

Overview

The Experiment Details (hidden) section had a word limit that was slightly too short. As a result, we provide more study details and information about outcomes in this document.

Please note that this is not a complete pre-analysis plan but rather a pre-registration document that provides what we would have written in the Experimental Details (hidden) section if we had more space.

Major changes made after initial registration

In this section, we document major changes made to the Study Details document. We made several minor clarifications between the initial registration and March 27.

March 27, 2022

1. Based on feedback from preliminary results ($N \approx 430$), we include a subexperiment that randomly varies the *stakes* of the altruism task. This subexperiment was designed to examine whether the participants' behaviors change as the stakes increase. More details on the variation in stakes are given below.
 - a. We slightly update the wording and presentation of the altruism task when implementing the variation in stakes. The main updates are: (i) we flag to the participant that their choices are private; (ii) we highlight that the funds for this task come from research dollars; (iii) we slightly change the wording about the probability of having one's choice implemented. We make change (i) to address the potential concern that participants make their choices due to social image concerns. We make choice (ii) for the government partner: we do not want participants to believe that the large potential payouts are coming from money intended for tenants in the ERA allocation. We make choice (iii) because we cannot implement the choice for larger stakes for as many as 10 participants. Instead, we now say that at least one participant's choice will be implemented.
 - b. Because of the wording updates and changes, we will present our main results on altruism including a control for whether the sample was collected before or after March 27.
2. We include several additional questions intended to elicit the participants' altruism (using questions from the Global Preferences Survey) and qualitative questions about the tenants' landlord. These changes are modest. They affect other tests to validate the altruism experiment.

Experimental Details

We recruit tenants to participate in an online survey experiment. We obtain tenants' emails and phone numbers via a data-sharing agreement with Memphis/Shelby County. Landlords participate in a small sub-survey, explained below under secondary outcomes.

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Altruism task and altruism treatments. Tenants begin by participating in an altruism task, in which we elicit tenants' indifference point $\$X$ between a bundle of ($\$Y$ self, $\$Y$ other) and ($\$X$ self, $\$0$ other). $\$X$ can vary between 0 and $2Y$; we elicit (bounds on) $\$X$ via a multiple price list and employ the strategy method to allocate $\$X$. ($\X is technically an amount of a gift card to Amazon.)

Each participant engages in the task twice (in random order). They engage in the task where the other person is a random tenant who participates in the survey (unnamed). Participants also engage in the task where the other person is either: (i) their own landlord (named), whose information they provide to us before the task, or (ii) a random landlord (unnamed), among landlords of tenants who participate. Whether they play against (i) or (ii) is randomized, where they play against their own landlord (named) with probability $\frac{2}{3}$. This activity is designed to be symmetric to a similar activity we conduct in #AEARCTR-0008053, switching landlords and tenants. The purpose of randomizing whether the opponent is own vs. a random landlord is to study whether people have different preferences toward their own landlord (e.g., have a damaged relationship with them).

Randomization of stakes. Before March 28, $Y = \$10$ for all participants: we elicited their indifference point between ($\$X$ self, $\$0$ other) and ($\$10$ self, $\$10$ other). Beginning March 28, we randomize the value of $\$Y$ in this task across the values: $\{\$10, \$100, \$1000\}$, with equal probability. For each opponent, the participant sees the same value of $\$Y$.

Beliefs. We elicit prior beliefs about: (i) the share of landlords whose indifference point is more than 20 when they play the altruism game described above with their own tenant, and (ii) the share of landlords who file an eviction in Memphis/Shelby County (formally known as an "FED warrant") who then settle or drop the case. We obtain (i) from our survey experiment in AEARCTR-0008053. In that experiment, we find that 69% of landlords prefer to evenly split a gift card with their own tenant rather than have $\$20$ for themselves and $\$0$ for their tenant, indicating a high amount of altruism from landlords when faced with the choice to divide a gift card with their own tenant. We obtain (ii) from court records, where we find that 31% of landlords file an eviction and then settle or drop the case. For each of (i) and (ii), we elicit prior beliefs about (a) the share of landlords on average who either split the giftcard evenly or settle or drop a case, as well as (b) whether their own landlord would split the giftcard/settle or drop the case. Beliefs about the share of landlords who engage in the behavior on average has a known ground truth; we incentivize accuracy in this elicitation using a quadratic payment. Because there is no known "ground truth" about one's beliefs about one's own landlord, we do not incentivize this belief. We also elicit a coarse measurement of uncertainty in each of these beliefs.

Information Treatments. We cross-randomize providing the truth about either fact. The idea behind our treatments is that providing truthful information about the average landlord should move beliefs about one's own landlord. Put another way, half of participants are told that 69% of landlords split the gift card evenly. Half of participants (cross-randomized) are told that 31% of

landlords on average file an eviction and then drop or settle the case. The order of the treatments is random (with probability $\frac{1}{2}$) for the $\frac{1}{4}$ of participants assigned to both. We then elicit posterior beliefs about whether one's own landlord would engage in the behavior about which we provide information. For example, half of participants who are told that 69% of landlords split the gift card evenly are then reminded that they said the average landlord would split the gift card with probability X and their own landlord would split the giftcard evenly with probability Y. They are then asked whether they would like to update probability Y (and what their update is).

Note that, depending on one's prior beliefs, participants may be expected to update up or down. Consider the belief update about landlord altruism. The belief update may not only depend on the relationship between 69% and Y. For instance, if some participants believe that their own landlord is much more altruistic than average (say), and the truthful information 69% is much larger than the beliefs about the average landlord X, that could cause them to update up about X even if Y is larger than 69. On the other hand, if the tenant learns that the average landlord is less altruistic than their own landlord ($69 < Y$), that could cause them to update down. Put another way, in theory, the relationship between 69 and X can have either a positive or negative effect on the belief update about Y. The same forces also apply to beliefs about bargaining propensity.

Primary outcomes

Overview. We focus on three core outcomes: (1) outcomes in the altruism task, (2) prior beliefs (and uncertainty) and belief updates, and (3) a bargaining exercise, described below.

Note that outcomes in the altruism task are elicited before randomization into the information treatment and therefore the treatment effect of information is not relevant here; we care about the behavior before treatment. Similarly, regarding (2), prior beliefs occur before randomization. The treatment effect is relevant for measuring belief updates.

Our bargaining exercise (3) is as follows. We ask tenants whether they would like to propose a "payment plan" for any back rents that they have. These back rents may include money that the Emergency Rental and Utilities Assistance Program cannot repay. The payment plan consists of: (i) a total amount of back rents to repay (e.g., the tenant may propose to the landlord to forgive some of the funds they owe), and (ii) a total duration of the payment. (Of course, the combination of (i) and (ii) implies a per-period payment.) We also elicit whether they would like payments to be weekly vs. monthly, and whether they propose to the landlord to drop any eviction filing.

Primary outcomes (details). For the altruism experiment, we focus on the following measures:

- Average indifference point between bundles, for a given category of opponent (own landlord, random landlord, random tenant)
- Share who are vindictive (i.e., have an indifference point less than 10, meaning they effectively forgo money to not pay their opponent), for a given opponent

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- The effect of the “random landlord” treatment (i.e., having the opponent be a random landlord vs. own landlord) on the indifference point and vindictiveness toward landlords
- Difference in indifference points between bundles (within- or across-person, differencing over indifference toward landlords and tenants)
- Difference in difference in indifference, across participants, e.g. computing the sample average statistic: (random landlord minus random tenant) minus (own landlord minus random tenant).
- Difference in difference in vindictiveness, across participants.
- Effect of stakes. To test the effect of stakes, we normalize the implied indifference point by the value the participant receives in the “equal payoff” bundle (i.e., we divide the estimated indifference point by \$Y). We then test whether the above outcomes differ by the value of \$Y. With this normalization, the indifference points are now expressed as the share of the participant’s payout when split evenly. We focus on the effect of stakes on landlord or tenant indifference points/vindictiveness (raw) and the effect of stakes on the “random landlord” treatment’s effects on indifference/vindictiveness. To increase power, we may study the effect of \$Y expressed parametrically (e.g., log or quadratic in logs) rather than studying the effect of each \$Y nonparametrically.

Given we have obtained similar measures in AEARCTR-0008053 and AEARCTR-0008436, we may also test for differences in the above measures to the symmetric estimates from those datasets.

For the beliefs elicitation, we focus on the following measures:

- Value of the prior belief.
- Value of the posterior belief, for the treated group.
- The treatment effect (average belief update, if shown a treatment).

For bargaining exercise, we focus on the treatment effect on the following primary outcomes:

- Whether the tenant wishes to implement a payment plan (extensive margin).
- Of their back rent owed, the amount that they propose to repay in a payment plan.

Secondary outcomes for the bargaining exercise are elaborated below.

Note that we only conduct the bargaining exercise among tenants who report having a positive back rent.

Secondary outcomes

Overview. After the bargaining exercise, we conduct several additional elicitation. First, we elicit the willingness to pay (WTP) for information about how landlords behaved. Next, we conduct two hypothetical exercises where we ask tenants (i) how much money they would require to accept to leave their rental unit, and (ii) how much money they would pay to remove an eviction from their record. Finally, we conduct a simple altruism task where we elicit whether tenants would like to enroll either a random tenant or their landlord in a lottery. In the WTP exercise, our focus is on both the levels and the treatment effect. In the hypothetical indifference elicitation, our focus is on the levels of and treatment effect on the indifference point for

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elicitation (i) and the level of the indifference point for elicitation (ii). In the other exercises, our focus is on the levels (with particular emphasis on the control group that receives no information). Finally, we pre-register several dimensions of heterogeneity that we study in secondary analyses.

Secondary outcomes (details):

Altruism task.

Beginning March 28, we elicit questions from the Global Preferences Survey (Falk et al., 2016, 2018) to measure altruism and whether the participant likes to engage in punishment. We correlate these outcomes with their behavior in the task.

Bargaining exercise.

- Whether they propose to drop an eviction filing.
- The amount that they propose to repay each period.
- The payment period (e.g., weekly vs. monthly).
- From the amount that they propose to repay and the repayment period, we will construct the present discounted value of proposed repaid back rents (assuming a representative discount rate).
- We may study the effects of being induced to bargain on eviction in the long-run, matching onto Shelby County data. This is secondary and suggestive, not because it is unimportant but because we are unsure we will be powered to study it since evictions are rare events.

Willingness to pay task. We give participants a \$10 gift card (implemented with 1% probability using the strategy method) and elicit how much of this gift card they would give up to learn the truth about the share of landlords who prefer to split the \$20 gift card evenly in AEARCTR-0008053. We will implement this elicitation using a multiple price list and Becker-DeGroot-Marshak mechanism, as in the division of the gift-card between tenants and landlords. We focus on the following outcomes:

- The level of, and treatment effect on, the share of tenants who have a positive WTP
- The level of, and treatment effect on, the reported WTP of tenants

The purpose of this task is to examine whether: (i) this information is indeed valuable to tenants, and (ii) providing information about landlords' bargaining propensity changes how valuable information about landlord altruism is to tenants. Intuitively, providing information about landlords' bargaining propensity may be a complement or a substitute to other information, depending on tenants' uncertainty and how close to the margin they are to bargaining.

Indifference points elicitation. We conduct two exercises, eliciting (1) the amount of money the participant would need to receive to leave their rental unit, and (2) the amount of money the participant would pay to have an eviction cleared from their record. Both these exercises are hypothetical and flagged as such to the participant. We focus on:

- The level of, and treatment effect on, the elicited indifference points for the first outcome.
- The level of the elicited indifference point for the second outcome.

We acknowledge that the levels are difficult to interpret, since these are hypothetical outcomes. The purpose is to see whether providing information about whether landlords are likely to bargain could affect whether tenants would raise their willingness to accept to leave the rental unit. The second elicitation is purely to study whether tenants consider the costs of eviction such as having a flag on one's credit report. We do not emphasize the treatment effect for this outcome.

Secondary outcomes (heterogeneity). Secondary analyses also explore heterogeneity. First, we examine outcome heterogeneity by tenant characteristics, including race (and whether race is the same as landlord), gender, whether they have previously been evicted, the self-reported ease of communication with the landlord, and the amount of back rents they owe. The reason heterogeneity by self-reported ease of communication with the landlord is important is that this measure coarsely proxies for the cost of bargaining (relative to going to court). Second, we examine outcome heterogeneity by the tenant's indifference point in the altruism task. Third, we examine outcome heterogeneity by the tenant's self-reported uncertainty about their prior beliefs in each belief elicitation. Finally, our belief updating specification naturally incorporates heterogeneity into our notion of the instrument.

Additional surveys

We conduct two additional surveys. Because we have limited scope to pilot them at this time (since they occur conditional on the main survey with tenants), we register outcomes from them as secondary.

Landlord response survey. As noted above, tenants can propose a payment plan. We email a link to the payment plan to landlords. Immediately following the payment plan, landlords can complete a short survey that asks them demographics and whether they intend to accept the payment plan (and if not, why not). All outcomes associated with the survey are secondary, not because they are unimportant but because piloting leads us to expect that a small share of landlords will complete the survey. We can also observe whether landlords open the payment plan, which may serve as a useful statistic or secondary outcome.

Tenant response survey. As noted above, tenants can propose a payment plan. We may contact tenants who propose a payment plan to complete a survey in several months indicating whether the payment plan was adopted. All outcomes associated with this survey are secondary, not because they are unimportant but because we are not able to predict whether a large enough share of tenants will take the survey, so we may have very low completion rates.

Specification for Information Treatment

For studying treatment effects of providing information, as in AEARCTR-0008053, we employ two methods. First, we study the reduced form effect of providing information on outcomes. Second, we use information as an instrument for posterior beliefs, captured before the

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outcomes. Using this instrument we can study the effect of beliefs on outcomes using an instrumental variables design. We note several details:

- The information may move beliefs up or down. We therefore construct an instrument that captures whether the information lies above or below people's beliefs. An example of such an instrument would be $\text{instrument} := 1(\text{treatment}) \times (\text{truth} - \text{prior})$ beliefs about own landlord. A second example of such an instrument would be $\text{instrument} := 1(\text{treatment}) \times (\text{truth} - \text{prior})$ beliefs about average landlord. A related specification could show the ITT of providing information, split across people whose prior beliefs lie above vs. below the prior.
- We can construct at least two instruments for each of the two treatments, because the information may induce beliefs to change based on the distance from one's belief about the average landlord or one's own landlord. We can instrument for posterior beliefs about both whether their landlord would split the gift-card and whether their landlord would settle or bargain if they filed an eviction. We may employ a joint specification that jointly instruments for both endogenous posterior beliefs as well as individual specifications that instrument for one given posterior beliefs separately (with each treatment) and controls for the other posterior.
- We randomize the order of the treatments. A concern about the instrumental variables specification we employ is that the information we provide could affect other beliefs that we do not measure. Since we elicit posterior beliefs about each belief shocked by the treatment, randomizing the order of the treatments yields a placebo test for this specification as follows. Call the treatments Treatment A and Treatment B. We can test whether the belief updates for about belief B are statistically different than the belief updates for people who see Treatment A and then Treatment B vs. people who see Treatment B and then Treatment A. If they are, then we may control for the belief order in our instrumental variables specification.

Other Details

- We conduct an attention check where we ask tenants what is their favorite color but instruct them to respond with "teal." We think this attention check provides a useful filter, and we will present results with participants who do and do not pass the check. However, piloting suggests it is not dispositive and a large number of tenants who are paying attention (based on text responses) fail the exercise. Given that we have a limited number of possible participants, we may include tenants who fail the check in our main sample for power. This is symmetric to AEARCTR-0008053. We also include an attention check where we ask tenants to report the number 6, as another potential check.
- As a part of our partnership with Memphis's Emergency Rental and Utilities Assistance Program, we elicit a number of questions about attitudes toward the Emergency Rental and Utilities Assistance Program and effects of the program. We elicit these questions prior to treatment. They may form the basis for exploratory heterogeneity but are not part of the core analysis.

References

Falk, A., Becker, A., Dohmen, T. J., Huffman, D., & Sunde, U. (2016). The preference survey module: A validated instrument for measuring risk, time, and social preferences. IZA Discussion Paper No. 9674.

Falk, A., Becker, A., Dohmen, T., Enke, B., Huffman, D., & Sunde, U. (2018). Global evidence on economic preferences. *Quarterly Journal of Economics*, 133 (4), 1645–1692.