs about her own tenant and the truth for the average tenant, (ii) the wedge between the landlord's beliefs about the average tenant and the truth, and (iii) an indicator variable for receiving the information treatment. We include prior beliefs in $\boldsymbol{X}_i$, so that the instruments compare outcomes induced by changes in beliefs among people with the same prior beliefs. These controls address the concern that the first two instruments are inherently correlated with prior beliefs (Fuster and Zafar, 2022), but our results are little affected by controls.

We find a large elasticity of requesting materials with respect to changes in beliefs. A 10-percentage-point reduction in beliefs about one's own tenant results in a 6.0-percentage-point increase in propensity to request ERAP materials (Figure A8B, $p = 0.037$). This finding is least driven by the variation induced by the second instrument, which uses landlords' priors about the average tenant. We also use a basic specification that simply instruments for the belief update with the treatment and obtain a slightly larger estimate.

Placebo Test. We collect data on other landlord beliefs to implement a placebo test. During our landlord study, Shelby County courts were open and processing evictions, but with a substantial backlog. Landlords' beliefs about court delays were therefore relevant to their assessment of costs and benefits of eviction. We inform landlords about how many evictions were filed in Shelby County courts between April 1 and June 30, 2021. We then elicit prior beliefs about the number of monetary evictions that were granted in that time. Following the information treatment, we ask treated landlords whether they wish to update their beliefs about the number of money evictions granted. Just 14 percent of landlords update their placebo belief, and the distributions of prior and posterior beliefs are quite similar (Figure A18). When we control for the belief update in the IV exercise, results are almost identical.

[64] In Figure 3, the orange bars illustrate that individuals who update their beliefs have substantially larger treatment effects than people who do not. Moreover, people with below-median prior beliefs have smaller treatment effects than people with above-median prior beliefs (blue bars).

E.4 Interaction Between Altruism and Misperceptions

The interaction between altruism and misperceptions is important for interpreting our results. First, there may be multiple constraints to bargaining: if relationships are severely hostile among those with misperceptions, then correcting beliefs may be insufficient to achieve efficient bargaining. Second, misperceptions and hostility could be self-reinforcing.

Correlation Between Beliefs and Altruism. Tenants' beliefs about their own landlord's behavior in the DG are correlated with tenants' behavior in the DG toward their own landlord (Figure S2A). They are still correlated with beliefs about the average landlord's behavior, though the slope falls by about half (Panel B). Just as in the DG, tenants appear to project their relationship with their own landlord onto beliefs about landlords as a class. Beliefs are also correlated with whether the tenant believes their own landlord will file an eviction and then settle (Figure S2C), and the slope for average landlords is insignificant (Panel D). On the other hand, landlords tend to be more generous to tenants whom they believe would not abscond (Figure A24A). Landlords' beliefs about average tenants are uncorrelated with behavior in the DG toward their own tenant (Figure A24B).

Information Treatment Effects and Altruism. We examine the *interaction* between altruistic relationships and information.

Tenants. We split the tenant sample by whether the tenant had a very altruistic relationship with her landlord, which we define as preferring the bundle ($x, $x) to ($2x, $0). We find that the effect of the altruism treatment increases in magnitude, although not significantly so (Figure S3A). However, the effect of the bargaining information treatment increases significantly (Panel B). In particular, providing information to people who were over-pessimistic about whether their landlord would bargain in court makes them less likely to bargain today. In both cases, the difference in updating behavior between people whose priors lie above and below the truth is significant, futher suggesting that beliefs about these facts mediate actions.

Why might the relationship affect the efficacy of information? One hypothesis is that damaged relationships affect information processing. We study whether participants with damagned relationships are less likely to update beliefs when presented with new information. Recall that before eliciting the quantitative belief update, we ask participants if their reported prior is too high or too low. We form binary measures of belief updating, $\mathbb{1}(\text{update})$, based on whether the participant chooses to update her belief. We regress:

$$\begin{aligned}\mathbb{1}(\text{update})_i = {} & \beta_1 \mathbb{1}\left(\text{Hostile}\right)_i + \beta_2 \mathbb{1}\left(\text{AltruismInfo}\right)_i + \beta_3 \mathbb{1}\left(\text{BargainingInfo}\right)_i \\ & + \beta_4 \left(\mathbb{1}\left(\text{Hostile}\right)_i \times \mathbb{1}\left(\text{AltruismInfo}\right)_i\right) + \beta_5 \left(\mathbb{1}\left(\text{Hostile}\right)_i \times \mathbb{1}\left(\text{BargainingInfo}\right)_i\right) + \varepsilon_i,\end{aligned} \tag{99}$$

where β_4 and β_5 represent the marginal effect of providing information to hostile tenants. When we study hostile tenants, we indeed find that they update beliefs less (Table A19B), though we do not find the same result for landlords (Table A19)A.

E.5 311 calls

Overview and Hypotheses. Are hostile tenants and landlords in properties that are more likely to violate city codes? We study whether hostile landlords or tenants are more likely to be involved in 311 calls about code violations. While 311 calls do not indicate whether a code violation actually occurred, they are nevertheless a proxy for animus between tenants and landlords. Calling 311 on one's landlord is a costly action that can cause the landlord to be fined or forced to make repairs.

How might we expect hostility to be linked to 311 calls? Hostile landlords may accrue 311 calls because they do not make repairs, since they have animus toward their tenants. Alternatively, they may become hostile because the tenant called 311. Similarly, tenants whose landlords violate code may be hostile, or hostile tenants may be more likely to rent from landlords who violate code. We cannot distinguish these hypotheses.

Data. We use publicly available 311 calls from the City of Memphis, which contain geocoded address information. We link them to study participants' addresses. We keep only 311 calls pertaining to code enforcement, drain maintenance, grounds maintenance, or street maintenance. We can only link to Memphis 311 calls and not calls in Shelby County, since we do not have 311 calls for the outlying areas.

311 calls are not linked to individuals. Therefore, we conduct fuzzy matches to the 311 records based on address only. For landlords, we cannot be sure whether they rented the property at the time of the 311 calls, but we keep only calls from 2020–October 2023. For tenants, we link to 311 calls at their current address.

Results. Table A27 shows that highly hostile tenants are more likely to be linked to 311 calls. Tenants who are highly hostile (have $S(x) < \frac{x}{10}$) to either landlord opponent or their own landlord opponent have more 311 calls (Columns 3–4). These effects are large in magnitude: highly hostile tenants have more than 10 pp higher rates of code enforcement complaints (25% of those who are not highly hostile). We detect no such results for hostile landlords (Table A27), although point estimates in some specifications are large (16 pp, Column 2). One hypothesis is that code enforcement problems drive tenant hostility (or vice-versa), but hostile landlords do not use code violations to discipline tenants.

E.6 Emergency Rental and Utilities Assistance Program Evaluation

This section conducts a policy evaluation of the Memphis/Shelby County Emergency Rental and Utilities Assistance Program (ERAP), with the goal of examining whether emergency rental assistance stops evictions.

ERAP Sample. We use administrative data from Memphis/Shelby County's ERAP records (Section 2). Our sample consists of households whose ERAP case was created after September 1, 2021 and who were paid by the time the program concluded in December 31, 2022. We use timestamps of changes to the household record to infer how the household progresses from creating an application, submitting an application, and receiving payment. Using personally identifiable information on the application, we conduct fuzzy merges on name and address to public evictions records, scraped from public records by the Legal Services Corporation and shared with us (Appendix D). Our merge strategy will not detect evictions if the eviction record only lists an occupant who does not appear on the ERAP application.

In Memphis, tenants may apply to the local ERAP, or landlords may apply on the tenant's behalf. Back rents are repaid to landlords, unless landlords decline or do not respond to ERAP, in which case tenants may receive a direct payment.[65]

A share of paid households also received representation from an attorney who could encourage landlords to accept payments and impose eviction forbearance periods. To focus on the most

[65] Landlords can decline payments as they can be subject to legal stipulations, such as right to random inspections of the property or an agreement not to evict the tenant within a certain period of time.

externally valid portion of the sample, and obtain the treatment effect of rental assistance payment alone, we drop households who reached a legal settlement.

Several features of ERAP's program affect the interpretation of the payment treatment effects. First, ERAP payments could also include one–two months of rent for future months. These payments were intended to cover back rents accrued during the processing period. The exact amounts of additional months of rent varied by month. Second, ERAP could also pay utility bills directly to the utilities providers. Third, if the landlord declined ERAP, the program could make direct payments to the tenant. We do not currently have complete information about to whom the payments were made.

Tenant Characteristics. Tenants in the ERAP administrative data are highly similar to those in the experimental sample, by dint of how the samples were constructed (Table 1). Households paid in the legal program are similar to those in the non-legal program (Column 5 versus Column 6). After this restriction, there are about 4,800 paid tenants in the sample. The legal sample is more than half of all paid households in our data. The reason so many households entered the legal program is that the legal arm also paid bulk settlements with many tenants to large landlords.

Outcomes. We focus on two stages of the eviction process: (i) the eviction *filing*, which is the formal legal petition filed by the landlord that initiates a court eviction hearing; and (ii) an eviction *judgment*, which is a formal eviction (Section 2).

E.6.1 Additional Institutional Details

Legal Program. As Table 1 shows, more than half the paid tenants appear in the legal services program. This program enrolled two types of tenants. First, some tenants whom the program perceived to be at risk of eviction were granted legal assistance. Second, some tenants were granted bulk settlements with landlords.

Legal program participants were subject to explicit legal contracts that forbade eviction for a 45-day period. Their payment was also expedited. The lawyers could also encourage landlords into accepting the terms of the legal agreements. For this reason, we want to exclude this sample for external validity.

Excluding the legal sample poses several challenges to the empirical analysis. To begin with, by excluding at-risk tenants, we may be dropping the tenants who are likely marginal to eviction. This concern is likely valid to some extent. Three points mitigate how worried one should be. First, many tenants do obtain eviction filings and judgments in the pre-period. Second, many tenants in the legal services arm were *not* at risk, because they were granted bulk settlements together with other tenants. Third, many tenants were at risk but missed by the legal services arm. Tenants were flagged for the legal services arm in several ways: if they listed that they had an eviction notice on their application; if they were found to have an eviction record by exact-matching based on name; or if they asked for help from their screener. Clearly, many tenants did not make it to the legal services arm even if eligible, as we observe hundreds of filings in the pre period.

A second, more subtle concern is mean reversion, which we address explicitly in the analysis.

Terms of ERAP Receipt. The bundle in an ERAP payment had different valuations depending on the period. Early in the program, ERAP only paid back rents or utilities assistance. Beginning in July 2021, some bulk settlements incorporated three months of future-rent payments. In April 2022, regular (non-legal) ERAP payments also included two months of future rent, as well as late fees for all other payments, including non-legal payments. As we include calendar time and cohort fixed effects, these changes should not materially affect our results.

E.6.2 Event Study

Formal Specification. We now leverage quasi-random variation in ERAP *payment timing* among households who *applied* at the same time.[66]

We estimate:

$$y_{it} = \gamma_r + \delta_c + \alpha_t + \sum_s \beta_s \left(\mathbb{1}\left(\text{event period} = s\right)_{i,s(it)} \times \text{After}_{it} \right) + \lambda \text{After}_{it} + \sum_{s<s'} \sigma_s \mathbb{1}\left(\text{event period} = s\right)_{i,s(it)} + \varepsilon_{it} \tag{100}$$

for household i who was paid in 14-day period r and who applied in 14-day period c, and where t indexes 14-day calendar period. We include fixed effects for payment-period cohort γ_r, application-period cohort δ_c, and calendar period α_t. Event-time s is defined relative to the period of ERAP payment. The outcome y_{it} is an indicator for whether a household i receives an eviction filing or judgment in period t. The coefficients of interest, β_s, represent standard event-study coefficients. We present event-study estimates at the two-month level.

The indicator After_{it} is an indicator that turns on once the tenant has applied to ERAP, but potentially before she is paid. The variables $\sum_s \sigma_s \mathbb{1}\left(\text{event period} = s\right)_{i,s(it)}$ allow for periods prior to application to be correlated with eviction risk. We explain below why these controls add to the credibility of the design. Because of collinearity, we can only identify σ_s in periods $s < s'$ where s' is the event period of payment.

We cluster standard errors by household.

Identification Assumption and Tests. Our identifying assumption is that a given payment period cohort's judgment and filing rates would have trended in parallel with other cohorts if the cohort were not paid. Our approach permits standard pre-trends tests of our parallel trends assumption.

The program had explicit scope to expedite payments for two reasons. First, payments made through the legal program could be expedited. As noted above, we drop people who successfully went through this program. In our primary specification, for power, we keep people who were flagged for the legal program but not ultimately paid by it.

Second, the program could expedite rent payments for households who apply with utility shutoff notices. We keep these households in our primary specification, since payments from applying with a utility shutoff would not be driven by a short-term change in filing risk *after* applying. We show results if we drop these households.

A sufficient (but not necessary) condition that ensures the parallel-trends assumption is if the timing of ERAP receipt, conditional on application date, is orthogonal to filing or judgment rates. Our approach is motivated by institutional context which suggests that there is a substantial exogenous component to the timing of ERAP payment among applicants. For instance, there is substantial variation in average time-to-payment across the screeners employed in the Memphis ERAP (Figure A3B), though we are underpowered to use exclusively this variation in a investigator-IV design. In support of our assumption, we find that applications with above- versus below-median payment times are balanced on applicant demographic characteristics like sex, race, household size, income, and monthly rent (Table A21). We do find that households with higher overdue rents received payments faster.

Level differences in demographics are not on-face concerning, since our specification is

[66]ERAP speed being associated with altruism does not necessarily complicate the analysis. As in any difference-in-differences design, our empirical strategy permits level differences in ERAP speed that owe to individual covariates. Receipt may not be driven by time-varying changes in potential outcomes (eviction risk).

dynamic. To further investigate this point, we present an anticipation test. We regress judgment and filing rates prior to payment on payment speed and find no evidence of a correlation (Table A22), once we control for calendar-date fixed effects. This table provides evidence against a remaining identification concern that landlords or tenants coordinate to return materials to ERAP in response to short-term spikes in eviction risk.

Interacting the indicator After_{it} with event-study coefficients in Equation (100) ensures that pre-trends are only estimated from tenants who have already applied for the program. Thus, the event-study coefficients are only identified from idiosyncratic payment-time variation once the tenant applied. This interaction reduces the risk of contamination from endogenous forces that could cause tenants to apply for the program in the first place (Ashenfelter, 1978).

The additional coefficients $\sum_{s<s'} \sigma_s \mathbb{1}\left(\text{event period} = s\right)_{i,s(it)}$ are useful but not necessary for the design. In particular, excluding these coefficients imposes that event time, indexed with respect to payment, is uncorrelated with eviction risk prior to application ($\sigma_s = 0$ for all s). This econometric restriction is justified if payment timing is quasi-random and can assist with power. Intuitively, if a household has not yet even applied, then their eviction risk should not be affected by whether it is 2 months or 4 months until payment. We call specifications where we omit the $\sum_{s<s'} \sigma_s \mathbb{1}\left(\text{event period} = s\right)_{i,s(it)}$ coefficients "semi-parametric," and the complete specification in Equation (100) "non-parametric."

Because we limit the sample to people paid 6 months before the end of our eviction data, our panel is balanced in the post-period. However, the pre-period event study coefficients are not identified from a balanced panel. That imbalance is required if we use only the quasi-random payment-time variation, as households will then by definition not be observed for the same number of pre-periods after applying.

E.6.3 Alternative Design (Non-Paid Households)

We call the above design, which leverages only payment timing among households who are paid, the "primary design." We additionally use an "alternative design" with an explicit untreated comparison group: households who apply to ERAP but never get paid, because they are lost in the screening process or do not finish the eligibility-certification process. There are more than 6,000 such households. This strategy relies on familiar difference-in-differences identifying logic: absent payment, trends for never-paid households would have been parallel to paid households in the post period.

The comparison group provides more power. On the other hand, the variation is less clean, as people who do not get paid may have unobservably different time-varying eviction risk.

We collapse to the week of payment by week of application level, which makes the source of variation explicit and eases estimation. For households that are never paid, we collapse only to the application period. That is, we obtain the mean outcome by calendar period $\times$ application period $\times$ payment period, where a period is two weeks, and the payment period is 0 if never paid.

The alternative design is similar to Equation (100):

$$y_{rct} = \gamma_r \times \delta_c + \alpha_t + \sum_s \beta_s \left(\mathbb{1}\left(\text{event period} = s\right)_{rcs} \times \text{After}_{rct}\right) + \lambda \text{After}_{rct} + \sum_{s<s'} \sigma_s \mathbb{1}\left(\text{event period} = s\right)_{rcs} + \varepsilon_{rct}, \tag{101}$$

where γ_r is 0 for all r if not paid and y_{rct} is the mean outcome at the $r \times c \times t$ level. Event dates s are 1 only if the household is paid. Equation (101) constitutes a "standard" event-study that compares paid households to household who applied in the same calendar week but were not paid. Another conceptual difference between Equation (101) and Equation (100) is that we interact the application by payment indicators. This interaction is natural once we think of cohorts as payment by application periods, and the interaction eliminates a modest pretrend in the filings specification.

In practice, to address concerns about negative weights, we use heterogeneity-robust estimators for Equation (101).

We focus on the "stacked" estimator, as in Cengiz et al. (2019). For each payment period r, we form a "dataset" $d(r)$ that is all the unpaid households and the households paid in week r, collapsed as described above. We stack each dataset $d(r) \in \mathcal{D}$ and estimate a stacked version of Equation (101):

$$y_{rctd} = \gamma_{rd} \times \delta_{cd} + \alpha_{td} + \sum_{s} \beta_s \left(\mathbb{1}\left(\text{event period} = s\right)_{rcs} \times \text{After}_{rct} \right) + \sum_{d} \lambda_d \text{After}_{rctd} + \sum_{s<s'} \sigma_s \mathbb{1}\left(\text{event period} = s\right)_{rcs} + \varepsilon_{rctd}, \tag{102}$$

where outcomes are collapsed as described above.

This equation augments the standard event study to include dataset-specific time and cohort fixed effects. We two-way cluster this specification at the dataset by 14-day payment period level, since the data have been collapsed, and we weight by the number of underlying observations in each dataset. As Cengiz et al. (2019) argue, the specification is robust to concerns about negative-weights because it compares each cohort of paid households to unpaid ("clean") controls.

Second, we use the Sun and Abraham (2021) estimator for Equation (101), where we cluster by 14-day payment period, employ the same collapse procedure, and weight by number of underlying observations.

Finally, we present the Two-Way Fixed Effect (TWFE) specification as a benchmark. For comparability, we estimate this specification by employing the same collapse procedure, clustering by 14-day payment period, and weighting by the number of underlying observations.

We limit the unpaid sample to the first three months (3×4-week periods) after applying. The reason is that we were concerned about linking evictions to households more than a few months after applying, if they did not get paid. The paid sample is confirmed to live at the address at least some point in the intervening time between payment and application, so this drop is not necessarily differential across samples.

E.6.4 Coefficient Interpretation and Rescaling

The event-study coefficients β_s characterize changes in filing and judgment rates after payment, with the units of percentage points per two-month event period. We make several adjustments to facilitate interpretability. First, the event periods are at the two-month level (for power), but the data are at the two-week level (which allows finer calendar-time fixed effects). To ease interpretation, we multiply each event-period coefficient and standard error by four, as there are four two-week observations per household per event period. Then the coefficient represents the effect in that two-month event period. We also report the average per-period effect over the 6-month post period. To obtain the cumulative effect, simply multiply the event period estimates by three.[67]

[67] Results are similar if we estimate event-period coefficients at the two-week level and average them to the two-month level. This specification further requires omitting the period-of-payment fixed effects, due to multicollinearity.

E.6.5 Results

Figure A22 displays estimates of our event-study specification (Equation 100). Panel A shows that ERAP payments have, at best, a small effect in the first two-month period which then attenuates. We present three specifications: a semi-parametric specification which imposes $\sigma_s = 0$ for all s (see above), a full non-parametric specification (Equation 100, corresponding to the dashed line and pooled estimate reported on the figure), and a version which excludes households who applied with eviction notices or shutoffs and may have been eligible for expedited filings.[68] We see no evidence of pre-trends in any specification, and worst-case specifications for ERAP (the restricted sample) yield positive post-period effects on judgments. The estimate reported on the figure, which averages the post-period coefficients and subtracts from the averaged pre-period coefficients, suggests a small and non-significant effect on judgments in the average two-month period after payment.

ERAP payments cause a sharp reduction in eviction filings for two months before plateauing (Panel B). The semi-parametric version of Equation (100) shows no pre-trends. The full non-parametric version of Equation (100) shows a discernible pre-trend in filings. We eliminate this pre-trend if we exclude households with eviction shutoffs or notices, who can be flagged for expedited payment.

The alternative design yields similar results (Figures A22C and D). Relative to the primary design, the alternative design yields a larger effect on filings and a smaller (more positive) effect on judgments. Notably, even if the program deferred filings, that it had a null or positive effect on judgments implies that the filings it stopped were less likely to yield judgments in the first place.

Further consistent with this point, we examine "non-suits," or explicit withdrawals from the court system, using the primary design (Figure S1). We find that the program yielded a sharp increase in non-suits. The null effect on judgments implies that these non-suits came from households that would have had informal arrangements outside the courts.

Reweighted Estimates. One concern about the above approaches is that people who received filings in the interim between application and payment could be pushed into the legal program. This force pushes toward small effects, as the riskiest households have selected out. Moreover, as receiving filings is unusual, if receiving a filing in the pre-period makes one less likely to get a filing in the post-period (because not in the sample), there is a risk of regression toward the mean.

We adjust the primary strategy using propensity-score reweighting (Figure A23). We regress an indicator for appearing in the legal program (and being dropped from the main sample) on an indicator for (i) obtaining a filing between application and payment, interacted with (ii) an indicator having an eviction notice at application.[69] We weight the primary strategy by $1/(1-p)$, where p is the propensity from this regression.[70]

Reweighting generates a persistent negative effect on filings and a more negative effect on judgments in some specifications.

Interpretation and Fiscal Costs of Eviction Prevention. Table A23 aggregates estimates across empirical strategies. We present the non-parametric specification in the primary design, the reweighted estimate, and the stacked version of the alternative designs. The effects across research

[68] Note that eviction *notices* are not the same as *filings* (Section 2). Not all households with filings listed that they had a notice on their application, and it was harder for them to be expedited if they did not do so.

[69] We include the second indicator because people with notices at application were supposed to be sent to the legal program directly.

[70] Intuitively, suppose one third of people who develop filings are put into the legal program. We upweight the remaining two thirds who are not in the legal program by $1/(1-1/3) = 3/2$, thus "filling in" the selected observations.

designs are consistent with a null or small negative effect on judgments (and positive in some specifications), and a moderate negative effect on filings.

We present estimates that are directly interpretable as percentage point effects. The reweighted estimates paint the most favorable picture of ERAP. For instance, the second column suggests that ERAP stopped 32 eviction judgments (s.e.: 22) and 87 eviction filings (s.e.: 32) per 1,000 paid households in the six-month period following payment (primary design, reweighted estimates). Effects are much larger in the first period. If the later periods had the same effects as in the first-period effects, then ERAP would have stopped about 160 filings per 1,000 paid households (Row 3, Column 2). Effects are smaller in the other designs.

Rows 4–9 of Table A23 interpret these treatment effects. Rows 4 and 5 present the "maximum simulated effect" on judgments or filings. These show the treatment effects on simulated data, where we replace all treated units in the post period with having zero filings or judgments. Dividing the judgments or filings estimates in Rows 1 and 2 by the estimates in Row 4 give one way of benchmarking how large the effects are. The best-case effect in Column 2 is moderate — about 38% of the maximal effect — but also implies that the remaining 62% accepted ERAP funds and pursued evictions anyway. Effects in other columns are much smaller.

From our administrative data, we estimate the average ERAP payment amount is approximately $5,400. We compute average prevention costs by dividing this payment amount by point estimates of the treatment effects. The fiscal costs of preventing judgments via ERAP are very high, since the point estimate is small (Row 6), whereas the costs of preventing filings are smaller but still meaningful (Row 7). In Rows 8–9, we present the minimum fiscal cost consistent with the lower bound of the 95-percent confidence intervals in Rows 1–2. Across specifications, the lower-bound cost is about $35,000 per filing and $70,000 per judgment. As discussed in the body, the fiscal cost is high in part because evictions are rare among this sample.

E.6.6 Interpretation and Discussion

ERAP payment defers filings for at least two months, and in some specifications, has a modest persistent effect. How important is stopping filings alone?

Filings are costly, but less costly than judgments. To Shelby County landlords, it costs $127.50 directly to file, and there additional legal costs. Tenants also face costs of filings: eviction filings are matters of public record and landlords often investigate the eviction history of potential tenants. Filings also may trigger informal moves that tenants may want to avoid.

However, filings are likely less costly than judgments. Collinson et al. (2024) present estimates of the effect of judgments relative to filings, as they use a judge-IV design among households with filings. They find moderate negative effects on credit scores, employment outcomes, and hospital admissions.

To provide some evidence on the costs of filings and judgments, we elicited tenant survey participants' hypothetical *(i)* willingness to accept cash in exchange for moving from their unit, and, if they reported previously being evicted, *(ii)* willingness to accept cash in exchange for erasing their eviction from their record (see Appendix C for details on these elicitations). Tenants value avoiding an eviction filing at or more than their subjective moving cost. 75 percent of surveyed tenants would decline $1,000 to avoid a move. 83 percent of surveyed tenants who reported having an eviction would decline $1,000 in cash to expunge their eviction record.