Social Preferences and Bargaining Failure in Eviction*

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Abstract

We study how social preferences shape landlord-tenant bargaining over eviction using labin-the-field experiments in Memphis, Tennessee. 24% of rent-delinquent tenants and 15% of landlords exhibit hostility, burning money to harm the other, yet majorities of both are highly altruistic. Social preferences correlate with eviction and related behaviors. Embedding these patterns in a bargaining model, hostility and misperceptions render 19% of evictions inefficient, even as altruism sustains others' efficient bargaining. Social preferences undermine policy targeting, as altruists more often take up rental assistance but evict less. Heterogeneous social preferences thus both destroy and create bargaining surplus, and limit gains from policy.

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We study how social preferences shape bargaining and policy design in theory, the laboratory, and the field. Absent social preferences, frictionless bargaining produces efficient outcomes. However, real-world bargaining often involves people with established interpersonal relationships: consider divorce proceedings, labor disputes, or political negotiations. In such contexts, hostility could undermine mutually beneficial bargaining, while altruism could facilitate it. Field evidence on social preferences mostly comes from non-confrontational settings, examining altruism or prosocial motivations and charitable giving (DellaVigna et al., 2012), worker behavior (Bandiera et al., 2005; Ashraf and Bandiera, 2018; Cullen and Perez-Truglia, 2023), family investments (Dizon-Ross and Jayachandran, 2023), and beyond. We know much less about social preferences in adversarial bargaining domains, especially the role of hostility (Krueger and Mas, 2004; Hjort, 2014; Kranton et al., 2016; Carry and Schoefer, 2024).¹

Our context is landlord-tenant bargaining and eviction from rental housing. Around 2% of U.S. renters face court-ordered eviction each year (Gromis et al., 2022; Graetz et al., 2023). Parties can avoid eviction with bargained settlements, but hostility or altruism may influence negotiations. For example, *The New York Times* (2021) describes a landlord whose tenants "curse and spit at her and owe more than \$23,000 in rent." Bargaining failure could have large welfare consequences, as evictions traumatize tenants (Desmond, 2016) and cause financial distress (Collinson et al., 2024).² Landlords also incur costs from court, vacancy, and property damage. Concern about evictions' impacts has spurred policy intervention, including the pandemic-era Emergency Rental Assistance Program (ERAP) which provided \$40 billion in funding for low-income rental assistance.

How prevalent are positive and negative social preferences among tenants facing eviction and their landlords? Do social preferences create or stop a meaningful share of evictions? How do social preferences influence the welfare impact of eviction? And, in light of social preferences, what are the consequences of emergency rental assistance for evictions?

We answer these questions in four parts. First, we use a simple model to capture how social preferences might affect eviction and policy intervention. Second, we test for the importance of social preferences among landlords and tenants facing eviction with lab-in-the-field experiments linked to court data. Third, we turn to policy, studying ERAP's causal effects and leveraging experimental data to explain its impacts. Fourth, for welfare analysis and policy counterfactuals, we estimate the model to match experimental and observational moments.

Section 1 illustrates how social preferences influence a stylized bargaining model adapted

¹ Laboratory experiments document negative social preferences and suggest a relationship to bargaining (Roth et al., 1991; Charness and Rabin, 2002; Abbink and Sadrieh, 2009; Kranton et al., 2016). It is unclear whether laboratory data explain bargaining outcomes in high-stakes contexts where agents know each other (Levitt and List, 2007). Field evidence in development or cultural-economics settings shows negative social preferences from conflicts or colonialism (e.g., Hjort, 2014; Lowes, 2021; Blouin, 2022; Ramos-Toro, 2022; Cavatorta et al., 2023; Ortiz, 2024; Dunia et al., 2025).

²We define eviction as formal (court-ordered) eviction and view informal evictions as implicitly bargained. The findings in Collinson et al. (2024) suggest formal evictions are costlier than informal evictions, as their research design involves comparisons of formally-evicted to likely-informally-evicted tenants.

to our eviction setting. Landlords and tenants bargain over rental debt. Eviction occurs when bargaining fails. In this frictionless Coasean benchmark, parties choose eviction when total private bargaining costs are higher than total private court costs (i.e., when efficient). For instance, efficient evictions occur if enforcing bargained agreements is challenging; or, if bargaining makes landlords seem "soft," reducing rent payments from other tenants.

We depart from the benchmark by introducing social preferences where parties place positive or negative weight on the other's payoff (Becker, 1974). We show that "hostile" (negative) social preferences work by exacerbating other bargaining frictions, raising the chances of inefficient outcomes like eviction despite low bargaining costs. Meanwhile, "altruistic" (positive) social preferences dampen frictions and facilitate efficient outcomes.³ To illustrate a potential friction, we model misperceptions from belief errors about eviction payoffs. For instance, if landlords are mildly optimistic, then hostility can turn a minor misperception into inefficient eviction. Anti-eviction policy that requires landlord–tenant cooperation risks "selection on altruism": it reaches altruists who would bargain anyway, muting the policy's impact. This framework suggests that the role of social preferences for eviction and anti-eviction policy is an empirical question depending on the distributions of social preferences, bargaining costs, and misperceptions.

This framework guides our analysis of eviction in Memphis, Tennessee, a city where both eviction and landlord-tenant bargaining are common (Section 2). We partner with the housing agencies that run Memphis's ERAP to recruit 1,808 tenant applicants and 371 landlords of tenant applicants for lab-in-the-field experiments and surveys. Our experiments measure social preferences among a policy-relevant population, in the midst of high-stakes negotiations. Tenants have back rents of around \$3,600, about four times their monthly income, and are at high risk for eviction. We link the experiments to court dockets and ERAP records, letting us connect lab-quality data on social preferences to actual bargaining behavior and policy outcomes.

We measure social preferences using more than 4,000 real-stakes Dictator Games (DGs) with landlords and tenants (Section 3). Landlords play against their own tenant, and tenants play against their own landlord. We use a modified DG where players can express hostility in addition to altruism. If hostile, participants forgo money to lower their opponent's payment. If altruistic, participants forgo money to raise their opponent'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. In other words, a material share of landlords and tenants burns money — sometimes hundreds of dollars — to ensure the other gets nothing. As one comparison, 0% of college students exhibit hostility in Charness and Rabin (2002).⁴ Yet, 69% of landlords and 53% of tenants are altruists who halve their payment to ensure

³These normative claims hold whether or not the social planner respects social preferences.

⁴As another comparison, we randomize if participants play against their own or a random landlord or tenant. Participants are significantly more hostile toward own opponents than random tenants. Other studies do detect some hostility (Abbink and Sadrieh, 2009; Kranton et al., 2016). Section 3 discusses our results in light of these benchmarks.

their opponent gets an equal payment. Several validation exercises suggest our findings are not artifacts of the laboratory environment (Levitt and List, 2007). Intense social preferences persist even at \$1,000 stakes, and correlate with text-analysis measures of relationship affect, qualitative responses, and other revealed-preference choices.

As the model suggests that social preferences affect evictions by interacting with other frictions, we test for misperceptions about the perceived payoffs of eviction (Section 4). With landlords, we elicit beliefs about back rents that they can expect to recoup in court. With tenants, we elicit beliefs about landlords' altruism and bargaining rates after initiating the court process. 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%. Experienced landlords have more accurate beliefs. Correcting beliefs with information affects some bargaining and ERAP take-up decisions.

Having found evidence of polarized social preferences, we next show that social preferences predict eviction (Section 5.1). We link the experiment to Memphis eviction records. Hostility toward own landlord or tenants correlates with a 25% higher rate of eviction filings, which initiate court eviction, and a 27% higher rate of ultimate eviction judgments.

Supporting evidence validates this correlation between social preferences and eviction (Section 5.2). First, hostility is approximately balanced on eviction risk predicted from observables. Therefore, the relationship between hostility and eviction is robust to including flexible controls. Second, a falsification exercise shows that generalized hostility toward random tenants or landlords, also elicited in the DGs, does not predict eviction. Third, we verify a key model prediction, that hostility and misperceptions are complements: hostility predicts eviction only in the presence of high rates of misperceptions. Fourth, other bargaining outcomes correlate with hostility. For instance, hostile addresses are 48% more likely to receive complaints about housing code violations. Finally, we do not find that hostility rises after eviction filings, arguing against reverse causality. We lack an exogenous shock to social preferences, so our correlational evidence should be treated cautiously. Still, the evidence suggests social preferences' relevance for bargaining failure.

Social preferences also predict take-up of government policy (Section 5.3). Linking the experiment to ERAP administrative data, we confirm that tenant enrollees are more altruistic in the DGs. An implication of these patterns is that ERAP could have muted impacts on eviction, as altruists take-up despite low counterfactual eviction risk.

To examine that implication, we study the Memphis ERAP's causal effects using administrative data on around 4,600 enrollees (Section 5.4). Event studies around payment reveal null effects on judgments (i.e., ultimate formal evictions) and moderate, but statistically significant, reductions in filings. Beyond confirming social preferences' relevance for policy, these reduced-form findings challenge advocates' views that ERAP was "the most important eviction prevention policy in American history" (The White House, 2022).

We conclude with welfare analysis using the model (Section 6). 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 remaining parameters by matching model-implied behavior among experiment participants to moments from the experiment and ERAP evaluation, using the Method of Simulated Moments. Estimation interprets the social preferences–eviction correlation as causal, conditional on model structure.

Our main result from the model is that 19% of evictions are inefficient, that is, caused by social preferences and misperceptions. The average eviction gives more than \$1,000 in surplus, so anti-eviction interventions, if untargeted, reduce welfare. Different modeling assumptions can give that around 40% of evictions are inefficient, but do not reverse our conclusion that most are efficient. Naturally, implications could differ if policies target especially inefficient evictions, or if negative externalities or high social marginal welfare weights on tenants (Saez and Stantcheva, 2016) outweigh eviction's private surplus.

Although most evictions are efficient, social preferences are still critical. First, hostility causes two-thirds of the inefficient evictions. Second, altruism sustains bargaining that otherwise fails. When we shut down altruism in model simulations, more than half of evictions are inefficient.

Finally, the model confirms that selection on altruism plays a quantitatively meaningful role in ERAP's small impacts. When we shut down altruism in the model, ERAP's treatment effects rise to 23–43% from a baseline of no impact. While we do not offer a complete solution to selection on altruism, simulations suggest that restricting ERAP funding based on observable characteristics would improve ERAP's treatment effects.

Contribution. This paper contributes to several literatures in economics. First, within the behavioral literature, we show social preferences' relevance for adversarial bargaining in a high-stakes field setting, examine how they interact with context-specific frictions, and demonstrate novel policy implications. Our focus on hostility is fairly unusual, as the literature often emphasizes altruism — in part because of the practical challenge of recruiting hostile parties into experiments.⁵

Second, we add to a growing literature on non-classical forces and housing markets. One related strand examines how beliefs shape housing choices (e.g., Bailey et al., 2018, 2019; Armona et al., 2019; Bottan and Perez-Truglia, 2021; Kuchler et al., 2023; Chopra et al., 2024; Fairweather et al., 2024). Another strand documents canonical behavioral forces like reference-dependence (Genesove and Mayer, 2001; Andersen et al., 2022), projection bias (Busse et al., 2012), or inattention (Andersen et al., 2020) in housing markets. Relative to prior work, we focus on eviction rather than choices like home-buying or mortgage refinancing, and we examine a different non-classical force (social preferences). Although law and sociology research emphasizes the role of landlord–tenant

⁵Exceptions include research discussed in the beginning of the introduction and footnote 1. Another area where the literature has emphasized hostility is when considering taste-based discrimination and, more recently, Allport (1954)'s contact hypothesis (Becker, 1957; Rao, 2019; Lowe, 2021, Forthcoming; Bursztyn et al., 2024). Carry and Schoefer (2024), another related paper, attribute inefficient settlements in labor disputes to hostility but lack as direct evidence on social preferences. Our finding that altruism offsets misperceptions parallels Friedberg and Stern (2014) on divorce.

relationships for housing insecurity, economists have not stressed this mechanism.⁶ An exception includes work on housing discrimination (e.g., Ewens et al., 2014; Lodermeier, 2025).

In examining eviction's causes and normative implications, we study different questions than important recent research on eviction's impacts (Collinson et al., 2024, 2025b). One antecedent is Hoy and Jimenez (1991)'s classical model of bargaining and squatter evictions. Humphries et al. (2025) show how financial distress and (rational) beliefs influence eviction. Along with Collinson et al. (2025a), we conduct the first evaluations of ERAP, and we likewise find small impacts.

Third, we add to research in behavioral public economics by studying how social preferences influence policy intervention and social welfare (Bernheim and Taubinsky, 2018). Public economics has long considered how altruism affects charitable donations (Andreoni, 1990, 1993; Andreoni and Miller, 2002), bequests (Becker, 1974), and contributions to public goods (reviewed in Ledyard, 1995). We add a new mechanism to the benefit take-up literature — selection on altruism — which complements prior research's focus on take-up costs and information frictions (e.g., Mullainathan and Shafir, 2013; Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019; Rafkin et al., 2024). This selection on altruism result flips the classic hypothesis that financial incentives crowd out prosocial behavior (Bénabou and Tirole, 2006; Ashraf et al., 2020); here, altruism crowds *in* take-up of an incentive. A similar selection on altruism mechanism might generalize to the take-up of child care, old-age assistance, or even diversity recruitment programs (Behaghel et al., 2015).

Last, we build on prior research about bargaining norms, biases, and heuristics (Binmore et al., 1985; Roth et al., 1991; Babcock and Loewenstein, 1997; Charness and Rabin, 2002; Andreoni and Bernheim, 2009; Birkeland and Tungodden, 2014; Pope et al., 2015; Backus et al., 2020; Keniston et al., 2022; Pollak, 2022). Our work on social preferences adds to seminal literatures on bargaining with incomplete information (e.g., Myerson and Satterthwaite, 1983; Yildiz, 2011; Larsen, 2021; Cullen et al., 2025) and court settlements (Priest and Klein, 1984; Silveira, 2017; Sadka et al., 2020).

1 A Model of Bargaining in the Shadow of Eviction

We write the model to describe landlord–tenant bargaining and eviction. With minor relabeling, the model captures many bargaining situations. It thus offers more general insights about social preferences and bargaining. Extensions and proofs are in Appendix B.

1.1 Setup and Classical Benchmark

Set-Up. 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

⁶Sociologists often describe eviction as emerging from coercive power dynamics (Vaughan, 1968; Desmond, 2016; Akers and Seymour, 2018; Chisholm et al., 2020). Yet, interviews suggest that altruism motivates landlord participation in housing programs (Aubry et al., 2015; Cossyleon et al., 2020). A sociology review summarizes that landlord-tenant relationships, together with economic and policy forces, create housing insecurity (DeLuca and Rosen, 2022).

bargaining offer $o_i \in \mathbb{R}$ to her landlord to repay exogenous rental debt. The offer o_i can be positive or negative; for instance, the landlord may pay the tenant in a "cash for keys" arrangement ($o_i < 0$). Take-it-or-leave-it simplifies exposition but is unimportant for model conclusions (see Appendix B for generalization to Nash bargaining). That rental debt is exogenous is more important, as social preferences could shape whether tenants accrue debt, and is explored in the appendix.

Formal eviction occurs when at least one party perceives it as profitable relative to bargaining. If the landlord evicts, she gets transfer $t_i \in \mathbb{R}$ from the tenant. For instance, t_i could include the expected value of possessing the unit or recouped back rents. We view informal evictions, including agreements to stay or leave but which avoid court, as a form of bargaining.

Landlords and tenants face net court costs of $k_{Li}, k_{Ti} \in \mathbb{R}$. We normalize bargaining costs to zero. Then, $k_{ji} > 0$ denotes that eviction is costlier to party *j* than bargaining. Conversely, $k_{ji} < 0$ denotes that bargaining is costlier than eviction. Landlord and tenant utility is linear:

$$V_{Li} = \begin{cases} t_i - k_{Li}, \text{ if evicts} \\ o_i, \text{ if bargains} \end{cases} \text{ and } V_{Ti} = \begin{cases} -t_i - k_{Ti}, \text{ if evicted} \\ -o_i, \text{ if bargains.} \end{cases}$$
(1)

Interpretation of Costs. Crucially, court costs are written *net* of bargaining costs, and they could be positive or negative. In standard settings, court is often modeled as costlier than bargaining (positive k_{ji}). For instance, court imposes legal fees on landlords, and evictions have negative financial impacts on tenants; bargaining could be free. Yet, in our setting, bargaining may be costlier than court (negative k_{ji}). For instance, parties could be hard to contact, or view bargained agreements as costly to enforce. Alternatively, the landlord could worry that bargaining makes her seem "soft," reducing her ability to collect rents from other tenants.

Net court costs capture credit constraints and some dynamic forces. On credit constraints, as long as they do not completely bind, tenants could always borrow and negotiate. For instance, they could take expensive debt like payday or pawn-shop loans. Then, credit constraints raise k_{Ti} . On dynamics, the cost terms can include net present discounted rent flows or vacancy costs.

Benchmark. With frictionless bargaining, a Coase Theorem holds:

Proposition 1. Eviction occurs if and only if

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

The positive implication of Proposition 1 is that evictions occur if and only if bargaining costs exceed court costs, and bargaining occurs otherwise.

The proposition also has normative content. Eviction occurs if and only if it is constrained privately efficient, that is, if total surplus from eviction is positive. This notion of *constrained*

efficiency takes existing costs as given, and says that parties choose the cost-minimizing outcome. Our concept is suitable if the normative goal is to say whether, fixing the bargaining technology, the less-costly outcome occurs.⁷ This notion of *private* efficiency excludes externalities on society including homelessness or uninternalized negative impacts on children (Collinson et al., 2024, 2025b). Private impacts of eviction are still important if they reduce or raise the magnitude of externalities that could rationalize policy intervention.

1.2 Adding a Friction: Misperceptions

Bargaining is probably not perfectly frictionless. Misperceptions are a natural friction to include. First, economic theory emphasizes beliefs as a cause of bargaining breakdown. 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.

Eviction with Misperceptions. Landlords and tenants may now disagree about the court transfer. The terms t_{Li} and t_{Ti} denote the landlord's and the tenant's beliefs about the transfer if evicted. An example of misperceptions, which we focus on empirically, is if landlords believe they will receive back rents if they evict. We say there are "misperceptions" if beliefs do not coincide, $t_{Li} \neq t_{Ti}$. We write the difference in beliefs, or "misperception wedge," as $\Delta t_i := t_{Li} - t_{Ti}$.⁸

It is not conceptually important that misperceptions are about the transfer t_i . Misperceptions about many aspects of the bargaining game, including court costs, produce inefficiency. However, this way of writing misperceptions corresponds to the experiments.

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).

Misperceptions change the eviction condition:

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

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

We illustrate Proposition 2 in Diagram 1. We often consider $\Delta t_i > 0$, as this condition implies that landlords see formal eviction as more favorable for the landlord's payoffs than tenants do. Without misperceptions, evictions occur only if joint surplus $k_{Li} + k_{Ti}$ is less than zero (green shaded region). If $\Delta t_i > 0$, then misperceptions generate more evictions (blue shaded region), as eviction now occurs for $k_{Ti} + k_{Li} \in (0, \Delta t_i)$. Intuitively, in the blue region, neither party can make

⁷To see what we mean, suppose bargaining is slow, so net court costs are negative. Calling eviction "efficient" places what some may call a friction — that bargaining is not instantaneous — as a normatively respected cost.

⁸/Misperceptions" is shorthand for "non-coincident beliefs." With equal misperceptions, the wedge is zero.

offers that the other party would accept. Yet, efficient side payments could make both parties better off. The additional evictions that misperceptions cause are inefficient, as bargaining would yield positive joint surplus.



Diagram 1: Comparative Statics

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

1.3 Adding Social Preferences

Becker (1974)-type social preferences $a_{Li}, a_{Ti} \in (-1, 1)$ now enter utility. The value $a_{ji} = 0$ implies that party *j* is classical and unaffected by the other party's utility; $a_{ji} > 0$ implies altruism; and $a_{ji} < 0$ implies hostility. Utility remains linear. Payoffs are:

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

(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, $a_{Ti} = -0.5$ means the tenant gets -10 utils plus her own payoff.⁹

⁹Parties do not internalize the others' social payoffs. That is, the tenant ignores if the landlord gets altruistic utility from giving the tenant money, and vice-versa. Richer models of social preferences include impure altruism (Andreoni, 1990), moral obligations (Rabin, 1995), inequity aversion (Fehr and Schmidt, 1999), or reciprocity (Charness and Rabin, 2002), among others (see Fehr et al., 2023). A material share of the population having genuinely hostile preferences could rationalize how posturing as hostile might persist (as in Abreu and Gul, 2000). We use the word "hostility," not "spite" (Levine, 1998), to avoid connoting dynamics.

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.

Social preferences change the eviction condition:

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

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

for "compound altruism" $\overline{a}_i \coloneqq (1 - a_{Li}a_{Ti})/((1 - a_{Li})(1 - a_{Ti}))$.

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

Equation (5) gives three insights. First, a positive implication is that hostility can cause evictions. Graphically, more hostility ($\bar{a}_i \downarrow$) flattens the slope of Diagram 1's red ray and increases the region in which eviction occurs (red shaded area), assuming positive court costs ($k_{Li} + k_{Ti} > 0$) and misperception wedge ($\Delta t_i > 0$). Intuitively, the hostile party must be excessively compensated so they do not impose extra costs on their counterpart. Meanwhile, altruism sustains bargaining that would otherwise fail due to misperceptions. Here, parties believe they get private utility from eviction, but want to avoid imposing costs on the other party.

Our second insight is that social preferences and misperceptions are complements for eviction. Absent misperceptions ($\Delta t_i = 0$), social preferences vanish from Equation (5), and the proposition reduces to the benchmark (Equation 2). Hostility is insufficient for eviction because parties get equal utility from harsher bargained side-payments as from imposing costs. However, hostility only vanishes if misperceptions are precisely zero. Otherwise, hostility blows up even minor misperceptions, as hostility can make \bar{a}_i arbitrarily small. Hostility could thus be important in real-world bargaining, where beliefs are unlikely to *exactly* coincide.

Third, a normative insight is that hostility creates inefficient outcomes, amplifying misperceptions that cause parties not to maximize joint surplus. As Diagram 1 shows, new evictions caused by hostility are always inefficient. And when misperceptions go in the other direction ($\Delta t_i < 0$), hostility amplifies them to cause inefficient bargaining. Meanwhile, altruism pushes toward efficient outcomes by reducing the wedge from misperceptions.

These conclusions about efficiency hold regardless of whether the planner includes social preferences in welfare (but see subtleties in Appendix B). As hostility only creates eviction by amplifying existing misperception wedges, any eviction created by hostility is already inefficient, whether or not hostility enters welfare. Hence, the sign of the welfare loss from a given eviction is "normatively robust" to reasonable philosophical disagreement about whether to value utils from hostile social preferences (Bernheim and Rangel, 2009; Naik and Reck, 2025). However, the

magnitude of the welfare impact does depend on whether social preferences count, so in empirical calibration, we show magnitudes both ways.

Extensions. Appendix **B** extends the framework to a two-period dynamic setting with endogenous rental debts, concave tenant utility, and Nash Bargaining. Enough poverty or impatience recovers the same forces as in the text.

1.4 The Role of Emergency Rental Assistance

The appendix models a stylized ERAP. The program pays tenants' back rents, if landlords pay a take-up cost.

This exercise yields two insights. First, ERAP's targeting is ambiguous. From a positive perspective, the program could have small or large impacts on evictions. From a normative perspective, it is unclear whether evictions averted would have been inefficient. The same key forces — net court costs, misperceptions, and social preferences — govern whether households most likely to take up ERAP would otherwise have faced (inefficient) eviction.

Second, altruism can reduce ERAP's impacts on eviction because of "selection on altruism." Intuitively, altruistic landlords are more willing to pay the ERAP take-up cost, as their outside option (whether eviction or bargaining) is less valuable. That is, program enrollees are often altruists. Yet, such altruists rarely evict to begin with. This theme relates to research on mechanism design with social preferences (Bierbrauer et al., 2017).

From the Model, to Empirics. Social preferences may influence eviction and its policy implications. The model suggests a roadmap for empirical work. First, we elicit social preferences via experiments, along with a friction (misperceptions). Second, we relate primitives to eviction, and study ERAP's impact. Third, we estimate eviction's costs by matching experimental and observational moments to model-implied behavior. Costs in hand, we perform normative analysis.

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. Tennessee has landlord-friendly eviction law. If the landlord seeks an eviction judgment, tenants rarely win in court.

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 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 (many leases waive the right to a notice). 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 (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 the sheriff to remove the tenant and their belongings 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 to obtain 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 March 2021 (several months before the national expiry of the moratorium), only 40% of filings yielded judgments. The lower level of judgments persists throughout our study period.

In the model, bargaining is any arrangement that avoids a court judgment. For instance, court settlements, cash-for-keys arrangements, repayment plans, or abandoning a filing so it does not yield a judgment — all reflect bargaining. Both the implicit and explicit forms of bargaining are common. 57% of surveyed tenants have formed repayment plans to repay back rents over time to landlords (see below), consistent with the more than 70% who report informal forbearance in Collinson et al. (2025a). Even outside the pandemic, around 40% of court processes do not conclude in judgments (Figure A1). The richness and prevalence of bargaining (and bargaining failure), together with eviction's high stakes and possibly important welfare consequences, underscore the appeal of this setting.

ERAP. Under the CARES Act and Consolidated Appropriations Act of 2021 (CARES II), state and local governments received federal 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 Memphis ERAP gave at least \$100 million in assistance to around 20,000 households. Additional ERAP details are in Section 5.4.

2.2 Experiment Overview and Recruitment

We conduct separate experiments with landlords and tenants. Figure 1 visualizes the survey flow. There are two main sections to each experiment. First, Dictator Games measure social preferences (Section 3). Then, we elicit misperceptions (Section 4). Appendix C gives experiment details including: examples of survey instruments; information on incentives; and minor deviations from pre-registration. Full survey instruments are posted online.

Experiment Sample Recruitment. We recruit participants by contacting any records with valid contact information in the Memphis ERAP system. Not all landlord or tenant participants ultimately received ERAP payment, because their application was incomplete, denied, or never processed by case workers.

Our sampling frame conditions on some involvement with ERAP. Up to selective participation, our study is internally valid for this sample of landlords and tenants who are "marginally attached" to ERAP. This sampling frame has two practical advantages. First, outreach materials include the ERAP logo, which probably raised response rates. Second, the ERAP database includes tenants with overdue rents but before the court process started. Finding and recruiting at-risk tenants before eviction is otherwise challenging. While our sampling frame is reasonable, it is not identical to the ideal but infeasible sampling frame of all at-risk tenants in Memphis or the U.S.

Recruitment materials, sent via personalized email and text, informed participants that they would receive a \$20 gift card for completing the survey and could earn other rewards. We conducted all surveys online via individual Qualtrics links associated to unique ERAP identifiers.

For the landlord survey, we contacted 3,966 unique email addresses associated to landlords and property managers listed on tenant ERAP applications from August 29 to October 28, 2021. We received 371 valid responses, for a response rate of 9%. For the tenant survey, we contacted 16,861 unique tenants from February 14 to May 30, 2022. We obtained 1,808 valid responses, for a response rate of 11%. In both surveys, we sent an invitation email, followed by at least one reminder, as well as text message invitations. Response rates are conservative as emails or texts sometimes bounced.

We recruit landlords and tenants separately, for two reasons. Practically, it is hard to get both landlords and their tenants to enroll in the same study. Ethically, we worried that jointly recruiting parties into the Dictator Games might cause harm or retaliation. Given low absolute participation rates, only a small number of matched landlord–tenant pairs happen to take the survey (by chance, without being informed that their counterpart did). A limitation is that, for quantitative estimation of the model, we require assumptions about landlord–tenant matching. As we describe below, DGs are one-way anonymous; that is, parties know that they play against their own landlord or own tenant, but the opponent typically does not participate in the study.

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). Tenants owe a median of around \$3,500 when they applied for ERAP and have average incomes of less than \$1,000. About a third have ever been evicted, and almost 90% have had overdue rents (Panel B).¹⁰

Landlord Characteristics. 58% are Black, and 62% of landlord participants are female (Table 2, Column 1). 62% are landlords, and 29% are property managers, and the remaining share is other employees at larger landlords or landlord legal counsel. We refer to all these 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). 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, and are slightly more likely to be white.

Balance and Attrition. Tables in Appendix A and the supplement show attrition and balance. Conditional on completing a demographic questionnaire to start the survey, 77% of tenants and 65% of landlords complete the survey. Our sample consists of participants who complete.¹¹ 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).

3 Altruism and Hostility: Measurement and Results

3.1 Measurement

Background. We adapt the laboratory experiment called the Dictator Game (DG). In a standard DG, the Dictator has an endowment. The experimenter elicits how much of the endowment the Dictator wants to give to another player, the Opponent. Classical models predict that the Dictator gives \$0. Engel (2011)'s meta-analysis finds that 65% of Dictators give positive amounts.

¹⁰We 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.

¹¹Attrition rates are constant across treatments (joint *p*-value > 0.2 for both surveys, Table A1). 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.

Modified Dictator Game. Consider a Dictator who gives \$0 in the standard DG. It is ambiguous whether she has classical preferences and maximizes her own payment, or if she has hostile preferences and derives utility from reducing her opponent's payment.

Our modified DG, similar to Charness and Rabin (2002), can detect both altruism and hostility in one elicitation. In the modification, the Dictator chooses between two bundles: (s to Dictator, 0 to Opponent) and (s to Dictator, s to Opponent). In the first bundle, the Dictator gets s and the Opponent gets nothing. In the second bundle, both players get s. 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 prefers whichever bundle gives a larger private payoff. With altruism, S(x) > x: the Dictator forgoes some private payoff so that the Opponent receives a payoff. With hostility, S(x) < x: the Dictator forgoes some private payoff so that the that the Opponent receives \$0.

The game exactly delivers the Becker (1974) altruism parameter. With linear prefrences, individual *i*'s altruism parameter $a_i \equiv (S_i(x) - x)/x$, as $S_i(x)$ must satisfy

$$\underbrace{S_i(x) + a_i \cdot 0}_{\text{Payoff from } (S(x), 0) \text{ bundle}} = \underbrace{x + a_i \cdot x}_{\text{Payoff from } (x, x) \text{ bundle}} .$$
(6)

Relative to the standard DG, this modification yields altruism and hostility in one shot.

Rates of hostility vary from zero (Charness and Rabin, 2002) to about 40% (Abbink and Sadrieh, 2009) in similar DGs or "joy of destruction" games. Most other estimates of social preferences are from university subjects (e.g., Charness and Rabin, 2002; Abbink and Sadrieh, 2009; Abbink and Herrmann, 2011; Kranton et al., 2016), or people who do not know each other (e.g., Ortiz, 2024). It is unclear how much hostility is present among parties with existing relationships in high-stakes bargaining situations.

Elicitation and Opponents. We summarize important aspects of elicitation (see details in Appendix C). We employ best practices in experimental economics. We implement a small share of DG choices to ensure incentive-compatibility. Bundles are Amazon gift cards. We provide plain-English instructions (see Figure A2A for an example elicitation). We tell participants that it is in their best interest to respond truthfully (Danz et al., 2022).

We elicit indifference points using a multiple price list (MPL). We repeatedly ask whether a participant prefers (s, 0) or (x, x). We vary s in increments of x/10 until the participant switches her response. The value of s could range from s = 0.1x to s = 2x. Elicitation imposes monotonicity.

We impute *s* as the midpoint of the bounds implied by the MPL. With preferences (0.1x, 0) > (x, x) and (x, x) > (2x, 0), we impute S(x) = 0.05x and S(x) = 2.05x.

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 Motives and Anonymity. For the DGs to capture social preferences, it is important that parties do not have instrumental reasons to appear altruistic or hostile. Most instrumental reasons apply only if the parties can credibly signal their DG behavior to others. An example is if landlords have classical social preferences ($a_{Li} = 0$) but nevertheless want to appear hostile in the DG, to signal toughness to other tenants. Alternatively, tenants could share money with their landlord, to appear trustworthy and avoid eviction.

Our protocol addresses this confound because Dictators are anonymous. Dictators know Opponents' identities, but are told that Opponents do not know Dictators' identity. We can truthfully maintain this one-way anonymity because we recruit landlords and tenants separately, and both parties do not need to participate. Therefore, we do not tell Opponents if they are enrolled in a DG. If the Dictator gives the Opponent nothing in a DG, including if hostile, we send the Dictator money and do not communicate anything to the Opponent. If the Dictator gives money, we send the Opponent money but do not say why, except that the money comes from MIT and ERAP.¹² To the Dictator, we never imply that the Opponent learns the Dictator's choice.

One worry is that participants misinterpret the protocol and incorrectly perceive it is not anonymous, so that our DGs partly reflect instrumental motives. Misinterpretations are always possible, but we think they are unlikely. We inform participants when they provide consent that their responses are anonymous. We remind tenants in the DG that "the gift card will not be associated with your name and won't count as rent" and include a confirmation check. 88% of tenants correctly answer the comprehension check about anonymity. We correct those who fail. That said, with landlords, we discuss anonymity only at the point of consent.¹³

We cannot rule out instrumental motives entirely. For instance, we cannot stop parties from talking outside the experiment. Participants could screenshot the experiment and show their Opponents what they chose. Or participants might not read consent language closely. Some free responses do mention instrumental reasons for DG choices.

After presenting the results, we give several pieces of evidence against this threat being

¹²Even if Opponents get a gift card, they cannot deduce that it came from their own landlord or tenant, as Opponents may also receive gift cards from random landlords or tenants and randomization probabilities were not disclosed.

¹³The tenant confirmation check and reminder was added on March 27–28, 2022 and covers about 70% of the sample. At the time of the landlord study, we worried that stressing anonymity in the DG would make it feel less realistic. We decided in the tenant survey that this cost was worth the benefit of shutting down any concerns regarding anonymity.

quantitatively meaningful.

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.

Strengths and Weaknesses 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.

DGs also have limitations. As List (2007) and others have argued, DG giving is sensitive to subtle changes in elicitation procedures. Another compelling criticism is that people do not give as much money to strangers outside the lab, so DG giving must reflect more than social preferences alone. Our setting sidesteps these concerns, at least partly, if they are more important for small-stakes DGs with anonymous parties. We build confidence with several validation exercises (Section 3.3) and show that DGs predict out-of-lab behavior (Section 5). Ultimately, while we take DG-elicited social preferences literally for welfare (Section 6), it is reasonable to view them as reflecting some measurement error.¹⁴

3.2 Results

Levels of Hostility and Altruism. Cumulative Distribution Functions (CDFs) in Figure 2A show raw data from DGs played against own landlords and own tenants. Table A2 summarizes the data and presents comparisons against random opponents.

Landlords and tenants exhibit substantial hostility. 15% of landlords are hostile toward their own tenant (cumulative share to the left of the dashed vertical line in Figure 2A). 24% of tenants are hostile toward their own landlord (*p*-value of landlord–tenant difference < 0.01). Most hostile parties are "highly hostile" and prefer (x/10,0) to (x, x) bundles (i.e., CDFs are fairly flat from 0 to 100). To express high hostility, participants must repeatedly reject the even-split bundle in favor of a hostile bundle, ruling out trembles or misclicks as complete explanations for hostility.

Altogether, 24–39% of landlords and tenants have at least one party expressing hostility toward their own tenant/landlord, depending on matching between hostile landlords and tenants.

¹⁴If the DGs partly reflect richer psychological forces (e.g., fairness), then DGs may measure "as-if" social preferences rather than the "deep" primitives suggested by a Becker (1974) model. Such as-if preferences could still capture how parties behave in actual bargaining, but they may yield different a normative interpretation.

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 D).

Yet, most participants are quite altruistic. In fact, more than two-thirds of landlords and half of tenants are "highly altruistic," meaning they prefer the even-split bundle to doubling their own payoff (i.e., $(x, x) \succ (2x, 0)$). These high rates of tenant altruism toward landlords are notable given their precarious housing situations.

On 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 A3).

Contrasting DG Behaviors with Comparison Groups. Parties are significantly more hostile to own landlords/tenants than random tenants (Rows 4–6 and 10–12 in Table A2). Landlords are 8.8-pp more hostile to own versus random tenants (57%, Row 4; *p*-value of difference < 0.01). Tenants are 9.7 pp more hostile toward their own landlords versus random tenants (40%, Row 12; *p* < 0.001). Meanwhile, landlords and tenants have similar rates of hostility to own tenants/landlords as they do to random landlords. The fact that parties are similarly hostile to random landlords may suggest some hostility to landlords as a class, even among other landlords.

Differences in high hostility are also significant. 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 < 0.05, Table A2).

Landlords are 6.5 pp more likely to be highly altruistic to their own tenant than a random landlord (p < 0.05, also Table A2). Tenants are less likely to be to be highly altruistic to their own or random landlord than random tenants.

Next, we compare our samples to random online survey participants recruited in Memphis (N = 275) and a nationally representative sample (N = 623) during the study period (see Appendix D for recruitment details). Random-sample participants play against random landlords and tenants in our study. Tenants' hostility toward own landlords exceeds the pooled random sample's hostility toward random tenants by 11 pp (Figure A4, p < 0.001) and toward random landlords by 4.4 pp (p = 0.01). Landlords in the study are 14 pp more likely to be highly altruistic toward their own tenant than the pooled random sample is to a random tenant (p < 0.001). Landlords' hostility and tenants' altruism is more similar to the general population.

Summing up, landlord-tenant social preferences are more polarized than those toward random parties or among random samples. In particular, tenants are more hostile to their own landlords, and landlords more altruistic to their own tenants, than comparison groups; additionally, landlords are more hostile to their own tenants than random tenants. That said, other differences between landlord-tenant DGs and comparison groups (including in the literature) are sometimes smaller. Our interpretation is that the landlord-tenant DGs elicit a combination of relationship-specific and

relationship-invariant preferences. The level of social preferences among landlords and tenants is what matters for eviction, not excess social preferences relative to others. If someone is hostile to everyone, that could certainly affect how they bargain with their landlord. We show that social preferences predict bargaining and policy outcomes, mitigating such concerns.¹⁵

It is perhaps surprising that parties are hostile or altruistic to random strangers at all, although that is what Kranton et al. (2016) and others find in the laboratory. Hostility toward random tenants could reflect negative affect toward low-income households, which sociology research documents among the poor (Shildrick and MacDonald, 2013). One caveat is that some hostility toward random opponents (but not own opponents) partly reflects anchoring. Recall that each participant does the DG twice, in random order. When we consider behavior in only the *first* DG, 3% of landlords and 8% of tenants are hostile to random tenants (Table A3). Crucially, Table A3 shows that anchoring in behavior toward *own* opponents is not affected by anchoring issues. That is, in the sample of first DGs only, levels of hostility toward own tenants and own landlords are similar to the pooled estimates, and statistically different from the random opponents. This motivates using the full sample in the rest of the paper, as anchoring only attenuates comparisons to benchmarks, and does not influence levels of the focal estimates.

3.3 Validations

One may worry that the high levels of altruism and hostility reflect inattention, confusion, or other artifacts of the lab setting. However, dropping inattentive participants does not affect results (Tables A4–A5). Significant differences by opponent allay many possible issues. For instance, tenants are unlikely to be more attentive to DGs played against a random tenant than against their own landlord, or more influenced by experimenter demand.

We now discuss several confirmatory tests using elicitations we built into the tenant survey (see Appendix C for details and similar checks for landlords).¹⁶

Test 1: Free-Response and Qualitative Questions. Free-response questions suggest the presence of strong social preferences. We ask tenants, "Do you have any thoughts about [landlord name] that you want to share? These responses will be kept private from them but may be used anonymously for research purposes." 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 was willing to give up \$600–\$700 so that their landlord would receive \$0. Yet, another tenant wrote that their landlord "is a very good person and is willing to help the very best she can."

These are not randomly selected anecdotes; in fact, we cherry-picked the best ones. A more principled analysis codes the sentiment of tenants' free responses, using Natural Language

¹⁵Whether to call relationship-specific affect a "social preference" is a fair question, as "preference" could connote generality. For our research question, interpretation is similar, if we call them preferences or something else.

¹⁶We added some validations on March 27, 2022, so they are available for about 70% of participants.

Processing. Free-response sentiment strongly correlates with hostility (Figure 2B).

Tenants also rate their relationship with their landlord using a Likert scale. Their ratings strongly correlate with own-landlord DG behavior (Figure 2C). As a falsification test, tenants' qualitative assessment of their relationship with their landlord exhibits far less correlation with random-tenant DG behavior.

Test 2: Stakes Sub-Experiment. One may worry that pervasive hostility and altruism are smallstakes phenomena or reflect minor trembles. We experimentally manipulate stakes by two orders of magnitude and find no evidence that stakes affect behavior (Figure 2D). More than 20% of tenants are hostile with x = \$1,000, meaning they give up at least \$100 when they could instead give \$1,000 to both parties (Appendix Figure A6C shows stability of high altruism by the stakes). It is harder to argue that trembles cause tenants to give up hundreds of dollars. This result contributes to a conflicted literature on the importance of stakes in social-preference experiments (e.g., Andersen et al., 2011).

Test 3: Additional Revealed-Preferences Measure. One may worry that the DG task is complicated. We embedded a simpler measure of revealed hostility, which gives similar findings.

We ask tenants whether they want to enroll their own landlord in a lottery for a gift card. Enrolling the opponent is costless, so not entering the opponent indicates hostility.

This elicitation is also useful because we emphasize anonymity. Thus, instrumental motives are less relevant.

Rates of hostility in the lottery are almost identical to the rates of hostility in the DG, and are positively correlated with DG behaviors (Figure A5A). 38% of tenants are hostile in either the DG or the lottery, and 14% are hostile in both. As a benchmark, we also elicit whether the tenant would enter a random tenant in a lottery, and behaviors toward own landlords are more hostile.

Test 4: Global Preferences Survey. Falk et al. (2018) validate survey questions that capture incentivized economic primitives. DG behaviors correlate with their measures of primitives like altruism and reciprocity (Figure A5B).

Other Concerns. One may worry 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, this concern cannot explain behavior of landlords who are highly altruistic or exhibit hostility (more than 80%). Second, we use landlords' elicited beliefs about the amount of money they can recoup from tenants in an eviction (see Section 4) to scale down their altruism. Naturally, landlords become less altruistic and more hostile. But preferences remain extreme (Figure A7).

One may worry about selection into the study. For instance, people who complete the study if invited could be more altruistic, which would 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 A2 vs. Table A7), suggesting that our main results on hostility are conservative. Differences are small, however. 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 A6).

4 Misperceptions: Measurement and Results

The model suggests that a friction — e.g., misperceptions — is necessary for social preferences to have bite. Therefore, we document the existence of misperceptions in this market.

It is reasonable to be skeptical of field-elicited beliefs data. Participants' lack of numeracy or pull-to-center reporting biases may lead us to overstate misperceptions. On the other hand, hostility can greatly amplify even minor belief disagreements (Section 1). Said otherwise, our empirical claim in this section is more narrow: it suffices if there exist misperceptions, even if there is uncertainty about magnitudes.

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.

We elicit participants' prior beliefs about an observed statistic that we can benchmark to ground truth, corresponding to the average behavior of landlords or tenants. We provide information about this statistic. Once we give information, it is not meaningful to elicit posterior beliefs about the treatment information. Instead, to capture belief updating, we also measure participants' prior and posterior beliefs about how their own landlord or tenant would behave.

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. 6% 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, 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.

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. We elicit beliefs about this fact soon after tenants play the DG, so the elicitation is natural. We also elicit the subjective probability that the tenant's *own* landlord would evenly split the gift card.¹⁷

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. 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. 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 beliefs about own tenants/landlords are correlated with beliefs about the average.

We design belief elicitations and treatments to be as natural as possible. Elicitations include confirmation questions and visual aids (Figure A2B shows an example). For instance, after some elicitations, we restate percentages as equivalent fractions (e.g., $20\% \rightarrow 1$ in 5) and ask: "This means you think there is a [fraction] chance that your tenant would repay. Is that right?" Participants must confirm or revise their initial response. To ensure that participants intend to update, we first ask participants whether their prior belief is "too high," "too low," or "still correct."

¹⁷Our focus on beliefs about others' altruism relates to laboratory evidence on how people may possess distorted views of others' social preferences (Di Tella et al., 2015).

Then they must report a posterior consistent with the direction of the reported belief update. We equate the control group's posteriors with their priors since they did not get information.¹⁸

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 giving information leads participants to change behaviors. Among landlords, we give opportunities to indicate interest in participating in ERAP; for instance, whether they choose to receive informational materials about applying. We give tenants with positive back rents a real opportunity to propose bargained agreements to their landlords.

Connection to the Model. Imagine the landlord sees the expected transfer as $t_{Li} = p_{Li}d_i + \bar{t}_{Li}$, where d_i is back rents, p_{Li} is the recoupment probability, and \bar{t}_{Li} is the landlord's expectations about other aspects of the transfer like possession of the unit. We elicit p_{Li} . We did not elicit tenants' beliefs about the court eviction process symmetrically because doing so introduced ethical concerns. For instance, we did not want to provide tenants with information that tenants rarely repay eviction judgments, as that could harm landlords or encourage tenants to break the law. 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 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.

4.2 Results

Landlords and tenants have misperceptions that push toward eviction. 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%.¹⁹

Tenants have two beliefs that may suppress bargaining. They underestimate landlord altruism (Figure 3B). The average tenant reports that 47% (s.e. 0.84) of landlords would choose (\$10 self, \$10 own tenant) to (\$20, \$0), when in fact 69% do (vertical red line). Similarly, we find that tenant

¹⁸This procedure assumes that the act of eliciting beliefs does not move beliefs. We verify this assumption with a pre-registered placebo belief for landlords (Appendix D).

¹⁹Landlords' 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). It seems unlikely that these landlords are accurate, since it would render these landlords extraordinarily effective relative to the average. Beliefs about own and average tenants are highly correlated (Figure A9A), which further suggests that landlords are biased about their own tenant.

beliefs about landlords' propensity to drop eviction filings in the future are overly optimistic (Figure A10C).

Turning to our second outcome, belief updating, 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).

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 A10). 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).

Landlord Experience. Landlords who have ever evicted a tenant have 7 pp more accurate beliefs on average (Figure A9B), although beliefs remain imperfect. Eviction experience is more than half as effective in reducing mean belief error as directly receiving information ($0.58 \approx 7/12$).

Summary of Misperceptions Data. We obtain lower bounds on the share with misperceptions by calculating the percent of participants whose prior beliefs are at least 20 pp (unsigned) from truth (Figure 3C). 33% of landlords and 79% of tenants have misperceptions by this measure. And 17% of landlords and 41% of tenants have such incorrect beliefs and also update when told the truth. These lower bounds suggest that at least one in five participants is truly misinformed.

One may be concerned that lack of numeracy or reporting biases would lead participants to report noise. Elicitation checks discussed above should mitigate this concern, but we test it in two ways (Table A8). First, we ask participants how certain they are about their prior beliefs about the average. Restricting to participants who express certainty actually raises misperception rates. Second, dropping inattentive participants or those with no college education sometimes improves beliefs, but magnitudes of misperceptions are large in the remaining sample.

Revealed Behaviors. Appendix D presents suggestive but mixed evidence that information affects behaviors. These results are consistent with moderate effect sizes in the information-provision literature (Haaland et al., 2023; Yang, 2023). For landlords, information raises the probability of requesting informational materials about ERAP by 17%. However, we find null effects on other outcomes like referring tenants to ERAP. For tenants, some specifications detect effects of providing information about landlord altruism on bargaining.

4.3 Information Experiments and Inefficiency

The information experiments suggest inefficient bargaining failure in two model-free ways.

First, experiment data suggest landlords' court behavior is inefficient. Recall that more than

half of landlords pursue "money judgments." Landlord behavior could be inefficient because tenants rarely repay money, and money judgments are costlier to obtain (Section 2). As we find that landlords erroneously believe tenants will repay, these behaviors are harder to reconcile with classical explanations like signaling toughness to other tenants.

Second, survey data on payment plans suggest inefficient absence of bargaining. As noted, the tenant experiment gives tenants with back rents an opportunity to propose a payment plan to their landlord. We sent landlords a survey along with the tenants' payment plans, asking whether they would accept the payment plan. Out of 691 payment plans sent (including bounced emails), we received 96 responses (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 under worst-case assumptions about non-respondents, a minimum of 6% of landlords accept or consider the payment plan. The payment plans were costless to propose to landlords, as we simply emailed tenants' survey responses. Taken together, these findings suggest that tenants under-bargain because they misperceive landlords as intractable.

5 Connecting Social Preferences to Evictions and Policy

5.1 Social Preferences and Eviction

We now show that social preferences correlate with eviction and bargaining outcomes. However, caution is warranted in interpreting the correlations, as we lack an exogenous shock to social preferences. Causal interpretation requires a selection-on-observables assumption that probably does not hold exactly. After presenting the evidence, we discuss remaining concerns. Quantitative estimation targets these correlations as causal (Section 6).

Main Test. We fuzzy-merge the experiment data to court records on evictions (Appendix D). We merge landlords' and tenants' DG behavior against their own tenant or own landlord, to whether the tenant has an eviction record at that address. We create indicators for having a filing or a judgment within 1 year before to 2 years after survey completion (2.5 years for judgments to account for the court process). We include court records from before the DGs, to avoid restricting to evictions that hostility did not trigger quickly. That is, if we only examined evictions after the DGs, but hostile parties receive evictions quickly, we could understate the correlation between hostility and eviction.

We restrict the sample to ease interpretation. Some tenants in the survey moved between ERAP application, when they provide contact information, and survey participation. Movers recently reset their relationship and may not be at risk of eviction. We drop about 5% of tenants who moved between ERAP and the survey and have zero back rents (implying less-housing-insecure). We do not need to make this restriction with landlords. Landlord survey participants are recruited from the contact information that their tenant supplied at ERAP application. So, their experiment

behavior corresponds to their ERAP tenant, who expressed housing-insecurity in this relationship. We do not restrict on possible ERAP receipt, so correlations are net of the impact of this policy.

We stack the landlord and tenant experiments and estimate the model

$$y_i = \beta \text{Hostile}_i + \lambda \text{Tenant}_i \ (+X_i\gamma) + \varepsilon_i, \tag{7}$$

where Hostile_i is an indicator that is 1 when participant *i* is hostile toward her own landlord or own tenant in the DG. X_i are optional controls. Tenant_i is an indicator that is 1 when *i* is a tenant. The outcome y_i is a filing indicator that is 1 when the tenant in the DG played by *i* ever has a filing at the address within the above window, or a judgment indicator that is 1 if she ever has a judgment. The coefficient of interest β shows the descriptive relationship between DG-hostility and eviction, pooling experiments.

Filings and judgments correlate with hostility (Table 3A). Panel A shows that being hostile toward one's own landlord or tenant correlates with a 9.5 pp (s.e. 3.3, p = 0.004) increase in having a filing, a meaningful increase off the non-hostile mean of 39%. We find similar magnitudes in percent terms for judgments (7.1 pp, p = 0.02). We disaggregate landlords and tenants in Columns 3–6. Although within-experiment relationships are naturally less precise than our stacked estimates, correlations are especially large for landlord hostility. Binned scatterplots visualize the data, leveraging the granular DG indifference points rather than binary indicators for hostility (Figure 4A).

5.2 Supporting Evidence

Controls and Sample Balance. The relationship between hostility and eviction persists when we add controls for observable characteristics or probe sensitivity to sample restrictions (Tables A9 and A10). Including controls also partially addresses the worry that hostility simply proxies for unobserved eviction risk from other classical forces (e.g., if tenants' income correlates with hostility and eviction). We have already shown how hostility in the survey relates to demographic characteristics (Figure A3). Magnitudes are not huge, and there is no clear pattern based on eviction risk. We quantify this point by using logit-Lasso models to predict filing or judgment probabilities using observables. We then assess balance on this measure of predicted eviction risk with respect to hostility. Reassuringly, hostility has a quantitatively small relationship with predicted eviction risk (Table A11). Balance on observable eviction risk explains why our specification is robust to controls.

Falsification: Random DG Opponents. Consistent with relationship-specific affect driving bargaining failure, hostility toward random opponents does not predict filings or judgments (Table 3, Panel B; Figure 4A, orange series).²⁰ For instance, hostility toward a random landlord or tenant

²⁰For landlords whose DGs are against random tenants, we merge eviction records to the tenant who would have

correlates with a small *reduction* in having a filing (Column 1).²¹

Other Bargaining Outcomes. Bargaining outcomes within experiments correlate with social preferences. We form an index of landlord engagement with ERAP, based on demanding ERAP materials, notifying or referring tenants about ERAP availability, and wanting to receive an ERAP contract. Hostility towards their own tenant correlates with a –0.49 s.d. reduction in the index (p < 0.05, Table A12A, Columns 1–4). In an unincentivized elicitation, hostile landlords are 21% less likely to accept an ERAP payment below-rent and waive the remaining rent, thus taking a "haircut"(Columns 5–8).²² For tenants, we form a measure of bargaining proposity, defined as the share of back rents that tenants offer in our incentivized payment-plan exercise. Hostile tenants have 6.5 pp lower bargaining proposity (from a mean of 33%, p < 0.05, Table A12B).

Code Violation Reports. Participants exhibiting high hostility toward own landlords or tenants (S(x) < 0.1x) are 48% more likely to live in addresses with public reports about possible propertycode violations (p < 0.05, Table A13).²³ At the address level, we link to public Memphis data on reports about possible violations. Reports can trigger government inspections, fines, or legal action (Appendix D). Examples of violations include having a "junky yard" or "substandard, derelict structure." Our interpretation is that these reports reflect bargaining failure, as parties could negotiate repairs privately without involving authorities.

In a placebo test (also Table A13), hostility does not correlate with reports of inoperable vehicles or missed trash pick up from the city's waste management department. Thus, hostile parties do not simply live in disinvested housing that gets more code-violation reports. Rather, they live in housing where surplus from landlord–tenant bargaining is left on the table.

Verification of Model Prediction. We test Section 1's prediction that hostility and misperceptions are complements. We separate participants into those with above- and below-median prior beliefs about the average, reversing beliefs when appropriate so above-median beliefs always push toward eviction.²⁴ In isolation, misperceptions or hostility do not correlate with filings (middle bar versus left bar, Figure 4B; judgments in Figure A12A). But having both high misperceptions and hostility correlates with a 11.5 pp (s.e. 4.2) increase in filings.

Testing Reverse Causality. A salient objection is that eviction causes hostility, not the other way around. To test this concern, we leverage cross-sectional variation in when participants take the

been shown in the DG, if we had randomized the landlords into playing against their own tenant.

²¹This finding does not necessarily imply that differences between own hostility and hostility toward random opponents are the key statistic from Section 3, versus the levels. Subtracting off hostility toward random parties would undercount true hostility since many tenants who are hostile toward random tenants are not hostile toward their own counterpart. (Correlation of hostility to own landlords and random tenants: 0.37.)

²²Results for the haircut outcome are marginally significant (p = 0.06 to 0.16 depending on the specification), and hostility does not predict the magnitude of the haircut that landlords accept (Columns 9–12).

²³Only high hostility correlates with these outcomes, and not hostility in general.

²⁴We use prior beliefs, not posterior beliefs, since our outcome includes evictions that take place before the survey. With tenants, we use the average misperception across both measures.

survey with respect to eviction. We see that hostility is flat over time and always higher among DGs linked to filings than DGs without filings (Figure A11). DGs with filings in the 100 days before the survey are almost exactly as hostile as DGs in the next 200 days. Reverse causality would suggest DGs with filings beforehand should be more hostile.

Summing Up and Remaining Concerns. Hostility correlates with eviction and bargaining. Supporting tests lend credibility to the correlations, and are consistent with a causal relationship.

Yet, important concerns remain, and the direction of bias in interpreting the correlation as causal is unclear. Classical measurement error in DG elicitations would attenuate our estimates. Some forms of omitted variables bias could lead us to overestimate the causal relationship; e.g., if hostility correlates with unobserved changes in financial circumstances. Measurement error in controls could also bias estimates away from zero.

A separate issue is whether social preferences are stable or endogenous to eviction-related behaviors. Relationships change over time. Hostility may be triggered by, say, missed rent or neglected repairs. Such stories remain consistent with a causal relationship between hostility and eviction, if deteriorating relationships impede bargaining that would otherwise prevent missed rent from escalating to court cases. Still, a concern is that these behaviors would cause eviction regardless of their effect on social preferences. The findings that hostility is balanced on observable eviction risk, and flat over time, partially address this concern.

5.3 Explaining ERAP's Effects: Selection on Altruism

The model raises the possibility of selection on altruism, if altruists are more likely to take up ERAP despite low counterfactual eviction risk.

Institutional knowledge suggests selection on altruism is plausible. ERAP 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 can sometimes obtain a check for the overdue rent directly.

We link tenant experiment participants to program receipt using administrative ERAP data. We focus on tenants who play the DG against their ERAP landlord. We restrict the sample to tenants where the program did not have discretion to expedite payments due to participation in the legal program or having a utility shutoff (Appendix D, Table A14). We cannot do the same exercise for landlords because we have a small number of landlord survey participants linked to payment status. However, we already showed in Table A12A that altruism correlates with landlord interest in ERAP.

Hostile tenants are 17 pp less likely to receive ERAP (45%; p < 0.01, Figure 5A). Kaplan-Meier failure curves plot the speed of ERAP receipt, and show hostile tenants receive payments more

slowly (orange versus blue lines, Wilcoxon *p*-value < 0.01). Regression versions of Figure 5A show similar results if we control for demographics or tenants' economic conditions (Figure A12B).

Additional evidence corroborates these findings. A first placebo test finds that hostility toward random opponents does not correlate with payment (Figure 5B, gray series). A second placebo test examines cases where ERAP had discretion to expedite payments (excluded from the above analysis). Here, we find an insignificant, wrong-signed correlation between hostility and payment speed (Figure A12C). Consistent with our mechanism, selection on altruism emerges when parties must cooperate, or when ERAP cannot easily circumvent landlord-tenant relationships.

5.4 Policy Evaluation

An implication of the previous patterns — ERAP selection on altruism, and that altruists have lower eviction risk — is that ERAP may have small treatment effects on eviction. We examine this implication directly with an impact evaluation of ERAP. The text presents visual evidence. Appendix D shows event studies and other details.

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 4,600 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 5B shows, among tenants who get ERAP, 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 follows the same 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. Our event studies use 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 compare paid and non-paid ERAP applicants using a differences-in-differences design.

Event studies confirm the graphical evidence and yield tight nulls for impacts on judgments (Appendix D). Even in best-case specifications, 95% confidence intervals include 0, and are consistent with at most a 2 pp reduction in judgments after 6 months. As households receive \$5,600 on average, it is perhaps surprising that the policy has a small effect. To obtain the cost of stopping an eviction, we assume homogeneity and divide the average payment amount by the average treatment effect. The fiscal cost to stop a judgment for six months exceeds hundreds of thousands of dollars (distributed across many landlord–tenant pairs).

Meanwhile, we detect effects on eviction filings at 1–2 months, and mixed evidence on filings at 6 months. The best-case fiscal cost to stop a filing for six months is above \$45,000.

Discussion. Results comport with Collinson et al. (2025a), who also report null impacts on eviction filings and other financial outcomes from Randomized Controlled Trials of five local ERAPs. Consistency with high-quality RCT evidence is reassuring. Three aspects of our study complement Collinson et al. (2025a). First, their RCTs took place during eviction moratoria, limiting scope for impacts on this outcome; their control group has eviction filing rates of 0.02–1.2% within 10 months of assignment, versus over 20% in our evaluation sample. Second, the authors study eviction filings but not judgments. Third, the Memphis ERAP's payments averaged \$5,600, substantially larger than the RCT payments of \$1,000–\$3,400.²⁵ The authors write, "Whether our results generalize to subsequent rounds of ERA (which were typically more generous, prioritized paying-down arrears, and were implemented during the tail end of the pandemic), is an open question" (p. 30).

Our analysis comes with several caveats. First, the event studies of filings exhibit moderate pre-trends. (Pre-trends for judgments are smaller.) The appendix shows pre-trends come from applications outside the normal functioning of the program, and dropping these applications reduces pre-trends without affecting treatment effect estimates. Even so, results are perhaps more credible for judgments than filings. Second, high fiscal costs partially owe to the fact that evictions are rare. Third, 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 also give legal aid, and because the legal arm may complicate identification (see discussion in Appendix D). This sample restriction could lead us to understate the program's impacts.

6 Quantitative Analysis of Eviction Model

We augment the model in Section 1 for empirical calibration. This exercise involves strong and perhaps debatable assumptions. Throughout, we highlight where results are most sensitive to researcher choices. Appendix B contains details and sensitivity checks.

6.1 Empirical Model

Payoffs. We model the landlord's decision to take ERAP payment, beginning from once the tenant has applied. Landlords get payoffs from four possible end states. 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 possibly less valuable as

²⁵In their Harris County RCT, the moratorium was not strictly enforced. There, 1.2% of the control group had filings, and the program paid \$1,200. The other RCTs with larger payments had enforced moratoria.

the tenant has no back rents.

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

$$V_{Li}^{\text{NoERAP,Evict}} = (1 - a_{Li})t_{Li}(d_i) - a_{Li}k_T - k_L + \varepsilon_{i1}$$
(8)

$$V_{Li}^{\text{NoERAP,Bargain}} = (1 - a_{Li})n_i^*(d_i) + \varepsilon_{i2}$$
(9)

$$V_{Li}^{\text{ERAP,Evict}} = d_i + (1 - a_{Li})t_{Li}(0) - k_L^P - a_{Li}k_T - k_L + \varepsilon_{i3}$$
(10)

$$V_{Li}^{\text{ERAP,Bargain}} = d_i + (1 - a_{Li})n_i^*(0) - k_L^P + \varepsilon_{i4}.$$
(11)

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 transfers $t_{Li}(\cdot)$ describe transfers when the tenant owes d_i or 0 to the landlord, which are relevant because they enter court payoffs.

The landlord's payoffs when she does not take ERAP (Equations 8 and 9) 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 a transfer $t_{Li}(0)$ (Equation 10). Alternatively, she can bargain with the tenant to avoid eviction and get the Nash Bargaining payoff (Equation 11).

We assume asymmetric Nash Bargaining, yielding standard expressions for $n_i^*(d_i)$ and $n_i^*(0)$ (see Appendix B). Nash payoffs depend on tenant bargaining power $\beta \in [0, 1]$. When $\beta = 1$, bargaining corresponds to a take-it-or-leave-it offer by the tenant, as in Section 1.

Timing and Landlord Maximization Problem. Landlords draw shocks ε_{ik} . These values are known to both parties. The landlord then chooses the actions ERAP/No ERAP and Evict/Bargain which maximize payoffs (see full problem in Appendix B).

6.2 Estimation

We let ε_{ik} be type-I extreme value with scale parameter σ . We estimate parameters $\Theta := \{k_L, k_T, \beta, \sigma\}$. Values $X_i := \{a_{Li}, a_{Ti}, t_{Li}, t_{Ti}, d_i, k_L^P\}$ are either observed or calibrated.

Random shocks ε_{ik} induce model-implied shares of landlords in each of the end states. The experiment delivers the distribution of beliefs, social preferences, and other data among the landlords and tenants. Given these inputs into model-implied landlord choices, we solve for the parameters that match a series of moments (described below), using the Method of Simulated Moments (MSM). Simulation-based estimation is useful because the shocks change the choice sets and give non-standard take-up probabilities.

Preparing the Data for Estimation. Data are landlord-tenant pairs. Much of X_i come from the experiment. For social preferences, we convert DG indifference points against own opponents to altruism via Equation (6). As preferring (x, x) to (2x, 0) in DGs implies $a_{ji} > 1$ outside the admissible interval $a_{ji} \in (-1, 1)$, we top-code at $a_{ji} = 0.95$.

We form expected transfers using the beliefs data. We assume transfers t_{ji} are given by

$$t_{Li}(x) = p_{Li}x + \bar{t}_i, \tag{12}$$

where p_{Li} are beliefs about court recoupment. Misperceptions do not enter with ERAP because x = 0. For tenants, we assume that beliefs p_{Ti} , which we do not measure, have analogous deviations from ground truth as the beliefs that we do measure (see Appendix B). We calibrate \bar{t}_i as worth two months rent, to capture the value of possessing the unit. While these assumptions are imperfect, they prove inconsequential. Extreme social preferences either blow up the misperception wedge or shrink it to zero (see appendix robustness checks).

Several choices facilitate estimation (Appendix B discusses and shows sensitivity to each). First, we use payment plans in the tenant survey to proxy for bargaining payoffs. Second, we simulate assortative matching on altruism at the midpoint of our suggestive estimates (Appendix D). Standard errors account for the assumed matching process. Third, we calibrate $k_L^p =$ \$500.

We allow correlated altruism and misperceptions by using the empirical distributions in the experiments. An important assumption, explored in robustness checks, is that landlord and tenant eviction costs are orthogonal to social preferences and misperceptions.

Moments. Moment conditions come from the experiment and the ERAP observational analysis. From the experiment, we target the ERAP take-up rate among landlords; the bargaining offer made by tenants in the payment plans; and, crucially, the correlations between altruism and beliefs and eviction judgments, ERAP take-up, and the tenant bargaining offer. From the observational analysis, we target ERAP's treatment effects on judgments (a "well-identified" moment) and the mean judgment rates among landlords who take up ERAP. We form standard errors by bootstrapping both the treatment effect moment and the experiment data.

We now intuitively describe identification. From the experiment data and our assumptions, each landlord–tenant pair is associated to surplus in the relationship $\Delta t_i/\bar{a}_i$. This value moves the threshold at which eviction becomes possible absent ERAP, depending on the total eviction costs $k_L + k_T$. Then, the correlation between evictions and individual preferences pins down $k_L + k_T$. We identify k_L and k_T from landlords' choices to take ERAP or not, as these terms no longer enter landlords' payoffs as a simple sum. Finally, tenants' bargaining offers pin down bargaining power β . Null impacts from the ERAP event study are inconsistent with some simulated eviction propensities with and without ERAP, and thus provide an overidentifying restriction.

6.3 Results

Parameter Estimates and Model Fit. The model delivers reasonable parameter estimates (Table 4). 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. Landlords' mean eviction cost is \$874, and tenants' exceeds \$2,500. Confidence intervals for landlords' costs contain the estimates in Humphries et al. (2025)'s structural model (costs of \$2,000). Tenants' costs are consistent with evictions' medium-sized private costs in Collinson et al. (2024). Positive average costs do not imply that the average eviction is inefficient, as average costs could still be negative among those who pursue eviction. The standard deviation of landlord eviction costs (given by σ) is about \$3,800, suggesting that eviction could be efficient for many parties.

While landlord and tenant-specific cost parameters are imprecisely estimated, the total estimated cost ($k_L + k_T$) is more precise (s.e. of about \$1,100). As a result, our estimates of inefficient versus efficient eviction will also be reasonably precise (our main objective).

Tenants have high bargaining power ($\beta > 0.85$), which means that they capture more surplus in bargaining than landlords do. One interpretation is that our imputed bargaining transfers from tenants to landlords are low, which we infer as tenants capturing surplus.

Simulated moments closely match targeted moments, which confirms model fit (Table A17).

Efficient and Inefficient Evictions. We now quantify the shares of evictions and bargaining that are efficient and inefficient. Figure 6A shows an empirical version of the model comparative statics in Diagram 1 (see Table A16 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, 20.5 pp of the simulated data is evicted. This low percentage reflects that eviction judgments are extreme events, even in our sample.

Of those evicted, 19% are inefficient (s.e. 4.2%). We obtain this share by counting the number who obtain evictions despite positive surplus $k_L + k_T + \varepsilon_{i2} - \varepsilon_{i1}$. The remaining 81% are efficient. 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). Hostility causes about two thirds of inefficient evictions.

We intuitively describe why we find most but not all evictions are efficient. If k_T and k_L were zero, then eviction depends on the magnitude of the idiosyncratic shock and how it compares to $\Delta t_i/\bar{a}_i$. With a large shock and large \bar{a}_i , we attribute fewer evictions to $\Delta t_i/\bar{a}_i$ than to the shock. On the other hand, positive k_T and k_L reduce the share of evictions that are efficient from that baseline scenario, and hostile households have small \bar{a}_i . Altogether, the inefficiency estimate is similar to the magnitude of the correlation between hostility and evictions in Table 3.

On the other hand, bargaining is far more common than eviction, and nearly all bargaining is efficient (Table A16B).

Given most evictions are efficient, policy that stops evictions reduces joint surplus on average (Figure 6B). The average eviction produces about \$1,300 of private surplus if we do not normatively respect social preferences and \$2,500 if we do.

Even though the average eviction has positive private surplus, social preferences are still key to the economics of eviction. First, Panel B shows that the average *efficient* eviction yields more than \$2,300 in surplus without respecting social preferences. Thus, the presence of inefficient eviction cuts the average surplus from eviction by almost half.

Second, altruism sustains bargaining that would not otherwise occur. To show this, we replace social preferences as being classical if altruistic, and resimulate behavior assuming the same value of other primitives (Panel C). In this case, over half of evictions would be inefficient. Thus, high rates of altruism stop many pairs from inefficiently evicting. Intuitively, altruism offsets misperceptions that otherwise push toward bargaining.

These findings challenge the view that evictions' private costs exceed their benefits on average. A given anti-eviction policy could improve welfare if: (i) the policy targets inefficient evictions better than average; (ii) evictions' externalities exceed their net private benefits; or (iii) the planner places high welfare weights on those with large eviction costs (e.g., tenants). The potential presence of bargaining failure is a less compelling policy rationale in isolation. However, bargaining failure meaningfully reduces the magnitude of externalities or welfare weights that justify eviction policy.

Selection on Altruism. Table 5 varies nonclassical forces and court costs (Panel A) and considers policy counterfactuals (Panel B). We first confirm that altruism inhibits ERAP's treatment effects. In our primary simulation (Row 1), ERAP has small impacts on eviction (Column 3). This is natural, as ERAP's small TOT is a targeted moment. More revealing is that shutting off social preferences (Row 3) or particularly landlord altruism (Row 4) would cause ERAP to have a –11.1 pp effect on judgments (43% of the judgment rate among enrollees in Column 5).

ERAP Counterfactuals and Targeting. Given the presence of selection on altruism, we next study whether counterfactual tweaks to ERAP could raise its impacts (Rows 11–13). As one example, we simulate restricting eligibility among those predicted from Lasso to have large treatment effects, using observable demographics (i.e., excluding variables like social preferences). This produces a TOT of 25% of the counterfactual judgment rate. Even if infeasible to fully restrict eligibility, the program could expand outreach to those groups.

The model does not suggest that ERAP stops especially inefficient evictions. 19% of the evictions that ERAP stops are inefficient (Row 1, Column 4), meaning it is about as well-targeted as "generic" anti-eviction policy.

Robustness. Appendix B shows that the main conclusions are reasonably robust to researcher choices (see Table A18). Results are especially sensitive to assumptions about assortative matching (Figure A13C). Random or perfect assortative matching would change the share of evictions that

are inefficient from 12–37%, respectively. More assortative matching raises inefficiency because compound altruism \bar{a}_i becomes large as altruism goes to 1 for either party, even if the counterparty is hostile. Our matching assumption is speculative, so additional research on assortative matching in landlord–tenant relationships would be valuable. Another test scales down altruism and hostility, as one could be worried that DGs overstate true social preferences. We find inefficiency rates of about 30% if we scale a_{ii} down by half.

7 Conclusion

Eviction is the culmination of a failed bargaining process. Landlord–tenant social preferences could undermine or facilitate such bargaining. Motivated by this idea, we conduct Dictator Games with landlords and tenants in Memphis, which detect both altruism and hostility. These social preferences predict eviction and take-up of anti-eviction policy. When we quantify the results with a model, we find that more than three quarters of evictions are efficient. However, hostility creates most of the inefficient evictions, and altruism sustains bargaining that would otherwise fail.

On the one hand, our findings suggest that private welfare losses from eviction cannot rationalize generic eviction policy. On the other hand, we find enough inefficiency to materially reduce the magnitude of other forces like externalities that would make intervention desirable. Yet, most eviction advocacy emphasizes eviction's private costs (usually for tenants), not its social costs. Our findings suggest a shift in focus, and the value of more research to quantify externalities like homelessness (Evans et al., 2021).

The role of social preferences in bargaining has important implications beyond eviction. First, social preferences may shape efficient bargaining in the household, labor relations, and other settings. Second, we find that "selection on altruism" in the Memphis Emergency Rental and Utilities Assistance Program attenuates its treatment effects. Intuitively, altruistic landlords are the ones willing to pay a take-up cost, but they rarely evict. Selection on altruism may exist beyond ERAP, as housing policies like Section 8 vouchers also rely on landlord–tenant cooperation. More speculatively, altruism could also affect take-up of programs like old-age assistance or child care. A general lesson from our work is that social preferences can influence the desirability and effectiveness of these policy interventions.

8 Figures



Figure 1: Survey Flow

Notes: This figure depicts the survey flow for the main elicitations in 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: Social Preferences: Cumulative Distribution Functions and Tenant Validations

(a) Dictator Game CDFs (Own Landlords/Own Tenants)

(b) Free-Response Sentiment Predicts Dictator Game Behavior



Notes: In Panel A, S(x) is the Dictator Game indifference point, expressed as a percent of x. Hostility means that S(x) < 100%, i.e., that the participant prefers $(x, x) \succ (x, 0)$ for s > 2x. Panels B–D are only available for the tenant experiment using data starting on March 27, 2022 (about 70%). Panel B uses the technique in Hutto and Gilbert (2014) to extract sentiment from tenants' free responses about their relationship with their landlord. The blue line corresponds to the sentiment estimated on a quotation that we mention in the text. We drop tenants who do not respond to this optional question. Panel C shows responses from a Likert scale. Panel D shows our stakes sub-experiment, where the horizontal lines show the post-period averages by group. s the elicitation changed slightly once we randomize stakes (Appendix C), we disaggregate the data for x = 10 in the two leftmost points of Panel C to show whether these subtle survey changes affected behavior when the stakes were the same. Appendix C provides details on each elicitation.

Figure 3: Misperceptions



Notes: 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 CDFs of tenant priors for beliefs about landlord altruism (i.e., the share of landlords who prefer (*x* self, *x* own tenant) to (2*x* self, 0 own tenant)). The truth is indicated in a vertical red line. In Panel C, bars show different samples, since we only compute the share who update among those who are exposed to information. For tenant bars about *both* beliefs, we restrict to tenants who see both information treatments.

Figure 4: Social Preferences and Eviction

(a) Social Preferences Predict Eviction Filings



(b) Social Preferences and Misperceptions Are Complements for Eviction Filings



Notes: The sample is as in Table 3, Panel A, pooling landlord and tenant survey participants. Panel A shows a binned scatterplot of eviction filings against behavior in the Dictator Game. The "indifference point" S(x) is the value that renders her indifferent between (s, 0) and (x, x), expressed as a percent of x. The outcome is whether the tenant receives an eviction filing at the address where she played the DG (see Appendix D.1). The areas of each dot represent the number of observations. Best-fit lines and coefficient estimates come from regressions on the microdata, also controlling for survey fixed effects as in Equation (7). In Panel B, "high misperceptions" means the participant has above-median misperceptions. The bars present raw means. The "xor" in the middle bar means "exclusive-or"; that is, it shows mean filings among the sample that has either hostility or high misperceptions, but not both. The regression-adjusted difference comes from a regression of the outcome on an indicator for having both above-median misperceptions and hostility, also controlling for survey fixed effects.

Figure 5: ERAP Impacts and Tenant Hostility

(a) Kaplan-Meier Failure Curves



(b) Visual Evidence of ERAP's Impacts on Filings and Judgments



Notes: Panel A shows Kaplan-Meier curves in the sample of tenants who: had not moved between applying for ERAP and participating in our study; did not receive expedited ERAP legal representation or utilities payments; and applied after September 1, 2021, since that is when the program exited its pilot phase. The correlation is from a regression of ever receiving ERAP payment on hostility. The blue and orange series have one observation per participant and is the main comparison in the figure (hostile vs. not hostile, among tenants who play the DG against their own landlord). The gray series is a placebo, showing that tenants who are hostile to random parties do not have different payments in ERAP. The first Wilcoxon *p*-value is for the blue vs. orange; the second is for the blue vs. gray. The gray series can have multiple observations per participant, since some participants play against multiple random opponents, and its corresponding Wilcoxon *p*-value does not account for clustering within participant. The reported *N* is the total number of unique tenants in any of the three lines. Panel B 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. Appendix D shows event studies and describes sample restrictions.

Figure 6: Decomposing Eviction: Efficient and Inefficient Evictions

(a) Empirical Analog of Diagram 1



Notes: 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 as described 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

	Experiment	Experiment	Difference	SE	Memphis	Memphis	Shelby
Sample:	Participant	Non-Participant	(1 - 2)	Difference	ERAP (Paid)	ERAP (Paid & Nonlegal)	County
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Observed for Experiment Partic	ipants and Non	-Participants					
Age	34	35	-1.1	0.27	37	39	45
Female	0.81	0.72	0.09	0.010	0.76	0.75	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	0.09	0.10	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	0.42	0.40	0.64
Household monthly income	891	733	157	29	1,183	1,184	5,408 ⁺
Monthly rent	872	900	-28	12	954	988	834
Back rent owed at application [†]	3,585	3,900	-315	173	4,081	4,275	
B. Observed for Experiment Partic	ipants 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			10,632	4,609	5,586

Notes: 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. The "ever evicted" row is from a self-reported question in the experiment; Section 5.1 shows means of eviction judgments and filings from fuzzy-merges to court data. †: displays median.

	(1)	(2)	(3)	(4)	(5)
Variable	Participant	Participant's Tenant	Non-Participant's Tenant	Difference $(2 - 3)$	SE Difference
A. Observed for Landlords and Te	rnants				
Age	49	39	37	2.2	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		0.03	0.02	0.01	0.010
Back rent owed [†]		2,700	2,400	300	222
Utilities owed		350	544	-195	80
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	3,584		

Table 2: Landlord Demographics

Notes: 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.

		All	Lan	dlords	Te	enants
	Filing (1)	Judgment (2)	Filing (3)	Judgment (4)	Filing (5)	Judgment (6)
Panel A. Own Landlord or	Tenant Op	ponent				
Hostile	0.095	0.071	0.245	0.201	0.075	0.053
	(0.033)	(0.031)	(0.094)	(0.089)	(0.035)	(0.033)
	[0.004]	[0.022]	[0.009]	[0.025]	[0.033]	[0.106]
Ν	1,361	1,361	211	211	1,150	1,150
Panel B. Random Landlord	or Tenant	Opponent				
Hostile	-0.019	-0.004	-0.014	0.022	-0.020	-0.007
	(0.029)	(0.026)	(0.084)	(0.075)	(0.031)	(0.028)
	[0.506]	[0.882]	[0.867]	[0.774]	[0.519]	[0.802]
Ν	2,685	2,685	453	453	2,232	2,232
Non-hostile mean	0.385	0.265	0.270	0.163	0.408	0.285
<i>p</i> -value: own = random	0.003	0.042	0.016	0.081	0.024	0.125

Table 3: Social Preferences Correlate with Eviction

Notes: Cells present estimates of β from Equation (7). The level of observation is a Dictator Game, which is unique at the experiment participant-level in Panel A, but not necessarily unique in Panel B since some participants play against multiple random opponents. The outcome y_i is a filing indicator that is 1 when the tenant in the DG played by *i* ever has a filing at the address from 1 year before to 2 years after the DG, or a judgment indicator that is 1 if she ever has a judgment in the same window (allowing 2.5 years after the DG so the court process can conclude). The sample consists of participants for whom we have valid addresses and, in the tenant survey, who did not move between ERAP application and the survey or who have positive back rents at the time of the DG. Table A9 shows sensitivity to sample restrictions. Columns 1 and 2 stack the landlord and tenant samples and include a survey fixed effect. Panel A uses DGs played against participants' own landlord or own tenant. Panel B uses DGs played against random landlords or random tenants and is a falsification test. Standard errors, presented in parentheses, are clustered at the level of the participant (landlord or tenant). *p*-values are in brackets. The last *p*-value comes from a difference-in-differences specification that compares the coefficient in Panel A to Panel B. The non-hostile mean is from participants who play against their own landlord or own tenant.

Explanation	Parameter	Estimate
Total unconditional eviction cost (\$)	_	3,398 (1,105) {3,824}
Landlord cost (\$)	k_L	874 (2,077)
Tenant cost (\$)	k_T	2,524 (2,067)
Tenant bargaining power	β	0.88 (0.12)

Table 4: Empirical Bargaining Model: Parameter Estimates and Model Fit

Notes: Estimates from 200 bootstraps. The table 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. Moments are in Table A17.

	Descriptiv	/es			
	Judgment % (absent ERAP)	Takeup (%)	TOT: judgments (pp)	Inefficient judgment (%) (if prevented)	Counterfactual judgment (%) (enrollees)
	(1)	(2)	(3)	(4)	(5)
A. Mechanisms				. ,	
1. Primary	20.5	65.8	0.7	19.2	17.9
2. No social prefs., misperceptions	16.6	67.2	7.0	0.0	13.1
3. No social preferences	29.6	60.1	-2.9	51.6	23.9
4. No landlord altruism	24.5	57.1	-11.1	29.4	25.9
5. No altruism	33.6	59.2	-6.3	58.8	27.4
6. No hostility	18.0	66.6	2.7	8.8	15.8
7. No misperceptions	16.6	67.5	3.8	0.0	14.6
8. High misperceptions	21.5	64.1	0.4	21.1	18.2
9. Mean T and L court costs are 0	53.0	65.7	12.0	8.0	46.1
10. Double T and L court costs	7.2	65.8	-2.6	41.8	6.6
B. Counterfactual Policies					
11. \uparrow eviction penalty	20.5	61.9	-4.1	19.2	17.5
12. \downarrow take-up costs	20.5	71.1	-0.2	18.9	18.4
13. Targeted on demographics	25.8	83.2	-6.7	34.3	24.1

Table 5: Empirical Bargaining Model: Mechanisms and Counterfactuals

Notes: 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 4. 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 10) or eliminate take-up costs ($k_L^P \rightarrow 0$). Row 13 conducts a targeting counterfactual by restricting ERAP eligibility to households whom a Lasso procedure determines are in the top 10% most likely to bargain without ERAP (Lasso estimated on 10% of the data, sampled at random). The landlord variables in the regression 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. Column 1 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 3 divided by Column 5 (×100) shows a treatment effect in percentage terms.

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Please see Rafkin's website for a Supplementary Appendix containing extra exhibits (prefixed with "S") and survey instruments (http://crafkin.github.io/rs_eviction_supplement/).

A Additional Figures and Tables



Figure A1: Eviction Prevalence: Descriptive Statistics

(a) Share with Judgments over Time

(b) Time between Filing and Judgment



Notes: 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. The gray and red lines show the median and mean.

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	l get \$900 and they get \$0
Which would you prefer?	0	0

(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.



tenants had fully repaid

Notes: 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: Dictator Game: Heterogeneity



(a) Landlords

Notes: 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 regress landlords' hostility toward own tenants on the indicated demographic and show the coefficient. In Panel B regress tenants' hostility toward own landlords on the indicated demographic and show the coefficient. Whiskers show 95-percent confidence intervals.



Figure A4: Tenants, Landlords, Random Memphians, and Random Americans

Notes: 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 A5: Additional Dictator Game Validations

(a) Alternative Lottery Measure Gives Similar Results



(b) Correlation Between Falk et al. (2018) Questions and Modified Dictator Game



Notes: Panel A shows responses from our lottery sub-elicitation. Appendix C provides details on the elicitation. Panel B 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 A6: Social Preferences: Additional Within-Experiment Validations



Notes: Analogous to Figure 2B–C, but with the indifference point S(x) as the outcome (Panels A and B) or "high altruism" (that is, rejecting (20x,0) in favor of (x, x) bundles; Panel C). Panel D shows the lottery outcome for landlords (see Figure A5A for tenants).

(b) Qualitative Questions Predict Dictator Game Indifference Point



Figure A7: Social Preferences: Rescaling Landlords' Altruism

Notes: 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 (Section 4). 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 A8: Posteriors and IV: Landlord Information

(a) Posteriors about recoupment



Notes: Panel A includes only treated individuals and shows the prior and posterior beliefs about recoupment. Panel B shows the effect of beliefs on the outcome of requesting materials about ERAP, instrumenting for landlords' belief update.

Figure A9: Landlord beliefs: heterogeneity





Notes: Panel A shows a binned scatterplot of the correlation between a landlords' beliefs about 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 A10: Tenants' Average Belief Updates

(a) Treatment effect: beliefs about landlord altruism





1

Notes: Panel A shows a cumulative distribution function of beliefs about landlord bargaining. Panels B and C show belief updates about landlord altruism and bargaining. We only show the sample of people who received each information. Treatments were cross-randomized.





Notes: The figure shows tenant hostility by the number of days until a filing, using the sample in Section 5.1. For instance, the bin 0 to 99 shows the share of tenants who are hostile, if they receive a filing between 0 to 99 days after completing the survey. The unit of observation is an eviction filing, which is not necessarily unique at the level of the tenant. Our interpretation is that hostility correlates with eviction filings both before and right after the survey elicitation, suggesting against reverse-causality. The sample is as in Table 3.



Figure A12: Section 5: Additional Results

(a) Misperceptions and Eviction Judgments Are Complements

(b) Regression version of Figure 5B

(c) Placebo: ERAP receipt for utilities-shutoffs



Notes: Panel A presents the analog of Figure 4 with judgments as an outcome. Panel B shows coefficients from regressions of receiving ERAP within the indicated number of days on tenant hostility. 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. Also controls for attention checks and survey week-fixed effects. The sample is as in Figure 5B. Panel C is analogous to 5B except run on a sample of households that received payments via the legal program or had payments made to address utilities shutoffs. These households could have payments expedited by ERAP. It is a placebo because, for this sample, we expect relationships to play less of a role in ultimate speed of payment. Everyone in the sample ultimately receives payment, so the test is for payment speed.



Figure A13: Measurement Error: Simulations

(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: $\hat{p}_{ji} := \max\{s, 1\}p_{ji} + (1-s)p_{true} + \mathbb{1}(s > 1)(s - 1)p_{ji}$ 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: $\hat{p}_{Ti} := \max\{s, 1\}sp_{Ti} + (1-s)p_{true} - \mathbb{1}(s > 1)(s - 1)p_{Ti}$. 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 D.5). All panels use the estimated and calibrated values of other parameters as in our primary specification. The vertical red lines show the primary estimate.





Notes: We produce this figure by the process in Section D.5. 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 D.5 in the simulated sample.

Figure A15: Effects of Rental Assistance on Eviction Judgments and Filings



Notes: Panels A and B show the effect on judgments and filings from Equation (92). The figure's point estimates come from the primary sample. The middle series drops households who apply with eviction notices or shutoffs, who were eligible to be expedited. The left-most series restricts the application dates to be between October 1, 2021 and July 31, 2022 (see appendix for explanation). Panels C and D show the alternative design which compares to non-paid households (Section D.7.3). The primary specification in Panels C and D refer to the estimates from Equation (92), nonparametric specification. The other estimates come from Equations (93) or (94), 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).

Table A1: Experimental Attrition

(4) 10		arvey manual	
Attrition Funnel	Ν	% 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 treat	nent (co	nditional on finishi	ng demographics)
Random landlord in DG		2	-0.026
			(0.018)
			[0.147]
Altruism info treatment			0.020
			(0.017)
			[0.236]
Bargaining share info treatm	lent		-0.013
			(0.017)
			[0.437]
Joint p across treatments			[0.252]
(b) Lar	ndlord	Survey Attrition	
		% of	% of total
Attrition Funnel	Ν	total consenting	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 treat	ment (c	onditional on finishi	ing demographics)
Random landlord in DG		2	0.035
			(0.040)
			[0.380]
Info treatment			-0.040
			(0.038)
			[0.289]
Joint p across treatments			[0.394]

(a) Tenant Survey Attrition

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 A4:	Landlords	DG Robustness
-----------	-----------	---------------

		(a) Hostility			
	(1)	(2)	(3)	(4)	(5)
	Raw	If attentive (one q)	Limited controls	Full controls	DS lasso
Random tenant	-0.088	-0.105	-0.082	-0.075	-0.085
	(0.032)	(0.039)	(0.030)	(0.028)	(0.028)
	[0.006]	[0.008]	[0.006]	[0.009]	[0.003]
Own tenant (constant)	0.154	0.160			
	(0.024)	(0.031)			
	[0.000]	[0.000]			
Observations	371	235	371	371	371
		(b) Indifference	2		
	(1)	(2)	(3)	(4)	(5)
	Raw	If attentive (one q)	Limited controls	Full controls	DS lasso
Random tenant	11.023	14.035	11.401	10.042	10.213
	(5.842)	(7.294)	(4.976)	(4.863)	(4.660)
	[0.060]	[0.056]	[0.023]	[0.040]	[0.028]
Own tenant (constant)	171.496	169.097			
	(4.214)	(5.450)			
	[0.000]	[0.000]			
Observations	371	235	371	371	371

Notes: This table presents robustness for the Dictator Game (DG) in the landlord experiment. Panel A focuses on the level of hostility toward own tenants (the constant) and the difference between hostility for own vs. random tenants (Row 4 of Table A2). Panel B focuses on the level of the indifference point and the difference between the indifference points S(x). Column 1 corresponds to Row 4, Column 1 of Table A2. Column 2 limits to landlords who correctly answer the attention check (Appendix D.4). 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 survey week 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. Columns 3–5 do not display a constant (for own-tenant hostility) because we include controls. Brackets show *p*-values.

		(a) Hostility			
	(1)	(2)	(3)	(4)	(5)
	Raw	If attentive (one q)	If attentive (both q's)	Controls	DS lasso
Random tenant	-0.097	-0.100	-0.096	-0.099	-0.099
	(0.013)	(0.013)	(0.022)	(0.013)	(0.014)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Own landlord (constant)	0.240	0.233	0.214		
	(0.012)	(0.013)	(0.020)		
	[0.000]	[0.000]	[0.000]		
Observations	3,032	2,780	1,002	3,032	3,032
		(b) Indifference			
		(b) mumerence			
	(1)	(2)	(3)	(4)	(5)
	Raw	If attentive (one q)	If attentive (both q's)	Controls	DS lasso
Random tenant	17.402	18.156	18.823	17.576	17.532
	(2.110)	(2.189)	(3.650)	(2.078)	(2.348)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Own landlord (constant)	153.521	154.477	156.916		
	(2.062)	(2.137)	(3.488)		
	[0.000]	[0.000]	[0.000]		
Observations	3,032	2,780	1,002	3,032	3,032

This table presents robustness for the Dictator Game (DG) in the tenant experiment. Panel A focuses on the level of hostility toward own landlords, and the difference between hostility for random landlords versus random tenants. Panel B focuses on the level of the indifference point, and the difference between the indifference points S(x). Column 1 corresponds to Row 11, Column 1 of Table A2. Column 2 limits to attentive tenants who correctly either attention check (Appendix D.4). 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; indicators for passing either attention check; and survey-week fixed effects. 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. We do not show constants in Columns 4–5 since we show regressions with controls. Brackets show *p*-values.
	(1)	(2) Indifference	(3) Highly	(4) Highly
	Hostile	point	hostile	altruistic
A. Landlord sample ($N = 371$)				
1. Own Tenant	0.154***	171.5***	0.090***	0.688***
N = 234	(0.024) [0.000]	(4.2) [0.000]	(0.019) [0.000]	(0.030) [0.000]
	[]	[0.000]	[0.000]	[0.000]
2. Random Tenant $N - 137$	0.066^{***}	182.5***	0.036**	0.693***
N = 157	[0.002]	[0.000]	[0.025]	[0.040]
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]

Notes: 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 betweer (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.

	(1)	(2) Indifference	(3) Highly	(4) Highly
	Hostile	point	hostile	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]

Notes: See notes to Table A2 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 A2 except it only keeps the first instance that each participant plays the DG.

	(1)	(2) Indifference	(3) Highly	(4) Highly
Λ Landlord comple (NI - 271)	Tiostile	point	nostne	annuishe
A. Lunuloru sample $(N = 5/1)$				
1. Own Tenant $N = 234$	0.124*** (0.022) [0.000]	177.4*** (4.1) [0.000]	0.075*** (0.018) [0.000]	0.726*** (0.032) [0.000]
2. Random Tenant $N = 137$	0.077*** (0.026) [0.004]	182.7*** (4.6) [0.000]	0.038** (0.017) [0.032]	0.716*** (0.042) [0.000]
3. Random Landlord $N = 371$	0.119*** (0.018) [0.000]	174.7*** (3.1) [0.000]	0.039*** (0.011) [0.001]	0.625*** (0.028) [0.000]
4. Own Tenant – Random Tenant (Row 1 – Row 2)	0.048 (0.034) [0.166]	-5.2 (6.1) [0.396]	0.037 (0.025) [0.143]	0.010 (0.052) [0.844]
5. Random Tenant – Random Landlord (Row 2 – Row 3)	-0.042 (0.029) [0.150]	8.0* (4.6) [0.081]	-0.001 (0.019) [0.963]	0.090** (0.039) [0.023]
6. Own Tenant – Random Landlord (Row 1 – Row 3)	0.005 (0.022) [0.806]	2.8 (3.7) [0.453]	0.036** (0.017) [0.030]	0.100*** (0.031) [0.001]
B. Tenant sample ($N = 1,808$)				
7. Own Landlord $N = 1,224$	0.233*** (0.012) [0.000]	154.4*** (2.1) [0.000]	0.126*** (0.010) [0.000]	0.535*** (0.015) [0.000]
8. Random Landlord $N = 584$	0.226*** (0.018) [0.000]	156.8*** (3.1) [0.000]	0.102*** (0.013) [0.000]	0.547*** (0.022) [0.000]
9. Random Tenant $N = 1,808$	0.141*** (0.009) [0.000]	171.4*** (1.4) [0.000]	0.063*** (0.006) [0.000]	0.628*** (0.012) [0.000]
10. Own Landlord – Random Landlord (Row 7 – Row 8)	0.006 (0.022) [0.780]	-2.4 (3.7) [0.521]	0.025 (0.016) [0.135]	-0.011 (0.026) [0.666]
11. Random Landlord – Random Tenant (Row 8 – Row 9)	0.085*** (0.019) [0.000]	-14.6*** (3.1) [0.000]	0.039*** (0.014) [0.004]	-0.081*** (0.021) [0.000]
12. Own Landlord – Random Tenant (Row 7 – Row 9)	0.091*** (0.013) [0.000]	-17.0*** (2.2) [0.000]	0.064*** (0.010) [0.000]	-0.092*** (0.014) [0.000]

Notes: See notes to Table A2 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 A2 except we weight to adjust for sample selection. 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).

	(1)	(2) Indifference	(3) Highly	(4) Highly
	Hostile	point	hostile	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]

Table A7: Behavior in Dictator Game: All Participants, Prior to Attrition

Notes: This table is the same as Table A2, 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 A8: Belief Errors: Sensitivity

	(1)	(2)	(3)	(4)
	Repayment	Repayment	Repayment	Repayment
Uncertain (avg)	-0.267			
	(0.046)			
	[0.000]			
Uncertain (own and avg)		-0.271		
		(0.050)		
		[0.000]		
Inattentive			0.150	
			(0.052)	
			[0.004]	
HS or less				-0.085
				(0.066)
	0.404	0.0		[0.197]
Constant	0.401	0.369	0.277	0.344
	(0.030)	(0.027)	(0.029)	(0.027)
	[0.000]	[0.000]	[0.000]	[0.000]
Mean (whole sample)	0.33	0.33	0.33	0.33
Observations	371	371	371	371

Panel A: Landlords (outcome is misperception rates)

Panel B: Tenants (outcome is misperception rates)

	(1) Altruism	(2) Altruism	(3) Altruism	(4) Altruism	(5) Bargaining	(6) Bargaining	(7) Bargaining	(8) Bargaining
Uncertain	0.029 (0.034)				-0.001 (0.028)			
Inattentive (one q)	[0.391]	0.055 (0.024) [0.023]			[0.979]	0.047 (0.025) [0.060]		
Inattentive (both q's)		[0:020]	0.097 (0.038) [0.010]			[0.000]	-0.025 (0.041) [0.539]	
HS or less			[0:010]	-0.014 (0.023) [0.540]			[0.007]	0.005 (0.024) [0.838]
Constant	0.611 (0.031) [0.000]	0.598 (0.020) [0.000]	0.627 (0.012) [0.000]	0.642 (0.015) [0.000]	0.447 (0.025) [0.000]	0.415 (0.020) [0.000]	0.449 (0.012) [0.000]	0.444 (0.016) [0.000]
Mean (whole sample) Observations	0.64 1,808	0.64 1,808	0.64 1,808	0.64 1,808	0.45 1,808	0.45 1,808	0.45 1,808	0.45 1,808

• Table shows coefficients from regressing beliefs about the outcome on the listed coefficients and a constant. Brackets show *p*-values.

• The outcome is a binary indicator for having beliefs be at least 20 points away from ground truth (unsigned), where column labels show the belief in question. Outcomes are beliefs about the average, where we can form ground truth.

- Uncertainty is a binary indicator constructed from an uncertainty question asked about the prior beliefs elicitation. Certainty for landlords is elicitated on a 0-100 scale (0 = "very uncertain," 100="completely certain"). We code a landlord as being uncertain if they report 25 or less. Certainty for tenants is a qualitative Likert scale, where we code "somewhat unsure" and "very unsure" as being uncertain.
- Inattentive in the landlord survey is an indicator if the landlord gets the attention check wrong. We asked two attention checks in the tenant survey. Inattentive (one q) means the tenant got at least one wrong.
- The constant shows the share who have incorrect beliefs when the indicator is 0. The table is designed so that the constant reports misperception rates among a sample where we would expect more correct beliefs. For example, the share who have incorrect beliefs when attentive is the constant. The main message of the table is that excluding groups who may report noisy beliefs does not move the misperception rates much (constant is stable).

	(1) Filing	(2) Filing	(3) Filing	(4) Filing	(5) Filing	(6) Filing	(7) Filing	(8) Filing	(9) Judgment	(10) Judgment	(11) Judgment	(12) Judgment	(13) Judgment	(14) Judgment	(15) Judgment	(16) Judgment
Hostile	0.095 (0.033) [0.004]	0.092 (0.033) [0.006]	0.087 (0.032) [0.007]	0.084 (0.032) [0.010]	0.112 (0.037) [0.003]	0.109 (0.038) [0.004]	0.095 (0.033) [0.004]	0.090 (0.033) [0.006]	0.071 (0.031) [0.022]	0.076 (0.032) [0.016]	0.068 (0.030) [0.023]	0.071 (0.031) [0.020]	0.069 (0.036) [0.052]	0.079 (0.037) [0.032]	0.061 (0.031) [0.050]	0.060 (0.031) [0.057]
Mean (non-hostile) Sample Controls Court data window	0.38 1 (main) X 1 (main)	0.38 1 (main) √ 1 (main)	0.38 2 x 1 (main)	0.38 2 √	0.39 3 X 1 (main)	0.39 3 √ 1 (main)	0.41 1 (main) X 2	0.42 1 (main) X 3	0.26 1 (main) X 1 (main)	0.26 1 (main) √ 1 (main)	0.26 2 X 1 (main)	0.26 2 √	0.28 ✓ ✗ 1 (main)	0.28 3 √	0.29 1 (main) X 2	0.30 1 (main) X 3
Observations	1,361	1,361	1,414	1,414	1,103	1,103	1,361	1,361	1,361	1,361	1,414	1,414	1,103	1,103	1,361	1,361

Table A9: Table 3 Sensitivity: Controls and Sample

Notes: This table shows sensitivity of Panel A, Table 3 to controls and sample restrictions. All data stack landlord and tenant samples and estimate Equation (7) on people who played the Dictator Game against their own landlord or tenant. All specifications include a survey fixed effect.

- Sample restrictions: Sample 1 (main) corresponds to the main sample in Table 3: all people excluding tenants who moved between ERAP application and survey completion and do not have positive back rents. Sample 2 does not make the latter sample restriction. Sample 3 drops any tenant without positive back rent.
- Court data windows: The motivation for our primary court data restriction is that we want to include court outcomes that occur in a reasonable window around the survey, but also give the court process time to play out. We show sensitivity to the window around which we include filings or judgments. Our primary sample window includes filings and judgments from 1 year before to 2 years after survey completion (judgments 2.5 years after, to allow time for the court process to conclude). Window 2 starts with Window 1, but includes any filing or judgment that takes place more than 1 year before the survey (drops the end-point restriction of the window). We prefer to exclude such earlier cases in our primary estimate (Window 1), as continued residence in August 2021 or later indicates some bargained agreement to rent must have been formed. Window 3 starts with Window 1, but extends the start-point restriction of the window to include any filing or judgment that takes place after January 1, 2019 (maintaining the same end-point restriction of the window). We prefer to exclude such alter cases in our primary estimate (Window 1), as we do not expect DG-hostility to be predictive of behaviors in perpetuity.
- Landlord controls: 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, survey-week fixed effects, and linear controls for: age, tenure, rent, and experience.
- Tenant controls: Indicators for being Black, female, having less than high school, being employed, indicators for passing each attention check, survey-week fixed effects, and linear controls for monthly rent and monthly income.

		(-)	(-)		(-)	(()	(2)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Filing	Judgment	Filing	Judgment	Filing	Judgment	Filing	Judgment
Panel A. All ($N = 1,361$)								
Hostile	0.095	0.071	0.089	0.070	0.091	0.076	0.092	0.072
	(0.033)	(0.031)	(0.033)	(0.031)	(0.033)	(0.031)	(0.033)	(0.031)
	[0.004]	[0.022]	[0.006]	[0.025]	[0.006]	[0.014]	[0.005]	[0.021]
Panel B. Landlords only $(N = 211)$								
Hostile	0.245	0.201	0.205	0.180	0.194	0.175	0.220	0.195
	(0.094)	(0.089)	(0.085)	(0.085)	(0.088)	(0.084)	(0.094)	(0.088)
	[0.009]	[0.024]	[0.015]	[0.034]	[0.027]	[0.037]	[0.019]	[0.027]
Panel C. Tenants only ($N = 1,150$)								
Hostile	0.075	0.053	0.082	0.053	0.079	0.061		
	(0.035)	(0.033)	(0.035)	(0.033)	(0.035)	(0.033)		
	[0.033]	[0.106]	[0.019]	[0.112]	[0.025]	[0.066]		
Controls	None	None	Full	Full	Small	Small	Ex. L size, exp, occ	Ex. L size, exp, occ

Table A10: Table 3 Sensitivity: Additional Controls

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Notes: This table shows sensitivity of Panel A, Table 3 to controls. The sample is as in Table 3. Relative to Table A9 it shows more detail on controls. Columns 1 and 2 are the same as Table 3. Columns 3–8 use post-double-selection Lasso to select controls (Belloni et al., 2014). In the post-double-selection Lasso, we always include a survey fixed effect. We run separate Lassos in each column. The baseline set of controls is:

- 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, survey-week fixed effects, 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, survey-week fixed effects, and linear controls for monthly rent and monthly income.

In Columns 3–4 we include those controls, as well as ZIP fixed effects and all two-way interactions between the baseline set of controls within landlords or tenants surveys (that is, we only interact tenant characteristics with other tenant characteristics and not with landlord characteristics, and vice-versa). Columns 5–6 restrict to just the baseline set of controls, excluding ZIP fixed effects and two-way interactions. Columns 7–8 exclude landlord experience, size, and occupation indicators. The rationale for the specification in Columns 7–8 is that these may be bad controls, as part of the mechanism may be that small landlords are more altruistic to tenants they have personal relationships with.

	(1) Filing	(2) Judgment	(3) Filing	(4) Judgment	(5) Filing	(6) Judgment
Panel A. All ($N = 1,361$)						
Hostile	0.003	0.001	0.008	-0.001	0.005	-0.000
	(0.002)	(0.008)	(0.003)	(0.007)	(0.005)	(0.005)
	[0.117]	[0.880]	[0.012]	[0.903]	[0.316]	[0.968]
Panel B. Landlords only $(N = 211)$						
Hostile	0.026	-0.008	0.060	0.041	0.045	0.034
	(0.012)	(0.029)	(0.017)	(0.027)	(0.027)	(0.018)
	[0.039]	[0.782]	[0.000]	[0.136]	[0.104]	[0.065]
Panel C. Tenants only $(N = 1,150)$						
Hostile	-0.000	0.003	0.001	-0.007		
	(0.001)	(0.009)	(0.003)	(0.007)		
	[0.904]	[0.773]	[0.609]	[0.345]		
Controls	Full	Full	Small	Small	Ex. L size, exp, occ	Ex. L size, exp, occ

Table A11: Hostility Balance on Predicted Eviction Risk

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Notes: This table shows coefficient estimates from regressing a measure of predicted eviction risk on hostility and a tenant-survey fixed effect. The sample is as in Table 3. To form the measure of predicted eviction risk, we run Lasso-logit models on controls (always including a survey fixed effect), and then run logit on the selected coefficients. The outcome is in probability units, ranging from 0 to 1. For instance, a value of 0.01 indicates that hostility is correlated with 1 pp higher predicted eviction filing risk (see non-hostile mean eviction probabilities in Table 3 which are are close to the average *predicted* eviction probabilities). To estimate the Lassos, the baseline set of controls is:

- 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, survey-week fixed effects, and linear controls for: age, tenure, rent, and experience.
- Tenants. Indicators for being Black, female, having less than high school, being employed, survey-week fixed effects, indicators for passing each attention check, and linear controls for monthly rent and monthly income.

In Columns 1–2 we include those controls, as well as ZIP fixed effects and all two-way interactions between the baseline set of controls within landlords or tenants surveys (that is, we only interact tenant characteristics with other tenant characteristics and not with landlord characteristics, and vice-versa). Columns 3–4 restrict to just the baseline set of controls, excluding ZIP fixed effects and two-way interactions. Columns 5–6 exclude landlord experience, size, and occupation indicators. The rationale for the specification in Columns 5–6 is that these may be bad controls, as part of the mechanism may be that small landlords are more altruistic to tenants they have personal relationships with.

Table A12: Bargaining and Social Preferences, Within-Experiment

(a) Landlord Outcomes

	(1) Engagement index	(2) Engagement index	(3) Engagement index	(4) Engagement index	(5) Takes baircut	(6) Takes baircut	(7) Takes baircut	(8) Takes baircut	(9) Haircut %	(10) Haircut	(11) Haircut	(12) Haircut	
Indifference point (÷ 100)	0.334 (0.128) [0.010]	-0.494	0.361 (0.144) [0.013]	-0 546	0.093 (0.051) [0.068]	-0.147	0.089 (0.051) [0.086]	-0.132	-1.991 (2.260) [0.379]	2 047	-1.867 (2.057) [0.365]	2 048	
TIOSHIE		(0.228) [0.032]		(0.252) [0.031]		(0.090) [0.105]		(0.094) [0.161]		(4.088) [0.617]		(3.751) [0.586]	
Mean (non-hostile) Demographic controls Observations	0.00 × 234	0.00 × 234	0.00 ✓ 234	0.00 ✓ 234	0.68 × 234	0.68 × 234	0.68 ✓ 234	0.68 ✓ 234	82.73 × 234	82.73 × 234	82.73 ✓ 234	82.73 ✓ 234	
(b) Tenant Outcomes													
	(1) Share repaid	(2) Share repaid	(3) Share repaid	(4) Share repaid	Wants rep	(5) payment pl	an Wan	(6) its repaym	ent plan	(Wants repa	7) ayment plar	Wants	(8) repayment plan
Indifference point (÷ 100)	0.046 (0.017) [0.007]		0.046 (0.017) [0.007]) (([0).043).025)).087]				0. (0. [0.	048 025) 058]		
Hostile		-0.065 (0.030) [0.029]		-0.065 (0.030) [0.030]	·	-		-0.050 (0.043) [0.246]		Ľ	-		-0.060 (0.043) [0.170]
Mean (non-hostile) Demographic controls Observations	0.33 × 905	0.33 × 905	0.33 ✓ 905	0.33 ✓ 905		0.55 × 905		0.55 × 905		0	.55 √ 05		0.55 ✓ 905

Notes: All observations are participants who do experiments against their own tenants or own landlords. Parentheses show standard errors. Brackets show *p*-values. Indifference point shows S(x) as a share of *x*, so a value of 1 indicates that the participant was indifferent between (s, 0) and (x, x). Notes to Panel A:

• The engagement index is made up of five variables: requesting materials; indicating they would notify tenants when future opportunities to apply for ERAP become available; the number of tenants they refer to ERAP wanting an ERAP offer to be sent to them for a given tenant; and whether they indicate they will always reject ERAP offers that prohibit eviction. We average the standardized inputs and then re-standardize the index, so units are interpretable as standard deviations of the index. We only include the last variable for landlords who take the survey after September 1, 2021, because the initial question confused landlords due to a double negative.

- Takes Haircut is a variable that is 1 if the landlord agrees to accept an agreement from ERAP where ERAP paid less than full rent for the tenant (and the landlord had to forgive the rest, i.e., take a haircut on the rent). This question was hypothetical. Haircut % is the indifference point in this elicitation. We randomized between two different elicitation modules (starting at different start points and with different intervls). All specifications with haircut outcomes include a fixed effect for the elicitation module.
- Controls: 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, survey-week fixed effects, and linear controls for age, tenure, rent, and experience.

Notes to Panel B:

- Sample restricts to tenants with positive back rents (the only ones who did the payment plan exercise in the experiment). Includes date-of-experiment fixed effects since the elicitation of the payment plan changed slightly over time. Share repaid is the share of back rents that tenants with back rents propose to repay in payment plans. As rejecting a payment plan is coded as zero, this measure combines the intensive and extensive margin of bargaining generosity. Wants repayment plan is a measure of the extensive margin only.
- Controls: Indicators for being Black, female, having less than high school, being employed, indicators for passing each attention check, survey-week fixed effects, and linear controls for monthly rent and monthly income.

	(1)	(2)	(3)	(4)	(5)
	Any	Housing	Placebo	Any	Any
	Code Violation	Code Violation	Code Violation	Code Violation	Code Violation
Highly hostile	0.091 (0.040) [0.021]	0.104 (0.039) [0.008]	-0.045 (0.028) [0.103]	0.101 (0.039) [0.010]	
Hostile					0.028 (0.028) [0.321]
Mean (non-highly hostile)	0.19	0.17	0.15	0.19	0.19
Controls	×	×	×	✓	×
Observations	1,264	1,264	1,264	1,264	1,264

Table A13: Hostility and Code Violations

Notes: This table shows the relationship between high hostility and possible code-enforcement violations. Brackets show *p*-values. High hostility is defined as having indifference point S(x) < 0.1x in the Dictator Games. Hostility is S(x) < x. Code enforcement data include open and closed tickets listed on the public City of Memphis 311 data archive. Anyone can file these tickets, and we include all tickets regardless of how they are resolved. The sample includes only landlords or tenants with an address in Memphis (excluding other residences in Shelby County) as we only have code-enforcement data for Memphis proper. We include any party who played the DG against their own landlord or tenant. Each cell presents the regression coefficient of the outcome (e.g., code violation) on high hostility and a survey fixed effect, estimated on the stacked landlord–tenant samples. The mean is the mean among the non-highly hostile group.

Definitions of code violations (see Appendix D.1):

- Any property code violation. Any violations from the code-enforcement department (excluding categories: vehicle violations or vacant lot codes); or solid waste management department (only category: code enforcement).
- Housing code violation. Violations from the code-enforcement department. Violation categories: substandard, derelict structure; junky yard; miscellaneous. (Excluding categories: vehicle violations; stagnant; open storage and furnishing; vacant lot; weeds occupied property.)
- Placebo violations: vehicle violations (code enforcement department), garbage-missed violations (solid waste management department).

Controls (Column 4):

- 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, survey-week fixed effects, 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, linear controls for monthly rent and monthly income, and survey-week fixed effects.

	(1)	(2)	(3)	(4)	(5)
	Legal program	Utilities	Cash arm	Cash arm	Cash arm
Hostile	0.090	0.017	-0.042	-0.148	-0.173
	(0.040)	(0.032)	(0.040)	(0.049)	(0.047)
	[0.024]	[0.604]	[0.296]	[0.002]	[0.000]
Mean (non-hostile)	0.30	0.18	0.62	0.46	0.39
Sample	All	All	All	Exclude legal	Exclude legal and utilities
Observations	836	836	836	566	496

Table A14: ERAP Participation and Hostility

Notes: This table shows the sample construction and relation between ERAP participation and hostility. The sample consists of tenants who play the Dictator Game against their own landlord, do not move between their ERAP application and the DG, and start an ERAP application after September 1, 2021 when the program exited its pilot phase. Column 5 shows the same regression as in Figure 5A. Columns 1 and 2 show the regression coefficient from regressing participation in the Memphis ERAP's legal program or having a utilities shutoff on hostility. The outcome in Column 1 is an indicator for receiving a payment via the legal program, which could expedite payments for tenants with eviction filings. The correlation is positive, in part because hostile tenants are more likely to have filings (and therefore be eligible for this special expedited process). The outcome in Column 2 is an indicator for receiving a payment via the utilities program and applying with a self-reported utilities shutoff. Such individuals could be expedited for ERAP screening. We view tenants in Columns 1 and 2 as outside the functioning of a standard ERAP, as these are tenants where landlord-tenant cooperation is less important for receiving ERAP payment. The outcome in Columns 3–5 is an indicator for receiving a payment via the ERAP cash arm (excluding utilities-only payments or legal payments). We show the impact of progressively excluding the sample that is 1 in Columns 1 and 2. *p*-values are shown in brackets.

<i>A. Treatment Effects (pp / 6 months)</i> 1. Average judgments	(1) 1.54 (1.48) [0.297]	(2) 1.40 (1.76) [0.425]	(3) 0.43 (0.62) [0.490]
2. Average filings	-0.57 (2.07) [0.782]	-7.16 (2.51) [0.004]	-2.94 (1.48) [0.047]
3. 0–1 month filings	-8.82 (1.38) [0.000]	-16.77 (1.85) [0.000]	-10.84 (1.36) [0.000]
<i>B. Interpretation</i> 4. Maximum simulated effect: judgments	-4.60	-6.60	-7.41
5. Maximum simulated effect: filings	-13.69	-20.53	-17.83
6. Fiscal cost per judgment (point estimate)			
7. Fiscal cost per filing (point estimate)	980,031	78,431	190,729
8. Fiscal cost per judgment (95% lower bound)	414,555	274,312	713,769
9. Fiscal cost per filing (95% lower bound)	121,147	46,501	96,024
N paid N non-paid Design:	4,609 Only-paid	4,609 Only-paid, reweighted	4,609 4,861 Non-paid (stacked)

Table A15: ERAP Treatment Effects

Notes: 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. Cumulative effects in Column 1 are the same as the average effects in Figure A15, but multiplied by 3. 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 92). Column 2 shows reweighted estimates. Column 3 shows estimates from the alternative design with non-paid applicants (Equation 94). Parentheses show standard errors. Brackets show *p*-values.

Table A16: Efficient and Inefficient Evictions

Panel A. Eviction	
Total eviction %	20.5
pp efficient	16.6
pp inefficient	3.9
pp from hostility	2.5
Panel B. Bargaining	
Total bargaining %	79.5
pp efficient	79.5
pp from altruism	13.2
pp inefficient	0.0

Notes: This table shows the share of evictions that are efficient and inefficient. The percentage points here correspond to the levels of efficient and inefficient evictions shown in Figure 6 A.

Table A17	: Match	to Moments
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Moments	Estimated Value	Targeted Value
ERAP mean judgment rate (unconditional)	0.186	0.093
Treatment effect on judgments (unconditional)	0.007	0.013
Landlord take-up rate	0.658	0.644
Payment plan rate (proportion of back rent)	0.326	0.318
Interaction: a_{Li} and take-up	0.446	0.462
Interaction: p_{Li} and take-up	0.266	0.274
Interaction: $a_{Li} \times p_{Li}$ and take-up	0.210	0.214
Interaction: a_{Ti} and payment rate	0.155	0.186
Interaction: \tilde{p}_{Ti} and payment rate	-0.058	-0.056
Interaction: $a_{Ti} \times \tilde{p}_{Ti}$ and payment rate	-0.022	-0.031
Interaction: d_i and payment rate	736	694
Mean: judgments (from tenants)	0.210	0.317
Interaction: a_{Ti} and judgment	0.079	0.153
Interaction: \tilde{p}_{Ti} and judgment	-0.039	-0.059
Interaction: $a_{Ti} \times \tilde{p}_{Ti}$ and judgment	-0.011	-0.024
Interaction: d_i and judgment	468	806
Interaction: a_{Li} and judgment	0.119	0.082
Interaction: \tilde{p}_{Li} and judgment	0.086	0.067
Interaction: $a_{Li} \times \tilde{p}_{Li}$ and judgment	0.053	0.037
Interaction: Takes _i and judgment	0.132	0.109
Interaction: $a_{Li} \times \text{Takes}_i$ and judgment	0.087	0.059
Interaction: $\tilde{p}_{Li} \times \text{Takes}_i$ and judgment	0.056	0.046
Interaction: $a_{Li} \times \tilde{p}_{Li} \times \text{Takes}_i$ and judgment	0.039	0.024

Notes: This table displays the match to the moments (see full list in Appendix B.3.2). The table displays unweighted values in their natural units. In estimation, all moments except the first two "macro moments" are scaled by the standard deviation of the data (the data underlying the second column), so they have comparable units (footnote 28). Moments labeled with "Interaction" mean that we are displaying the oaverage of the first variable multiplied with the second variable (mean(x_iy_i)).

Change to quantitative model	(1) Efficient eviction rate	(2) Inefficient eviction rate	(3) Hostile eviction rate	(4) % TOT if no L altruism
1. Primary estimate (no changes)	16.6	3.9	2.5	-43.0
2. ERAP TOT: 50% of judgments & filings	20.6	3.7	2.4	-64.4
3. Different take-up proxy	16.6	5.3	2.6	-46.3
4. Different bargaining moment	16.9	3.9	2.5	-44.8
5. Costlier ERAP: $k_L^P = $ \$1,000	16.6	5.5	2.5	-52.0
6. Cheaper ERAP: $k_L^P = \$0$	17.0	3.4	2.4	-42.9
7. Correlation: k_{Li} and p_{Li}	14.3	3.8	2.6	-45.7
8. Assume tenants have correct beliefs	19.0	2.2	1.4	-46.9
9. Use beliefs about the average	17.0	3.5	2.4	-47.9
10. Use identity weight for ERAP moments	19.0	3.9	2.5	-50.3
11. Use bootstrap bias correction	16.6	3.9	2.5	-43.0

Table A18: Section 6 Robustness

Notes: Rows 1 and 7 are estimated on the same 200 bootstraps as in the main analysis. Other rows are estimated on 100 bootstraps and involve re-estimating parameters. Columns 1–3 show the efficient and inefficient eviction rates (×100), similar to Table A16. 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 5 (dividing Column 3 by Column 5 of that table). Row 2 of the Table A18 shows the effects if ERAP had a 50% TOT. Row 3 changes the take-up proxy to be 1 if and only if the landlord requested materials in the experiment. Row 4 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 5–6 vary the ERAP cost parameter k_L^p . Row 7 permits correlation between k_{Li} and p_{Li} that roughly matches the empirical correlation between these. In particular, we simulate above-median p_{Li} as having 0.2 standard deviation higher k_{Li} , where standard deviations are measured from the simulated distribution of $\varepsilon_{i1} - \varepsilon_{i2}$. Row 8 assumes tenants have perfect beliefs. Row 9 uses the beliefs about the average in the experiment, rather than beliefs about own landlord or tenant. Row 10 uses the identity weight matrix to weight the ERAP "macro" moments. Row 11 implements a bootstrap bias correction.

Table A19: Landlord-tenant assortative matching

	(1)	(2)		(3)	(4)	
	Tenant: indifference p	oint Maximal alt	ruism H	ostile	Maximal hos	stile
Landlord indifference	e 10.6	0.061	-(0.015	-0.018	
	(10.2)	(0.053)	(0).069)	(0.036)	
	[0.305]	[0.257]	[0	.831]	[0.625]	
Observations	372	372		372	372	
	(b) Landlo	ord hostility				
	(1)	(2)	(3)		(4)	
Te	nant: indifference point	Maximal altruism	Hostile	Max	kimal hostile	
Landlord hostile	-24.2	-0.11	0.049		0.053	
	(16.2)	(0.086)	(0.11)		(0.055)	
	[0.146]	[0.203]	[0.652]		[0.349]	
Observations	372	372	372		372	

(a) Landlord indifference point

(c) 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
	1221

Notes: Panels A and B show 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 D). Panel C 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.

B Model Appendix

B.1 Efficiency with Social Preferences

Two different normative treatments of social preferences — one that respects equal social preferences and one that does not respect social preferences — yield equivalent efficiency conditions on eviction. For this reason, we can be agnostic between these approaches. But subtleties arise.

Consider three normative treatments of social preferences:

- 1. Social preferences are not normatively respectable. Then, eviction is efficient if and only if $k_{Ti} + k_{Li} < 0$. That is, eviction is efficient if bargaining is costlier than eviction.
- 2. Social preferences are fully normatively respectable. Then, eviction is efficient if and only if $(1 + a_{Ti})k_{Li} + (1 + a_{Li})k_{Ti} < 0$. This expression scales own court costs by the utility impacts on the counterpart from social preferences.

However, if $a_{Ti} \neq a_{Li}$, then the more altruistic party generates infinite social utility by transferring infinite wealth to the other in bargaining. To see why, suppose without loss of generality that the tenant is more altruistic than the landlord. Then, in a bargained settlement, the tenant gets $-o_{Ti} + a_{Ti}o_{Ti}$ and the landlord gets $o_{Ti} - a_{Li}o_{Ti}$. Consequently, maximizing the sum of utilities sends o_{Ti} to infinity. One could view this outcome as efficient, as it produces infinite social utility. Equivalently, eviction or any interior bargaining outcome is always inefficient. This degeneracy does not arise in the first concept because utility is transferable, and the social planner is indifferent among bargained outcomes.

3. Equal social preferences are normatively respectable. A middle-ground is to say that the social planner's normative altruism parameter is $a \in (-1, 1)$, which applies equally to both parties. For instance, *a* could be the average, maximum, or minimum of parties' social preferences. With equal *a*, social utility is now transferable, and interior bargained outcomes and eviction are not always inefficient. Specifically, eviction is efficient if and only if $(1 + a)k_{Li} + (1 + a)k_{Ti} < 0$. We see this concept as reasonable if the planner does not respect "excessive" social preferences that push toward degenerate allocations.

Concepts 1 and 3 coincide for determining whether eviction is efficient, as the (1 + a) terms cancel. Thus, when classifying evictions as efficient or not (our main normative exercise), we can apply either concept 3, which normatively respects equal social preferences, or concept 1, which does not. However, concepts 1 and 3 do not imply equivalent social utility from eviction. That is, the overall welfare impact of eviction differs across all three concepts. In Figure 6 and Table 5, we show different costs or benefits depending on whether one wants to respect social preferences.

B.1.1 Extension of Section 1's model)

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:

$$u(c_0) \coloneqq u(n_0 - d(1 - a_T)),\tag{13}$$

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 $[\underline{s}, \overline{s}]$. If $s \geq d_0 - d$, tenants can then pay the full remaining balance and consume

$$c_1 = s - (d_0 - d)(1 - a_T).$$
(14)

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 $x \in [0, d_0 - d]$:

$$u(s - j_T(x)) := u(s - \underbrace{(p_T x (1 - a_T) + k_T + (1 - a_T) k_L)}_{:=j_T(x)})$$
(15)

$$j_L(x) := p_L x (1 - a_L) - k_L - (1 - a_L) k_T,$$
(16)

where p_T and p_L denote tenant and landlord beliefs about how much the tenant would transfer to the landlord.

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

$$b^{*}(x) := \arg\max_{b(x)} \left(u(s - (1 - a_{T})b(x)) - u(s - j_{T}(x)) \right)^{\beta} \left(b(x)(1 - a_{L}) - j_{L}(x) \right)^{1 - \beta},$$
(17)

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

$$b^{*}(x) = \frac{j_{L}}{1 - a_{L}} + \left(\frac{1 - \beta}{\beta}\right) \left(\frac{u(s - (1 - a_{T})b^{*}(x)) - u(s - j_{T})}{u'(s - (1 - a_{T})b^{*}(x))}\right),$$
(18)

which nests the solution in Equation (36) if tenant preferences are linear.

Bargaining is therefore possible if and only if:

$$(1 - a_L)b^*(x) \ge j_L(x)$$
 (19)

$$u(s - b^*(x)(1 - a_T)) \ge u(s - j_T(x)),$$
(20)

which implies that bargaining occurs if and only if:

$$\frac{\Delta px}{\overline{a}} \le k_L + k_T,\tag{21}$$

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

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

$$U(d) = u(n_0 - d(1 - a_T)) + \delta \left(\int_{\underline{s}}^{d_0 - d} u(s - b^*(s)(1 - a_T))\mathcal{B}(s)dF + \int_{\underline{s}}^{d_0 - d} u(s - j_T(s))(1 - \mathcal{B}(s))dF + \int_{\underline{s}}^{\overline{s}} u(s - b^*(d_0 - d)(1 - a_T))\mathcal{B}(d_0 - d)dF + \int_{d_0 - d}^{\overline{s}} u(s - j_T(d_0 - d)))(1 - \mathcal{B}(d_0 - d))dF \right).$$
(22)

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

$$\mathcal{B}(x) = 1 \implies \mathcal{B}(y) = 1 \text{ for all } y < x.$$
 (23)

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

$$U(d) = u(n_0 - d(1 - a_T)) + \delta\left(\int_{\underline{s}}^{d_0 - d} u(s - b^*(s)(1 - a_T))dF + \int_{d_0 - d}^{\overline{s}} u(s - b^*(d_0 - d)(1 - a_T))dF\right)$$
(24)

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

$$u'(n_0 - d_1(1 - a_T)) = \delta \int_{d_0 - d_1}^{\overline{s}} b'^*(d_0 - d_1)u'(s - b^*(d_0 - d_1)(1 - a_T))dF,$$
(25)

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

$$\mathcal{B}(s) = 1 \iff s \le \frac{\overline{a}}{\Delta p} (k_L + k_T).$$
 (26)

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

$$U(d) = u(n_0 - d(1 - a_T)) + \delta \left(\int_{\underline{s}}^{\frac{\bar{a}}{\Delta p}(k_L + k_T)} u(s - b^*(s)(1 - a_T)) dF + \int_{\frac{\bar{a}}{\Delta p}(k_L + k_T)}^{d_0 - d} u(s - j_T(s)) dF + \int_{d_0 - d}^{\overline{s}} u(s - j_T(d_0 - d)) dF \right).$$
(27)

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

$$u'(n_0 - d_2(1 - a_T)) = p_T \delta \int_{d_0 - d_2}^{\overline{s}} u'(s - j_T(d_0 - d_2)) dF,$$
(28)

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 $(d = d_1 \text{ or } d = d')$. 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 $\delta \rightarrow 0$ or $n_0 \rightarrow -\infty$, 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*.

Concave tenant utility and Nash bargaining, rather than take-it-or-leave-it, does not change the fundamental bargaining solution in Equation (21). 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 or altruism *outside* the concave utility function can change it.

B.2 Simple ERAP Model (Appendix to Section 1.4)



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

Diagram A1: Timing and Game Tree

Setup. We now consider an emergency rental program, intended as a stylized version of ERAP. Diagram A1 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.²⁶

The payoffs are identical to the main text 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.

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

²⁶We 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.

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

$$k_{Li} + k_{Ti} + w_i \ge \frac{\Delta t_i}{\bar{a}_i},\tag{29}$$

where the "program wedge" w_i is defined as:

$$w_{i} = \frac{1}{\overline{a}_{i}} \left(\underbrace{\frac{d_{i} - k_{Li}^{P}}{1 - a_{Li}}}_{Altruism-adjusted} - \underbrace{\left(t_{Ti} + \frac{k_{Ti} + a_{Ti}k_{Li}}{1 - a_{Ti}} \right)}_{Outside option adjustment} \right).$$
(30)

Thus, ERAP can "crowd out" (cause not to occur) bargaining, efficient eviction, or inefficient eviction. The inequality in (29) reverses (5), but is augmented with the program wedge w_i . If $w_i < 0$ and take-up occurs, then bargaining would have occurred without ERAP. 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.

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

$$S_{Li} := \underbrace{d_i - k_{Li}^P}_{\text{ERAP payoff}} - \underbrace{(t_{Li}(1 - a_{Li}) - k_{Li} - a_{Li}k_{Ti})}_{\text{Outside option}}.$$
(31)

Enrollment occurs if and only if landlord net surplus S_{Li} is positive, as the condition $S_{Li} \ge 0$ is equivalent to (29). 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.

B.3 Quantitative Model Details (Appendix to Section 6)

B.3.1 Model Set-up

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

$$V_{Li}^{\text{NoERAP,Evict}} = (1 - a_{Li})t_{Li}(d_i) - a_{Li}k_T - k_L + \varepsilon_{i1}$$
(32)

$$V_{Li}^{\text{NOEKAP, Bargain}} = (1 - a_{Li})n_i^*(d_i) + \varepsilon_{i2}$$
(33)

$$V_{Li}^{\text{ERAP,Evict}} = d_i + (1 - a_{Li})t_{Li}(0) - k_L^P - a_{Li}k_T - k_L + \varepsilon_{i3}$$
(34)

$$V_{Li}^{\text{ERAP,Bargain}} = d_i - k_L^P + (1 - a_{Li})n_i^*(0) + \varepsilon_{i4}.$$
(35)

Nash Bargaining yields the following solutions for bargaining payoffs (see proofs in Appendix

B.4):

$$n_{i}^{*}(d_{i},\varepsilon_{i2},\varepsilon_{i1};\beta) = \underbrace{\beta\left(t_{Li}(d_{i}) - \frac{k_{L} + k_{Ti}a_{Li} + \varepsilon_{i2} - \varepsilon_{i1}}{1 - a_{Li}}\right)}_{\beta \times \text{ altruism-adjusted outside option of landlord}} + \underbrace{\left(1 - \beta\right)\left(t_{Ti}(d_{i}) + \frac{k_{Ti} + (k_{L} + \varepsilon_{i2} - \varepsilon_{i1})a_{Ti}}{1 - a_{Ti}}\right)}_{-(1 - \beta) \times \text{ altruism-adjusted outside option of tenant}}$$

$$n_{i}^{*}(0,\varepsilon_{i4},\varepsilon_{i3};\beta) = \underbrace{\beta\left(t_{Li}(0) - \frac{k_{L} + k_{Ti}a_{Li} + \varepsilon_{i4} - \varepsilon_{i3}}{1 - a_{Li}}\right)}_{\beta \times \text{ altruism-adjusted outside option of landlord}} + \underbrace{(1 - \beta)\left(t_{Ti}(0) + \frac{k_{Ti} + (k_{L} + \varepsilon_{i4} - \varepsilon_{i3})a_{Ti}}{1 - a_{Ti}}\right)}_{-(1 - \beta) \times \text{ altruism-adjusted outside option of tenant}}$$
(37)

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 $\mathcal{E}^{No\hat{E}RAP}$ be an indicator that is 1 if and only if

$$k_L + k_T + \varepsilon_{i2} - \varepsilon_{i1} \le \frac{t_{Li}(d_i) - t_{Ti}(d_i)}{\overline{a}_i}$$
(38)

(36)

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

$$k_L + k_T + \varepsilon_{i4} - \varepsilon_{i3} \le \frac{t_{Li}(0) - t_{Ti}(0)}{\overline{a}_i}$$
(39)

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

$$V_{Li} = \begin{cases} \max \begin{bmatrix} V_{Li}^{\text{NoERAP,Evict}}, V_{Li}^{\text{ERAP,Evict}} \end{bmatrix} & \text{if } \mathcal{E}^{\text{NoERAP}} = 1 \text{ and } \mathcal{E}^{\text{ERAP}} = 1 \\ \max \begin{bmatrix} V_{Li}^{\text{NoERAP,Bargain}}, V_{Li}^{\text{ERAP,Evict}} \end{bmatrix} & \text{if } \mathcal{E}^{\text{NoERAP}} = 0 \text{ and } \mathcal{E}^{\text{ERAP}} = 1 \\ \max \begin{bmatrix} V_{Li}^{\text{NoERAP,Evict}}, V_{Li}^{\text{ERAP,Bargain}} \end{bmatrix} & \text{if } \mathcal{E}^{\text{NoERAP}} = 1 \text{ and } \mathcal{E}^{\text{ERAP}} = 0 \\ \max \begin{bmatrix} V_{Li}^{\text{NoERAP,Evict}}, V_{Li}^{\text{ERAP,Bargain}} \end{bmatrix} & \text{if } \mathcal{E}^{\text{NoERAP}} = 1 \text{ and } \mathcal{E}^{\text{ERAP}} = 0 \\ \max \begin{bmatrix} V_{Li}^{\text{NoERAP,Bargain}}, V_{Li}^{\text{ERAP,Bargain}} \end{bmatrix} & \text{if } \mathcal{E}^{\text{NoERAP}} = 0 \text{ and } \mathcal{E}^{\text{ERAP}} = 0. \end{cases} \end{cases}$$

$$(40)$$

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. Fourth, we assume that all landlords receive payment if they choose to take up, abstracting from administrative issues in receiving payment.

B.3.2 Estimation

Method of Simulated Moments. 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:

$$\hat{\theta} = \operatorname*{arg\,min}_{\theta \in \Theta} \left[\hat{m}(X_i; \theta)' W \hat{m}(X_i; \theta) \right]$$
(41)

for weight matrix *W*.

This problem involves minimizing a global, nonsmooth objective. We estimate the global minimizer using the TikTak algorithm in Arnoud et al. (2019). We initialize the TikTak algorithm with N = 50,000 Sobel' points and then run Nelder-Mead on the 500 most promising seed points (those with the lowest objective). Optimization uses the TikTak mixing weight suggested in footnote 29 of Arnoud et al. (2019). Adjusting the number of Sobol' or seed points has only small effects on the estimated minimum. Parameters are constrained to lie within reasonable intervals. Parameter estimates approach the constraints only for a small share of bootstrap estimates (except in the case of the tenant bargaining power parameter, which is estimated to be high).

We do not observe matched landlord-tenant pairs, since they participate in the experiments separately. We randomly simulate landlord and tenant pairs to match based on an assortative-matching benchmark. Given that we see moderate assortative matching on altruism, we simulate this assortative matching by assortatively matching 22.5% of landlord-tenant pairs based on their rank in the altruism distribution of each group (Appendix D.5). Since there are more tenants than landlords, we resample landlords at random within the landlord sample.

We bootstrap the entire procedure, including matching the landlord-tenant pairs. We bootstrap the treatment effect moments and experimental data separately. Standard errors represent the standard deviation of bootstraps.

To summarize, estimation involves the following procedure:

- Bootstrap the data and moments, forming Σ^b datasets for $b \leq B$. Draw one vector of shocks ε^b for each bootstrap.
- In each bootstrapped dataset, simulate the landlord-tenant match.
- In each bootstrapped datset, estimate parameter vector Θ^b given that vector of shocks and match, using the TikTak algorithm.

Because all our parameter estimates use a matching procedure that involves randomness, we report the average value of the parameter across bootstraps ($\hat{\Theta} = \frac{1}{B} \sum_{b} \Theta^{b}$). For any function of the data and parameters $f(\Theta, \Sigma)$ (e.g., counterfactuals and welfare estimates), we report $\hat{f} = \frac{1}{B} \sum_{b} f(\Theta^{b}, \Sigma^{b})$.

Standard errors do not account for two sources of error. First, we do not bootstrap one moment, the average judgment rates from the ERAP evaluation. As this is a sample mean, the judgment rate is very precisely estimated so this source of error is de minimus. Second, we do not account for error from simulation noise in the shocks or convergence algorithm. Tests indicate simulation error is reasonably small, but present. Standard errors are therefore conservative.

Preparing the Experimental Data for Use in the Quantitative Model. The dataset includes 892 tenants with clean names and addresses (so we could merge them to eviction data), positive back rents at the time of the tenant survey, and who played the DG against their own landlord; and 211 landlords with clean names and addresses, 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 \overline{a}_i is well-defined, we top-code $|a_{ji}|$ at 0.95. The precise top-coding and bottom-coding value is possibly consequential, since most participants have $a_{ji} = 0.95$ and that determines the value of \overline{a}_i . Figure A13 shows sensitivity to how we scale altruism.
- Transfers $t_{ji}(\cdot)$ follow Equation (12). We parameterize landlord beliefs from 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:

$$\tilde{p}_{Ti} \coloneqq -\frac{1}{2} \left(\left(p_{1i}' - 0.31 \right) + \left(0.69 - p_{2i}' \right) \right) + 0.06, \tag{42}$$

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.

As these are strong assumptions, we also conduct an exercise where we simulate $p_{Ti} = 0.06$ (Table A18). The table also conducts an exercise where we use beliefs about the average landlord or tenant, rather than own landlord or tenant and find similar results. Assumptions about beliefs generally makes a smaller difference than assumptions about social preferences because extreme social preferences amplify the misperception wedge or drive it to zero (Figure A13).

- 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 proxy for 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 A18).
- Judgments (Judgment_i). We use landlord and tenant judgments from links to court data (Section 5). As we randomly match landlords and tenants, 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. The moment conditions use the correlation between tenant social preferences/beliefs and whether the tenant ends up in court; and vice-versa for landlord social preferences/beliefs.
- 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 solution. 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 elicitation 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. While $n_i^*(d_i)$ was elicited after information provision, given the small impacts on this outcome, we use the data from both treatment and control.²⁷

• For power, we 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.

Moments. Notation: hats $(\widehat{\ldots})$ represent simulated outcomes. Moments are:

1. ERAP's treatment effect on judgments, where

$$TOT - \left(\mathbb{E}[Judgment_{Li}|ERAP_{Li}] - \mathbb{E}[Judgment_{Li}|NoERAPExist_{Li} and ERAP_{Li}]\right) = 0.$$
(43)

Here TOT is the Treatment Effect on the Treated from Section 5.4, 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 D) and bootstrap the treatment effects.

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

$$p - \mathbb{E}[\operatorname{Judgment}_{l,i}|\operatorname{ERAP}_{Li}] = 0, \tag{44}$$

where *p* comes from the ERAP data. We do not bootstrap this moment.

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

$$\mathbb{E}\left[\mathrm{ERAP}_{Li} - \widehat{\mathrm{ERAP}}_{Li}\right] = 0. \tag{45}$$

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

$$\mathbb{E}\left[n_{Li}^*(d_i) - \widehat{N}_{Li}^*(d_i)\right] = 0.$$
(46)

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

$$\mathbb{E}\left[a_{Li}\left(\mathrm{ERAP}_{Li} - \widehat{\mathrm{ERAP}}_{Li}\right)\right] = 0 \tag{47}$$

$$\mathbb{E}\left[p_{Li}\left(\mathrm{ERAP}_{Li} - \widehat{\mathrm{ERAP}}_{Li}\right)\right] = 0 \tag{48}$$

$$\mathbb{E}\left[a_{Li}p_{Li}\left(\mathrm{ERAP}_{Li} - \widehat{\mathrm{ERAP}}_{Li}\right)\right] = 0 \tag{49}$$

$$\mathbb{E}\left[\operatorname{Judgment}_{Ti} - \operatorname{Judgment}_{i}\right] = 0 \tag{50}$$

²⁷There 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.

$$\mathbb{E}\left[a_{Li}\left(\text{Judgment}_{Li} - \text{Judgment}_{i}\right)\right] = 0$$
(51)

$$\mathbb{E}\left[p_{Li}\left(\mathrm{Judgment}_{Li} - \mathrm{Judgment}_{i}\right)\right] = 0$$
(52)

$$\mathbb{E}\left[a_{Li}p_{Li}\left(\operatorname{Judgment}_{Li}-\operatorname{Judgment}_{i}\right)\right]=0$$
(53)

$$\mathbb{E}\left[a_{Ti}\left(n_{Li}^{*}(d_{i})-\widehat{N}_{i}^{*}(d_{i})\right)\right]=0$$
(54)

$$\mathbb{E}\left[\tilde{p}_{Ti}\left(n_{Li}^{*}(d_{i})-N_{i}^{*}(d_{i})\right)\right]=0$$
(55)

$$\mathbb{E}\left[a_{Ti}\tilde{p}_{Ti}\left(n_{Li}^{*}(d_{i})-\widehat{N}_{i}^{*}(d_{i})\right)\right]=0$$
(56)

$$\mathbb{E}\left[d_i\left(n_{Li}^*(d_i) - \widehat{N}_i^*(d_i)\right)\right] = 0$$
(57)

$$\mathbb{E}\left[\mathrm{ERAP}_{Li}\left(\mathrm{Judgment}_{Li} - \mathrm{Judgment}_{i}\right)\right] = 0 \tag{58}$$

$$\mathbb{E}\left[a_{Li} \mathbb{ERAP}_{Li}\left(\mathsf{Judgment}_{Li} - \mathsf{Judgment}_{i}\right)\right] = 0 \tag{59}$$

$$\mathbb{E}\left[p_{Li} \mathrm{ERAP}_{Li}\left(\mathrm{Judgment}_{Li} - \mathrm{Judgment}_{i}\right)\right] = 0 \tag{60}$$

$$\mathbb{E}\left[a_{Li}p_{Li}\mathrm{ERAP}_{Li}\left(\mathrm{Judgment}_{Li}-\mathrm{Judgment}_{i}\right)\right]=0$$
(61)

$$\mathbb{E}\left[a_{Ti}\left(\text{Judgment}_{Ti} - \text{Judgment}_{i}\right)\right] = 0 \tag{62}$$

$$\mathbb{E}\left[\tilde{p}_{Ti}\left(\text{Judgment}_{Ti} - \text{Judgment}_{i}\right)\right] = 0 \tag{63}$$

$$\mathbb{E}\left[a_{Ti}\tilde{p}_{Ti}\left(\text{Judgment}_{Ti} - \text{Judgment}_{i}\right)\right] = 0 \tag{64}$$

$$\mathbb{E}\left[d_i\left(\mathrm{Judgment}_{Ti} - \mathrm{Judgment}_i\right)\right] = 0. \tag{65}$$

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. For the intercept, we use the tenant-side observed judgment (Equation 50). We simulate whether the household gets a judgment either in or out of ERAP and compare to whether they have a judgment in the court data, which corresponds to the correlations we target and in Table 3 (which do not restrict or condition on ERAP receipt in any way).

Weights. We weight the "micro" moments by the inverse of the variance of the data at the household (*i*) level.²⁸ To more precisely match the "macro" moments (Equations 43 and 44), we

²⁸ This is equivalent to normalizing the moments into comparable units, by dividing each moment by the standard deviations of the data. In particular, if moments are of the form $\mathbb{E}[X_i(Y_i - \hat{Y}_i)]$, we divide micro moments by $SD(X_iY_i)$ and use the identity weight matrix. This is equivalent to using a diagonal weight matrix with $Var^{-1}(X_iY_i)$ as the weight on these moments.

weight the treatment effect by 10 and the mean by 2. We use these heuristic weights because the treatment effect, as a well-identified moment, is valuable to match precisely. Results are similar if we weight these moments by the identity matrix (Table A18).

B.3.3 Robustness and Sensitivity of the Quantitative Model

Table A18 shows robustness to calibration assumptions. We discuss several especially important tests. The main conclusions are relatively robust to our assumption about landlords' take-up cost k_L^p . We also explore the importance of the assumption that landlords' eviction cost k_{Li} is uncorrelated with p_{Li} and \bar{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 but raises the inefficient share to about one third. Other rows explore different assumptions, as discussed in table notes.

While measurement error could affect the magnitude of our results, measurement error would need to be severe to overturn them. 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 A13A) and posit the same value of estimated and calibrated parameters. If social preferences are overstated by half (s = 0.5), then about 30% evictions would be inefficient. Second, we scale misperceptions for various $s \in \mathbb{R}^+$ (Panel B) and find our conclusions are also reasonably insensitive.

B.4 Proofs

Proof of Proposition 3.

Proof. 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:

$$-(1 - a_{Ti})o_i \ge -t_{Ti}(1 - a_{Ti}) - k_{Ti} - k_{Li}a_{Ti}$$
(Tenant constraint)
$$(1 - a_{Li})o_i \ge t_{Ti}(1 - a_{Li}) - k_{Li} - k_{Ti}a_{Li}.$$
(Landlord constraint)

Such an offer o_i exists if and only if:

$$t_{Li} - \frac{k_{Li} - k_{Ti}a_{Li}}{1 - a_{Li}} \le t_{Ti} + \frac{k_{Ti} + k_{Li}a_{Ti}}{1 - a_{Ti}}.$$
(66)

$$\iff \frac{k_{Li} + k_{Ti}a_{Li}}{1 - a_{Li}} + \frac{k_{Ti} + k_{Li}a_{Ti}}{1 - a_{Ti}} \ge \Delta t_i \tag{67}$$

$$\iff (k_{Li} + k_{Ti}a_{Li})(1 - a_{Ti}) + (k_{Ti} + k_{Li}a_{Ti})(1 - a_{Li}) \ge \Delta t_i(1 - a_{Ti})(1 - a_{Li})$$
(68)

$$\iff (k_{Li} + k_{Ti})(1 - a_{Li}a_{Ti}) \ge \Delta t_i(1 - a_{Ti})(1 - a_{Li}) \tag{69}$$

$$\iff k_{Li} + k_{Ti} \ge \frac{\Delta t_i}{\bar{a}_i} \tag{70}$$

meaning that eviction occurs if and only if

$$k_{Li} + k_{Ti} < \frac{\Delta t_i}{\bar{a}_i},\tag{71}$$

as desired.

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

$$d_i - k_{Li}^P \ge \max\left\{ (1 - a_{Li})o_i, V_{Li}^E \right\}$$
(72)

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

$$d_{i} - k_{Li}^{P} \ge (1 - a_{Li}) \left[t_{Li} - \frac{k_{Li} + a_{Li}k_{Ti}}{1 - a_{Li}} \right].$$
(73)

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

$$t_{Li} \le \frac{k_{Li} + a_{Li}k_{Ti}}{1 - a_{Li}} + \frac{1}{1 - a_{Li}} \left[d_i - k_{Li}^P \right].$$
(74)

We then subtract t_{Ti} from both sides:

$$\Delta t_i \le \frac{k_{Li} + a_{Li}k_{Ti}}{1 - a_{Li}} + \frac{1}{1 - a_{Li}} \Big[(1 - t_{Ti}(1 - a_{Li})) - k_{Li}^P \Big].$$
(75)

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

$$\Delta t_i \le \bar{a}_i (k_{Li} + k_{Ti}) - \frac{k_{Ti} + a_{Ti} k_{Li}}{1 - a_{Ti}} + \frac{1}{1 - a_{Li}} \left[(1 - t_{Ti} (1 - a_{Li})) - k_{Li}^p \right].$$
(76)

Rearranging, we obtain:

$$\Delta t_i \le \bar{a}_i (k_{Li} + k_{Ti}) + \frac{1}{1 - a_{Li}} \left[(1 - t_{Ti} (1 - a_{Li})) - \frac{1 - a_{Li}}{1 - a_{Ti}} (k_{Ti} + a_{Ti} k_{Li}) - k_{Li}^p \right].$$
(77)

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

$$\frac{\Delta t_i}{\bar{a}_i} \le k_{Li} + k_{Ti} + \frac{1}{\bar{a}_i(1 - a_{Li})} \left[(1 - t_{Ti}(1 - a_{Li})) - \frac{1 - a_{Li}}{(1 - a_{Ti})} (k_{Ti} + a_{Ti}k_{Li}) - k_{Li}^P \right].$$
(78)

Then the landlord takes up if and only if:

$$k_{Li} + k_{Ti} + w_i \ge \frac{\Delta p_i d_i}{\overline{a}_i} \tag{79}$$

where

$$w_{i} = \frac{1}{\overline{a}_{i}} \left(\underbrace{\frac{d_{i} - k_{Li}^{p}}{1 - a_{Li}}}_{\text{Altruism-adjusted}} - \underbrace{\left(t_{Ti} + \frac{k_{Ti} + a_{Ti}k_{Li}}{1 - a_{Ti}} \right)}_{\text{Outside option adjustment}} \right).$$
(80)

Proof of Equations (36) and (37). Suppress dependence on *i*. Given altruism, Nash bargaining solves:

$$n^{*} = \arg\max_{n} \left(-(1 - a_{T}) n - \mu_{T} \right)^{\beta} \left((1 - a_{L}) n - \mu_{L} \right)^{(1 - \beta)}$$
(81)

where μ_L and μ_T represent the parties' outside options inclusive of their own altruism. Then

taking logs, the first-order condition is:

$$-\frac{\beta(1-a_T)}{-(1-a_T)n^*-\mu_T} + \frac{(1-\beta)(1-a_L)}{(1-a_L)n^*-\mu_L} = 0.$$
(82)

From here, rearrangement gives:

$$n^* = \frac{\beta \mu_L}{1 - a_L} - \frac{\mu_T (1 - \beta)}{1 - a_T}.$$
(83)

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

$$\mu_L \equiv t(d_i)(1-a_L) - k_L + k_{Ti}a_{Li} + \varepsilon_{i2} - \varepsilon_{i1}, \qquad (84)$$

and

$$\mu_T \equiv -t(d_i)(1 - a_T) - (k_{Ti} + (k_L + \varepsilon_{i2} - \varepsilon_{i1})a_{Ti}),$$
(85)

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

C Experiment Details

C.1 Sample and 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. The sample also drops tenants who do not confirm that they applied or started applying for the ERA.

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. We keep only the first response from likely duplicates as identified by names, emails, or other information. The ERAP application had a contact email to opt out of research, and we exclude landlords and tenants from being contacted if they opt out (and from Table 1, Column 2 and Table 2, Column 3).

Completion Times. Participants could complete the survey partially, then begin again where they left off (typically after a reminder). Among the 57% of landlords (85% of tenants) who completed the survey in one day, the median survey completion time was 21 (29) minutes.

Survey Changes. There were limited opportunities to pilot the study, and we made several minor changes to the experiments after recruitment was underway. These are documented in Appendix C.6.

Ethics. Beyond separate recruitment, 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 with potential for adverse outcomes (e.g., choices in Dictator Games) were 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.

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 instance, if x = 100, then values of *s* could include $\{10, 20, \ldots, 90, 100, 110, \ldots, 190, 200\}$. We enforce monotonicity. The first question of the MPL asked about (9x/10, x) versus (x, x). To reduce the expected number of clicks, the second question of the MPL incremented *s* up or down by 4x/10 (to (x/2, 0) or (3x/2, 0)), depending on participants' responses.

On monotonicity, we assume that if a participant prefers *A* to *B* for a given *s*, she will also prefer *A* to *B* for s' > s.

We obtain upper and lower bounds on the indifference point, 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 $S_{\ell} = 12.5$. Consistent with this procedure, for people who prefer (x, x) to (2x, 0), we assume the indifference point is 2.05x.

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 is a tenant of another landlord participant, and similarly for tenants playing against random, unnamed landlords.

While some tenants move between ERAP application and the landlord survey, the surveys took place earlier in the program, during a period with fewer evictions, so move rates are lower. Therefore, our design is such that almost all participants play against their current landlord or tenant (if they play against own opponents).

C.3 Selected Experiment Instructions

The complete instruments are available on Rafkin's website. We reproduce select instructions here.

C.3.1 Tenants Dictator Games

We reproduce the case where tenants play against their own landlord.

At least 10 participants in the survey will be randomly selected to receive an additional Amazon gift card.

If you win one of the gift cards, you may be able to share it with [landlordname]. Or, you may be able to share it with another tenant who answered the survey.

Whether you can share the gift card with a tenant or landlord will be determined randomly.

Next screen (if playing against own landlord):

One or more survey participants will be randomly selected to get as much as [\$20/\$200/\$2000] in an Amazon gift card that can be shared with **a landlord**.

So, if you win the gift card, you can choose to split it with [landlord name]. Or you can choose not to share it.

Please note that this gift card would be allocated from separate research money and NOT the ERAP funds for tenants.

We will ask you a series of questions about how you would like to divide up the gift card. We will randomly choose one of the possible questions about how to split the gift card. Using your answers, we will determine how you want to divide it.

Your answers to the following questions will determine what happens if you are chosen to win the gift card. However, your responses will not influence your chances of winning. Whether or not you win is completely random.

It is always in your best interest to answer the questions truthfully.

Your response will be not be shared with the landlord. If you win and it is shared with the landlord, the gift card will not be associated with your name and won't count as rent.

Confirmation question:

This is to confirm you understand.

Will your response about how to treat the gift card be shared with the landlord?

Yes, my response will be shared. No, my response will not be shared.

After the confirmation question, we either say:

That is incorrect. Your response will not be shared with the landlord directly. However, your answers can affect how much you and your landlord receive, if you are chosen to win the gift card.

That is correct. Your response will not be shared with the landlord directly. However, your answers can affect how much you and your landlord receive, if you are chosen to win the gift card.

Then, we ask repeated questions like the following, in a repeated price list (note that stakes were also randomized).

Would you prefer to get [\$9/\$90/\$900] and [landlordname] gets \$0, or you get [\$10/\$100\$1000] and [landlordname] also gets [\$10/\$100/\$1000]?

As discussed in Appendix C.6, some of the extra language confirming anonymity was added partway through the study. We disaggregate responses before and after the language was added and find no meaningful differences.

C.3.2 Landlords Information

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?

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:

belief bonus = max
$$(0, 22 - 22 \times (\text{truth} - \text{response})/44)^2) + 3,$$
 (86)

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:

belief bonus = max
$$(0, 22 - 22 \times (\text{truth} - \text{response})/3500)^2) + 3,$$
 (87)

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 MPL 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, for 10 landlords in total. 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 D.3, 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:

belief bonus = max
$$(0, 22 - 22 \times (\text{truth} - \text{response})/44)^2) + 3,$$
 (88)

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

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

belief bonus = max
$$(0, 22 - 22 \times (\text{truth} - \text{response})/44)^2) + 3,$$
 (89)

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.

On March 27, we switched the incentive language to say that one or more tenant would be chosen (holding constant any incentive language across all stakes). For implementation, we select 5 tenants to have their choice implemented for DGs played against landlords if they play the (10, 10) DG; 2 tenants to have their choice implemented against landlords if they play the (100, 100) DG; and 1 tenant to have their choice implemented if they play the (1000, 1000) DG. We repeat this process for 5 tenants who have their DGs against tenants implemented, for 16 (= $2 \times (5 + 2 + 1)$ implementations in all.

As the language before March 27 was identical to the landlord sample, our statements are always truthful. Note that the stakes sub-experiment in Figure 2A shows no impact of changing the magnitudes of the DGs among tenants who play after March 27.

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

WTP for Information. A secondary elicitation asks people their willingness to pay for information about the truth. We implement using a multiple price list (similar to above) for 1% of tenants.

C.5 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 omit some heterogeneity if listed in the preregistrations as secondary.

The tenant survey pre-registration was posted as a document on the AEA registry website and then lightly updated several times over the course of the study, as we added collected new data. Unfortunately, the AEA registry does not allow a reader to see updates to uploaded documents (that is, readers can click to the entry but not see tracked-changes or previous versions of uploaded documents). For transparency we post all the study details documents to the supplement folder on Rafkin's website (http://crafkin.github.io/rs_eviction_supplement/).

Exhibits or sections in brackets indicate where to find a given exhibit or analysis in the paper.

C.5.1 Landlord outcomes

Primary Outcomes.

- Behavior in DG, including indifference points, vindictiveness, differences across groups, and the impact of stakes [Figure 2A, Table A2, Figure 2D].
 - The text shows DG behavior against each of the three opponents, for transparency and consistency with the tenant survey. The pre-registration also describes computing differences between all landlords and tenants (aggregating own and random). In the pre-registration, we wrote: random versus own tenant comparisons were secondary "due to concerns about power, based on our expectations about the number of landlords who will participate (from piloting). They are not secondary because they are necessarily less important." We therefore pool them in Table S10. Levels of DG behaviors are similar, but hostility differences rely on comparing own tenants to random tenants, as in Table A2.
- Choice of whether to enroll landlords or tenants in lottery (simple lottery). [Table S13]
- Prior beliefs, both accuracy of prior beliefs ("objective," by which we meant beliefs about the average tenant), and beliefs about landlord's own the tenant. [Figure 3]

Secondary Outcomes.

- Belief updating, as well as the belief updates about the placebo belief [Figure A8, Figure S1].
- Treatment effects of information on 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, and show results from both ITTs and IV specifications [Figures S2 and A8, Tables A12, S6 and Table S9]

• Heterogeneity.

C.5.2 Tenant outcomes

Primary Outcomes.

- Behavior in DG. The tenant experiment pre-registered a focus on levels across the three groups, as in the main text. [Table A2]
 - On March 3, we added a clarification in the pre-registration that we would also examine differences between own and random landlords, which was (always) one of our treatments. We limit to participants after the survey was frozen on March 30 in Table S8.
- Beliefs (priors, posteriors, and belief update). [Figure 3, Figure A10, Appendix C]
- Payment plan outcomes: extensive margin (proposed payment plan), intensive margin (amount proposed to repay). [Appendix D.6; Table S6]

Secondary Outcomes. Several are not described in the main text, so we explain them and why we collected these data in more detail here. We omit outcomes pre-registered as exploratory or optional.

- Falk et al. (2018) survey outcomes' correlation with DG behaviors [Figure A5B]
- Willingness to pay for information. We conduct a task that asks tenants for their the willingness to pay for information about average landlord altruism. This is incentivized using a BDM mechanism with probability 0.01. We study the treatment effect of receiving one piece of information (the bargaining information) on this outcome. We originally collected these data because we thought they were useful for a different version of the structural model. [Figure S6]
- Additional payment plan outcomes. [Table S7]
- Validations of the altruism task. We added these to the pre-registration on March 27 as secondary outcomes (except for the lottery, we did not collect them prior to March 27). [Figure 2B–D]
- Tenant behavior in entering landlord into gift-card lottery. [Figure A5A]
- 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 that we would not examine the treatment effect of information on the second outcome. We collected these data because we thought having benchmarks of tenants' self-reported eviction costs would be useful. We use these outcomes in Appendix D.7.6, and report the treatment effect on the first outcome in Figure S6.
- Survey of landlord responses to payment plans, described in Section 4.3.
- Heterogeneity.

C.5.3 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 A4. Beliefs about the eviction process are in Figure S5.

C.5.4 Deviations from Pre-registration

- Changes to tenant survey pre-registration. The tenant survey launched February 14, 2022. On March 27, we updated the tenant survey to include a subexperiment that varied the DG stakes. We also collected additional DG validation outcomes (Section 3.3). We updated the pre-registration materials before making the changes. The full pre-registration history and dates are available there. We made minor tweaks to the pre-registration on Feb. 14, March 3, and April 8.²⁹
- In the landlord survey, we wrote that "we intend to link landlords' behavior to their decision of whether to accept the City/County's offer to repay back rent." These linkages proved challenging to implement in the landlord survey, since we have incomplete data on tenants' ERAP payments for those who applied before 9/1/2021 (all tenants in the landlord survey). We implement this for tenants in Section 5.
- As DG outcome in both landlord and tenant pre-registrations, we write that we would create "difference in differences" measures for the DG. For the DGs with landlords as dictators, this corresponds to: *E*[(Own tenant DG behavior – random tenant DG behavior) – (Random landlord – random tenant DG behavior)]. We realized after completing the study that these differences are not informative, as "random tenant" behaviors are subtracted from both terms due to linearity of expectations.
- Optional analysis. The tenant experiment pre-registered several optional analyses (e.g., a follow-up tenant survey, or studying the impact of information on eviction filings), which we omit.
- Tenant IV specification. The tenant experiment pre-registered doing an IV specification with the information treatment. We omit these because we find the ITTs cut by whether beliefs are above or below the treatment information to be more transparent.

C.6 Survey Changes

C.6.1 Landlords

We made two 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

²⁹On Feb. 14, we added that we would look at the impact of information on ultimate eviction. On March 3, we clarified that we would also study the difference in DG behavior betwen own landlord and random landlord. The original pre-registration indicated that we would study the levels of each independently, but omitted to pre-register looking at the difference. We had always included the randomization of own vs. random landlords, and the original experiment description describes the treatment; this change clarified that we would use the treatment. On April 8, we changed an item in the tenant-survey pre-registration pertaining to the sample size across the information treatments. This does not change what we report.
outcome about declining to sign legal agreements (a secondary outcome for landlords) from a double negative that confused participants. Second, we added a qualitative question about why the participants wanted to split the DG bundle evenly, if they reported being maximally altruistic.

C.6.2 Tenants

We made 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 30% of the sample had been collected. Changes are detailed below. Except for secondary parts of the study, the tenant survey was frozen on March 30.

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 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 government sources 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 DG interpretation.

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 2D 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.

We had limited opportunities to pilot and therefore made several tweaks based on responses between March 27 and March 30. About 10% of participants completed in this window.³⁰ The survey elicitation was unchanged after March 30. Results are similar if we exclude the March 27–30 completes, or limit only to tenants who start after March 30 (Table S8).

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

³⁰Specifically, we added language stressing that the gift card would not count as rent on March 28; the confirmation check about whether the response was anonymous on March 29; and language stating that "if you win and the gift card is shared with the landlord, the gift card will not be associated with your name."

- Between Feb. 14 (survey launch) and March 30, we made adjustments to the belief elicitation and payment plan language. 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. The DG elicitations were fixed.
- Feb 16: We added a filter question asking participants to confirm they applied or began applying for ERAP. From Feb. 16 and later, we drop 29 tenants who do not confirm this.

With the exception of changes made in the payment plan elicitation, the survey was frozen on March 30, 2022. The changes made after March 30 were:

- April 11: Adding demographic question asking how the participant found out about ERAP. Updating language in the the payment plan elicitation indicating that participants may want to ask for a full set of eviction protections, and that they should discuss the payment plan with their landlord.
- April 12: Added language indicating the payment plan was "proposed" and an option in the payment plan that the tenant needed to remain in residence.
- April 27: ERAP changed its policy on second payments. The original language on the payment plan indicated applicants could only be paid once, so we changed this to indicate future payments were possible.

The above changes were made either due to explicit ERAP requests or policy changes, or to address questions from landlords or tenants about the payment plans.

D Empirics Appendix

D.1 Data Linkages

We now discuss the process of merging landlord/tenant experiment data to external data sources.

Fuzzy Merges to Eviction Data. Eviction court data for Shelby County were shared by the Legal Services Corporation. We process first names, last names, addresses, and ZIP codes from the ERAP application or experiment. We match only on tenant names and not landlord names, as eviction records do not consistently contain landlord information. 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 reclink2, with equal weights on first name, last name, address, and ZIP. In practice, after dropping invalid households, nearly all merges are exact. We require an exact match to the street address number, since inexact matches could indicate a different landlord. Based on inspecting the merge, we add several other rules to improve merge quality (e.g., requiring only that the first letter of either the first or last name matches if last/first names and addresses also match exactly).

In Section 5.1's test of whether DG behavior predicts eviction, we fuzzy merge using names, addresses, and ZIP codes. For the landlord survey matches to eviction records, addresses come from the ERAP application (since we do not re-elicit address). For the tenant experiment, addresses come from survey reports. A limitation is that if the eviction record does not list the tenant in question, we will not merge.

In Tables 3 and related tests, unless otherwise noted, we construct indicators that are 1 if the participant ever has a filing from 365 days before to 2×365 days after survey completion ($2 \times$

365 + 183 days [i.e., granting another 6 months] for judgments, to allow time for the court process to conclude). The court data end in May 2025.

Merges to ERAP Reciept. In Section 5.3, merges from the tenant data to ERAP receipt 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 exactly track how they proceed through the ERAP receipt process. However, individuals can be merged to multiple ERAP records because of duplicate applications.

We drop any records for an individual before September 1, 2021, as the program was still in a pilot phase until then. Among remaining records, we consider the first ERAP payment we observe. With legal payments, we do not have perfect information about payment timing and therefore consider any ERAP record linked to a legal payment as in the legal sample. We consider the individual in the utilities shutoff sample if their record of first payment is associated to an application that received a utilities payment and they applied with a shutoff.

Merges to Possible Code Violations. In Table A13 we use publicly available reports or tickets in a case-management system about potential code violations in the City of Memphis. These tickets are filed via Memphis's public 311 reporting system. Reports can be made by anyone, including anonymously, and contain address information. We link reports to study participants' addresses. We can only link to Memphis and not Shelby County, since we do not have potential code violations data for the outlying areas.

Reports are not linked to individuals. We conduct exact matches to the records based on address only (excluding units, since those are inconsistently reported). For landlords, we cannot be sure whether they rented the property at the time of the report. For tenants, we link to reports at their current address as of the survey. Code violations data are from 2020–October 2023. We cannot be sure that the tenant in question resides at the address during that period. So, proper interpretation is about hostility at the address-level rather than between the landlord–tenant pair.

Violations are associated to a department and a "request type" or category. Notes to Table A13 describe what requests and categories we keep. These tickets may not ultimately resolve as a true code violation or a fine. For instance, notes in the tickets about junky yards often indicate that junk was removed when an inspection was made. We see the report itself as indicating bargaining friction and present this as the outcome. In many cases we cannot tell from data what the ultimate resolution was, which is why we do not present that as well.

D.2 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.³¹ 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.

D.3 Validation of Preference Measures (Appendix to Section 3.3)

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

³¹In 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.

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).³² The classifier gives an aggregate sentiment score for each response. The question was: "Do you have any thoughts about [landlord name] that you want to share?"

- 2. *Likert Scale.* We asked tenants to subjectively rate their relationship with their landlord.
- 3. *Simple Lottery.* In both the landlord and tenant surveys, 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.³³ 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.

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.

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

Tenants' free-response and Likert scales were only collected after March 27, 2022 (\approx 70% of tenant survey responses). Table S12 shows sample sizes for each elicitation. We did the lottery for all tenants and landlords.

Aggregating Across Measures. Following Kling et al. (2007), we combine these measures via an index that is the average of the standardized measures. Panel A of Table S12 shows the index is highly correlated with tenant behavior in the DG toward their own landlord. Panels B, C, and D of Table S12 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).

Results for Indifference Point. Figure A6A–B presents a version of validations in Figure 2 with the indifference point S(x) as the outcome instead of hostility. Figure A6C shows "high altruism" (rejecting (20,0) in favor of (10,10) bundles) as the outcome.

Landlords. We implemented the lottery with landlords to rule out their inattention, confusion, or the importance of anonymity. We find symmetric results as with tenants (Figure A6D, Table S13), suggesting none of these play an important role.

D.4 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 that we would not drop participants who fail this attention check, as it seemed to be too hard. 64% of landlords pass this check. In the tenant experiment, we also used a

³²VADER has been pre-trained on social media data and is designed to handle standard challenges with sentiment analysis like negation and slang.

³³Specifically, we write: "Your response will be kept private from your tenant" or "Your response will be kept private from your landlord."

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, suggesting that the "teal" check was too hard. Our primary findings do not condition on passing the general checks, following our pre-registrations. However, we show that dropping inattentive participants does not influence results in the DGs (Tables A4–A5). Because of concerns about attention among Lucid participants, we do condition on the most stringent attention checks in the Memphis and National Samples. Confirmation checks in Section 3 raise confidence that our participants are attentive for our key elicitations. It is more reasonable for tenants and landlords to be attentive because they were recruited via surveys associated to a government program that could pay them money, and therefore have an incentive to pay attention and report truthfully.

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. Finally, the fact that the DG responses are correlated 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.

D.5 Assortative Matching

We have two objectives with assortative matching. First, we want to estimate 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, 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 multiple tenants linked to 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.³⁴

We regress tenant behavior on the landlord's mean indifference point across observations for that landlord (Table A19, 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 10 percent increase in the tenant's indifference point S(x), but it is not significant (p = 0.3).

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:

$$p_i = \beta_1 \mathbb{1}(\text{highly altruistic toward own } L)_i + \beta_0 + \varepsilon_i.$$
 (90)

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 (Table A19C). 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 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 *i*th most altruistic tenant with the *i*th 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.

Figure A14 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, around 1 in 3 relationships feature at least one hostile party and $q \in [0.1, 0.35]$.

In Section 6, we assume assortative matching by splitting the difference, positing that 22.5% of

³⁴We 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.

landlords and tenants are matched assortatively (i.e., q = 0.225).

D.6 Information Treatments: Revealed Outcomes (Appendix to Section 4)

D.6.1 Landlords

Providing information about tenants' recoupment probabilities causes landlords to be more likely to request ERAP materials (Figure S2A). Intent-to-treatment estimates are significant and 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.

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). Our specification is

$$y_i = \beta \text{Update}_i + X_i \delta + \varepsilon_i, \tag{91}$$

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 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 10percentage-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 S1). When we control for the belief update in the IV exercise, results are almost identical.

Null Results on Other Outcomes. 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 S6 for ITT and Table S9 for IV; 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. Focusing on the ITT, 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.

D.6.2 Tenants

Payment Plans. 74% of tenant survey participants are eligible for payment plans because they owe their landlord money for rent at the time of our survey. In the exercise, we offer tenants the chance to propose how much back rents to repay, as well as the total duration and payment period. We tell tenants that we will send the payment plan to their landlord. We send the payment plan via email.

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 A10B and no detectable effect for the bargaining correction (Panel C). 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. We present results on preregistered outcomes in Table S7 and find small impacts on other outcomes.

D.7 ERAP Evaluation (Appendix to Section 5.4)

This section evaluates 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. 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.1). Our merge strategy does not detect evictions if the eviction record only lists occupants who do 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.³⁵

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 externally valid portion of the sample, and obtain the treatment effect of rental assistance payment alone, we drop households who reached a legal settlement. We also drop households that received a utilities-only payment.

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 over the course of the program. 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 have complete information about

³⁵Landlords 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.

to whom the payments were made.

Tenant Characteristics. Tenants in the ERAP administrative data are similar to those in the experimental sample, by virtue 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 our restrictions, there are about 4,300 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 order (Section 2).

D.7.1 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. By excluding at-risk tenants, we may be dropping the tenants who most at risk of eviction. This is a reasonable concern. Three points are worth noting. 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 before payment.

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

D.7.2 Event Study

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

We estimate:

$$y_{it} = \gamma_r + \delta_c + \alpha_t + \sum_s \beta_s \left(\mathbb{1}(\text{event period} = s)_{i,s(it)} \times \text{After}_{it} \right) \\ + \lambda \text{After}_{it} + \sum_{s < s'} \sigma_s \mathbb{1}(\text{event period} = s)_{i,s(it)} + \varepsilon_{it}$$
(92)

for household *i* who was paid in 14-day period *r* and who applied in 14-day period *c*, and where *t* indexes a 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 $\sum_{s} \sigma_{s} \mathbb{1}(\text{event period} = s)_{i,s(it)}$ 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. Since all people in this sample are paid, because of collinearity, we can only identify σ_{s} in periods s < s' where s' is the event period of payment.

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.

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. It could be the case that once people apply, ERAP ultimately paid people in a quasi-random order, without respect to potential eviction risk.

Our approach is motivated by institutional knowledge which suggests a quasi-random component to payment timing. For instance, there is substantial variation in average time-to-payment across the application screeners (case-managers) employed in the Memphis ERAP (Figure S8B), though we are underpowered to use exclusively this variation in a investigator-IV design. If screeners were assigned to applicants without respect to potential eviction risk, that would generate quasi-random variation in payment timing. In support of quasi-random payments, 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 S11). We do find that households with higher overdue rents received payments faster.

Quasi-random payment timing is unlikely to hold exactly.³⁶ Interviews with ERAP officials indicate that tenants could and did lobby screeners for faster payment. Importantly, our differencein-differences design addresses *level* differences in potential eviction risk (e.g., high rates of eviction each period), so only time-varying differences in potential eviction risk are concerning for identification. Pre-trends diagnose the magnitude of such violations.

D.7.3 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 4,500 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 and a clean control group. On the other hand, the variation is arguably less credible, as people who do not get paid may have unobservably 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

³⁶The 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. 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.

 \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 (92):

$$y_{rct} = \gamma_r \times \delta_c + \alpha_t + \sum_s \beta_s (\mathbb{1}(\text{event period} = s)_{rcs} \times \text{After}_{rct}) \\ + \lambda \text{After}_{rct} + \sum_{s < s'} \sigma_s \mathbb{1}(\text{event period} = s)_{rcs} + \varepsilon_{rct},$$
(93)

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 (93) constitutes a "standard" event-study that compares paid households to household who applied in the same calendar week but were not paid.

To address concerns about negative weights, we use heterogeneity-robust estimators for Equation (93). One such approach is a "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 $d(r) \in D$ and estimate a stacked version of Equation (93):

$$y_{rctd} = \gamma_{rd} \times \delta_{cd} + \alpha_{td} + \sum_{s} \beta_{s} (\mathbb{1}(\text{event period} = s)_{rcs} \times \text{After}_{rct}) \\ + \sum_{d} \lambda_{d} \text{After}_{rctd} + \sum_{s < s'} \sigma_{s} \mathbb{1}(\text{event period} = s)_{rcs} + \varepsilon_{rctd},$$
(94)

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.

Additionally, we use the Sun and Abraham (2021) estimator for Equation (93), 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 six months (6×4 -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.

D.7.4 Other details

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 interpretation. 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.

Multicollinearity and Estimation Details. We have event-time, calendar time, and cohort fixed effects, which presents a standard multicollinearity problem in the always-paid specification. We identify our event-time coefficients because they are aggregated at the two-month level, whereas the other fixed effects are at the two-week level.

D.7.5 Results

Figure A15 displays estimates of our primary event-study specification (Equation 92). Each coefficient represents the percentage point impact of ERAP receipt on the flow of eviction filings or judgments. Panel A shows that ERAP payments have no impact on eviction judgments and, if anything, point estimates are positive after six months. Panel B shows a discernible impact on filings in the first 0–1 month period after payment, and null impacts in subsequent months.

There is a positive impact on judgments and negative impact on filings, neither of which is significant. These are average per-period difference-in-differences estimates (impacts on *flows* of filings or judgments). To obtain the cumulative impact (stock of filings or judgments at six months), multiply by three.

Interpretation is subtle. We reject small impacts on judgments or filings in absolute terms. To answer whether ERAP is cost effective at stopping evictions, the absolute impact is what matters. On the other hand, judgments and filings are rare, so small absolute changes can still be meaningful in percent terms. If we replace all post-period judgments and filings with zeros, indicative of a maximally effective ERAP program, we easily reject this maximal effect (blue lines labeled "maximal avg. effect").

Still, the last exercise shows that some of the small absolute impact is mechanical. Most households who apply are not evicted, so the program never had a shot of being cost effective. That is, even maximal impacts are small in magnitude.

That the magnitudes are small in part for mechanical reasons is consistent with our social preferences mechanism. ERAP may have a small treatment effect because landlords and tenants who apply are altruistic. Then, ERAP application may be negatively selected on levels of counterfactual eviction risk — our selection on altruism point.

Investigating Pre-Trends. Our primary specification exhibits evidence of pre-trends in the three to four months before ERAP receipt. Pre-trends are small in magnitude for judgments and moderate for filings. They are concerning if they indicate payments were correlated with time-varying eviction risk. Bias could be in either direction. Overall, given the small magnitudes and the fact that they do not meaningfully affect judgments (only filings), we are not overly concerned.

Nevertheless, we investigate the source of the pre-trends. First, we restrict the sample to those who applied between October 1, 2021 and July 31, 2022 (yellow series, Panels A and B). The program intentionally shut off applications in September 2021 for a month or two to clear its backlog. Meanwhile, after September 2022, the program stopped applications completely. In August, the program made a concerted effort to enroll people with high eviction risk just before the deadline. These facts suggest that payments outside the 9/2021–8/2022 window are more likely to vary with eviction risk. Consistent with this interpretation, the pre-trend in the first application period for judgments falls by around half and is no longer significant. This sample restriction has little impact on other periods. In sum, pre-trend violations come from applications that applied outside the program's normal functioning; restricting to a less problematic sample does not affect conclusions.

Second, restricting the sample to households without explicit utility shutoff or eviction notice submitted with their application materials does not affect the pre-trends (red squares). This result suggests pre-trend issues do not reflect other problematic violations of our identification assumption.

Building Up to the Main Specification. We show the role of our non-standard departures from the simplest event-study design by building up to our primary specification (Figure S3). The left-most series is a standard event study with unit and calendar-time fixed effects only. It omits application-date fixed effects and the After_{*it*} indicator and interactions from Equation (92). The second series adds application-date fixed effects. The third series is our main specification, which adds the After_{*it*} interactions. Altogether, our main specification is similar to the simpler event study strategies.

Alternative Design. The alternative design with non-paid households yields similar results (Figures A15C and D). The stacked design eliminates pre-trends in judgments.

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 S3C and D). 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.³⁷ We weight the primary strategy by 1/(1 - p), where *p* is the propensity from this regression.³⁸ Reweighting generates a persistent negative effect on filings and a more negative effect on judgments in some specification.

Summing Up. Table A15 aggregates estimates across empirical strategies. We present the primary design, the reweighted estimate, and the stacked version of the alternative designs. Effects across research designs are consistent with at best a null or small negative effect on judgments (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. Effects are much larger in the first period for filings.

Rows 4–9 of Table A15 interpret these treatment effects. Rows 4 and 5 present the "maximum simulated effect" on judgments or filings, similar to our discussion of Figure A15. They are not identical to the simulations in the figure, because we difference the maximal average effect (navy line in Figure A15) with the estimated pre-period coefficients (black dashed line in pre period), for comparability.

From our administrative data, we estimate the average ERAP payment amount is approximately \$5,600. We compute average prevention costs by dividing this payment amount by point estimates of the treatment effects. Assuming homogeneity, this value is the total amount of money, distributed across households, to stop a single eviction. 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).

³⁷We include the second indicator because people with notices at application were supposed to be sent to the legal program directly.

³⁸Intuitively, 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.

D.7.6 Discussion of ERAP's Impacts on Filings

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.