Legal Assistance for Evictions: Impacts, Mechanisms, and Demand*

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Abstract

We randomize provision of attorneys to tenants facing eviction in Memphis, Tennessee ($N = 307$ attorneys provided), who otherwise seldom have legal representation. Despite landlord-friendly eviction law, providing an attorney reduces tenant eviction judgment rates within 180 days by 23 percentage points (37%). However, attorneys' effects persist only when they can connect tenants to other services. Once a concurrent emergency rental assistance program expires, effects on judgments at 180 days shrink by about three quarters and are indistinguishable from zero. Attorneys have little effect on informal outcomes and bargaining. Incentivized surveys suggest tenants' demand for an attorney is double attorneys' price, and eight times attorneys' implied impacts on tenants' incomes via stopping evictions. This high willingness to pay does not appear to result from elicitation errors, misperceptions, or binding budget constraints. We contrast lawyers' Marginal Value of Public Funds from using elicited willingness to pay (MVPF = 2.7 without rental assistance, ignoring impacts on landlords or general equilibrium) versus a standard calibrated approach (MVPF = 0.3).

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1 Introduction

Each year, 7% of U.S. renter households receive eviction filings, which initiate the court process in the civil legal system (Gromis et al., 2022). In Shelby County, Tennessee, the setting for this study, 91% of landlords and just 5% of tenants have attorney representation in eviction cases (Innovate Memphis, 2023), which is comparable to representation rates across the U.S. (Engler, 2010). These evictions are costly. For tenants — often among society's most vulnerable — formal (court) evictions cause reductions in many measures of financial and personal well-being (Collinson et al., 2024b), as well as personal upheaval (Desmond, 2016). Landlords rely on evictions to collect rents, but court evictions cost money to file and can introduce delays or personal conflicts.

Seeking to assist tenants, 17 cities and four states recently passed "Right to Counsel" programs that guarantee defense attorneys in eviction cases.[1] This expansion represents perhaps the most significant shift in U.S. eviction policy in the past two decades, aside from temporary pandemic-era measures. Yet whether attorneys actually stop evictions is unclear. Many localities have landlord-friendly housing laws that hinder attorneys from raising successful defenses, especially in cases of rent nonpayment.

Even given causal estimates of representation on evictions, measuring the effects on social welfare is challenging but essential given the considerable expense of lawyers and alternate ways to assist the poor or redistribute. A key input to any welfare analysis is the value created for tenant recipients. Yet attorneys could have many diffuse effects and thus tenant value created by lawyers is difficult to calibrate.[2] Given the momentum of policy expansion, evidence on lawyers' effects and mechanisms, as well as tenants' demand, is urgently needed.[3]

What is the impact of free provision of tenant eviction lawyers on evictions? How much do tenants value defense lawyers, and how do their valuations influence welfare analysis? We make progress on these questions with a field experiment that randomizes provision of attorneys to tenants facing eviction, and direct elicitation of tenants' demand for lawyers. Relative to existing research on Right to Counsel (discussed in the end of the introduction), we bring more power for formal outcomes, sharp policy variation to address mechanisms, and new data on informal outcomes and tenant demand.

We start with a simple Nash Bargaining framework to interpret lawyers' effects and propose normative implications (summarized in Section 2, for more details see Appendix C). Landlords and tenants pursue eviction only if they are unable to negotiate out of court. Tenant attorneys can affect either party's outside option or increase tenant bargaining power. Bargaining power

[1]All Right to Counsel programs in the U.S. were passed since 2017. The cities include New York City, Newark, and San Francisco, and the four states are CT, MD, MN, and WA. Source: NCCRC, accessed 11/2/2023.

[2]Tenants may value the treatment effect on eviction and also factors like the psychological security that lawyers provide. On the other hand, tenants are low-income and have a high marginal utility of cash.

[3]Another important consideration, highlighted in recent work (Abramson, 2023), is that free lawyers could have negative general equilibrium impacts by raising rents.

exclusively affects the division of surplus *if* bargaining occurs, and not *whether* bargaining occurs at all. Consequently, if attorneys reduce court eviction rates, they must change real bargaining constraints. The upshot is that data on court eviction, together with randomized attorney provision, are sufficient to test the hypothesis that lawyers do not change outside options. Additionally, data on informal bargaining can be used to test whether lawyers transfer resources from landlords to tenants in informal bargaining.

This framework structures the Randomized Controlled Trial (RCT) that we conduct in partnership with The Works Initiative (TWI), a nonprofit in Memphis, Tennessee (Section 3). TWI has provided Memphis tenants with housing and legal services since 2012. Memphis/Shelby County has a large number of evictions (an average of more than 2,000 monthly filings pre-pandemic), which makes it a fitting setting to examine housing insecurity. For this trial, TWI provided a combination of staff attorneys and recruited contract attorneys who supplemented their day jobs by providing representation to tenants in eviction court. We randomize tenants to the offer of full attorney representation (treatment, $N = 307$) or control ($N = 833$).[4]

We first study formal outcomes. As registered, we focus on court "judgments," or the court eviction order; "nonsuits," or agreements that settle or drop the case from the court docket; and legal tactics like filing "continuances," which delay court cases.

On every court metric we observe, lawyers dramatically improve outcomes for tenants (Section 4). Using the offer of assistance to instrument for being represented by a lawyer, we find that attorney representation causes a 22.5 pp reduction in eviction judgments over 180 days (standard error: 4.9) off a control mean of 61.0%. Lawyers reduce the amount that tenants owe in a judgment by $1,196 (s.e.: 290), a 52% reduction from the control mean of $2,311. Attorneys increase nonsuits by 17.4 pp (s.e.: 4.8) over a control mean of 32%.

To explain lawyers' effects, we next leverage sharp policy variation that took place during our study. During the first half of the program (March–December 2022), lawyers could assist tenants in receiving funds from the local Emergency Rental and Utilities Assistance Program (ERAP), a $100-million pandemic-era program that paid overdue back rents. In January 2023, the program shut down. We compare treatment effects between pre- and post-expiry of ERAP.

The large effects of lawyers fall dramatically once ERAP expires. Without ERAP, attorneys significantly reduce judgments at 30 days post-filing, suggesting that attorneys can still delay fast evictions. Yet the effect on judgments attenuates by 180 days and becomes indistinguishable from zero, although splitting the sample reduces power (coefficient at 180 days: -8.0 pp, s.e.: 6.9). Differences between pre- and post-expiry are large and statistically significant. Once ERAP expires, lawyers are 28.7 pp (78%; s.e.: 9.7 pp) less effective in reducing judgments over the

[4]When we notify tenants that they were not selected, they also receive a small amount of information about their rights in court. Because we had to notify tenants anyway, it was more ethical to provide the information to the control group than to provide nothing.

180 days after a filing. They are 16.8 pp (s.e.: 9.5) less effective in increasing nonsuits. These results suggest that lawyers work well when they can connect tenants to other available resources. Otherwise, they delay quick judgments but have smaller medium-run effects.[5]

While we lack randomized evidence on this mechanism, several tests all point to the ERAP environment, rather than other forces, as decisive. First, we link the RTC data to administrative data on ERAP receipt, and confirm in the pre-expiry period that attorneys significantly increase the chance of receiving assistance (by 23.8 pp). Second, we perform a mediation analysis, controlling for ERAP receipt. When we do so, the coefficient on attorneys' impacts on judgments falls by about two thirds and is indistinguishable from post-expiry impacts. In this way, the analysis using post-expiry data serves as an out-of-sample validation of the mediation analysis that uses pre-expiry data alone. As a simple mediation analysis relies on strong assumptions, we also instrument for ERAP receipt by leveraging idiosyncratic changes in ERAP's "congestion" (the likelihood of program receipt) around the time of RTC application. If anything, our IV moderately increases the share of the RTC effect that we attribute to ERAP. Third, in a cousin of a "visual IV," we find that demographic groups with the largest attorney effects on ERAP receipt are those with the the largest attorney effects on judgments. Fourth, we conduct a suite of tests for changing conditions around ERAP expiry and detect no evidence of other forces changing. For instance, reweighting on applicant observables only amplifies the pre-/post-expiry differences.

We next turn to the effects of lawyers on informal outcomes (Section 5), collected via endline survey (phone and web). Although attorneys have small effects on formal outcomes unless ERAP is available, bargaining remains common throughout the sample — around 50% of the treatment group does not end up with a judgment. Conditional on bargaining, lawyers could achieve better outcomes for tenants, albeit by redistributing from landlords. We reach 39% of contacted tenants for a sample of $N = 439$, which is high relative to contemporary response rates in financially distressed populations in the United States but low in absolute terms. A major concern in this setting is attrition, as evicted tenants are often harder to reach. We indeed find differences in response rates among treatment versus control ($p = 0.04$) and on whether the case got a judgment ($p = 0.003$).

Given the paucity of existing evidence and the potential importance of informal evictions, the surveys make a useful descriptive contribution but should be interpreted cautiously. Reassuringly, our estimates of outcomes that can be compared to administrative court data are virtually identical. Effects on informal evictions, moves, attempts to bargain, and propensity to make out-of-pocket payments to landlords are all economically small and statistically indistinguishable from zero. We are still in the process of exploring ways to maximize what we can learn from these surveys given the attrition concerns and report the current results and attrition corrections

[5] The fact that lawyers have large effects when combined with ERAP does not imply that ERAP alone has large effects on eviction. Collinson et al. (2024a) and Rafkin and Soltas (2024) find ERAP alone also has small effects.

in Appendix F.

Setting aside the causal analysis, the surveys provide rich descriptive evidence about the prevalence of landlord–tenant bargaining. About 66% of tenant respondents are ultimately evicted, of which 24 pp are informal evictions.[6] Bargaining is common: more than half of tenants report either offering a payment to their landlord or agreeing to pay the landlord at a later date. Yet less than half of those who offer a payment ultimately make out-of-pocket payments. Tenants employ a wide range of bargaining strategies. Of those who make out-of-pocket payments to their landlord, 23% pay immediately, 25% pay in installments, and about 52% make a lump-sum payment after a delay. Both attempting to bargain and eventually paying large shares of amounts owed are especially prevalent among the group who did not receive any eviction, formal or informal.

Viewed through the lens of the Nash framework, the results on formal evictions suggest that lawyers are most effective when they can change tenant outside options by gaining access to resources. Despite filing continuances that delay proceedings, lawyers have relatively small effects on landlord outside options, since lawyers have limited medium-run effects once ERAP expires. The modest effects on bargained settlements conditional on bargaining further suggest small effects on bargaining power.

Finally, Section 6 investigates demand for attorney provision. To interpret our demand estimates for welfare, we use a Marginal Value of Public Funds (MVPF) framework (Hendren and Sprung-Keyser, 2020). Our point of entry is that calibrating tenants' willingness to pay for the in-kind transfer of lawyers (the MVPF numerator) is challenging with observational methods. To make progress, we use lab-in-the-field tools to elicit demand for lawyers directly from tenant applicants. Clearly, neither observational methods nor lab techniques dominate the other, but we highlight the benefits of the latter in this setting.

We begin by providing a demand and MVPF benchmark using the standard toolkit. We focus on demand for lawyers after ERAP expires, since that is likely most generalizable. To estimate the MVPF, we use the only treatment effect estimate that we observe and can credibly monetize, the impact of attorneys on the following two years' wages operating through eviction judgments. We ignore general-equilibrium effects or potential negative effects on landlords. This approach gives a willingness to pay estimate of $75. Meanwhile, attorneys cost between $250–325 per lawyer. Fiscal externalities, via the impacts of lawyers on eviction, reduce the net cost to $260. The calibrated MVPF is then 0.3 for the period after ERAP expires.

This estimate of demand has two limitations. First, attorneys may have direct impacts on unmeasured yet welfare-relevant outcomes, like tenant stress about engaging in the legal system. Second, even the outcomes that we measure well (e.g., attorneys' effects on eviction judgments)

[6] The share of informal evictions is lower than in other surveys (e.g., Gromis et al., 2022) because our sample conditions on having a filing.

could have knock-on effects on outcomes which are challenging to monetize, like time to find another unit. Absent more complete data, analysts often monetize channels that they do have data on and discuss other aspects informally.

We instead elicit the welfare-relevant willingness to pay parameter directly in a real-stakes, incentivized baseline survey embedded into the program application ($N = 227$). This method gives an average tenant willingness to accept (WTA) cash versus a lawyer of \$691 (s.e.: 25).[7] 42% of tenants prefer a lawyer to receiving \$1,000 in cash, a striking result given program applicants have an average monthly income of about \$1,400. As lawyers have moderate effects on judgments without ERAP, and the market price of eviction defense attorneys is \$250–\$350, this value is hard to reconcile with an approach that monetizes lawyers' impacts. Taking tenants' demand seriously, the MVPF from this method is at least 2.7 — about nine times above the traditional approach, and implying that providing lawyers is more efficient than providing equivalent cash to the same population. While the lab-in-the-field MVPF still misses important forces (e.g., impacts on landlords and rents), our finding of very high tenant demand for lawyers is relevant to any welfare analysis of this policy intervention, including in general equilibrium.

The elicited WTA is high and naturally induces skepticism, but we embedded several tests that raise confidence in our lab-in-the-field measures. First, attention and comprehension were high (90%) based on standard survey checks. Second, we elicited demand for a comparison good in the survey using the same elicitation technique (Dizon-Ross and Jayachandran, 2023). Demand for this good, an iPad, is low (\$108) and does not exhibit the extreme left skew of the lawyer WTA distribution. Third, we leverage the iPad WTAs to estimate a statistical model of survey inattention, and when we attention-correct the lawyer WTAs, results fall by only 6–12%.

Next, we examine whether the WTA reflects behavioral biases. We test for misperceptions using baseline survey questions about the program's treatment effects. We find that the average belief is optimistic, but not hugely so. We tie these results together with the attention model and other elicitations to construct a "rational consumer benchmark" of demand without attention issues, misperceptions, or binding budget constraints, thus enriching the framework in Allcott et al. (2019). Across normative assumptions, we find that the mean WTA remains above \$600 (with an MVPF above 2.3), and confidence intervals rule out WTAs below about \$550.

Then what does explain tenants' high valuations? Notably, studies measuring low-income households' demand for health insurance, another in-kind good, have found relatively low demand (Finkelstein et al., 2019a,b). When we ask tenants why they value lawyers, unmeasured or unmonetized factors appear more valuable than the channels that enter traditional welfare analysis. In particular, tenants report valuing lawyers because they reduce stress (63% of respondents)

[7] Willingness to accept is the appropriate parameter for comparing the welfare impact of two policy tools, the in-kind transfer or a lump-sum cash transfer with identical incidence. Surveys correspond to the period after ERAP expires and where lawyers have small impacts.

or can help negotiate out of court (56%), which both exceed the share who value lawyers because they believe lawyers could fight an eviction in court (39%). Further bolstering the validity of the WTA estimates, we elicit experimental measures of tenants' trust in lawyers, via a trust game, and find trust highly correlates with demand. Asking tenants to explain their valuations directly — and validating those responses with incentivized lab elicitations — can help explain what differentiates attorneys in this setting from low-demand in-kind goods. While our explanation for tenants' high valuations remains incomplete, the exercise highlights that lab-in-the-field tools can complement observational welfare calculations.

1.1 Contribution and Related Literature

First, we contribute to the nascent literature on Right to Counsel. Compared to previous work on Right to Counsel, we have four advantages. First, our trial has more power to study evictions than any previous RCT; is more relevant to today's housing markets than studies from over a decade ago; and provides evidence from the Memphis housing market that greatly differs from previous work in localities with more tenant protections.[8] Relative to roll-out designs, our RCT avoids concerns about the Stable Unit Treatment Value Assumption (SUTVA): for instance, tenants or landlords in control localities could change behavior after Right to Counsel begins in a nearby treatment locality, as rental markets are inherently subject to spillovers.[9] Second, we measure demand for lawyers and its implications for welfare assessments. Third, we exploit a sharp policy change to study how lawyers' effectiveness depends on the local policy environment — a potential explanation for previous mixed effects. Finally, we collect detailed survey outcomes on informal outcomes, which lets us study lawyers' effects on bargaining and whether lawyers just shift tenants from formal to informal evictions. Four previous RCTs, of which one took place in the past decade, show mixed effects of legal representation from relatively small samples (Table A1). Of the four RCTs, Greiner et al. (2012) and Greiner et al. (2013) are the most informative. These studies are highly credible and take important first steps in this literature. However, the studies were conducted more than a decade ago, have sample sizes of fewer than 200 total units, say little about informal outcomes, and find conflicting evidence. Both papers lack a pure control group, and instead study the impact of full representation relative to giving limited legal services. We examine the effects of attorneys relative to a (virtually) pure control, which is a

[8] One reason that lawyers may have different impacts today is that renting increased over the 2010s, which led to lower eviction filing rates per renter household (Gromis et al., 2022). As a result, households with eviction filings now may have different chances in court than in the past.

[9] On the one hand, such spillovers could be policy-relevant for RTC programs when they scale up. On the other hand, spillovers from the roll-out period are not guaranteed to extend to spillovers from a *fully* rolled out program. And to extrapolate spillovers to general equilibrium, it is also helpful to obtain partial-equilibrium treatment effects from a clean randomized trial (Abramson, 2023).

more relevant comparison for policymakers deciding whether to implement Right to Counsel.[10] Besides the RCTs, Cassidy and Currie (2023) conduct a quasi-experimental analysis of New York City's Right to Counsel roll-out, finding large effects on tenant outcomes. Abramson (2023) considers general-equilibrium implications.

Our second contribution is to the broader literature on housing insecurity and eviction (Collinson et al., 2015, 2024b; Humphries et al., 2024). Related studies examine emergency rental assistance (Collinson et al., 2024a), vouchers (e.g., Collinson and Ganong, 2018), homelessness interventions (e.g., Cohen, 2022; Phillips and Sullivan, 2023), rent control (Autor et al., 2014; Diamond et al., 2019; Geddes and Holz, 2022), or other supply-side forces. We study a proposal for addressing housing insecurity that conceptually differs from other policies, as it can directly affect the informal negotiations that are key to low-income housing markets (Rafkin and Soltas, 2024).

Third, we add to a law and economics literature on bargaining (Silveira, 2017), lawyers (Greiner and Pattanayak, 2012), and the court process (Kennan and Wilson, 1993). Recent empirical papers have studied the importance of attorney quality (Abrams and Yoon, 2007; Agan et al., 2021; Shem-Tov, 2022), caseloads (Caspi, 2023), incentives (Lee, 2021), and agency problems (Sadka et al., 2020). We provide evidence on whether and how lawyers work, as well as when policy amplifies their effectiveness.

A fourth contribution of this study is that we conduct behavioral welfare analysis by eliciting tenants' incentivized demand for legal assistance and consider how non-classical forces affect normative interpretation (Bernheim and Taubinsky, 2018). A few papers, from which we take inspiration, use lab-in-the-field tools to study public-finance topics in a developed-country setting (e.g., Allcott and Taubinsky, 2015; Mas and Pallais, 2017; Allcott et al., 2019, 2022; Lockwood et al., 2024; Rafkin and Soltas, 2024). Relative to prior work, we use these tools to measure demand in the context of a high-stakes impact evaluation. We also develop several methodological innovations that build confidence in the WTA elicitations (e.g., experimentally manipulating budget constraints when eliciting WTA). More broadly, we think these tools are portable to study any in-kind intervention — including direct assistance, job training, or wrap-around services — as willingness-to-pay for the program is a key parameter for welfare.

2 Framework Summary

We notate lawyers' effects in a simple Nash Bargaining framework which lets lawyers operate through several distinct mechanisms (Appendix C). Lawyers can change the outside options to

[10] Meanwhile, Seron et al. (2001) present results from a trial that took place in 1993–1994, which we see as useful but outdated. More recently, two policy white papers (Judicial Council of California 2017 and Jarvis et al. 2020) find mixed results when evaluating a 2015 state-run RCT in California. The RCT is difficult to interpret, and there are concerns about its validity (see notes to Table A1).

bargaining for both landlords and tenants and can change relative bargaining power in negotiations. The framework explains what different data can reveal about lawyers' mechanisms, why these mechanisms may have different welfare implications, and how to interpret different configurations of empirical results.

The framework has three key takeaways. First, administrative data on the frequency of court outcomes are sufficient to test whether lawyers affect outside options, but insufficient to separately identify effects on landlord versus tenant outside options. This is because whether bargaining is successful depends on the joint surplus of bargaining relative to outside options, not the division of that surplus. Conveniently, ERAP's expiry affects only tenant outside options, providing necessary variation to separate the effects on tenants versus landlords. Second, collecting informal bargaining outcomes is a key input to measuring the effects of attorneys on bargaining power, which cannot be observed in administrative court data. Third, the welfare effects of attorneys critically depend on whether they affect bargaining power or outside options. Changing outside options can generate a range of externalities while changing bargaining power simply redistributes from landlords to tenants. Thus, each of our combination of administrative data, policy variation, and informal survey measures are necessary to make progress towards measuring welfare.

3 Setting, Design, and Data

3.1 Setting

Background on Memphis and Shelby County. Shelby County, the county associated to Memphis, has a population of nearly 1 million and had more than 2,000 eviction filings per month before the pandemic. More than 50% of Shelby County is Black. Memphis, which has a population of more than 600,000, is one of the most economically distressed large cities in the U.S. Its poverty rate exceeds 20%, and less than 30% of the adult population have a Bachelor's degree. For instance, Memphis contrasts with Nashville, Tennessee, which has a poverty rate of 15% and a college-educated share of 44%.

Eviction Process in Shelby County. Landlords initiate the eviction process by serving the tenant with an eviction *notice*. These notices do not appear in court or other data. Many leases also waive tenants' rights to receive a notice. After 14 days, they may *file* an eviction, which initiates the court process. If the court rules in favor of the landlord, she receives a *judgment*, which grants her the right to obtain a *Writ of Possession* from the county sheriff. Following the literature, we consider formal evictions to take place with a judgment. Judgments may typically be made for "possession" or "money and possession," where the latter entitles the landlord to claim back rents in addition to possession of the property. There was no pandemic-era eviction moratorium in place for any part of this study.

The Works Initiative (TWI). TWI is a nonprofit in Memphis that, among other housing activities, represents tenants facing eviction. Most of the tenants represented as part of the program were assigned a lawyer employed full-time by TWI. TWI also recruited attorneys in the Memphis area to represent eviction tenants for a fixed, discounted fee ("low-bono"). Those attorneys have various specialties (e.g. estate law, personal injury) and received brief training from TWI attorneys on how best to represent tenants facing eviction.

Emergency Rental and Utilities Assistance Program (ERAP). Shelby County's ERAP was funded by Coronavirus Aid, Relief, and Economic Security Act (CARES) I and II to provide assistance to tenants with overdue rents. Each locality could set different rules for its program. Lawyers associated with this RCT could expedite the process with the court to have their tenant receive ERAP funds.

ERAP payments typically consisted of a payment for documented overdue back rents plus one month's future rent. When arranged through the court process, the attorney could pressure the landlord to agree to drop the eviction filing in exchange for receiving expedited ERAP.

Landlords could decline to receive ERAP and continue to pursue the eviction. In some cases, attorneys could help tenants receive ERAP even if the landlord declined, in which case the tenant would receive a check from the program with no stipulations.

The Shelby County program ended on December 31, 2022, allowing us to test the extent to which attorneys on their own or the interaction of attorneys and rental assistance assist tenants. ERAP's expiration was announced in August 2022.

Eviction Law in Tennessee. Tennessee has landlord-friendly eviction law. Any amount of non-paid rents can trigger an eviction. Beyond raising procedural objections about giving sufficient notice, there are few defenses against nonpayment of rent. For instance, tenants cannot raise failure to repair as a defense unless they provided notice to the landlord prior to withholding rent. Tennessee contrasts with California, Massachusetts, and New York — the other states with recent Right to Counsel RCTs — which all have more robust "affirmative defenses" against eviction. For instance, all three states give presumption that the landlord is retaliating if the eviction occurs within six months of the tenant exercising a legal right like reporting a code violation.[11]

3.2 Application and Outreach

TWI began accepting applicants in February 2022 and started providing assistance in March 2022.

[11] New York has additional defenses. For instance, the tenant can make a *laches* defense, in which she claims that the landlord is claiming debt that is too old. Alternatively, she can claim that the landlord is overcharging the tenant for the unit, or can make counterclaims based on habitability.

Tenants apply for a TWI program advertised as providing legal services to tenants facing eviction. The application takes less than 10 minutes and is conducted online. Tenants are informed in the application that the program has limited resources and not all will receive assistance.

TWI advertised the program in several ways. First, TWI posted information about the program on government homepages dedicated to assist tenants with housing problems. Second, TWI has the email addresses of tenants who previously applied for ERAP and emails them to inform them about the legal assistance program. Third, TWI sends postcards to all addresses that have eviction filings in Shelby County.

3.3 Experiment Design

An eligibility screen takes place before treatment assignment. Tenants may apply but be ineligible if: they do not have an eviction filing; their court date is too soon; or they are already assigned a lawyer through the legal arm of Emergency Rental and Utilities Assistance Program (ERAP). In rare cases, the program selected some some tenants to receive assistance automatically, without entry in a lottery. Our sample consists of tenants who are deemed eligible and whom we enter in the lottery.[12]

We randomized eligible tenants into one of three conditions: a control group, the offer of an attorney, or the offer of a meeting with an eviction counselor. The size of the counselor treatment is small (about 200 contacted households, many of whom are contacted long after the court process) and mainly done in deference to our research partner, who wanted to provide as much assistance as possible given their available resources.

Tenants selected for the control group received an email informing them of their status and providing basic information about legal rights. This information was fairly generic and extremely light-touch (see Appendix D.2); if anything, it attenuates the size of our treatment effects relative to an absolutely pure control.

Tenants selected to receive an attorney were assigned one from among the pool of attorneys with excess capacity. The tenant was contacted to sign a retainer agreement — hiring the attorney through TWI — or had the opportunity to reject assistance at that time.

3.4 Methodology

We use randomized lotteries to assign tenants to the offer of an attorney and present both Intent-To-Treat (ITT) and IV estimates. There are three differences in our setting relative to a simple RCT which we discuss in turn.

[12] Limiting eligibility to those with filings may underestimate the effects of an attorney. However, providing attorneys to all tenants at risk of eviction would be more expensive. Cities that have implemented universal provision of attorneys have made them available no earlier than the filing of an eviction notice (Cassidy and Currie, 2023). We thus see this eligibility restriction as policy relevant, even if it understates the potential impact of counsel.

Counselor Lotteries. Periodically, tenants who lose the initial lottery for a lawyer are entered in separate lotteries to receive "eviction counselors." Counselors are social workers or law students who provide coaching about how to handle eviction cases but do not serve as attorneys, go to court, or interact with landlords.

Our main specification excludes the roughly 200 applicants who received counselors, comparing people who received full legal assistance to a pure control group. Excluding them preserves randomization since the people who received counselors were also selected at random. Appendix exhibits show results comparing tenants who received lawyers to the combination of control group and those who received counselors, and find very similar results (Tables A2, A3). We reweight the control group by the inverse of their propensity to be selected in a waitlist lottery. This reweighting procedure preserves unbiasedness since we exclude people in the control group who are randomly selected for the counseling, and being *entered* in a lottery for counseling is potentially non-random.[13]

Waitlist Lotteries. When lawyers have more bandwidth than anticipated, tenants who were initially not selected to receive assistance were sometimes entered into small "waitlist lotteries": they were re-enrolled in a second lottery to be selected for assistance. We exclude this variation and instrument for Lawyer_i with being offered an attorney in the first lottery in which we entered a tenant ($\text{WinsFirstLottery}_i$).[14]

Treatment Propensities. Treatment propensities changed over time based on the number of applications and number of lawyers available to assist the program. The operational treatment assignment process also varied over time, based on logistical constraints with our research partner, and is fully detailed in Appendix D. For any lottery, we always have full knowledge of and control over the underlying treatment propensity. We obtain an unbiased IV estimate if we include (potentially nonparametric) controls for the treatment propensity $f(p_i)$ in X_i (Rosenbaum and Rubin, 1983). Thus, all our specifications saturate in the 12 unique treatment propensities.

Estimating Equations. Both the IV and ITT are policy-relevant. The IV estimate represents the Local Average Treatment Effect (LATE) of receiving an attorney on tenants who respond to an offer. The ITT represents the treatment effect of receiving an attorney offer, accounting for non-compliance.

[13]In practice, counseling lotteries select people who applied within a certain date and are quasi-random. Beginning in 2023, selection for counseling lotteries followed a pure random lottery based on the same eligibility criteria as the main study. However, as being *entered* in a counseling lottery is not necessarily random for some of the study, weighting the people who remain unselected for any lottery restores unbiasedness. For intuition, suppose that 40 people in the control group are placed in a given lottery for counseling, of whom 10 are selected. Then, we reweight the 30 who are not selected by $w_i = 1/(1-0.25)$.

[14]Since being entered in a waitlist lottery is potentially non-random, leveraging that additional variation requires stacking lotteries and yields complications for generating IV estimates. Thus, we focus on the "clean" variation that generates an unbiased Local Average Treatment Effect.

ITT estimates are from:

$$y_i = \beta \text{WinsFirstLottery}_i + X_i\gamma + \varepsilon_i, \tag{1}$$

where we include a potentially nonparametric control for the treatment propensity $f(p_i)$ in X_i.

The attorney-assignment lotteries yield a simple Instrumental-Variables (IV) strategy, for which we target the following second-stage regression:

$$y_i = \beta \text{Lawyer}_i + X_i\gamma + \varepsilon_i, \tag{2}$$

where Lawyer_i is an indicator for if tenant i receives a TWI attorney and X_i are controls. As expected, there is imperfect compliance. Some tenants win the lottery and are deemed ineligible after the fact, e.g., because they have received legal representation through other assistance programs. Some tenants do not respond to lawyers' outreach, even if selected. Finally, some tenants are denied counsel after winning the lottery because their court date is too soon.

In addition to controls for the treatment propensity, our main specification uses the post-double-selection Lasso method of Belloni et al. (2014) to select auxiliary controls for both the IV and ITT. We always impose that the propensity control enters the regression and allow Lasso to select from a vector of demographic controls.[15]

We select controls separately for each IV or ITT specification and outcome. As a result, the IV is not generally exactly equal to the reduced form divided by the first stage, and sometimes IV p-values are slightly lower than ITT p-values. We also show main tables excluding controls (Tables A6 and A7).

3.5 Data

We collect data on tenant outcomes from two sources: (i) administrative court data, and (ii) endline surveys. The administrative court outcomes are observed for 98% of the sample — all except 21 tenants (Appendix D), since program eligibility depends in part on being confirmed to have an eviction filing in the court records. In addition, we field baseline surveys to measure tenant demand for legal services (see details in Section 6).

Baseline and endline surveys are both optional. Response rates are high for the setting but low in absolute terms. We use them to augment our main analysis, but they are more subject to concerns about potentially important selection.

[15]The controls are: month fixed effects; indicators for: being female, Black, single, reporting that they cannot pay what the landlord requested, reporting that they previously took ERAP, reporting that they did not know if they took ERAP, or being on a housing voucher; and the continuous variables: age, monthly rent, monthly income, total amount owed, household size, and number of months in unit.

Court Outcomes. We obtain scraped court data generously shared by the Legal Services Corporation. We focus on the following court outcomes:

1. Eviction judgments. Judgments are the formal court orders confirming an eviction. Judgments can be made for possession only or possession and money, i.e. for back rents. When the court orders tenants to repay back rents, it formally records the "judgment amount," which we also observe as an outcome.

2. Nonsuits. Court cases that do not conclude in judgments are formally settled, dismissed, or nonsuited.[16] We group all such cases as "nonsuits." Nonsuits and judgments do not partition all possible resolutions for court cases: : Landlords frequently settle out of court but leave cases open, preserving the option to pursue judgment later without refiling. Thus, nonsuits typically represent a better outcome for tenants than idle cases.

3. Time until resolution. We also study whether the case has resolved, i.e. concluded in a nonsuit or judgment. This measure is less clear to interpret, since a fast resolution for a nonsuit may be valuable for both parties. This drawback notwithstanding, the outcome provides one way of studying the burden on the legal system.

4. Writs. After obtaining a judgment, the landlord has the right to obtain a Writ to have the sheriff evict a tenant. In practice, while we do see writs executed, they are fairly rare. For this reason, we focus on judgments as the conclusion of the eviction case. One weakness of this approach is that landlords and tenants may still negotiate a settlement in which the tenant can stay at the unit even after a judgment has been obtained.

5. Continuances. Parties in court may file "continuances," which delay a case resolution. By introducing a delay into the time at which a landlord can turn over the unit, we consider continuances as one objective measure of the court cost for landlords.

As another way of examining the effects on delays, we form a measure called "days left in unit." We do not have the full terms of the lease for all tenants. For cases that do not receive judgments, we impute that the case has 180 days left in the unit from when they apply (half a year). Other cases that do receive judgments have the number of days until the judgment takes place. This measure aggregates effects on judgments at different horizons.

Survey Outcomes. We complement the above by collecting surveys, which allow us to measure informal tenant outcomes. Cases that do not result in a judgment may resolve in a variety of ways. The tenant may leave their apartment under duress, commonly called an "informal eviction." Alternatively, the landlord and tenant may reach a resolution under which the tenant may

[16] In a rare share of cases, the tenant can obtain a judgment against the landlord, for instance if the court finds that the landlord was not fulfilling legal responsibilities to make certain repairs. We do not focus on these because they are so rare.

stay in the unit for some period of time. A consistent limitation of past studies has been an inability to evaluate the resolution of cases that do not result in formal evictions.[17] If representation helps tenants negotiate more favorable informal arrangements, this limitation is a first-order concern. We thus field endline surveys 4-6 months after tenants apply to track informal outcomes regarding tenant moves and payment plans that are unobservable in court data. Response rates for surveys of low-income populations are low and have been trending down in recent years (Heffetz and Reeves, 2019). We perform several additional randomizations to account for non-response bias, which we discuss in Appendix F.

In the surveys, we focus on the following outcomes:

1. Moves. We ask the tenant where they live, which we compare to their original place of residence at application. This outcome permits us to measure whether lawyers enable tenants to stay in their unit.

2. Out-of-pocket payments to the landlord. We ask tenants a series of detailed questions about negotiations between landlords and tenants. We use these questions to form measures of how much the landlord initially claimed was overdue and how much the tenant ultimately paid.

Differential attrition is always a concern in lagged outcome surveys. For example, if tenants who received an attorney from TWI see a larger increase in their probability to respond to a survey request from receiving a favorable outcome than tenants who do not receive an attorney from TWI, we may mistakenly conclude that attorneys increase the likelihood of favorable outcomes. Our study has several advantages relative to garden-variety attrition concerns. First, we have access to the complete administrative outcomes. We can therefore test for selection directly. Second, lawyers record similar information as tenants, so for treated individuals we have a useful additional benchmark. Third, we randomized some tenants into receiving more intensive outreach, which allows us to implement additional attrition tests (Dutz et al., 2022). Appendix F presents attrition and further tests.

The surveys make two contributions. First, at a minimum, they provide a wealth of information about the distribution of tenant outcomes that occur out of court. Relative to other surveys of tenants at risk of eviction (e.g., Desmond and Shollenberger, 2015), ours provides an unusual level of detail about informal settlements and negotiations that are typical in this setting. Second, the surveys provide novel, though suggestive, information about the effects of legal counsel on informal outcomes, especially relative to the existing empirical literature on Right to Counsel.

Given the importance of informal evictions for tenant and landlord outcomes (and the economics of eviction in general), and how little we know about them, we believe that giving in-

[17] Cassidy and Currie (2023) find that $\sim$ 30% of eviction cases do not have an outcome recorded in NYC court records.

formation on the effects of counsel on this margin is very valuable. However, concerns about attrition bias are well-founded, so we present the causal effects on survey outcomes with caution.

Informal Versus Formal Eviction. Why should policymakers or economists care about formal versus informal evictions? Formal evictions are costly for tenants and landlords, since they require court costs to file and are especially observable to credit agencies and other landlords. The negative causal effects of eviction that Collinson et al. (2024b) estimate come from a judge-IV design that compares tenants at the margin of formal eviction to tenants who receive filings, do not get formally evicted, but may still get informally evicted. Meanwhile, the sociology literature often emphasizes the effects of housing insecurity and the trauma of forced moves irrespective of formality.

The Nash bargaining framework (Appendix C) makes these points precise. Bargained settlements consist of many non-court agreements that resolve the court case, including agreements for the tenant to leave (informal eviction), agreements for the tenant to stay, payment plans, cash-for-keys, side payments, or for the landlord to simply drop her demands. The effect of attorneys on informal outcomes reveals information about changing bargaining power, which has an ambiguous normative interpretation, as it entails transferring from landlords to tenants.

3.6 Sample Descriptives and Balance

Table 1 shows sample descriptives and balance. We compare the experimental sample (Column 3) to American Communities Survey aggregates for Shelby County (Column 1) and those with annual household incomes below $36,000 (Column 2), which places them around the bottom quartile in the United States. Participants are more than 90% Black, nearly 80% female, and about 90% single. They are severely rent-burdened, since they have individual monthly incomes of about $1,300, rents of more than $900, and overdue rents of about $2,000. About a third have ever been evicted. Even compared to the relatively low-income population in Shelby County, households in our experiment are considerably more financially distressed. Consistent with randomization, treatment and control are balanced on observable characteristics.

4 Court Outcomes

4.1 Main Results

Overall Effects. We begin by showing Kaplan-Meier curves of receiving judgments, non-suits, writs, and continuances by whether the tenant was offered an attorney in the first lottery (Figure 1A and Figure A2). The vertical distance between the blue (offered an attorney in the first lottery) and orange (not offered) lines represents an estimate of the ITT effect of an attorney offer on the given outcome at that point in a tenant's eviction history, relative to filing. The figures suggest large and persistent effects on judgments, nonsuits, writs, and continuances, and modest effects on case resolution. Receiving an attorney offer appears to reduce the amount the tenant owes in money judgments (Figure 1B). However, in the spirit of plotting the raw data, these figures do not control for the offer propensity or reweight, so the figures should be interpreted cautiously.

Formal estimates of the treatment effects confirm the graphical evidence and suggest lawyers cause large improvements for tenants' court outcomes (Table 2). We focus on a time horizon of 180 days, and present both ITT and IV estimates. We find a strong first-stage: receiving an offer for representation causes a 61 pp increase in representation by the program (t-stat of the first offer > 20). Consistent with Figure 1, the ITT on judgments at 180 days is -13.9 pp (Column 4; $p < 0.001$), a 23% reduction off the control mean of 61%. The impact on judgments is mirrored by an increase in nonsuits (11 pp, Column 6), but not quite one-to-one. Attorney offers reduce the amount owed in judgments by about $733 (Column 5; $p < 0.001$) off a control mean of $2,311. IV estimates, which account for non-compliance, are even larger in magnitude. Lawyers reduce judgments by 23 pp (or nearly 40%), cause a 17 pp increase in nonsuits, and reduce writs by 20 pp.

We visualize the time path of the effect on judgments (Figure A1). The effects appear within 15 days and grow steadily.

Effects of Experience. A natural question is whether these large pooled results are driven by particular abilities or expertise at TWI. Recall that tenants may receive either an attorney who is a full-time employee of TWI ($N = 2$ attorneys) or an attorney who was recruited from outside firms ($N = 14$ attorneys). TWI attorneys regularly represent tenants in eviction proceedings and develop considerable expertise. By contrast, non-TWI attorneys have day jobs unrelated to eviction law and receive limited training.

We test the extent to which attorney type matters for tenant outcomes. While attorney assignment is not randomized, the lead TWI attorney typically assigns tenants based on attorney availability in a queuing system. A joint F-test of attorney type (TWI versus other) on the covariates in Table 1 suggests balance on observables ($p = 0.33$). Plotting Kaplan-Meier failure curves for TWI and non-TWI attorneys, we find that TWI attorneys are likelier to avoid judgments but differences are not significant (Figure 2A).[18] However, we find that TWI attorneys are significantly more likely to file continuances for their clients, buying them potentially valuable time (Figure A4).[19] In our setting, the returns to experience are present but subtle.

[18] Wilcoxon p-values do not reflect weights or propensity adjustments.

[19] This is despite non-TWI attorneys receiving training to file a continuance before doing anything else.

4.2 Mechanisms and the Effects of ERAP

What role do attorneys play when representing tenants in eviction cases? Naturally, represented tenants may be more likely to prevail in court. Lawyers may fill other roles as well. They may delay court proceedings to give tenants more time to move, negotiate more favorable arrangements outside of court, or help tenants access other resources. Past studies have been unable to distinguish these roles, as the relevant data are not recorded in administrative court filings and because coincidental policy variation in available resources is rare. But it is policy-relevant to understand why attorneys work. Attorneys are expensive. There are cheaper ways to provide tenants with more time to move or easier access to social services.

To examine a key mechanism that could influence RTC's impacts, we turn to spillovers between RTC and the concurrent Emergency Rental Assistance Program. As discussed in Section 3, before January 1, 2023 (about halfway through the study), attorneys could help tenants apply for emergency rental assistance through the federally-funded, locally-administered Emergency Rental Assistance Program (ERAP). ERAP was implemented as part of the response to the COVID-19 pandemic. On December 31, 2022, ERAP closed and no longer assisted tenants. ERAP's expiration partway through the Randomized Controlled Trial lets us study program spillovers and mechanisms for RTC's effectiveness.

The potential spillovers between ERAP and RTC are critical to understand for at least three reasons. First, RTC roll-out is often motivated by a desire to reduce eviction rates. It is therefore important to know whether access to ERAP is a necessary ingredient for RTC to prevent evictions before investing more resources in RTC programs. Second, welfare analysis of RTC involves measuring the effective cost of each eviction prevented. ERAP has a fiscal cost of at least the dollar value of back rents, exceeding the lawyers fees in this study by an order of magnitude. Lastly, ERAP serves as a transfer to landlords, wiping away tenant debts that often go unpaid, and thus acts as a zero-sum transfer with ambiguous welfare implications.

We do not have randomized variation in ERAP take-up. Relative to the straightforward analysis of the RCT's average impacts, this part of our study relies on more assumptions, and involves more of the applied micro toolkit. Our analysis proceeds in three steps. First, we show RTC's impacts on court outcomes fall dramatically once ERAP expires on January 1, 2023. Second, we perform a formal mediation analysis, showing that our estimate of RTC's impacts falls when we control for ERAP receipt in the pre-period. Third, we explicitly test for other confounding channels.

A useful aspect of our approach is that treatment effects after ERAP expiry serve as an out-of-sample validation of the pre-expiry mediation analysis. That is, RTC's treatment effects using post-expiry data are similar to what we find when we control for ERAP using pre-expiry data alone, which is not guaranteed *ex ante*. A benefit of the mediation analysis is that it does not suffer from concerns about other forces being bundled with ERAP expiry: the analysis is pre-expiry. A benefit of the pre-/post-expiry comparisons is that we do not rely on the strong assumptions required for mediation analysis.

While some skepticism is appropriate, complementarities between ERAP and RTC are a natural candidate explanation for the large effects we observe in this study. ERAP was a $100-million program in Memphis that fully extinguished tenants' back rents and provided additional months of rental support. TWI explicitly coached attorneys in the program to bargain with landlords by raising the possibility of receiving ERAP. As we anticipated ERAP would have a large effect on outcomes, we registered that we would examine this heterogeneity in our initial AEA registration on December 21, 2022 (before ERAP expiry).

4.2.1 Descriptive Evidence and RTC's Impacts, With and Without ERAP

We plot the raw time series trend in ERAP enrollment over our study sample, fuzzy matching participants in our sample to administrative payment records from the Memphis/Shelby County ERAP (Figure 3A).[20] About 60% of tenants received ERAP assistance before the program stopped accepting applications, in August 2022. Between September 1, 2022 and January 1, 2023, ERAP nominally required that tenants have an ERAP application in the case management system to receive an ERAP payment. We find that the chance of receiving payment during this period fell, about linearly. Kaplan-Meier curves in Panel B show that RTC has large impacts on ERAP receipt, with approximately a 20 pp gap between treatment and control. We confirm this treatment effect in Table 5, Column 1, which shows the impact of RTC on ERAP receipt before January 1, 2023. Panel C converts the Kaplan-Meier curves to ITT estimates, showing that receiving an attorney offer causally raises the probability of ERAP receipt (figure notes give specification details).

Now, we study how ERAP's expiry influenced RTC's effectiveness. We plot the raw data for judgments and non-suits, split by before versus after ERAP expires (Figure 4). Before ERAP expires, the results look similar to the pooled estimates (Panels A and C). After ERAP expires, the difference between the offer and no-offer groups is large at 30 days but moderates for judgments by 60 to 90 days after filing (Panel B). The gap between the judgment curves early on suggests that lawyers may delay eviction. There is only a small discernible effect on non-suits without ERAP being available (Panel D).

Formal tests confirm ERAP's quantitative relevance (Table 3), showing large impacts of lawyers on tenant outcomes before ERAP expiry and small impacts after. For instance, before ERAP expiry (Panel A), IV estimates give that lawyers cause a 37 pp reduction in eviction judgments at 180 days (s.e.: 6.9 pp), a 65% reduction from the control mean in this period of 57%. However, without ERAP, effects on judgments attenuate to 8.0 pp at 180 days (s.e.: 6.9, Panel B), an 11%

[20] We fuzzy match on name, address, and age, using identified payment data.

reduction relative to the post-expiry control mean. Naturally, cutting the sample reduces power, and we cannot reject a meaningful 21 pp reduction in the post-ERAP expiry period. Yet the difference in treatment effects at 180 days is economically large and significant (Panel C). Lawyers are 28.7 pp more effective in stopping judgments at 180 days when ERAP is available (s.e.: 9.7; p-value: 0.003). Thus, lawyers in the post-ERAP expiry period are about 78% less effective in stopping judgments at 180 days.

Without ERAP, lawyers still stop fast judgments. They reduce judgments at 30 days by an economically meaningful 18.6 pp (s.e.: 7.2). These effects at 30 days without ERAP are smaller than the with-ERAP effects, but remain significant.

We plot the ITT effects on judgments at varying horizons, before and after ERAP expiry (Figure 5). Before expiry, lawyers' effect increases in time after filing and then flattens. We observe no evidence that effects attenuate (light blue series). By contrast, after expiry, we find that lawyers delay judgments, but the result attenuates substantially (navy series).

To further study potential delays, we estimate the treatment effect on the days left in unit (Column 9). After ERAP expires, we find that lawyers increase the number of days left in unit by about 21 days (s.e. 11). This estimate is of moderate magnitude, despite finding that lawyers dramatically reduce judgments at 30 days, because the total number of cases with judgments at 30 days is small (21% after ERAP expires).

Effects on other outcomes also attenuate considerably. After ERAP expires, we find no detectable effects on judgment amounts, nonsuits, or writs. Notably, ERAP does not appear to change lawyers' court tactics, as they still file continuances at rates that are indistinguishable across periods (Column 8).

Without ERAP available, we cannot statistically detect a medium-run impact of providing legal assistance on our primary court outcomes.[21] In support of the point that ERAP was genuinely important relative to other variation, we present heterogeneity tests that interact indicators for different characteristics with the treatment (Figure 2B). An indicator for ERAP availability has the largest coefficient, relative to other forces that one might believe would affect attorneys' efficacy.

The major concern about attributing these impacts to ERAP is that other changes are bundled with ERAP's expiry. However, there was no other meaningful change to the eviction policy environment in Memphis coincident with ERAP expiry (see Section 4.2.3).

[21]Note that nothing in these results suggests that ERAP on its own is effective. Rather, it suggests that the combination of attorney provision with ERAP is effective. One reason ERAP could be more effective when paired with attorneys, rather than in isolation, is that attorneys could use ERAP as a bargaining chip. For instance, they could urge landlords to drop filings in exchange for assisting the tenant with getting ERAP payments.

4.2.2 Mediation Analysis

So far, we have presented simple cuts of the RCT data, pre- and post-ERAP expiry. We now perform a mediation analysis, leveraging the fact that we can observe ERAP take-up in the pre-period. We focus on estimating our ITT, augmented with a control for receiving ERAP:

$$y_i = \beta_1 \text{WinsFirstLottery}_i + \beta_2 \text{ERAP}_i + \gamma X_i + \varepsilon_i, \tag{3}$$

and we estimate this model in the pre-ERAP expiry period. We compare the β_1 coefficient in Equation (3) to the main β coefficient in the ITT specification (Equation 1). Unlike in the main analysis, we only include the treatment-propensity controls in X_i and do not use Lasso. We want to hold fixed which demographics get included in each specification to make it clear what role adding the ERAP control plays.

The main idea of the mediation analysis is that we test if, once we control for ERAP take-up, the pre-expiry ITT falls in magnitude. If it matches the post-expiry coefficient, we can attribute the entire pre-/post-expiry gap to the end of ERAP.

Mediation analysis along these lines requires a unconfoundedness assumption, in which the mediator (ERAP enrollment) is orthogonal to potential outcomes, conditional on treatment (Imai et al., 2010). We evaluate this assumption by examining how our mediation analysis changes as we add demographic controls (Oster, 2019), and by instrumenting for the mediator.

In our primary analysis, we generalize Equation (3) in two ways. First, we have reason to think that ERAP and receiving an attorney could interact. For instance, rental assistance may be more effective if accompanied by a lawyer, who can confirm the landlord actually expunges tenants' rental debts. Second, as we see in Figure 3A, ERAP's availability differs greatly during the pre-expiry period, so we allow the mediation to differ by calendar month. Our model is

$$y_i = \sum_t \sigma_t \left(\beta_1 \text{WinsFirstLottery}_i + \beta_2 \text{ERAP}_i + \beta_3 \left(\text{WinsFirstLottery}_i \times \text{ERAP}_i \right) \right) + \gamma X_i + \varepsilon_i \tag{4}$$

where σ_t are month fixed effects. In words, we fully interact the attorney offer with ERAP take-up and month fixed effects. We then study the average marginal effect of the attorney offer, estimated via OLS, and compare this parameter to the average marginal effect from Equation (1), which is simply the estimate $\hat{\beta}$.

The generalized model makes the unconfoundedness assumption more palatable. The mediator needs to be unrelated to potential outcomes, controlling for the presence of an attorney offer and how that affects ERAP's impacts. Intuitively, lawyers may "level the playing field," so that tenants' ability to take-up ERAP would not depend on potential outcomes as much as random forces.

Primary Mediation Results. Our mediation analysis largely confirms the pre-/post-expiry differences analysis. Table 4 shows the average marginal effect of receiving an attorney offer, when we include various forms of controls for ERAP. In Column 1 and 2, we show the pre- and post-ERAP coefficients on the lawyer offer, which are -0.21 and -0.04 respectively.[22] In Column 3, we add a single control for ERAP, which reduces the coefficient on the attorney offer to -0.14. Under the assumptions for mediation analysis, the ERAP control thus explains 41% of the gap between the pre- and post-ERAP expiry coefficients $((0.21 - 0.14) \div (0.21 - 0.04) \approx 0.41)$. The large impact of ERAP even in this simple specification argues against concerns about the more flexible approach in Equation (4).

In Column 4, we augment the model to include the full set of calendar month fixed effects and ERAP-by-treatment interactions. The average marginal effect falls in magnitude to about -0.1, suggesting that ERAP can explain 65% of the pre- and post-expiry gap. In Column 5, we convert our ERAP variable into a flexible series of ERAP-receipt-time fixed effects, motivated by the idea that receiving ERAP quickly could yield different results than receiving it slowly. In Column 6, we add demographic controls interacted by offer status. In the most flexible model, the average marginal effect of an attorney offer in the pre-expiry period falls in magnitude to -0.088, suggesting that ERAP explains 72% of the gap.

The stability of the mediation estimates to inclusion of controls provides a reassuring test of the unconfoundedness assumption (Oster, 2019). While these controls are informative — raising the R^2 by one to two times as much as the more flexible mediation controls did relative to the simple mediation — they have only modest effects on the coefficient of interest.

We show the impact of the ERAP controls visually in Figure 5, where the green series shows that controlling for ERAP closes the gap in judgments beginning at about 30 days. The relative similarity between the coefficients in the mediation analysis conducted on pre-period data and the post-expiry coefficients was not guaranteed; the post-expiry coefficients serve as an out-of-sample validation of the mediation analysis, lending it credibility.

We summarize results for all outcomes in Table 5. Rows 1 and 2 show the average marginal effects of receiving an attorney offer on the indicated outcome, split by pre- and post-ERAP expiry, without demographic controls. To make Rows 1 and 2 as comparable to the specification with a mediator, we also fully interact the attorney offer with month fixed effects. Row 3 shows the same specification as Row 1, but interacting the offer-by-month fixed effects with the flexible ERAP-receipt-time fixed effects (as in Column 5 of Table 4). We show p-values for testing equality in average marginal effects across specifications, using a bootstrap.

We find that the impacts are generally similar across outcomes: ERAP can explain one to two thirds of the pre- and post-ERAP differences in judgments, writs, and non-suits. It cannot explain

[22] These columns slightly differ from Table 3 because they exclude demographic controls selected in Lasso, for comparability with the rest of the table.

the reduction in judgment amounts. Controlling for ERAP also does not change the average marginal effect of an attorney offer on continuances — a reassuring non-effect, as attorneys were supposed to immediately file continuances separately from receiving ERAP. With the exception of judgment amounts and continuances, we can typically reject that controlling for ERAP does not affect our estimate of the pre-expiry average marginal effect.[23] We can typically not reject that, after controlling, the pre-expiry average marginal effect equals the post-expiry average marginal effect. While we are unsure why ERAP appears not to explain the decline in judgment amounts after January 1, 2023, we note that these are more noisily estimated.

4.2.3 Other Confirmatory Evidence

"Visual IV." As another way of visualizing the mediation effect, we show that demographic groups with the largest impacts on ERAP take-up have the largest impacts on judgments (Figure A6, see notes for specification details). This analog to a "Visual IV" plot suggests that the aggregate mediation relationship holds between demographic groups. We find this evidence persuasive, as alternative stories about confounds for the mediation effect need to explain why the confound would be remarkably stable across groups.

IV Estimates. With a valid instrument for the ERAP mediator, we can relax the unconfoundedness assumption (Imai et al., 2010). We use the simple mediation specification (Equation 3), and focus on how instrumenting for the indicator ERAP_i changes the results (to avoid using multiple instruments for the flexible mediation specification). We leverage the idiosyncratic "congestion" variation apparent in Figure 3A. After August 2022, ERAP became more difficult to access, but even before August, there was great variation in whether someone who applied to RTC in a given week could get ERAP. It is plausibly exogenous whether a tenant happened to apply in a high or low congestion week.

We form instrument Z_i, the leave-out mean of the share of tenants who applied in the same week as tenant i and received ERAP in that week based on the RTC data. The exclusion restriction is that ERAP congestion is independent of potential judgments, except through receiving ERAP. Exclusion is violated, if, for instance, ERAP congestion correlates with time-series trends in the eviction environment that also generate application.

The IV specification confirms our results, producing an even more attenuated coefficient on the attorney offer relative to the non-instrumented mediator (Table A4, Column 4 versus Column 5).[24] Column 1 shows that the first stage is strong (F-statistic $= 12.8$). We show similar results from an IV where instrument Z_i as an indicator for applying to RTC after August (when the

[23] The effect on non-suits is large but not quite significant at conventional levels. One reason may be their relative rarity.

[24] In these specifications, we control linearly for the propensity score instead of saturating, as propensity score fixed effects are close to collinear with the instrument since they vary with time.

program closed), and where the first stage is considerably stronger (Table A5).

Other Concerns. We additionally test for concerns regarding bundling, changing attorney composition, and changing attorney tactics in Appendix E. Our mediation analysis mitigates these concerns by testing whether controlling for observed ERAP receipt closes the pre/post gap in the pre-expiry period alone. The additional analysis confirms that interpretation: if anything, any changes in demographics and attorney composition widen the pre/post gap. The expiration of ERAP is the only mechanism we find evidence for.

4.3 Specification Robustness

Results from our main specification without dropping the tenants who received counselors are very similar (Tables A2 and A3). No reweighting is necessary for these tables. We also show similar results if we only control for the propensity score and do not use Lasso to select controls (Tables A6 and A7).

5 Informal outcomes

5.1 Design

Timing and Collection Details. We survey all tenants at least four months after they applied for assistance to collect data about how their case resolved, with an eye toward outcomes that are difficult to measure in administrative records. Between four and six months after applying, all tenants receive three emails and four phone calls from professional phone surveyors (in randomly varying order) unless they either participate or actively reject the invitation to participate (see Appendix F for details). Tenants were immediately informed that they would be compensated with a $15 gift card for participating in the survey.

Outcomes Collected. The online and phone surveys ask identical questions.[25] We collect detailed information about the case outcomes, any bargaining processes and agreed upon payment plans, and the tenant's current housing and economic situation. The outcomes are listed in Appendix F and summarized in Table 7.

Demographics, Response Rates, and Attrition. Table 6 shows response rates and attrition. We have attempted to reach all eligible participants across both modes. Altogether, we have a conditional participation rate of 39% (439 respondents).

Low response rates are expected in our setting as low-income groups who are on the verge of eviction routinely lose access to traditional means of communication when they stop paying

[25] The only exception is that online surveys additionally ask participants to complete attention checks interspersed throughout the survey.

bills. As discussed above, we invited tenants to participate in the endline survey both using links sent to their email on file and phone calls from surveyors. We randomly varied whether tenants were contacted first by an email or over the phone, which allows us to test for differences in the mode of survey.

We find reasons for concern about differential attrition. There is a difference both between treatment and control (coefficient: 8 pp, $p = 0.036$) and between tenants who have judgments in administrative data (coefficient: -12 pp, $p = 0.003$). Because of this, we consider the causal analysis to be suggestive and report it in Appendix F. We are in the process of exploring corrections, leveraging randomizations and other tests we built into the design.

The demographics of the sample we reach for endline surveys are fairly similar to the full contacted sample. A few notable differences are that the sample participating in endlines has a significantly higher share female, had more months in their unit, and is more likely to have enrolled in ERAP. We decisively reject that the endline sample is identical to the main sample ($p < 0.001$), suggesting selection into participating into endline surveys. We also test for treatment–control balance on demographics within the endline sample. We also reject balance on demographics at $p = 0.014$, which motivates attrition adjustments.

5.2 Results

In all results, we focus on the entire (pooled pre- and post-ERAP expiry) sample for power.

Descriptive Evidence. We first document several novel descriptive facts separately for tenants who were formally evicted, informally evicted, and not evicted (Table 7). All tenants in our sample had an initial eviction filing, perhaps explaining the higher share of all evicted tenants who received formal court judgments (76%) than in other samples.

Panel A describes the negotiated agreement details of each group. Unsurprisingly, we find tenants who were not evicted were more likely to attempt to bargain with their landlord (59% versus 50% and 47% for formally and informally evicted tenants, respectively) and agreed to pay a higher share of the amount they owed (86% versus 66% and 74% for formally and informally evicted tenants, respectively). More surprisingly, tenants who were informally evicted were directionally less likely to bargain or pay out of pocket than those who were formally evicted.

Panel B documents patterns of informal outcomes. Tenants who were not evicted were much less likely to stay in homeless shelters or move, and reported paying higher rent at the time of the survey despite possessing no higher wages or likelihood of being employed.[26] They also reported a lower likelihood of going to court, which is consistent with tenants avoiding eviction primarily through out of court settlements or access to ERAP.[27] Tenants who were informally

[26] Reassuringly, we find similar rates of tenants who did not receive formal judgments report having moved ($\sim 45\%$) as other studies (Collinson et al., 2024b).

[27] We do find significantly higher rates overall of tenants reporting they went to court than other studies. This

evicted were most likely to stay in a homeless shelter (24% versus 20% and 7% for those who were formally evicted and not evicted, respectively). As expected, tenants who were not evicted were likeliest to report having a good or very good relationship with their landlord (28% versus 17% and 24% for formally and informally evicted tenants, respectively; Panel C).

Broadly, the descriptive evidence highlights the importance and difficulty of bargaining in this setting — over 50% attempt to bargain yet nearly 75% avoid making out of pocket payments — and reinforces the downstream effects of eviction on tenants' economic outcomes.

Causal Effects. We report suggestive causal analysis, attrition adjustments, and connections to our motivating framework in Appendix F. The results that we find are directionally and quantitatively consistent with the effects on formal outcomes. We find no evidence of attorneys substituting tenants from formal into informal evictions. Taken together, the results imply that the welfare effects of lawyers primarily operate through their effects on tenants' outside options.

6 Demand and Welfare

6.1 Set-Up

We present illustrative estimates of the Marginal Value of Public Funds (MVPF), which Hendren and Sprung-Keyser (2020) define as

$$\text{MVPF}^j = \frac{\sum_i \text{WTP}_i^j}{G^j}. \tag{5}$$

Here, WTP_i^j is individual i's willingness to pay for policy j and G^j is the net cost of providing j (cost of provision less fiscal externality).

In our analysis, we particularly focus on demand for the in-kind good, contrasting a traditional, calibrated approach and direct elicitation. Demand is intrinsically relevant, as it is a key parameter in any model of optimal provision of in-kind goods like lawyers. We then scale demand by the MVPF numerator, which helps with interpreting whether the difference in calibration vs. elicitation is large.

When estimating MVPFs, we consider the impacts on tenant welfare only. Landlords likely have a negative WTP for tenants they are evicting to be represented by an attorney.[28] Excluding this force is an imperfect abstraction, particularly as many landlords in our setting are small or middle-income themselves.

is likely due to a combination of reasons. First, tenants frequently report they went to court but their hearing was rescheduled. We count each appearance at the courthouse. Studies that report whether tenants attend their hearings do not. Second, tenants who participate in surveys may be more likely to go to court than those who do not. Unfortunately, the administrative records do not note whether a tenant was present, so we are unable to test for selection into the survey on this margin.

[28] In the MVPF framework, we set $\hat{\eta}_L$ — the average welfare weight on landlords relative to tenants — equal to 0.

We also ignore general equilibrium effects. The RCT is too small to meaningfully affect general equilibrium, so the MVPFs we compute are valid for the trial itself. However, general equilibrium forces are an issue for extrapolating these estimates to inform city- or state-level policy. For instance, providing attorneys at a rental-market level could change landlord behavior by either deterring eviction filings in the first place or raising rents (Abramson, 2023).

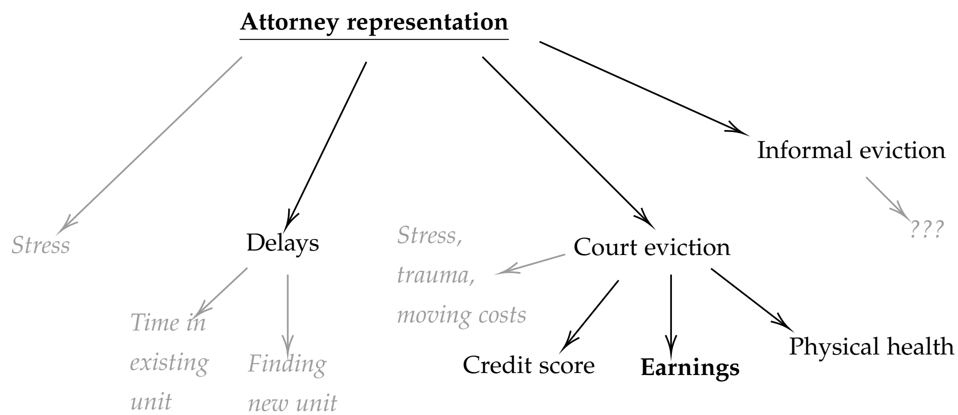
Net Costs. The net cost of providing attorneys to tenants facing eviction is the sum of the administrative cost of hiring the attorney and the fiscal externality on other government spending. TWI pays external attorneys $325 per case, and calculates a per-case cost of $250 for internal attorneys. For the purpose of this analysis, we use the average of the two. We rely heavily on Evans et al. (2021) and Collinson et al. (2024b) to convert our IV estimates on decreased evictions in the post-ERAP period into the fiscal externalities for other government programs. Table 8 summarizes our calculation of the fiscal externality generated by these and other effects. We find that providing an attorney generates a fiscal externality of -$28, implying it is socially beneficial and recovers $\sim$ 10% of the direct costs, which reduces the net cost of an attorney to $260. These estimates do not yet capitalize the effect of assistance on direct burdens on the legal system, but future drafts will.

Calculating WTP. How should we measure WTP? Traditionally, researchers calibrate the magnitude of the participant willingness to pay using estimated treatment effects. Sometimes this is straightforward: if expanding eligibility to social security insurance results in $46,100 dollars of additional benefits to program recipients, it is natural to conclude participants would be willing to pay $46,100 for the program (Deshpande and Mueller-Smith, 2022). Health programs sometimes employ Quality of Life Year (QALY) conversions, combined with a Value of a Statistical Life monetization (Hendren and Sprung-Keyser, 2020).

Such WTP calibrations require *measuring* all downstream effects of treatment and *monetizing* (i.e., generating a tenant willingness-to-pay for) each in turn. In particular, attorneys have many direct effects; for instance, on formal eviction, informal eviction, delays, and tenant stress/anxiety. Meanwhile, formal evictions, informal eviction, and delays are welfare-relevant because they affect downstream outcomes like earnings, physical health, and stress. We do not measure many of these outcomes (e.g., stress). It is difficult to monetize other ones that we do measure, like the tenant value of delays.

These are not idle concerns. To illustrate, consider a wheelchair-bound tenant who relies on veteran's disability payments and was selected for treatment. In an interview, he told us that he knew having an attorney would not change the fact that he would be evicted given the higher rents set by the new building owners. However, the delays that his attorney engineered gave him time to find a wheelchair-accessible apartment and avoid homelessness. The tenant said that he would prefer the attorney to having received $1,000 in cash, for this reason.

We visualize this challenge in the diagram below. Though representation has many welfare-relevant effects, we can only credibly measure and monetize the bolded one (effects on earnings). The italicized outcomes are not measured but are welfare-relevant. The unbolded, plain-text outcomes are measured in this or other studies, but difficult to monetize. Some of the unmonetized outcomes like credit scores or physical health are conceivably possible to monetize, albeit with assumptions that introduce further uncertainty. Others like delays are essentially impossible.



An alternative approach is to elicit the WTP for a program from individuals directly. Elicited WTPs have the advantage of recovering program effects that are observed by the participant but difficult for researchers to calibrate. However, they rely on: (1) high-quality elicitation, which is most credible when incentivized with real stakes, and (2) participants being well-informed about the impacts of the program during the elicitation. In fact, these two conditions can conflict. For instance, elicitations before receiving a lawyer can easily be incentivized. But elicitations after receiving a lawyer may reflect better information. Related to (2), expressed willingness-to-pay is not always normatively relevant, as it may reflect behavioral biases or misperceptions which affect decision utility but not experienced utility (Bernheim and Taubinsky, 2018).

Both calibrated and directly elicited WTPs have particular advantages and limitations. In some settings one may be clearly more appropriate, but researchers in settings with diffuse and hard-to-monetize effects should consider direct elicitations as a complement.

6.2 MVPF with calibrated valuations

We first calculate the MVPF using traditional, calibrated WTP. This approach can be interpreted as a lower bound on normative WTP: How should affected individuals value a policy given the set of effects that can be credibly calibrated? A natural tradeoff exists between capturing more welfare-relevant effects and limiting to the most credibly monetized. To highlight the contrast between WTP elicitation methods, we consider only the most credibly calibrated effects in this section, at the risk that this estimate is a lower bound.

We can credibly calibrate the welfare-relevant effects of attorneys mediated through reductions in eviction judgment rates using Collinson et al. (2024b)'s estimates of the effect of eviction. In this draft, we consider only the effects of eviction on earnings over the following two years. We estimate a WTP of \$75. This WTP estimate implies an MVPF of 0.3 ($\approx 75 \div 260$). According to this estimate, providing attorneys is a highly inefficient use of government funds.

6.3 MVPF with elicited valuations

We elicit applicants' willingness to accept (WTA) attorney representation versus cash using real-stakes multiple price lists embedded in a baseline survey at application. We collect this data for $N = 227$ tenants.[29] We survey tenants only after ERAP expiry, meaning that elicited WTAs do not reflect beliefs about accessing ERAP. Appendix G presents survey and elicitation details.

Our sample of 227 tenants conditions on the 90% of tenants who correctly answer a general attention check. Among this sample, tenants have a high level of comprehension. For instance, about 90% in this sample also correctly answer a separate confirmation question about WTA experimental procedures. We correct those who fail the question.

Our WTA elicitation method uses standard techniques from experimental economics. We elicit WTAs using an incentive-compatible multiple price list. We ask tenants whether they prefer to receive a lawyer or \$x in cash, varying \$x until we find (bounds on) the tenants' indifference point. We vary \$x between $\{100, \ldots, 1{,}000\}$ and impose monotonicity. Crucially, the questions are asked in a direct manner. We simply ask tenants whether they would prefer to get a lawyer or a cash endowment. We use the strategy method: tenants are selected to have choices implemented with a small probability. If selected, we randomly draw one value across the 10 possible questions to implement based on tenants' responses.

Tenant applicants exhibit very high demand for lawyers (Figure 6). Approximately 40% of tenant applicants prefer a lawyer to receiving \$1,000. The other 60% are spread relatively uniformly across the other values.

Point-estimating moments of the WTA distribution requires assumptions to account for all the top-censoring. Assuming conservatively that tenants who prefer a lawyer to \$1,000 in cash have a WTA of \$1,050, the average tenant demand for an eviction attorney from TWI is \$691 (s.e. 25). Alternatively, one can obtain a lower Manski bound by coding all WTA values at the bottom of the permissible interval, which yields a mean of \$641. Meanwhile, using a generalized Tobit

[29] The baseline survey takes place immediately after application. Tenants may not all be eligible for lawyers, in that they do not always have eviction filings, but perceive themselves to be eligible enough to apply.

to extrapolate the censored values implies a larger mean WTA value of more than \$800.[30] No matter what we assume here, WTAs are high.

If we use \$691 directly for the MVPF numerator, we obtain an MVPF of 2.7 ($\approx 691 \div 260$) in the post-ERAP period. This MVPF is relatively high, particularly for a policy that does not directly target children (Hendren and Sprung-Keyser, 2020) and which targets the poor (and thus, for which the social planner may permit a lower MVPF in order to achieve redistributive objectives). Even if the elicited WTA is overstated by a factor of two, the implied MVPF implies it is more efficient than providing cash in a non-distortionary way.

Such high willingness to pay is perhaps surprising, as tenants are very low income. One might have thought that tenants' high marginal value of a dollar, or the temptation of a fungible \$1,000 in cash, would drive tenants to express low WTA values for lawyers. These results are particularly high as we elicit them during a period in which lawyers have small impacts on court judgments. And, since we could hire lawyers for a few hundred dollars, they suggest a violation of fungibility: tenants could, in principle, take the cash (if selected), hire a lawyer, and pocket the difference. However, if tenants believe RTC lawyers are high quality, there are search costs, or tenants did not consider fungibility, then high WTAs are plausible. In the remaining sections, we explore the causes of high WTA.

6.4 Inattention

We now investigate concerns about inattention or comprehension. We took several steps within the survey to achieve high-quality responses. We randomize the order of whether cash or lawyers are presented first in the multiple price list, across participant. Participants do not seem to select lawyers first because they simply click the button to the right, for instance. We included many comprehension checks and bolded wording indicating that the choice could be implemented.

Including or excluding tenants who fail additional comprehension or attention checks throughout the survey has little impact on the results (Figure A10). Since observed measures of attention are uncorrelated with WTA, unobserved measures would need to be highly predictive to explain results (Oster, 2019). In fact, even dropping all tenants who prefer a lawyer to a \$1,000 check would still give a mean willingness to pay above \$400, which dramatically differs from the MVPF numerator in the calibrated approach.[31]

Moreover, we elicited the willingness to accept money or an iPad using a similar elicitation procedure (Dizon-Ross and Jayachandran, 2023). The WTA distribution for this comparison good

[30] In the first specification, we put WTA at the midpoint of WTA bins, \$50 for the minimum (the midpoint between \$0 and the first bin), and \$1,050 for the maximum. The generalized Tobit estimates a normal distribution to match the observed lawyer WTA data, accounting for binned WTA data and top- and bottom-censoring.

[31] Moreover, we randomly flip whether selecting "lawyer" or "money" is on the left or the right of the multiple price list. Consequently, random clicking left or right until the task concludes would generate symmetry which we do not observe.

has a low mean and far less top-coding (Figure A11). This test casts doubt on most stories about mechanical elicitation issues.

Inattention Model. The raw data in Figure A11 suggest that inattention cannot explain the results. However, there *is* a small spike in WTA at the maximum iPad amount. To quantify this residual inattention, we estimate a statistical model of inattention on the iPad data, and then use the results to inattention-adjust the lawyer WTA. Our exercise is conservative, as we have already dropped tenants who exhibit inattention and remaining tenants show high levels of comprehension. We assume that iPad WTA is distributed according to the mixture of a lognormal and a point-mass at the survey maximum:

$$\text{iPad}_i \sim (1-\alpha)\text{Lognormal}(\mu, \sigma) + \alpha\delta_{\max}. \tag{6}$$

We posit a lognormal distribution since the data are right-skewed. We interpret α as the share of people who are inattentive and always report the max. We estimate the vector (μ, σ, α) using Maximum Likelihood. Then we drop share α who report the maximum WTA value for lawyers.[32] The share who prefers \$300 to an iPad (Figure A11), together with the parametric assumption on iPad_i's distribution, identify inattentiveness α.

We estimate that 10.5% (s.e.: 3.3%) of iPad respondents are inattentive (Table A13), which yields a reasonably close match to the data (Figure A15). Dropping the corresponding 10.5 pp of those who report the max WTA for lawyers would reduce the mean WTA by 6% (from \$691 to \$650, Table A13), or the generalized Tobit mean by 12% (from \$845 to \$750). In other exercises, we additionally use the mass of people reporting less than \$20 WTA for an iPad to estimate the inattentive share. Such checks are even more conservative. Some needy tenants may genuinely prefer \$20 to an iPad. Nevertheless, the checks reduce the lawyer WTA to at least \$510, still much larger than the calibrated WTA.

This exercise contributes to the growing literature on correcting survey-elicited measures for inattention, particularly complementing Mas and Pallais (2017)'s study of inattention in job choice. Here, we demonstrate the advantage of using a comparison-good distribution to pin down inattentiveness α (Dizon-Ross and Jayachandran, 2023). While α naturally depends on the parameterization in Equation (6), the impact of α on lawyer WTA is governed by the *difference* in excess mass at the right tails between the iPad and lawyer distributions. Corrections for inattention can only reduce lawyer WTA so much, since fewer tenants report the maximum valuation for iPads than lawyers. Without the iPad data, inattention-adjusted WTA would be sensitive to assumptions about the WTA distribution in the censored region above \$1,000.

[32]Since we use this exercise only to inattention-adjust mean lawyer WTA, we do not need to identify inattention at the participant level.

6.5 Why Are WTAs High?

6.5.1 Set-Up

The previous section rules out inattention. We now examine other explanations, using data we collected on two key forces: trust and beliefs. We use these data, together with the inattention model, to adjust for behavioral biases and estimate the demand for lawyers that would enter welfare analysis (Bernheim and Taubinsky, 2018; Allcott et al., 2019).

Trust Games. Trust is a natural force to correlate with demand, given the literature on the importance of racial concordance between patients and doctors (Alsan et al., 2019), and that many of the program's lawyers were white.

At the end of the survey, tenants play "Trust Games" (TGs, Berg et al., 1995) against a TWI lawyer. In the Trust Game, a tenant is endowed with \$100, and can choose to transfer all, none, or some of the \$100 to the opponent. The opponent will receive an endowment of three times whatever the tenant gave, and can choose to return any amount of their new endowment to the tenant. We implement some of tenants' choices. If TG behaviors correlate with WTAs, we see that as corroborating evidence that the WTAs reflect real demand for lawyers.

Misperceptions. One reason WTAs may be high is due to behavioral biases. Consider the possible role of misperceptions. If tenants believe that lawyers are more effective than they are, that could drive high WTAs. Biased WTAs would not be normatively respectable in welfare analysis (Bernheim and Taubinsky, 2018; Allcott et al., 2019).

In the baseline survey, we collected two types of data on misperceptions. For our main estimates, we elicit beliefs about the average judgment rates within 90 days, among tenants assigned and not assigned lawyers. In particular, we purposefully elicited *second-stage beliefs* to sidestep having to discuss the lottery process or causality. That said, one should keep in mind that these estimates combine beliefs about treatment effects with beliefs about lottery compliance. We incentivize beliefs for accuracy.

As another measure, we ask tenants why they want a lawyer. If they tell us they want a lawyer because they think a lawyer will help them win in court, we can view these tenants as having a misperception.

6.5.2 Results

First, providing positive evidence about what underlies the WTAs, we find that Trust Game responses correlate with demand (see binned scatterplot in Figure A13, which also controls for misperceptions). For instance, a \$30 increase in the amount given to a TWI lawyer in the trust game (about one s.d.) correlates with about a \$59 increase in WTA.

Second, we find modest evidence that WTAs may be inflated by misperceptions. Figure A12A presents a histogram of the difference in second-stage beliefs. For instance, believing tenants without lawyers have a 10 pp higher rate of judgments than tenants with lawyers shows up as 10. The vertical line plots our estimate (about 2%) against tenants' beliefs. Tenants misperceive the second-stage correlation between having an attorney and judgments, but not hugely so. Their mean is about 21.8 (s.e.: 2.4), which is not far from the lower bound of our estimate in the post-ERAP period (21.5). After controlling for Trust Game responses, beliefs are correlated with lawyer WTA, though less so than trust (Figure A13B).

Third, tenants tell us that they value aspects of lawyers aside from whether they stop evictions. The baseline survey provides direct evidence on the importance of unmeasured or unmonetized values to tenants. We asked tenants what they thought an attorney would accomplish for them (Figure A14). Suggestively, 63% of tenants value the reduction in stress that attorneys grant, 56% want attorneys to help negotiate with their landlord, and 39% value the attorney's ability to fight the eviction. We can think of the 39% as biased, but it is also clearly not the main force that tenants say drives demand.

6.5.3 Rational Consumer Benchmark

To quantify the overall WTA, adjusting for misperceptions, we enrich the "rational consumer benchmark" approach in Allcott et al. (2019) with the above inattention model. Our goal is to estimate the mean WTA purged of biases like inattention and misperceptions.

Set-Up. We conduct a two-step procedure. In the second step, which closely follows Allcott et al. (2019), we estimate the following model of tenants' WTA for a lawyer:

$$\mathrm{WTA}_i = \mathrm{bel}_i\beta + X_i\gamma + \varepsilon_i \tag{7}$$

using OLS. Beliefs bel_i are a pair $(\mathrm{bel}_{1i}, \mathrm{bel}_{2i})$, where bel_{1i} is the difference in second-stage beliefs, and bel_{2i} is an indicator for whether a tenant wants the lawyer to help them win in court. We say that a rational benchmark is a tenant whose bel^*_{1i} is accurate (that is, ≈ 2), and $\mathrm{bel}^*_{2i} = 0$. X_i includes demographic variables and trust game responses. Then, we predict $\widehat{\mathrm{WTA}}_i$ at $\mathrm{bel}_i = \mathrm{bel}^*_i$.

In the first step, we use our inattention model in Section 6.4 to estimate the share of tenants who are inattentive α and report the highest WTA value. Assuming no heterogeneity in inattention that is correlated with i's characteristics, we drop share α from the data before proceeding to the second step.

The first step purges inattention — which, interpreted broadly, could encompass any non-normatively relevant force that generates artificially high iPad and lawyer WTAs. The second step regression-adjusts demand to account for misperceptions. As in related work, the second step relies on a selection on observables assumption (Allcott et al., 2019; Lockwood et al., 2024), which may be invalid if beliefs are correlated with other forces not captured in X_i. Another caveat is that our estimates may underestimate the role of misperceptions, since we only collect two beliefs and they may be subject to mismeasurement (Gillen et al., 2019).

We bootstrap the entire procedure to form standard errors. The bootstrap accounts for the fact that, when dropping a random share α, these dropped tenants may have different characteristics that feed into Equation (7).

Results. The rational consumer benchmark for demand exceeds \$600 (Figure 7A). We progressively add assumptions to the unadjusted WTA data. As in Table A13, adding the inattention model moves the WTA estimate down by about \$40. Eliminating misperceptions ($\text{bel}_{1i} = \text{bel}^*_{1i}$) or adjusting people who say they think a lawyer will win in court ($\text{bel}_{2i} = \text{bel}^*_{2i}$) moves the estimates down another \$30. Adding trust games or demographics, however, moves the estimates back up slightly. The apparently modest role of demographics provides some evidence about our selection on observables assumption (Oster, 2019).

In the final row, we add one more check using an elicitation which experimentally manipulates budget constraints. When designing the survey, we were concerned WTAs would be artificially *low* because tenants are so low-income. In particular, we elicit WTAs and tell tenants that their choice will be implemented alongside being given \$X, where \$X is either \$50 or \$500. (We pooled both elicitations in all other estimates above.) The idea is that, if tenants' demand for lawyers is higher or lower in the unconstrained state, that is informative about the impact of budget constraints on demand. We then say that normatively relevant WTA is when tenants' WTA was elicited for the unconstrained state. In particular, we augment bel_i to be a triple now containing variable u_i, and evaluate $\widehat{\text{WTA}}_i$ in the unconstrained state, setting $u_i = u^*_i = 1$. Here, this adjustment makes little difference to demand. However, we think this technique could be useful in other settings where demand is low, due to tight budget constraints.

6.5.4 Comparative Statics and Discussion

Ultimately, we find very little evidence that our high WTA estimates reflect inattention or misperceptions. How should we interpret them? In Figure 7B, we show that the WTAs *do* depend highly on measures of trust. For instance, evaluating the WTA at 0 trust yields a predicted WTA of less than \$500, versus a predicted WTA of more than \$700 with complete trust.[33] This large difference is informative, especially compared with the modest effects of beliefs. In fact, the correlation with Trust Games probably understates why WTAs reflect genuine, normatively respectable demand, as Trust Games are a noisy proxy so we expect attenuation. These data are corroborated by the aforementioned data that tenants value lawyers for reasons other than winning in court, and the

[33] This exercise differs from the one above. Previously, we used trust as a control to regression-adjust beliefs. Here, we predict WTAs at different levels of trust.

fact that tenants who want lawyers for those other reasons do not have lower demand for them (bottom row of Figure 7B).

7 Conclusion

Right to Counsel programs for tenants facing eviction have gained momentum as anti-eviction policy, despite limited empirical evidence on their effects. We randomize the provision of lawyers to tenants facing eviction in Memphis, Tennessee. We find large and positive effects of lawyers on tenant formal outcomes. However, we find that these results are largely driven by the combination of legal representation and access to the Emergency Rental and Utilities Assistance Program. We contrast two approaches to compute tenants' demand for lawyers without access to ERAP, ignoring general equilibrium impacts. Taking tenants' high valuations for attorneys at face value, direct elicitation yields a seven-times larger willingness to pay for lawyers (and thus MVPF) than calibration.

Methodologically, we contribute to a growing literature that embeds laboratory techniques into field experiments, with the goal of direct elicitation of welfare-relevant parameters. Economists involved in other randomized trials of in-kind transfers or services (such as job training programs, homelessness interventions, or others) might consider eliciting willingness to pay for the transfer. Such data are informative about the extent to which participants truly value the program, and thus complement other evidence about the program's treatment effects.

Empirically, we find mixed evidence on this Right to Counsel program. The results from the pre-ERAP period suggest attorneys have the potential to dramatically reduce evictions, if lawyers have the right set of bargaining chips. Absent ERAP, however, the impacts are about three quarters smaller. We find clear evidence that emergency rental assistance is an important complement to RTC programs. Given the cost of Right to Counsel programs, and their rapid adoption across many cities and states, we hope that this study motivates additional research about how to maximize the effectiveness of these policies.

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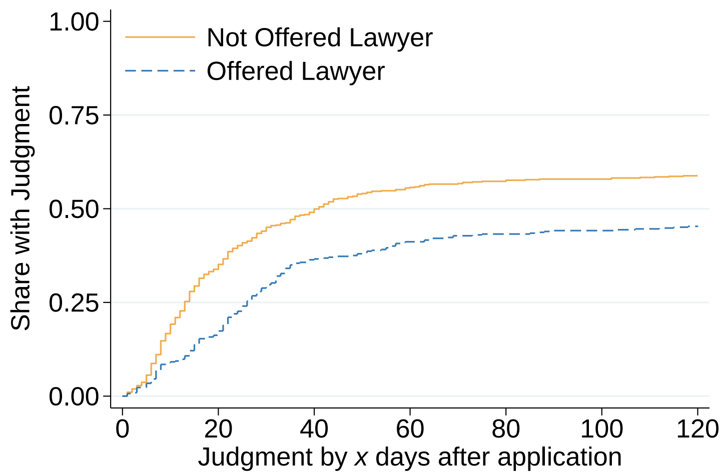
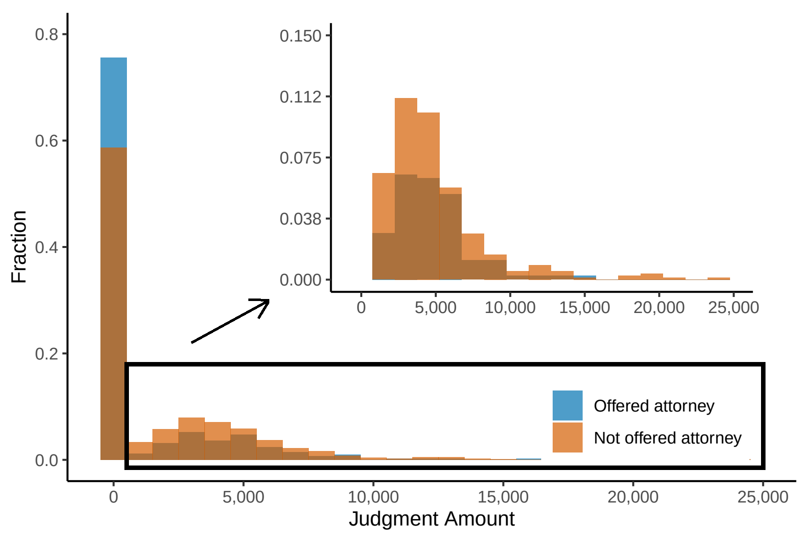
8 Figures

Figure 1: Effect of Legal Offer on Eviction Judgments

(a) Judgments by Attorney-Offer



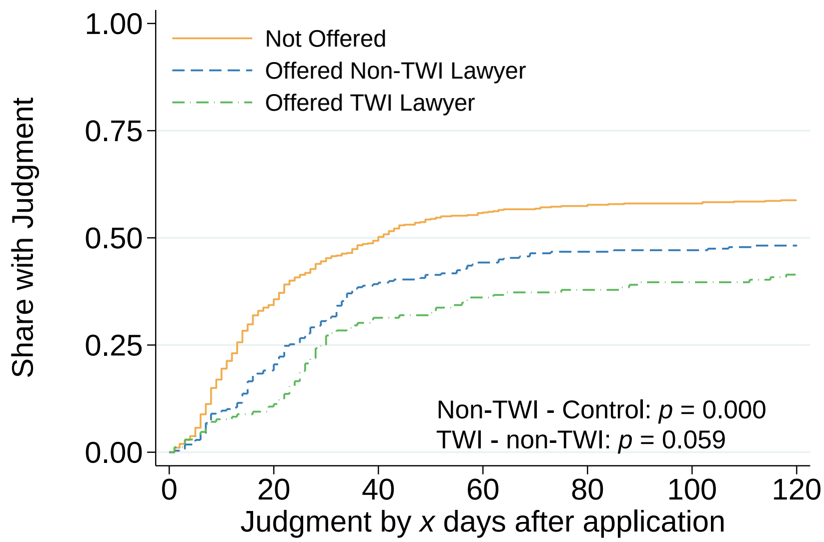
(b) Judgment Amounts by Attorney-Offer



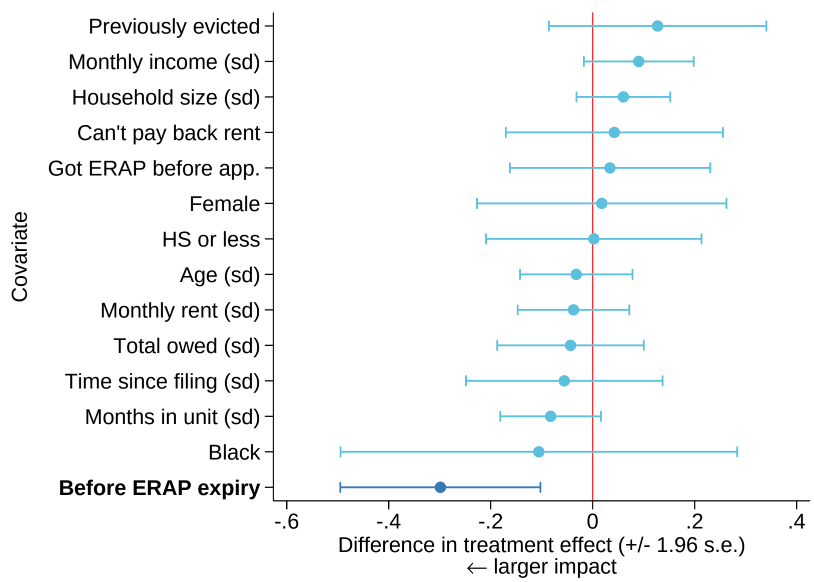
Note: This figure shows the difference in eviction judgment rates (Panel A) and judgment dollar amounts (Panel B) among those offered and not-offered a lawyer in the first lottery in which they are entered. Panel A plots Kaplan-Meier failure curves, which present the rate at which a group has achieved a certain outcome within a given number of days. The orange, solid line plots judgment rates for the group not offered attorneys and the blue, dashed line plots rates for the group offered attorneys. Panel B plots raw distributions of judgment amounts by treatment. The embedded figure shows the intensive margin (excluding $0 judgments).

Figure 2: Heterogeneity in Treatment Effects

(a) Judgments by TWI vs. other attorneys



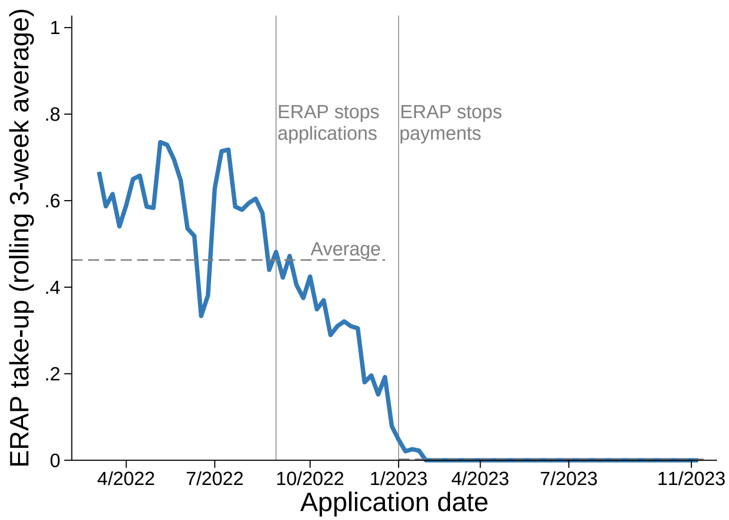
(b) Heterogeneity in IV estimates on judgments within 60 days



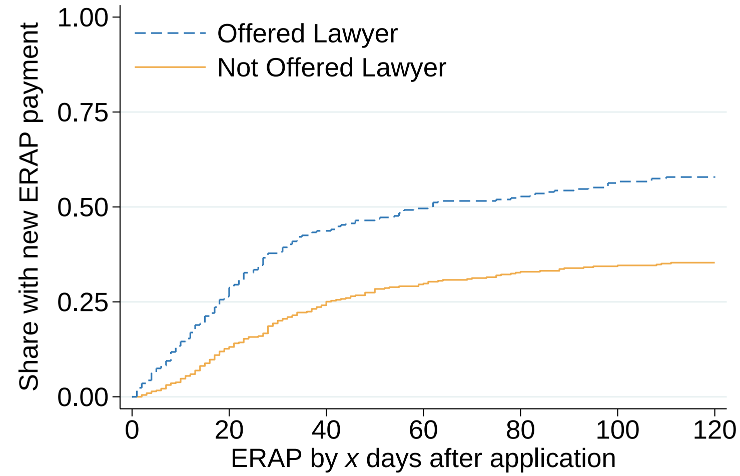
Note: Panel A separates the treatment effect on judgment between those who receive TWI attorneys versus those who receive contract attorneys. The *p*-values should be interpreted cautiously, since we do not reweight or adjust for time-varying assignment propensities. Panel B interacts the main specification (Equation 2) with an indicator for the listed demographic and presents the difference in coefficients. Thus, as the effect of attorneys on judgments is negative, a negative coefficient corresponds to *increasing* the treatment effect. We use a different post-double-selection Lasso procedure for each coefficient.

Figure 3: The Role of Emergency Rental Assistance During the Right to Counsel RCT

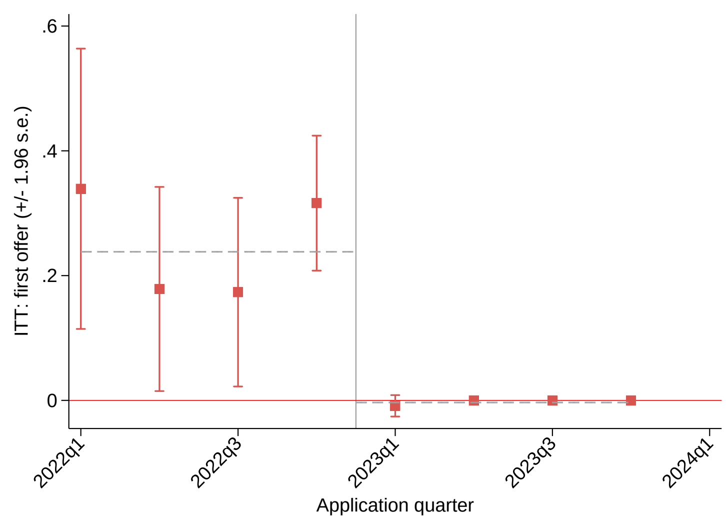
(a) Time Series of ERAP Take-up



(b) ERAP Take-Up by Attorney Offer Status



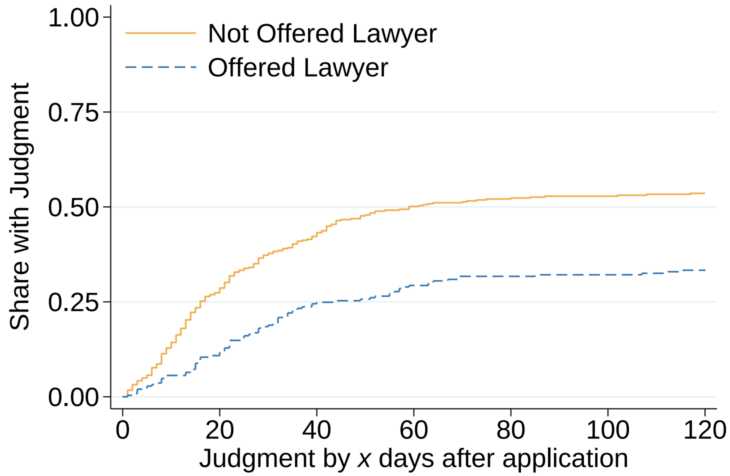
(c) Treatment Effects on ERAP Take-Up by Application Quarter



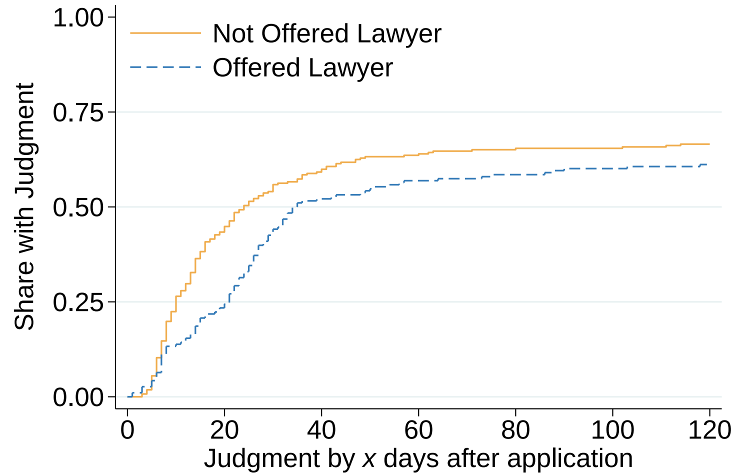
Panel A shows the time series of ERAP take-up among Right to Counsel applicants. Panel B shows Kaplan-Meier curves akin to Figure 1, with ERAP take-up as the outcome. Panel C shows treatment effects on take-up by application quarter. The specification saturates in the propensity score and reweights, but we do not use double post Lasso to select controls.

Figure 4: Judgments and Nonsuits by Attorney Offer Status, Pre- and Post-ERAP Expiry

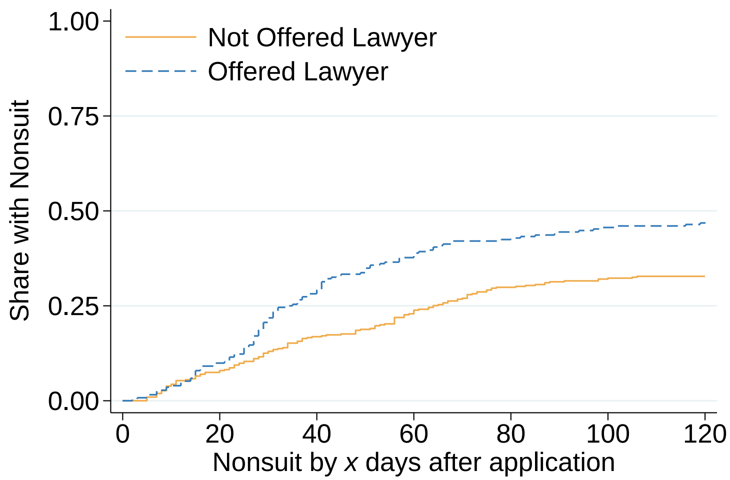
(a) Judgments, pre-ERAP expiry



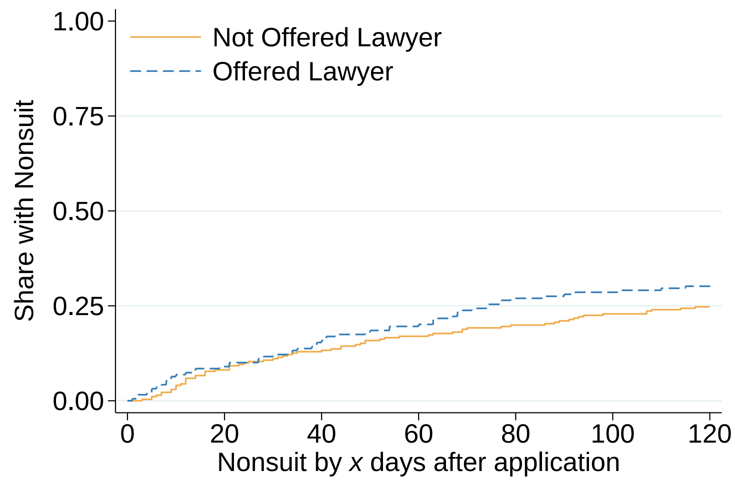
(b) Judgments, post-ERAP expiry



(c) Nonsuits, pre-ERAP expiry

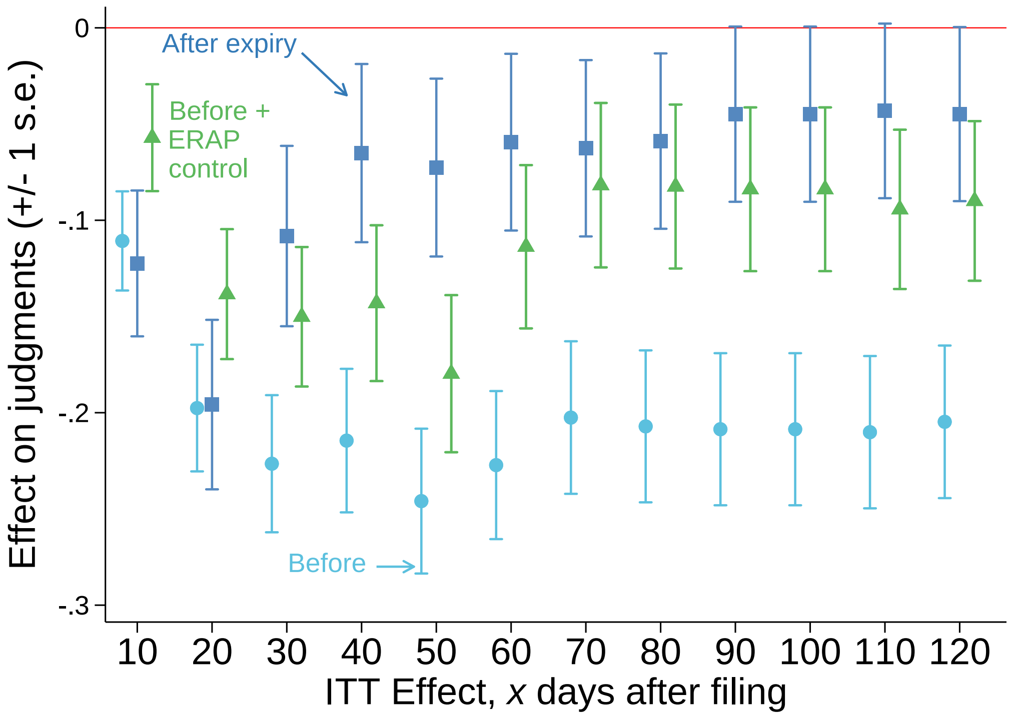


(d) Nonsuits, post-ERAP expiry



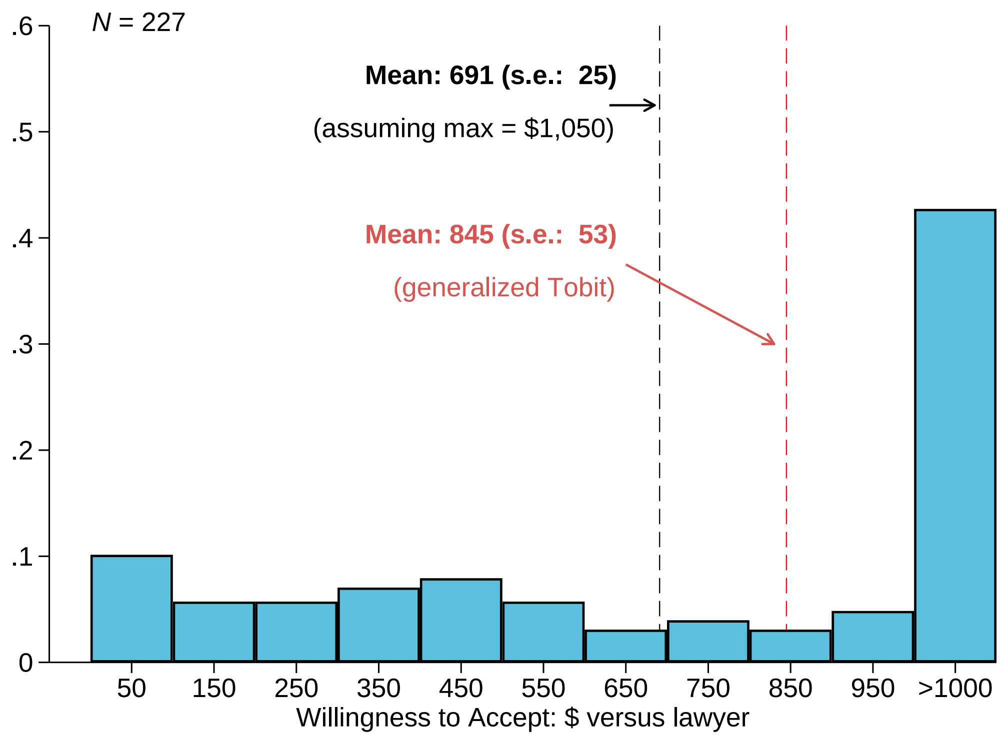
Note: These figures present Kaplan-Meier curves for judgments and nonsuits before and after ERAP expired. An individual is assigned to being post-ERAP expiry if they applied for assistance from TWI after January 1, 2023.

Figure 5: Right to Counsel Treatment Effects Before and After ERAP Expiry, and Mediation



Note: This figure shows the ITT estimate of receiving an attorney offer on judgments at the indicated period. The dark blue series shows the impacts after ERAP expires, whereas the light blue series shows the impacts before ERAP expires. The green series shows the estimates from Equation (4), using a flexible control for time until ERAP receipt. It is estimated entirely on pre-expiry RTC data. To maximize comparability between all the series, none of them use Lasso to select controls, but all reweight and saturate in the propensity score.

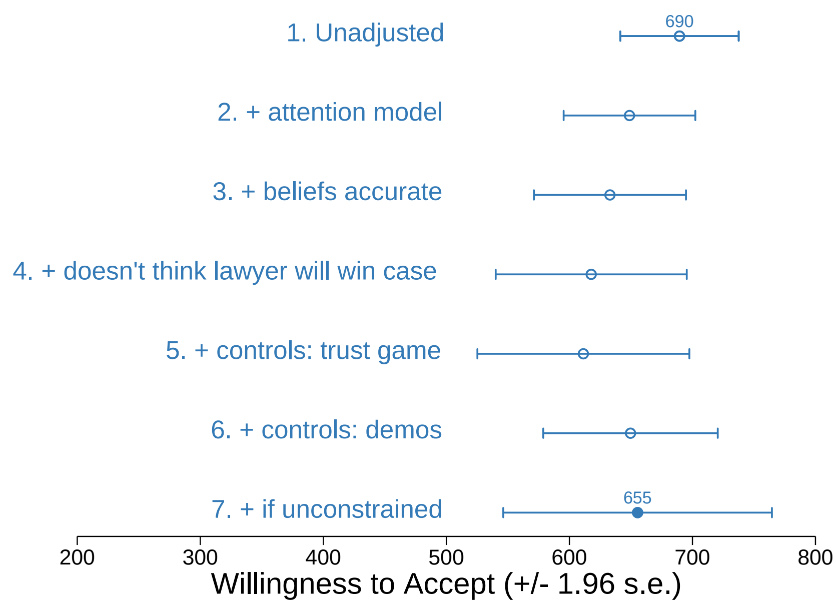
Figure 6: Willingness to Accept Money versus Lawyer



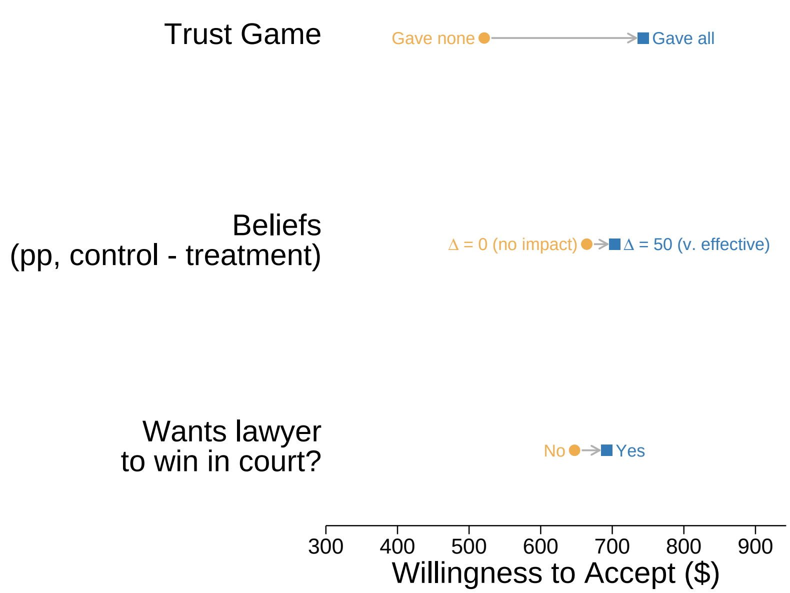
Note: This figure presents the distribution of Willingness to Accept (WTA) cash versus attorney representation by The Works, Inc. Choices are incentivized using the strategy method, and elicited using multiple price lists. The vertical black line indicates the mean, if we assume tenants who prefer a lawyer to $1,000 have a WTA of $1,050. The vertical red line uses generalized Tobit regression to estimate the mean, fitting a Normal distribution to the top- and bottom-censored and binned WTA data. See Appendix G for details on the elicitation method and checks used to ensure data quality.

Figure 7: Willingness to Accept Money versus Lawyer, Adjusted for Attention and Misperceptions

(a) Rational Consumer Benchmark



(b) Comparative Statics



Note: Panel A plots results from the rational consumer benchmark exercise in Section 6. The attention model drops those who both report the maximum WTA for a lawyer and whom we determine to be inattentive based on the model estimates in Table A13, Row 2. Rows 3 and 4 control for beliefs, as in Equation 7 and then evaluates the willingness to pay estimates at the "benchmark level" (having accurate beliefs or not saying they want a lawyer to win in court). Row 5 controls for the amount the tenant gives in a Trust Game against a TWI lawyer. Row 6 additionally controls for the following demographic variables: indicators in being female, Black, having less than a high school education, being single, being on a voucher; continuous variables of age, monthly income, monthly rent, back rents owed, tenure in unit (months), and the number of people in the household. Row 7 controls for an indicator variable in whether the WTA was elicited in an "unconstrained" state, where we first endow the tenant with an extra $450 if their choice is implemented. We then present estimated WTA for tenants who are unconstrained. In Panel B, we present three focal comparative statics, evaluating the model in Row 6 of Panel A where the tenant gives $0 vs. $100 in the Trust Game; has beliefs $b_{1i} = 0$ or $b_{1i} = 50$; or does versus does not want the lawyer to win in court. Since we drop a random tenant to match the share we estimate are inattentive, we bootstrap all estimates 200 times and show the mean and standard deviation of the bootstrapped mean.

9 Tables

Table 1: Data Description and Balance

	Shelby County (1)	Shelby County, monthly income $\leq$ 3,000 (2)	Experimental sample (3)	Treatment – Control (4)
Demographics:				
Age	45.0	47.0	33.8	-0.2
	[17.0]	[18.0]	[10.0]	(0.6)
Black	0.53	0.72	0.94	-0.01
				(0.01)
Female	0.53	0.61	0.80	-0.01
				(0.02)
Household size	2.0	2.0	2.7	0.1
	[1.0]	[1.0]	[1.5]	(0.1)
HS or less	0.45	0.69	0.65	0.03
				(0.03)
Single	0.40	0.56	0.90	-0.00
				(0.02)
Economic status:				
Monthly income	5,408†	1,100†	1,367	-85
	[115,769]	[631]	[1,361]	(86)
Monthly rent	834	682	967	-37
	[376]	[333]	[491]	(29)
Housing security:				
Applied after ERAP expiry			0.40	0.01
				(0.02)
Ever evicted			0.33	-0.01
				(0.03)
Months in unit			24.6	0.7
			[24.0]	(1.5)
Previously took ERAP			0.35	-0.07
				(0.03)
Total owed			2,959	-358
			[2,584]	(158)
F-statistic				1.22
p-value				0.262
N	5,586	814	1,140	

Note: This table shows the composition of the sample relative to all of Shelby County (2019 ACS) and Shelby County individuals with household monthly incomes of less than or equal to $3,000. Outcomes come from self-reports in the application at intake. Column (4) shows treatment minus control differences from an OLS regression, controlling linearly for treatment propensity as in our main specification. Estimates in Column (3) are weighted to adjust for excluding the counseling lotteries. The F-statistic comes from a joint test of the significance for listed covariates, saturating in the assignment propensity and reweighting as in our main specification. Parentheses show robust standard errors. Brackets show standard deviations. †: median.

Table 2: Treatment Effects of Lawyers on Formal (Court) Outcomes

	Has lawyer (1)	Judgment within 30 days (2)	Judgment within 60 days (3)	Judgment within 180 days (4)	Amount owed in judgment (5)	Nonsuit within 180 days (6)	Writ within 180 days (7)	Continuance within 180 days (8)	Days left in unit (9)
ITT: first offer	0.608	-0.180	-0.165	-0.139	-733	0.110	-0.123	0.220	26.1
	(0.024)	(0.029)	(0.030)	(0.030)	(177)	(0.030)	(0.029)	(0.029)	(4.8)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
IV: has lawyer		-0.295	-0.268	-0.225	-1,196	0.174	-0.204	0.353	42.3
		(0.047)	(0.049)	(0.049)	(290)	(0.048)	(0.047)	(0.047)	(7.8)
		[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Control mean	0.000	0.472	0.577	0.610	2,311	0.320	0.383	0.486	83.9
N total	1,140	1,140	1,140	1,140	1,140	1,140	1,140	1,140	1,140
N assigned attorneys	307	307	307	307	307	307	307	307	307

Note: This table shows the treatment effects of lawyers on the indicated court outcomes. Parentheses show robust standard errors. Brackets show p-values. The specification saturates in the propensity score, reweights to adjust for excluding the counseling lotteries, and uses Lasso (Belloni et al., 2014) to select controls from a set of demographics listed in Section 3.1.

Table 3: Treatment Effects of Lawyers: Court Outcomes, Before and After ERAP Expiry

	Has lawyer (1)	Judgment within 30 days (2)	Judgment within 60 days (3)	Judgment within 180 days (4)	Amount owed in judgment (5)	Nonsuit within 180 days (6)	Writ within 180 days (7)	Continuance within 180 days (8)	Days left in unit (9)
Panel A. With ERAP Available (March–December 2022)									
ITT: first offer	0.573	-0.229	-0.227	-0.211	-1,020	0.155	-0.210	0.201	36.4
	(0.032)	(0.036)	(0.039)	(0.040)	(205)	(0.040)	(0.036)	(0.038)	(6.2)
	[0.000]	[0.000]	[0.000]	(0.000)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
IV: has lawyer		-0.390	-0.395	-0.366	-1,795	0.262	-0.366	0.331	63.2
		(0.063)	(0.068)	(0.069)	(367)	(0.069)	(0.064)	(0.066)	(10.7)
		[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Control mean	0.000	0.408	0.528	0.570	2,110	0.352	0.374	0.501	91.7
N total	674	674	674	674	674	674	674	674	674
N assigned lawyers	162	162	162	162	162	162	162	162	162
Panel B. Without ERAP Available (January–November 2023)									
ITT: first offer	0.654	-0.119	-0.076	-0.055	-343	0.054	-0.004	0.249	14.4
	(0.036)	(0.047)	(0.046)	(0.045)	(309)	(0.043)	(0.047)	(0.045)	(7.3)
	[0.000]	[0.012]	[0.102]	[0.222]	[0.267]	[0.211]	[0.927]	[0.000]	[0.048]
IV: has lawyer		-0.186	-0.128	-0.080	-639	0.094	-0.009	0.379	21.1
		(0.072)	(0.070)	(0.069)	(484)	(0.065)	(0.071)	(0.065)	(11.1)
		[0.010]	[0.069]	[0.246]	[0.187]	[0.151]	[0.900]	[0.000]	[0.056]
Control mean	0.000	0.573	0.653	0.672	2,625	0.271	0.397	0.462	71.6
N total	466	466	466	466	466	466	466	466	466
N assigned lawyers	145	145	145	145	145	145	145	145	145
Panel C. Difference in Treatment Effects: After Minus Before									
ITT: first offer	0.081	0.110	0.152	0.156	676	-0.101	0.206	0.048	-22.0
	(0.048)	(0.059)	(0.060)	(0.060)	(372)	(0.059)	(0.059)	(0.059)	(9.5)
	[0.093]	[0.064]	[0.012]	[0.010]	[0.069]	[0.088]	[0.001]	[0.417]	[0.021]
IV: has lawyer		0.204	0.267	0.287	1,156	-0.168	0.358	0.048	-42.1
		(0.095)	(0.098)	(0.097)	(601)	(0.095)	(0.095)	(0.092)	(15.4)
		[0.032]	[0.006]	[0.003]	[0.054]	[0.077]	[0.000]	[0.603]	[0.006]

This table shows the treatment effects of lawyers on the indicated court outcomes. Parentheses show robust standard errors. Brackets show p-values. The specification saturates in the propensity score, reweights to adjust for excluding the counseling lotteries, and uses Lasso (Belloni et al., 2014) to select controls from a set of demographics listed in Section 3.1. Individuals are assigned to being in the post-ERAP expiry period if they applied for assistance from TWI after January 1, 2023.

Table 4: Controlling for ERAP Attenuates Pre-Expiry Attorney Offer ITT on Judgments

	Before Expiry: Judgment (1)	After Expiry: Judgment (2)	Before Expiry: Judgment (3)	Before Expiry: Judgment (4)	Before Expiry: Judgment (5)	Before Expiry: Judgment (6)
First Offer (Avg. Marg. Effect)	-0.208	-0.042	-0.139	-0.100	-0.088	-0.092
	(0.040)	(0.045)	(0.040)	(0.040)	(0.041)	(0.042)
R^2	0.075	0.014	0.146	0.191	0.271	0.238
Percent of gap explained			41.1	64.9	72.1	69.7
ERAP Variable	None	None	Dummy	Dummy	Receipt Time Bins	Dummy
Control: Offer × ERAP × Month FE	No	No	No	Yes	Yes	Yes
Control: Offer × Demo	No	No	No	No	No	Yes
N	674	466	674	674	674	674

Note: This table shows the impact of controlling for ERAP on the average marginal effect of receiving an attorney offer (Equation 4). Each cell comes from a separate regression. Columns 1 and 2 show the raw pre- and post-expiry coefficients, controlling only for propensity score fixed effects and reweighting. The estimates differ slightly from Table 3 because we do not use Lasso, to maximize comparability with the rest of the sample. Columns 3–6 present different mediation analysis specification. Column 3 includes a single control for ERAP receipt after applying for RTC (Equation 3). Column 4 interacts this indicator with month fixed effects and treatment (Equation 4). Column 5 modifies the ERAP indicator to be fixed effects for days until ERAP receipt (never received, 1–29 days, 30–59 days, 60–89 days, 90 plus days), and interacts these with month fixed effects and treatment as in Column 4. Column 6 includes a vector of demographic controls: indicators for female, Black, having less than high school degree, single, being on a voucher, ever being evicted, and having received ERAP before; continuous variables in age, monthly income, monthly rent, total amount owed, the number of people in the household, the time since receiving the filing. We always show the average marginal effect of the lawyer offer on the outcome, including in interacted specifications.

Table 5: Controlling for ERAP Attenuates Pre-Expiry Attorney Offer ITT, All Outcomes

	ERAP (1)	Judgment 180 days (2)	Judgment Amount (3)	Non-suit 180 days (4)	Writ 180 days (5)	Continuance 180 days (6)	*N* (7)
1. Before ERAP expiry	0.238	-0.196	-1,173	0.137	-0.204	0.199	674
	(0.038)	(0.040)	(208)	(0.041)	(0.036)	(0.039)	
2. After ERAP expiry		-0.038	-440	0.048	0.018	0.251	466
		(0.045)	(350)	(0.044)	(0.048)	(0.044)	
3. With ERAP control		-0.088	-1,091	0.069	-0.110	0.186	674
		(0.041)	(251)	(0.042)	(0.041)	(0.041)	
p-value: Row 1 = Row 2		0.012	0.073	0.146	0.000	0.359	
p-value: Row 1 = Row 3		0.004	0.678	0.102	0.004	0.714	
p-value: Row 2 = Row 3		0.448	0.178	0.779	0.054	0.309	

Note: This table shows the impact of controlling for ERAP on the average marginal effect of receiving an attorney offer (Equation 4). Specifications come from Column 5, Table 4. We always show the average marginal effect of the lawyer offer on the outcome, including in interacted specifications. Rows 1 and 2 differ slightly from Table 3 because we do not use Lasso, to maximize comparability with the rest of the sample. p-values come from a bootstrap, since we want to compare average marginal effects. We always saturate in the propensity score and reweight.

Table 6: Survey Participation Rate and Attrition

	Survey (all) (1)	Survey (phone) (2)	Survey (web) (3)
N completes	439	348	91
Share attempted to reach	0.99	0.99	0.79
Conditional participation rate:	0.38	0.30	0.09
Offer − No Offer	0.07	0.07	0.02
	(0.03)	(0.03)	(0.03)
	[0.025]	[0.028]	[0.513]
Judgment − No Judgment	-0.12	-0.11	-0.10
	(0.03)	(0.03)	(0.03)
	[0.000]	[0.000]	[0.002]

Note: This table shows the response and attrition rate for endline surveys. Section 5 describes the recruitment process. We attempted to reach some participants in multiple ways. The Treatment − Control row regresses an indicator for participating in the endline survey on being offered an attorney in the first lottery, saturating in the propensity score and reweighting to exclude counseling lotteries as in the main specification. The Judgment − No judgment row shows the same specification, but regressing the participation indicator on receiving a judgment. Parentheses show robust standard errors. Brackets display p-values.

Table 7: Survey Descriptive Results

	Formally evicted	Informally evicted	Not evicted
Share of total evicted	0.76	0.24	
Panel A. Agreement details			
Agreed to pay as share of owed	0.66	0.74	0.86
	(0.04)	(0.08)	(0.05)
Tried to bargain	0.50	0.47	0.59
	(0.03)	(0.06)	(0.04)
Out of pocket (extensive)	0.26	0.17	0.28
	(0.03)	(0.04)	(0.04)
Out of pocket (amount)	362	161	443
	(64)	(54)	(97)
Panel B. Outcomes			
Stayed in homeless shelter	0.20	0.24	0.07
	(0.03)	(0.05)	(0.02)
Moved	0.66	0.73	0.32
	(0.03)	(0.05)	(0.04)
Current rent	710	673	869
	(38)	(67)	(39)
Employed	0.62	0.63	0.63
	(0.03)	(0.06)	(0.04)
Current income	1951	2054	2174
	(96)	(195)	(136)
Went to court	0.88	0.79	0.68
	(0.32)	(0.41)	(0.47)
Panel C. Landlord relationship			
Never saw landlord	0.34	0.30	0.28
	(0.03)	(0.06)	(0.04)
Good landlord relationship	0.17	0.24	0.28
	(0.03)	(0.05)	(0.04)
N	219	71	149

Note: This table reports descriptive results from the endline survey separated by tenant eviction status. Tenants are labeled as "formally evicted" if they received a judgment in the administrative court records. Tenants are labeled as "informally evicted" if they report having been evicted but did not receive a court judgment. All other tenants are labeled as "not evicted." Most of the reported variables are self-explanatory (i.e. "employed" records whether the tenant reported they were employed at the time of the survey). Nonstandard ones are calculated as follows. "Agreed to pay as share of owed" records the ratio between the amounts the tenant reports agreeing to pay their landlord to the amount they report the landlord initially demanded. "Tried to bargain" records whether the tenant reports attempting to bargain with their landlord or offer a payment plan. "Never saw landlord" records whether the tenant reports never having contact with their landlord during the eviction proceeding. "Good landlord relationship" records the share of tenants who reported having a "good" or "very good" relationship with their landlord on a Likert scale. Estimates are not reweighted.

Table 8: Fiscal Externality for MVPF Post-ERAP

	Treatment effect of eviction (1)	Implied effect on gov budget (2)	Budget effect scaled by IV estimate (3)
Effects mediated by judgments			
Emergency shelter use	3.4 p.p.	$103.65	-$8.29
	(1.7)		
Hospital visits	0.188 visits	$118.96	-$9.52
	(0.094)		
Earnings (over two years)	-$936	$120.74	-$9.66
Other effects			
Writs	50%	$67.5	-$0.07

Note: All dollar amounts are converted to 2022 dollars. The positive values in column (2) reflect that eviction increase government costs. The negative values in column (3) reflect that counsel decreases eviction rates, resulting in spending reductions. Estimates of the effect of eviction on shelter use, hospital visits, and earnings come from Collinson et al. (2024b). Costs of shelter use come from Hao et al. (2022), who report that a median emergency shelter visit is one month and costs $2,100 in 2006 dollars. Moore and Liang (2020) reports an average emergency department visit costs $530 in 2017 dollars. Reliable government costs for non-emergency visits were unavailable; Collinson et al. (2024b) found similar size (but statistically insignificant) effects on emergency hospital visits. We follow the literature and use a 12.9% tax and transfer rate for low income populations (Hendren and Sprung-Keyser, 2020). We find that about half of judgments result in writs, which are executed by detectives. King County estimates that fulfilling a writ takes no less than $135 of detective time. We scale effects by the following IV estimates on the effects of counsel over 180 days in the post-ERAP period: reduces the rate of eviction judgments by 8.0 p.p.s, reduces the rate of writs by 0.01 p.p.s.

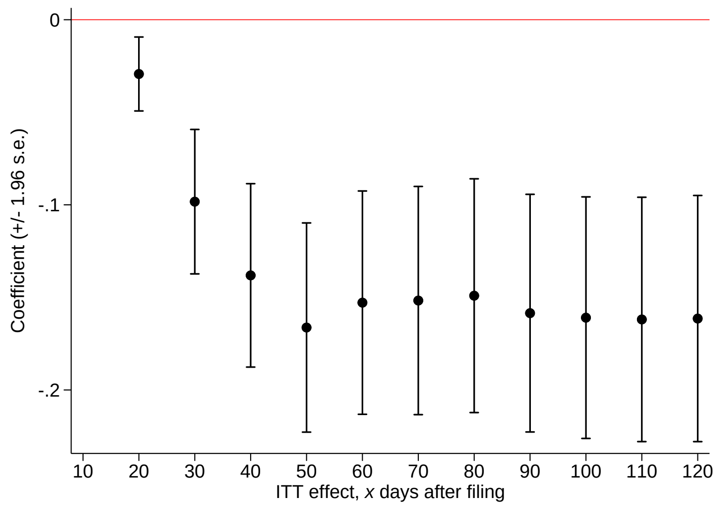
Appendices for Online Publication

Contents

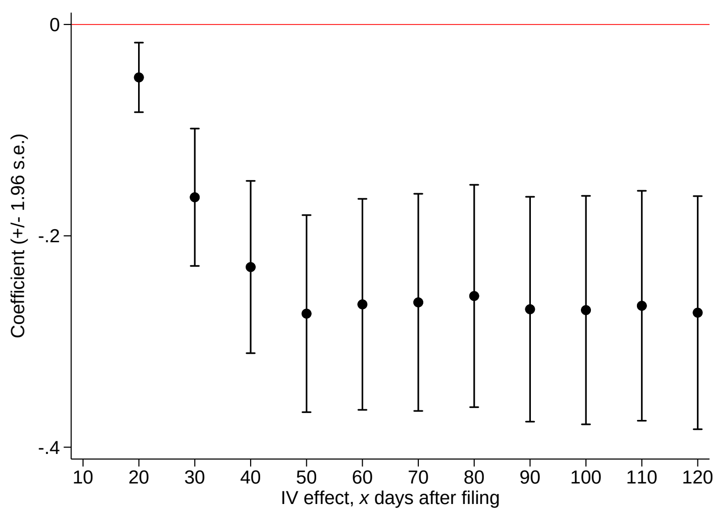
A Additional Figures

Figure A1: Effect of Legal Offer on Eviction Judgments (Full Sample)

(a) ITT on Judgments at 10, 20, . . . , 120 days



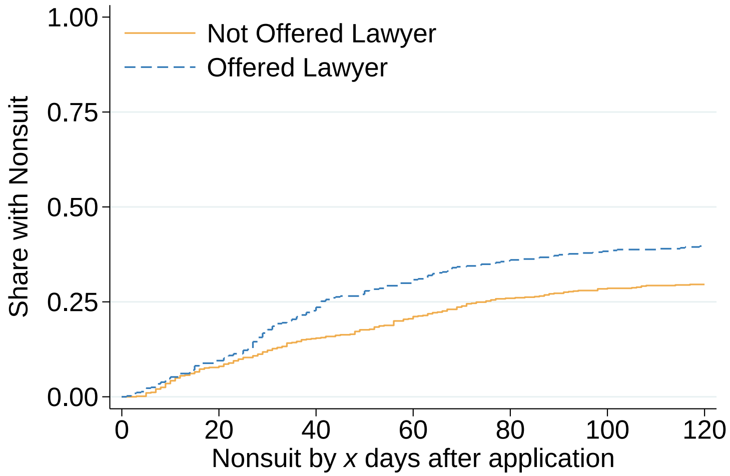
(b) IV on Judgments at 10, 20, . . . , 120 days



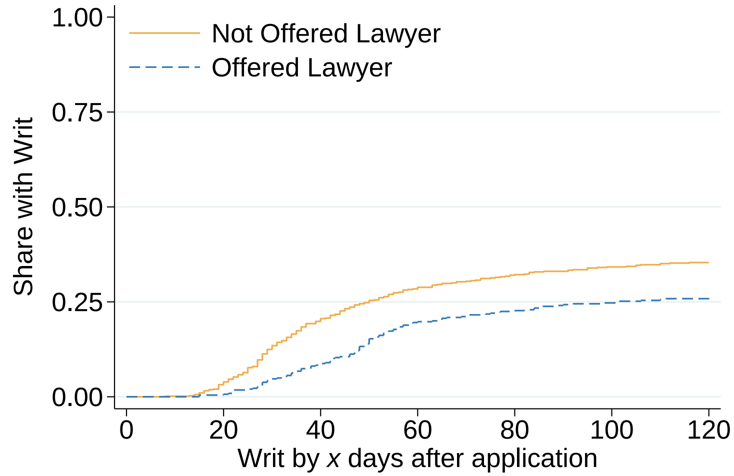
Note: This figure shows the estimates on judgments at various windows of time since eviction filing. Panel A plots ITT estimates and Panel B plots IV estimates for different window lengths. We use the specification in Table 2.

Figure A2: Nonsuits, Writs, Continuances, and Time in Court by Attorney Offer Status (Full Sample)

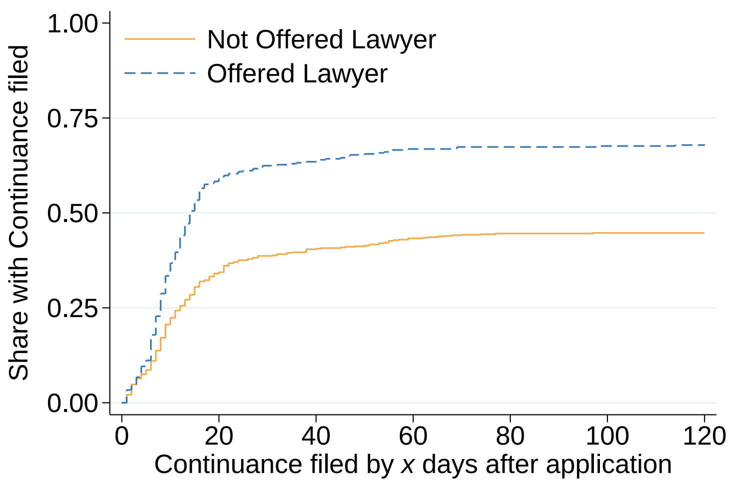
(a) Nonsuits



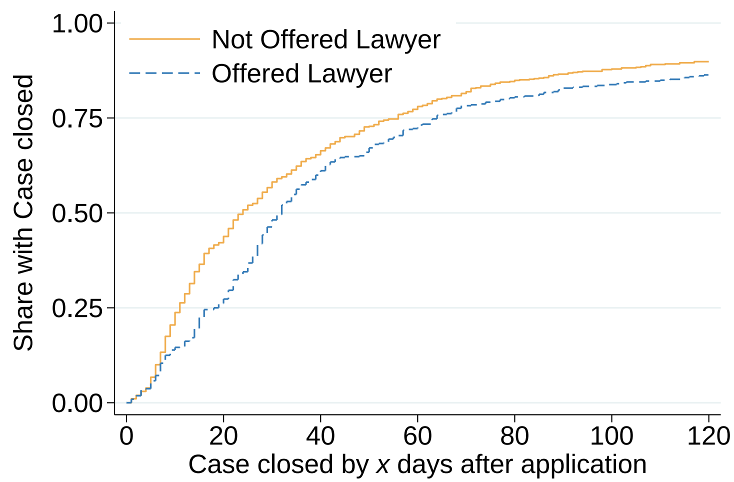
(b) Writs



(c) Continuances



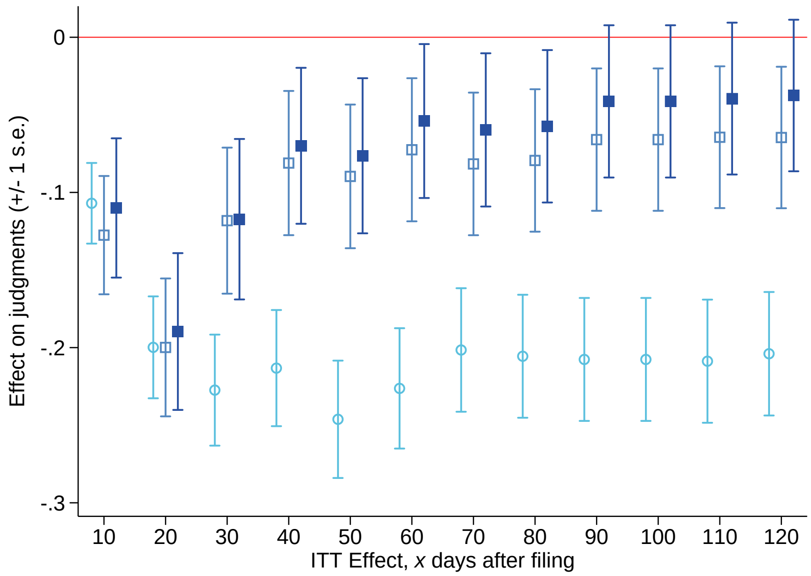
(d) Time in court



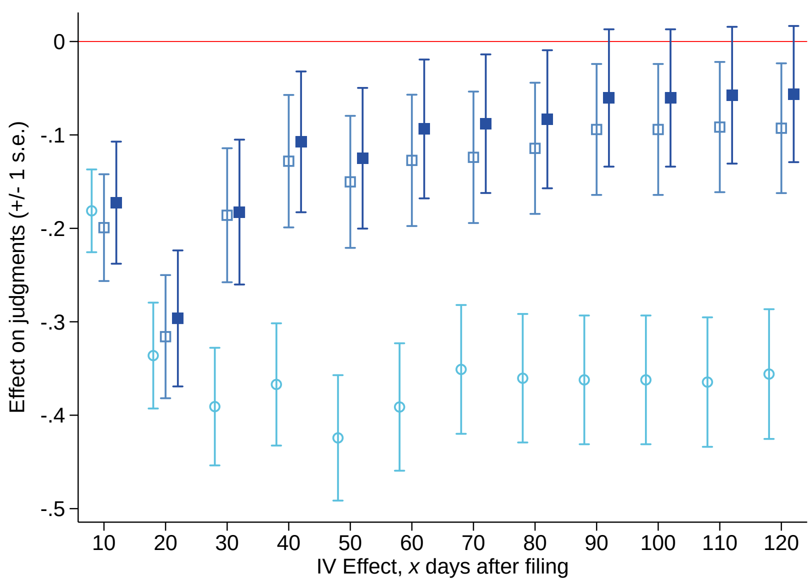
Note: This figure plots secondary outcomes using Kaplan-Meier failure curves, split by whether the individual was or was not offered at attorney. Panel A shows tenants offered an attorney (blue, dashed line in all plots) are significantly more likely to resolve their case with a nonsuit than those not offered an attorney (orange, solid line in all plots). Panel B shows treated tenants are less likely to be subject to writs of eviction. Panel C shows treated tenants are more likely to file continuances with the court. Panel D shows cases for treated tenants are less likely to be closed for any fixed window of time above 20 days.

Figure A3: Effect of Legal Offer on Eviction Judgments After ERAP, Re-weighting Demographics

(a) Heterogeneity in IV estimates on judgments within 60 days

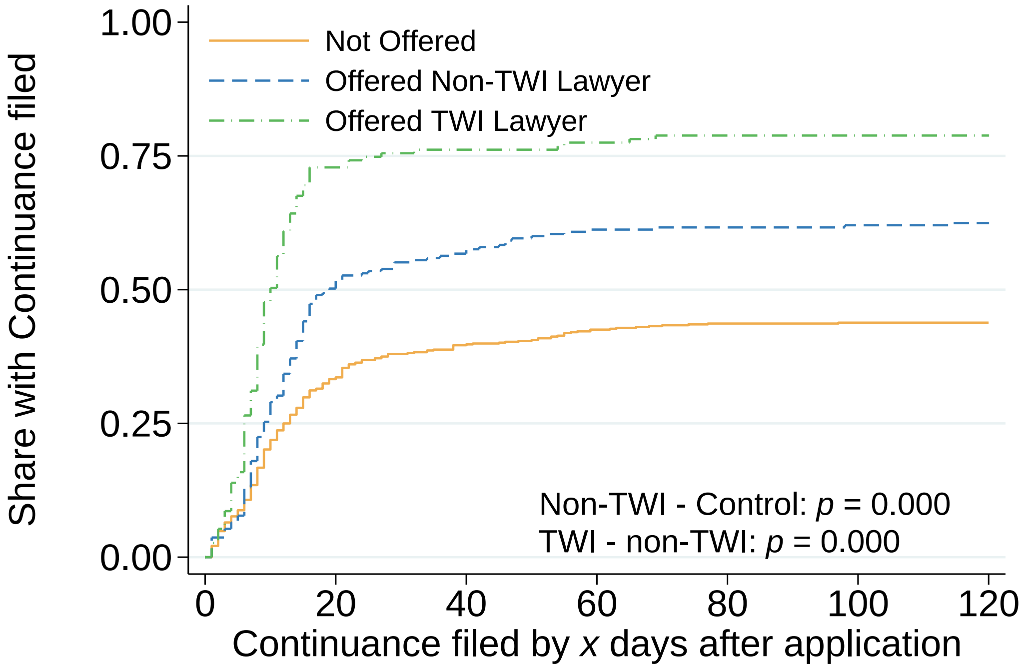


(b) Judgments by TWI vs. Other Attorneys



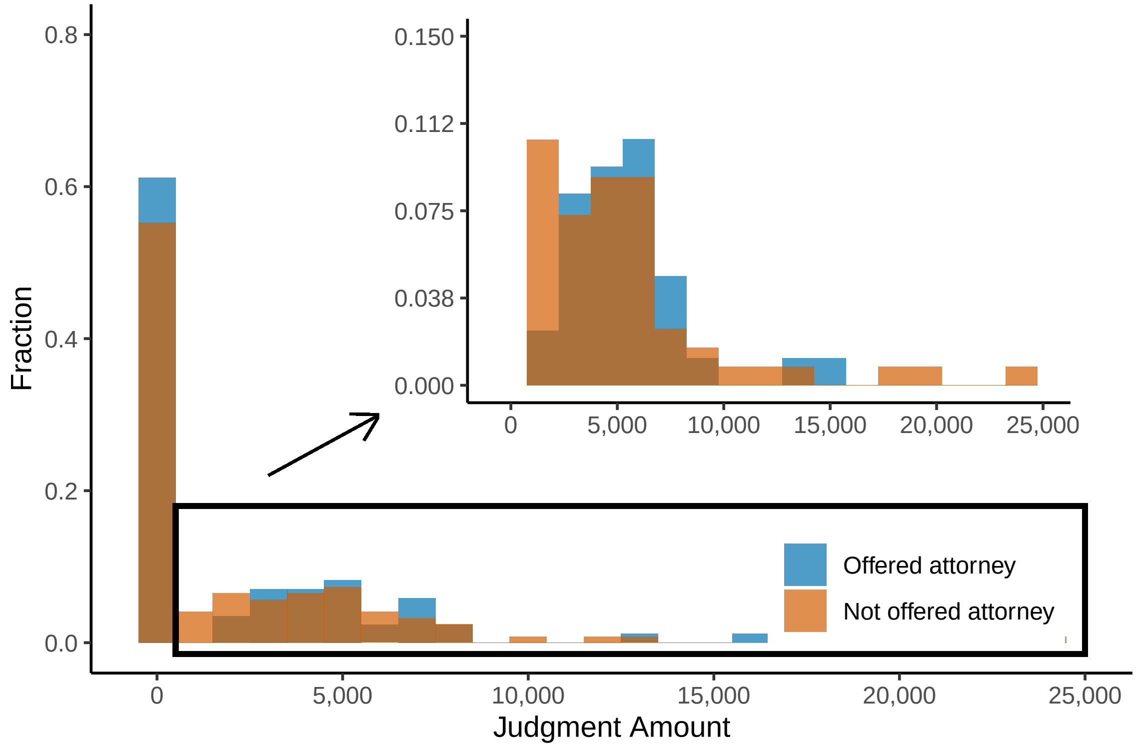
Note: The unadjusted estimates in this figure are identical estimates to Figure 5. The adjusted estimates use entropy rebalancing to reweight individuals in the post-ERAP expiry period to match those in the pre-expiry period (Hainmueller, 2012). The rebalancing demographics are the same as those selected for Lasso (see Section 3 for the list).

Figure A4: Continuances by The Works vs. Other Attorneys



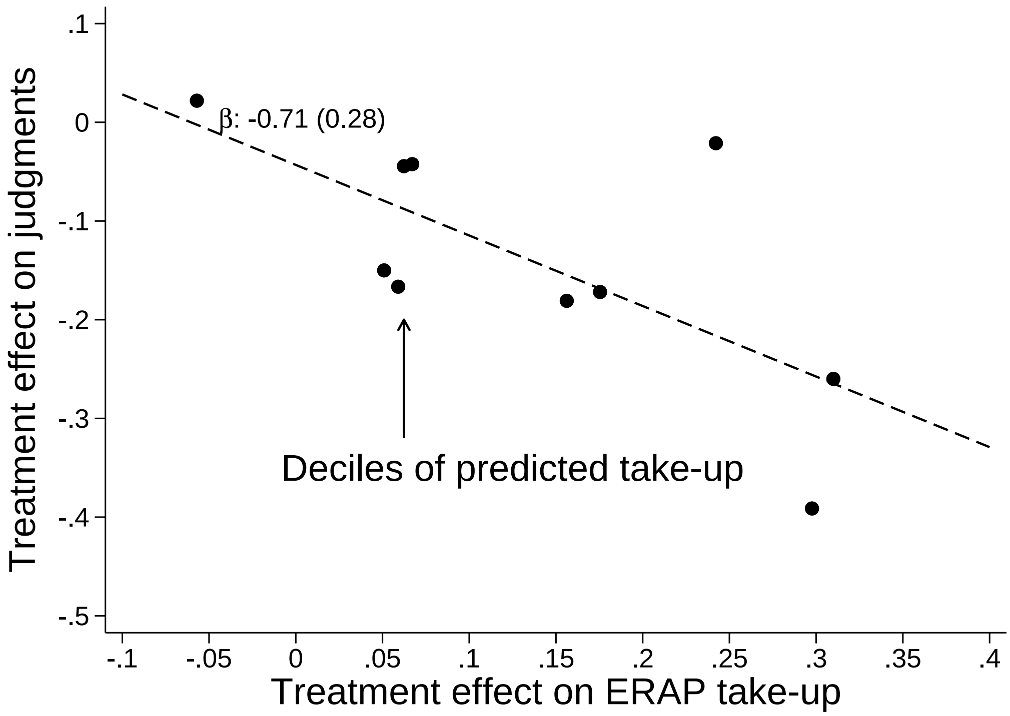
Note: This figure plots Kaplan-Meier failure curves separately for the likelihood of filing a continuance for TWI (green, dashed line) and non-specialized, ”low bono” attorneys recruited by TWI (blue, dotted line) relative to the control group (orange, solid line). *p*-values should be interpreted cautiously, since we do not reweight or control for the propensity score.

Figure A5: Judgment amounts post-ERAP



Note: This figure plots the distributions of judgment amounts separately for tenants offered attorneys (blue) and not offered attorneys (orange) in the post-ERAP period. Consistent with the regression estimates in Table 3, we find these distributions overlap.

Figure A6: Visual IV

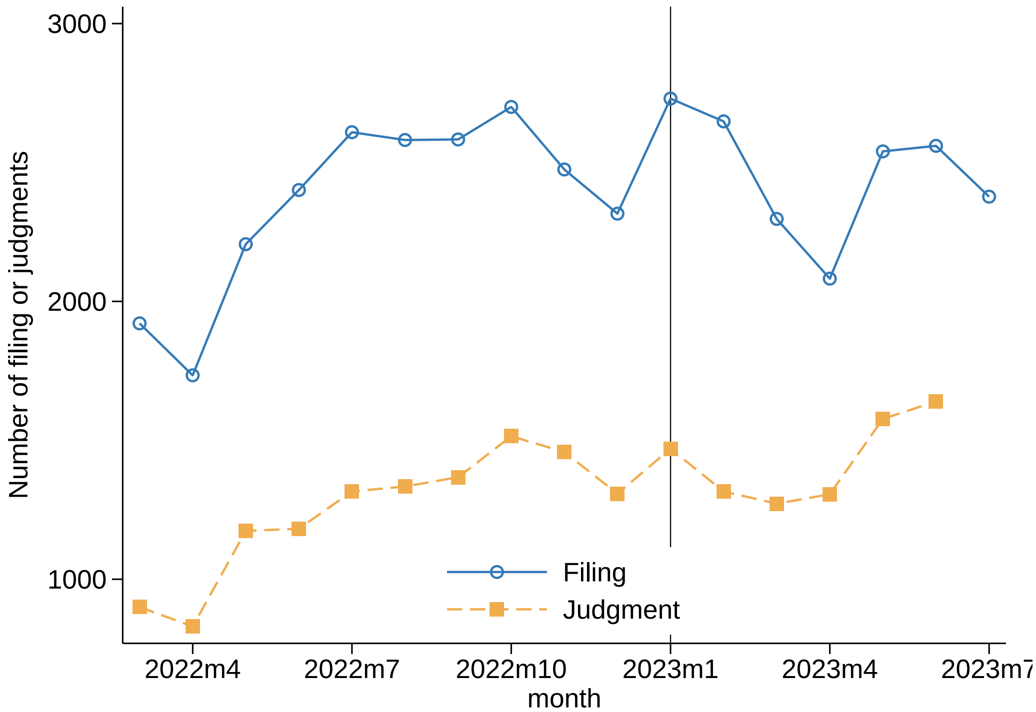


Note: To form the deciles in this figure, we first estimate

$$y_i = X_i\gamma + \varepsilon_i \tag{8}$$

using a logit on the pre-period data, where y_i is ERAP receipt and $X_i$is a vector of demographic variables (see Table 4 for the list, also augmented with month fixed effects). We form deciles of predicted estimates $\hat{y}_i$. Then, within each decile, we regress ERAP$_i$ on the lawyer offer variable, controlling for a continuous propensity score variable, and reweighting to account for not including tenants selected to get a counselor. We do not saturate in the propensity score because that would be close to collinear with the month fixed effects. We run the same specification but with judgments. We then plot the coefficients on the x and y axis. The best-fit line comes from a regression run on the 10 plotted points.

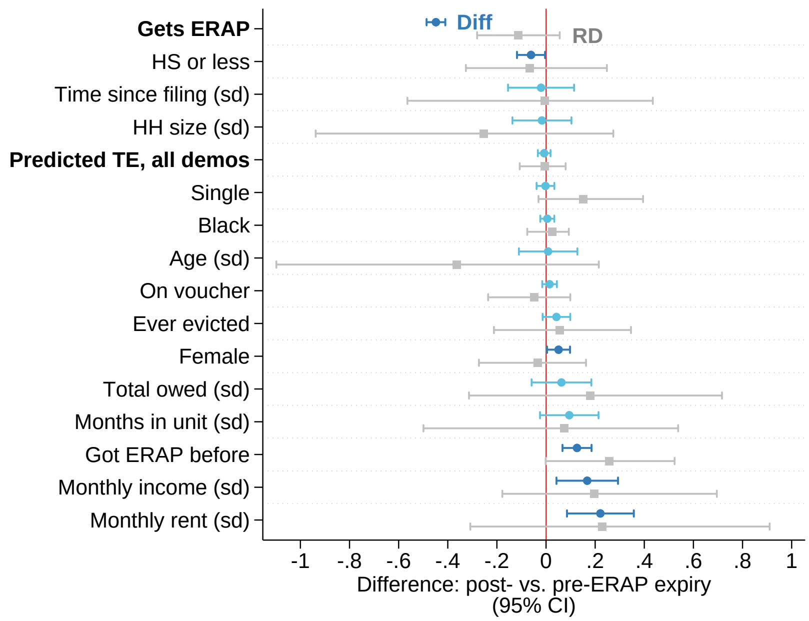
Figure A7: Monthly Evictions: March 2022–July 2023



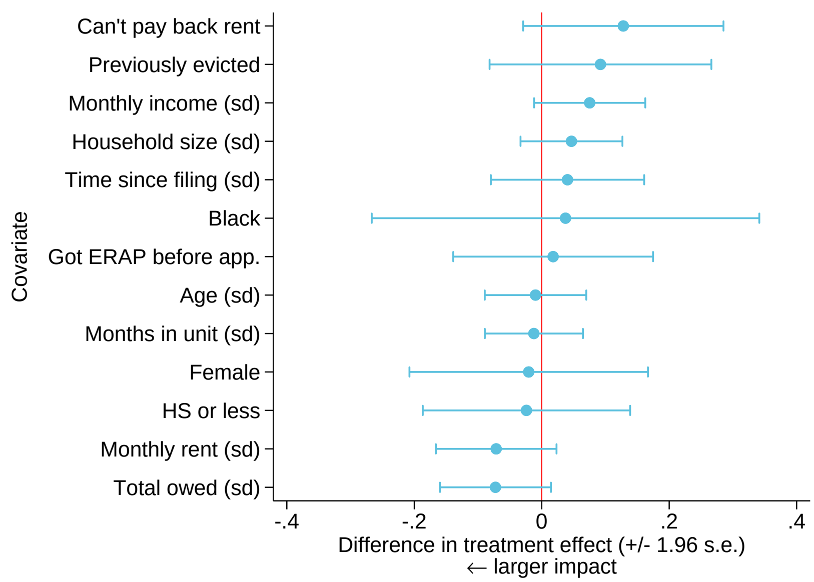
Note: This figure plots the monthly eviction filings and judgments during the listed time period. We list judgments only through June, as they would not have time to resolve by July.

Figure A8: Demographics Do Not Change at ERAP Expiry

(a) Demographics Before Versus After ERAP Expiry



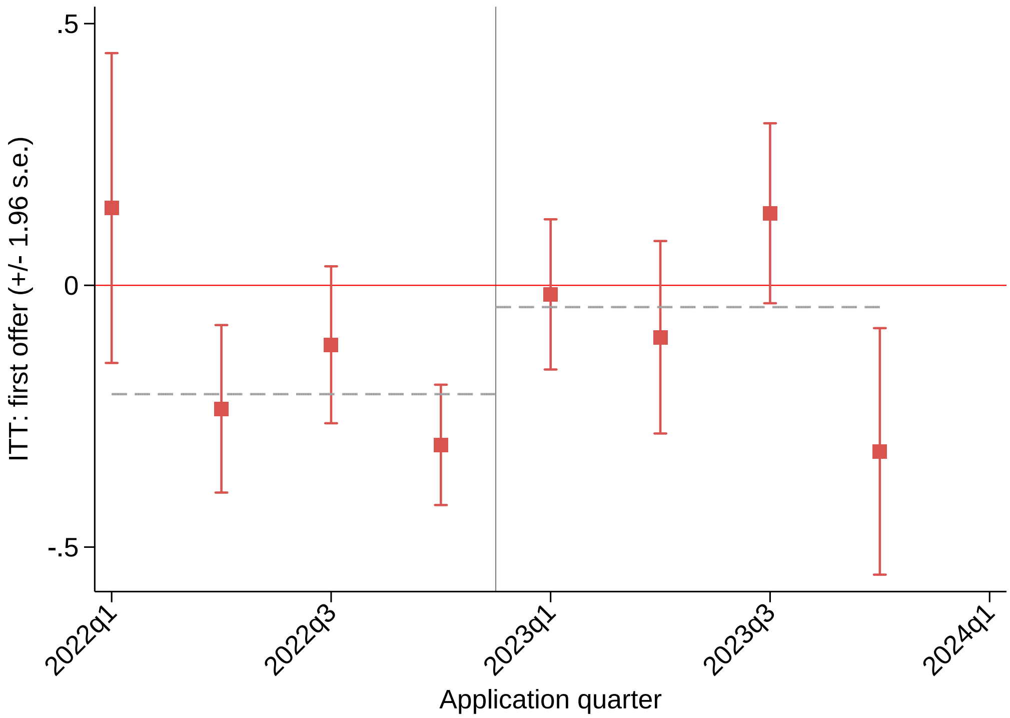
(b) ITT Estimates on Judgments in the Pre-Expiry Period



Note: Panel A presents regression coefficients from Equation (15) (light blue/navy series, where navy indicates significant at the 5% level), as well as a regression-discontinuity version of these estimates (gray/black series, where black indicates significant at the 5% level). Panel B presents a version of Figure 2, estimated only in the pre-expiry period.

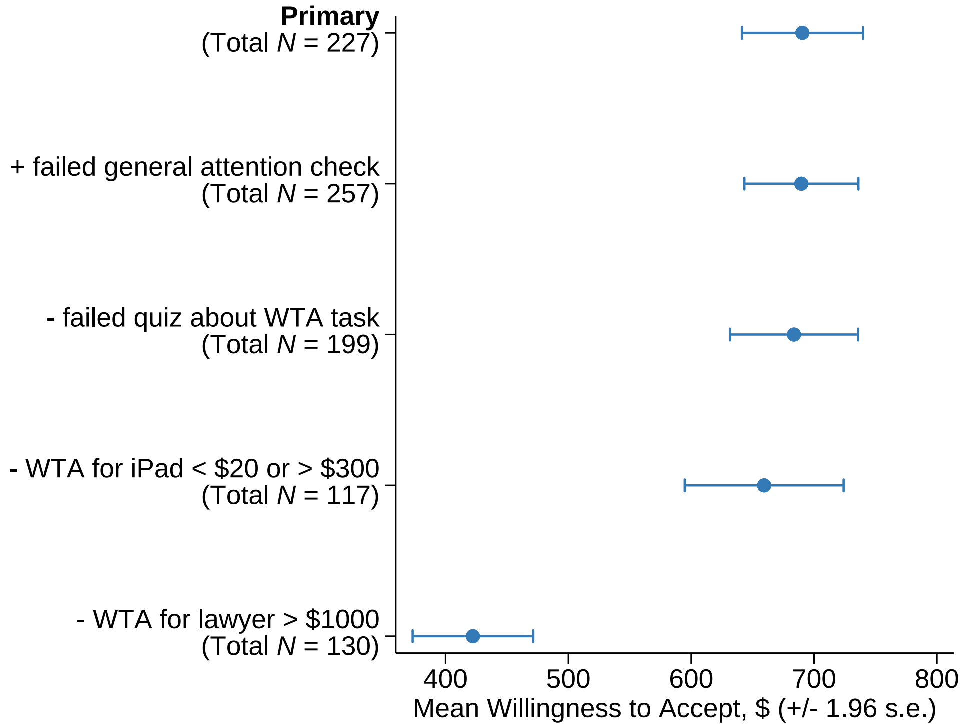
- In the RD, the running variable is calendar time, and the forcing variable is January 1, 2023. We form confidence intervals for the RD using the robust approach in (Calonico et al., 2017), so they may not be symmetric.
- The "predicted TE, all demos series" in bold forms outcome $\tilde{y}_i$ as follows. In the pre-ERAP expiry period, we run one regression which stacks all demographics into vector D_i and interacts D_i with the attorney-offer variable. Then, for each demographic cell d, we can form a predicted interaction coefficient: $\tilde{y}_i := \overline{y}_i^1 - \overline{y}_i^0$, where these represent the average judgment of this cell with and without a lawyer. This difference then represents an estimate of how much we expect this demographic cell to derive additional benefit from having a lawyer. We use $\tilde{y}_i$ as the outcome.
- All specifications control for propensity score fixed effects and reweight. We do not use Lasso procedure to select controls.

Figure A9: Finer Time Variation: Before and After ERAP



Note: This figure plots treatment effects on judgments at 30, 60, and 90 days using the main specification (Table 2). We reweight to adjust for dropping people selected in counseling lotteries but do not use the Lasso procedure to select controls. Applicants in Feburary 2022 and July 2023 are aggregated at the endpoints. The vertical line indicates the beginning of ERAP. The flat effects on judgments at 30 days is consistent with Table 3.

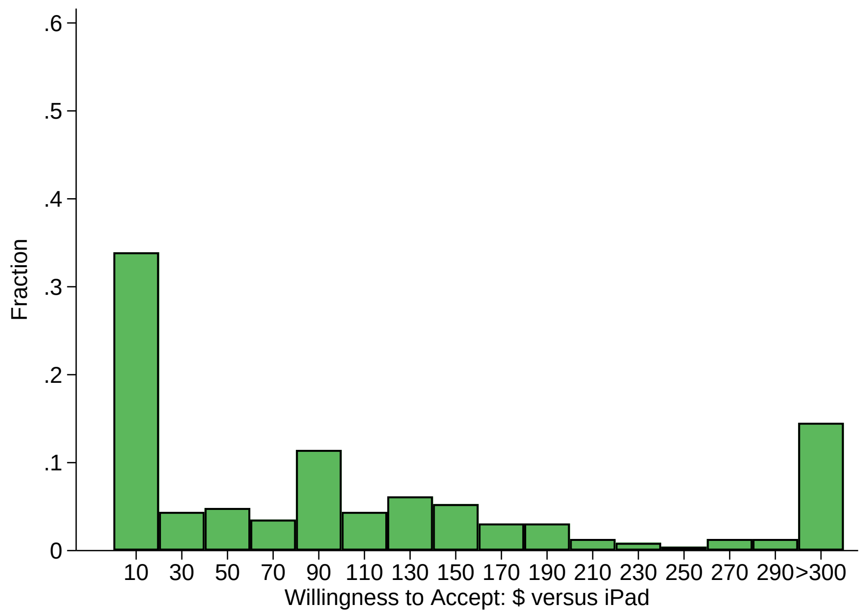
Figure A10: Robustness to Elicitation Error



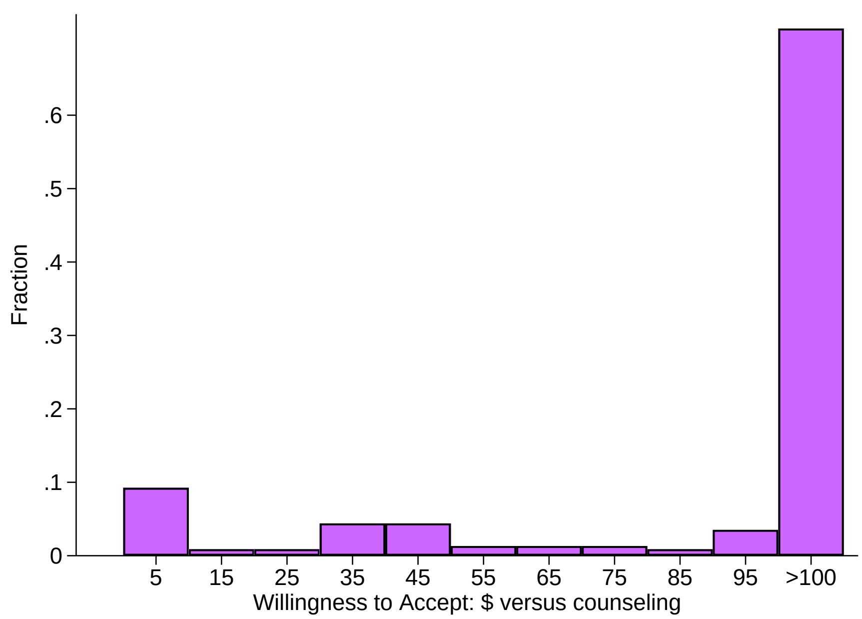
Note: This figure presents tests for the validity of the Willingness to Accept (WTA) elicitation. The first row shows our main estimate. The second row includes participants who failed a general attention check. The third row includes people who failed a confirmation check, prior to the elicitation, about the amount they would be endowed with if they are selected to have their choice implemented ($50 or $500). The fourth row drops people whose WTA for the iPad (Figure A11a) is below $20 or above $300, under the logic that these participants are just clicking buttons as fast as possible. The fifth row drops people whose WTA for a lawyer exceeds $1,000.

Figure A11: Histograms of Willingness to Accept for Other Goods

(a) iPads



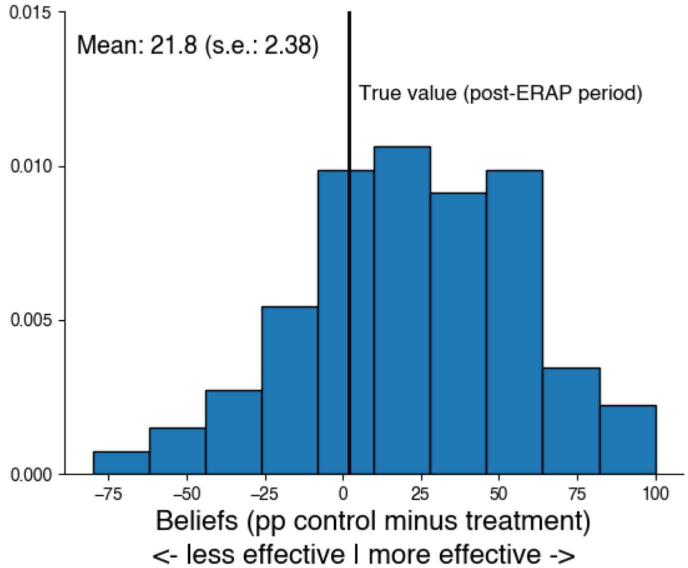
(b) Counselor



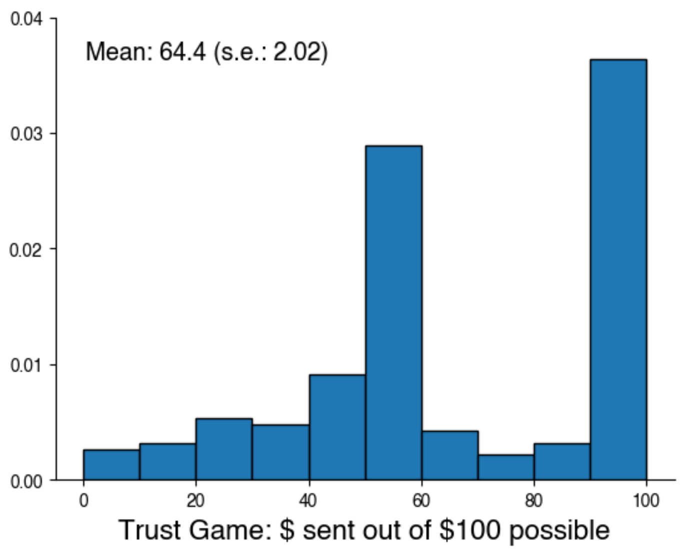
Note: This figure presents the distributions of Willingness to Accept for a reference good, an iPad, and for a counselor (social worker). Appendix G provides details about eliciting these measures.

Figure A12: Histograms of Second-Order Beliefs and Trust Game Responses

(a) Second-Order Beliefs



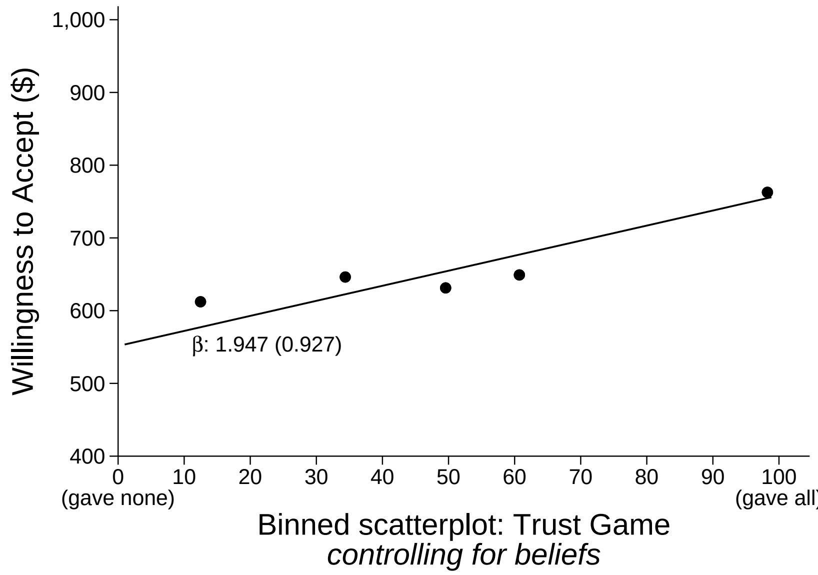
(b) Trust Game Responses



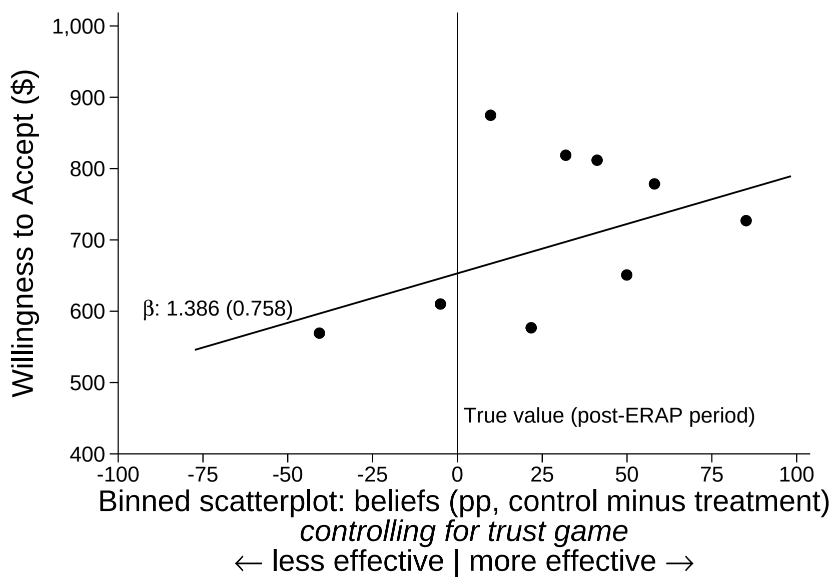
Note: This figure presents the distributions of second-order beliefs about the effectiveness of counsel and responses on the trust game with a lawyer from TWI. The mean belief is $\tilde{2}2$ pp, which is higher than the true value in the post-ERAP period of 2, marked with a vertical line.

Figure A13: Binned Scatterplots: Trust and Beliefs

(a) Trust Game

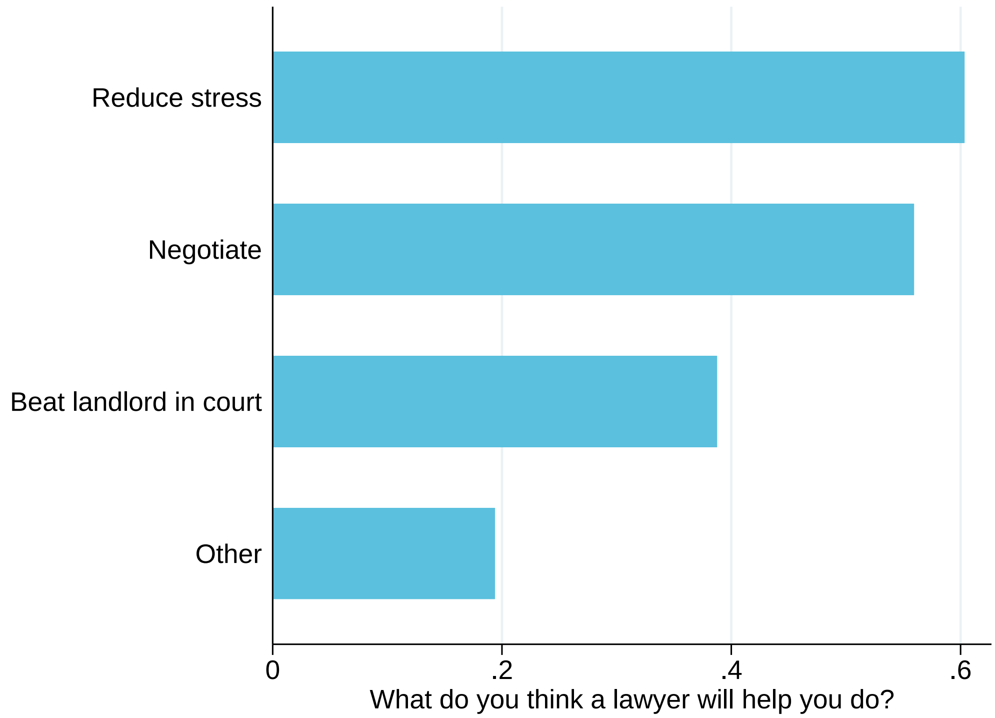


(b) Beliefs



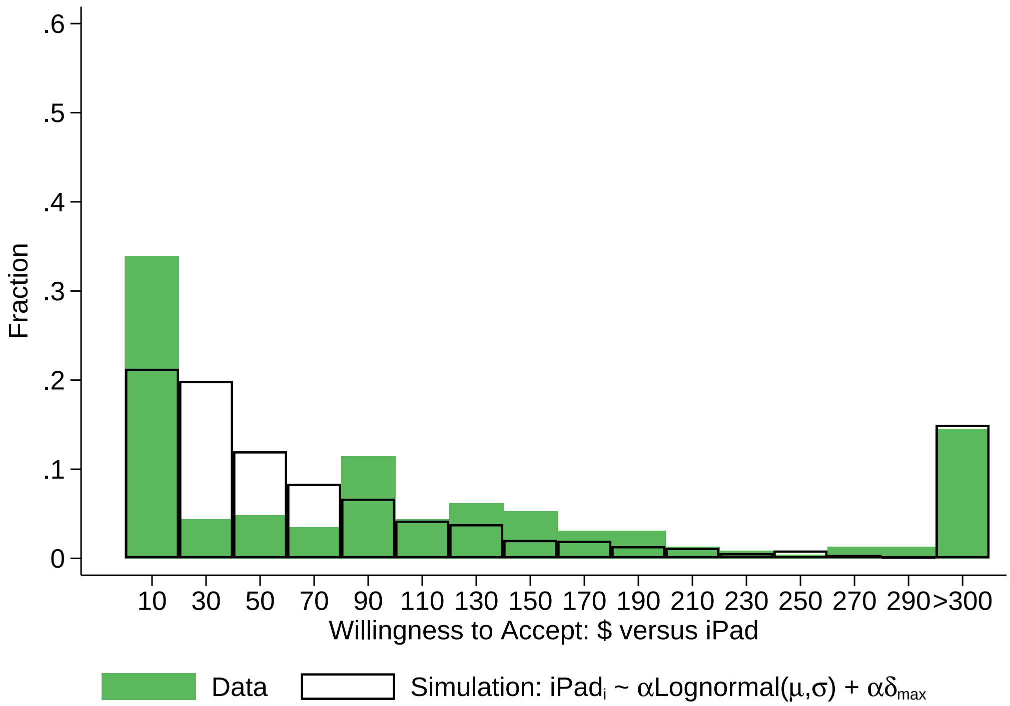
Note: Panels A and B report binned scatterplots of the amount the tenant would accept for a lawyer, versus amounts given in a trust game or beliefs about the second stage impact of receiving a lawyer. The regression coefficients come from multivariate regressions of their WTA on trust and beliefs.

Figure A14: What Do Tenants Want from a Lawyer?



Note: This figure reports what role tenants who apply to TWI expect attorneys to fulfill. Tenants are asked in the baseline survey "What do you think a lawyer will help you do?" They were permitted to choose as many of the options as applied to them. The majority selected that they think attorneys will help them negotiate and reduce stress.

Figure A15: Maximum Likelihood Estimates of Equation (6) versus Data



Note: This figure reports a histogram of 1,000 draws of a lognormal-Dirac mixture distribution at our estimates (μ, σ, α) from Equation (6). The estimates and standard errors themselves are reported in Table A13. Data are presented in green bars. Simulations are in white bars. The inattention parameter α will nearly perfectly match the mass at larger than 300.

B Additional Tables

Table A1: Summary of Past RCTs

	Seron et al. (2001)	Greiner et al. (2012)	Greiner et al. (2013)	Shriver Act: Judicial Council of California (2017) Jarvis et al. (2020)
Panel A. Study basics				
N treated	133	85	76	249
N control	124	99	53	134
Location and date	NYC, 1993-1994	MA, no date reported (2012 or before)	MA, 2010-2011	CA, 2014-2015
Study recruitment	In line at the courthouse	Word of mouth applicants	Targeted recruitment	
Panel B. Outcomes				
Formal outcomes	Judgments, tenant failure to appear, warrant for eviction, number of motions	Notice to quit, possession of unit, money judgments, times judge reviewed	Possession of unit, money judgments, times judge reviewed, jury trial demands	Possession of unit, money judgments, credit protection
Informal outcomes	NA	Time to move	NA	Moves, living situation
N (informal outcomes)	NA	57	NA	66
Panel C. Results and assessment				
ITT on judgments	-0.202 ($p = 0.001$)	0.05 ($p = 0.84$)	-0.58 ($p < 0.01$)	0.01† (p-value unreported)
Control group	Pure control	Received limited representation and referral to same-day representation	Instructional sessions and assistance with forms	Pure control (though control tenants who secured representation are excluded)
Empirical strengths	Compelling design with pure control	Careful empirics: balance tables and s.e.s reported	Careful empirics: balance tables and s.e.'s reported	Large sample, pure control
Empirical weaknesses	No balance tables, took place three decades ago	Control group received significant treatment	Control group received significant treatment	Randomization & sample selection concerns, no s.e.'s reported

Note: This table reports details from past RCTs that provided free attorneys in eviction cases. †: The outcome is court-ordered landlord possession, not judgments. A few additional notes about the Shriver Act evaluations:

- Validity. Both reports compare mean outcomes among people who are offered lawyers and get lawyers against those who are not offered lawyers and do not get lawyers. That is, they drop a selected group of 31 never-takers and 10 always-takers (Judicial Council of California, 2017, p. 112). This sample restriction undoes random assignment.
- Interpretation. 115 treatment and 50 control tenants in Los Angeles County were provided representation only if they passed a screen for the case merit and tenant vulnerability (Judicial Council of California, 2017, p. 57). The authors acknowledge this screen as a limitation of the study (p. 198). On the other hand, Jarvis et al. (2020) report that "no merit screening was conducted during this time" (p. 19). It is unclear which is accurate. If merit screening occurred, then the trial's estimates would reflect the effect of lawyers on cases where lawyers believe they are more likely to win. In that case, the trial would not generally deliver the effect of lawyers for the average tenant.
- Assessment. Both reports find no effects of representation on court-ordered landlord possession (e.g., Table H7 in Jarvis et al., 2020). Despite finding null effects on landlord possession, the reports stress that representation benefited tenants. For instance, Judicial Council of California (2017) writes: "Representation by Shriver counsel helped tenants avoid evictions" (p. 4). It is difficult to assess these claims.

Table A2: Formal Outcomes: Including Counseling Group

	Has lawyer	Judgment within 30 days	Judgment within 60 days	Judgment within 180 days	Amount owed in judgment	Nonsuit within 180 days	Writ within 180 days	Continuance within 180 days	Days left in unit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ITT: first offer	0.609	-0.174	-0.166	-0.140	-713	0.112	-0.118	0.216	26.1
	(0.024)	(0.028)	(0.029)	(0.029)	(169)	(0.028)	(0.028)	(0.028)	(4.5)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
IV: has lawyer		-0.285	-0.271	-0.230	-1,244	0.189	-0.195	0.353	42.8
		(0.045)	(0.047)	(0.047)	(280)	(0.047)	(0.045)	(0.044)	(7.4)
		[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Control mean	0.000	0.470	0.579	0.612	2,335	0.316	0.385	0.490	83.5
N total	1,266	1,266	1,266	1,266	1,266	1,266	1,266	1,266	1,266
N assigned attorneys	310	310	310	310	310	310	310	310	310

Note: This table shows the treatment effects of lawyers on the indicated court outcomes. Parentheses show robust standard errors. Brackets show p-values. Relative to Table 2, it includes the people dropped because they were chosen in the counseling lotteries and thus does not reweight. It still uses the Lasso procedure to select controls.

Table A3: Formal Outcomes, Before and After ERAP Expiry: Including Counseling Group

	Has lawyer (1)	Judgment within 30 days (2)	Judgment within 60 days (3)	Judgment within 180 days (4)	Amount owed in judgment (5)	Nonsuit within 180 days (6)	Writ within 180 days (7)	Continuance within 180 days (8)	Days left in unit (9)
Panel A. With ERAP Available (March–December 2022)									
ITT: first offer	0.576	-0.214	-0.222	-0.204	-985	0.150	-0.198	0.211	35.3
	(0.032)	(0.034)	(0.037)	(0.038)	(195)	(0.038)	(0.034)	(0.036)	(5.9)
	[0.000]	[0.000]	[0.000]	(0.000)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
IV: has lawyer		-0.371	-0.386	-0.353	-1,735	0.261	-0.344	0.365	61.2
		(0.059)	(0.065)	(0.066)	(343)	(0.066)	(0.060)	(0.062)	(10.2)
		[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Control mean	0.000	0.403	0.526	0.569	2,139	0.346	0.370	0.494	91.8
N total	759	759	759	759	759	759	759	759	759
N assigned lawyers	162	162	162	162	162	162	162	162	162
Panel B. Without ERAP Available (January–November 2023)									
ITT: first offer	0.652	-0.118	-0.085	-0.051	-399	0.062	-0.010	0.225	13.6
	(0.036)	(0.046)	(0.045)	(0.044)	(304)	(0.042)	(0.046)	(0.043)	(7.0)
	[0.000]	[0.010]	[0.058]	[0.245]	[0.190]	[0.139]	[0.821]	[0.000]	[0.053]
IV: has lawyer		-0.185	-0.129	-0.080	-632	0.098	-0.018	0.340	20.9
		(0.070)	(0.069)	(0.067)	(459)	(0.064)	(0.069)	(0.063)	(10.8)
		[0.008]	[0.059]	[0.234]	[0.169]	[0.124]	[0.793]	[0.000]	[0.053]
Control mean	0.000	0.580	0.667	0.683	2,657	0.267	0.410	0.483	69.8
N total	507	507	507	507	507	507	507	507	507
N assigned lawyers	148	148	148	148	148	148	148	148	148
Panel C. Difference in Treatment Effects: After Minus Before									
ITT: first offer	0.075	0.096	0.137	0.152	586	-0.089	0.187	0.014	-21.6
	(0.048)	(0.057)	(0.058)	(0.058)	(361)	(0.057)	(0.057)	(0.056)	(9.2)
	[0.114]	[0.093]	[0.019]	[0.009]	[0.104]	[0.118]	[0.001]	[0.805]	[0.019]
IV: has lawyer		0.187	0.256	0.274	1,103	-0.163	0.326	-0.025	-40.4
		(0.092)	(0.094)	(0.094)	(570)	(0.091)	(0.091)	(0.088)	(14.9)
		[0.042]	[0.007]	[0.004]	[0.053]	[0.075]	[0.000]	[0.773]	[0.007]

This table shows the treatment effects of lawyers on the indicated court outcomes. Parentheses show robust standard errors. Brackets show p-values. Relative to Table 3, it includes the people dropped because they were chosen in the counseling lotteries and thus does not reweight. It still uses the Lasso procedure to select controls.

Table A4: Mediation: Instrumental Variables, Congestion Instrument

	First Stage: ERAP (1)	Before Expiry: Judgment (2)	After Expiry: Judgment (3)	Before Expiry: Judgment OLS (4)	Before Expiry: Judgment IV (5)
Congestion instrument:	18.8				
Weekly take-up %, leave-out mean	(5.9)				
1(ERAP) (endogenous variable)				-0.303	-0.457
				(0.039)	(0.298)
1(First Offered)	0.256	-0.220	-0.042	-0.141	-0.101
	(0.039)	(0.040)	(0.045)	(0.040)	(0.087)
Montiel Olea-Pflueger F-statistic	11.4				
N	674	674	466	674	674

Note: This table shows estimates from the Instrumental Variables specification described in Section 4.2. We instrument ERAP_i in Equation (3) with instrument $Z_{w(i)}$, which is the leave-out mean ERAP receipt among people who applied in the same calendar week w. We report the F-statistic from Olea and Pflueger (2013). Columns 2 and 3 show the treatment effect on judgments pre- and post-ERAP expiry. We do not saturate in the propensity score in any of Columns 2–5 for consistency, so these estimates differ from Table 3. We cannot saturate in the propensity score since this would be nearly collinear with $Z_{w(i)}$. Column 4 shows the OLS mediation analysis. Column 5 shows the instrumented version. All specifications reweight to adjust for dropping the people assigned to counselors.

Table A5: Mediation: Instrumental Variables, August Instrument

	First Stage: ERAP (1)	Before Expiry: Judgment (2)	After Expiry: Judgment (3)	Before Expiry: Judgment OLS (4)	Before Expiry: Judgment IV (5)
1(Post-August) (instrument)	-0.263				
	(0.045)				
1(ERAP) (endogenous variable)				-0.303	-0.376
				(0.039)	(0.170)
1(First Offered)	0.242	-0.220	-0.042	-0.141	-0.122
	(0.038)	(0.040)	(0.045)	(0.040)	(0.059)
Montiel Olea-Pflueger *F*-statistic	38.3				
N	674	674	466	674	674

Note: This table shows estimates from the Instrumental Variables specification described in Section 4.2. It is identical to Table A4, except we define Z_i to be an indicator that is 1 if i applied to RTC after August 1, 2022.

Table A6: Formal Outcomes, Robustness: No Controls

	Has lawyer (1)	Judgment within 30 days (2)	Judgment within 60 days (3)	Judgment within 180 days (4)	Amount owed in judgment (5)	Nonsuit within 180 days (6)	Writ within 180 days (7)	Continuance within 180 days (8)	Days left in unit (9)
ITT: first offer	0.604	-0.176	-0.155	-0.136	-849	0.105	-0.119	0.213	25.4
	(0.024)	(0.029)	(0.030)	(0.030)	(186)	(0.029)	(0.029)	(0.029)	(4.7)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
IV: has lawyer		-0.292	-0.257	-0.225	-1,406	0.173	-0.196	0.353	42.1
		(0.048)	(0.050)	(0.050)	(310)	(0.049)	(0.048)	(0.047)	(7.9)
		[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Control mean	0.000	0.472	0.577	0.610	2,311	0.320	0.383	0.486	83.9
N total	1,140	1,140	1,140	1,140	1,140	1,140	1,140	1,140	1,140
N assigned attorneys	307	307	307	307	307	307	307	307	307

Note: This table shows the treatment effects of lawyers on the indicated court outcomes. Parentheses show robust standard errors. Brackets show *p*-values. Relative to Table 2, it does not use Lasso to select controls and exclusively saturates in the propensity score.

Table A7: Formal Outcomes, Before and After ERAP Expiry: No Controls

	Has lawyer (1)	Judgment within 30 days (2)	Judgment within 60 days (3)	Judgment within 180 days (4)	Amount owed in judgment (5)	Nonsuit within 180 days (6)	Writ within 180 days (7)	Continuance within 180 days (8)	Days left in unit (9)
Panel A. With ERAP Available (March–December 2022)									
ITT: first offer	0.573	-0.223	-0.225	-0.208	-1,144	0.147	-0.209	0.188	35.9
	(0.032)	(0.036)	(0.038)	(0.040)	(205)	(0.040)	(0.036)	(0.038)	(6.1)
	[0.000]	[0.000]	[0.000]	(0.000)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
IV: has lawyer		-0.390	-0.392	-0.363	-1,997	0.256	-0.365	0.328	62.7
		(0.062)	(0.068)	(0.069)	(365)	(0.069)	(0.063)	(0.066)	(10.8)
		[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Control mean	0.000	0.408	0.528	0.570	2,110	0.352	0.374	0.501	91.7
N total	674	674	674	674	674	674	674	674	674
N assigned lawyers	162	162	162	162	162	162	162	162	162
Panel B. Without ERAP Available (January–November 2023)									
ITT: first offer	0.645	-0.115	-0.064	-0.042	-477	0.050	0.002	0.251	11.7
	(0.036)	(0.047)	(0.046)	(0.045)	(339)	(0.043)	(0.047)	(0.044)	(7.2)
	[0.000]	[0.015]	[0.161]	[0.356]	[0.159]	[0.244]	[0.966]	[0.000]	[0.107]
IV: has lawyer		-0.178	-0.100	-0.064	-740	0.078	0.003	0.389	18.1
		(0.072)	(0.071)	(0.069)	(521)	(0.067)	(0.072)	(0.065)	(11.2)
		[0.014]	[0.159]	[0.353]	[0.156]	[0.242]	[0.965]	[0.000]	[0.105]
Control mean	0.000	0.573	0.653	0.672	2,625	0.271	0.397	0.462	71.6
N total	466	466	466	466	466	466	466	466	466
N assigned lawyers	145	145	145	145	145	145	145	145	145
Panel C. Difference in Treatment Effects: After Minus Before									
ITT: first offer	0.072	0.109	0.160	0.166	667	-0.096	0.211	0.063	-24.3
	(0.048)	(0.059)	(0.060)	(0.060)	(396)	(0.059)	(0.059)	(0.059)	(9.5)
	[0.137]	[0.065]	[0.008]	[0.006]	[0.092]	[0.100]	[0.000]	[0.284]	[0.011]
IV: has lawyer		0.212	0.292	0.298	1,257	-0.178	0.368	0.061	-44.6
		(0.095)	(0.098)	(0.098)	(636)	(0.096)	(0.096)	(0.092)	(15.5)
		[0.026]	[0.003]	[0.002]	[0.048]	[0.063]	[0.000]	[0.510]	[0.004]

This table shows the treatment effects of lawyers on the indicated court outcomes. Parentheses show robust standard errors. Brackets show p-values. Relative to Table 2, it does not use Lasso to select controls and exclusively saturates in the propensity score.

Table A8: Treatment Effects of Lawyers on Informal Outcomes

	Has attorney (1)	Judgment within 60 days (2)	Formal eviction (any) (3)	Informal eviction (4)	Move (5)	Tried to bargain (6)	No out-of-pocket payments (7)	Fraction out-of-pocket ÷ landlord ask (8)	Fraction out-of-pocket ÷ total arrears (9)
ITT: first offer	0.604	-0.188	-0.171	0.033	0.002	-0.040	0.029	-0.062	-0.090
	(0.038)	(0.048)	(0.048)	(0.037)	(0.049)	(0.050)	(0.042)	(0.034)	(0.053)
	[0.000]	[0.000]	[0.000]	[0.374]	[0.967]	[0.429]	[0.501]	[0.071]	[0.092]
IV: has lawyer		-0.312	-0.283	0.055	0.003	-0.066	0.047	-0.101	-0.145
		(0.079)	(0.080)	(0.061)	(0.080)	(0.082)	(0.069)	(0.054)	(0.085)
		[0.000]	[0.000]	[0.366]	[0.967]	[0.420]	[0.492]	[0.062]	[0.085]
Control mean	0.000	0.540	0.574	0.149	0.549	0.549	0.736	0.177	0.209
N total	439	439	439	439	439	439	439	439	439
N assigned attorneys	139	139	139	139	139	139	139	139	139

Note: This table shows the treatment effects of lawyers on the indicated court outcomes. Parentheses show robust standard errors. Brackets show p-values. The specification saturates in the propensity score, reweights to adjust for excluding the counseling lotteries, and uses Lasso (Belloni et al., 2014) to select controls from a set of demographics listed in Section 3.1. Outcomes are measured in endline surveys (Section 5).

Table A9: Treatment Effects on Informal Outcomes, Reweighted for Attrition (Demographics)

	Has attorney (1)	Judgment within 60 days (2)	Formal eviction (any) (3)	Informal eviction (4)	Move (5)	Tried to bargain (6)	No out-of-pocket payments (7)	Fraction out-of-pocket ÷ landlord ask (8)	Fraction out-of-pocket ÷ total arrears (9)
ITT: first offer	0.601	-0.168	-0.150	0.038	0.014	-0.049	0.020	-0.054	-0.090
	(0.040)	(0.049)	(0.051)	(0.041)	(0.051)	(0.052)	(0.045)	(0.033)	(0.057)
	[0.000]	[0.001]	[0.003]	[0.353]	[0.782]	[0.354]	[0.663]	[0.108]	[0.115]
IV: has lawyer		-0.279	-0.250	0.063	0.023	-0.081	0.033	-0.087	-0.150
		(0.083)	(0.084)	(0.066)	(0.083)	(0.086)	(0.073)	(0.052)	(0.093)
		[0.001]	[0.003]	[0.345]	[0.779]	[0.345]	[0.657]	[0.097]	[0.108]
Control mean	0.000	0.540	0.574	0.149	0.549	0.549	0.736	0.177	0.209
N total	439	439	439	439	439	439	439	439	439
N assigned attorneys	139	139	139	139	139	139	139	139	139

Note: Parentheses show robust standard errors. Brackets show p-values. Relative to Table A8, this table reweights such that endline participants match the main sample of participants' demographics, using the Hainmueller (2012) procedure. Columns (2)–(3) come from administrative data and are intended to assist with comparing to Table 2. Column (2) shows the treatment effect on judgments at 60 days within this sample. Column (3) shows the treatment effect on having any judgment within this sample. Outcomes in Columns (4)–(9) are measured in endline surveys (Section 5).

Table A10: Treatment Effects on Informal Outcomes, Reweighted for Attrition (Admin)

	Has attorney (1)	Judgment within 60 days (2)	Formal eviction (any) (3)	Informal eviction (4)	Move (5)	Tried to bargain (6)	No out-of-pocket payments (7)	Fraction out-of-pocket ÷ landlord ask (8)	Fraction out-of-pocket ÷ total arrears (9)
ITT: first offer	0.600	-0.191	-0.171	0.034	0.005	-0.042	0.027	-0.063	-0.093
	(0.039)	(0.048)	(0.048)	(0.034)	(0.049)	(0.051)	(0.043)	(0.035)	(0.057)
	[0.000]	[0.000]	[0.000]	[0.316]	[0.926]	[0.414]	[0.533]	[0.071]	[0.107]
IV: has lawyer		-0.319	-0.285	0.056	0.008	-0.069	0.045	-0.102	-0.152
		(0.081)	(0.080)	(0.055)	(0.081)	(0.083)	(0.071)	(0.055)	(0.092)
		[0.000]	[0.000]	[0.309]	[0.925]	[0.405]	[0.525]	[0.062]	[0.099]
Control mean	0.000	0.540	0.574	0.149	0.549	0.549	0.736	0.177	0.209
N total	439	439	439	439	439	439	439	439	439
N assigned attorneys	139	139	139	139	139	139	139	139	139

Note: Parentheses show robust standard errors. Brackets show *p*-values. Relative to Table A8, this table reweights such that endline participants such that endline participants match the main sample of participants' administrative outcomes, using the Hainmueller (2012) procedure. Outcomes are measured in endline surveys (Section 5).

Table A11: Demographics of Survey Sample and Additional Attrition Tests

	Sample (1)	Endline (2)	Endline − sample (3)	Endline: Treatment − Control (4)
Demographics:				
Age	33.9	34.1	0.3	-1.2
			(0.6)	(0.9)
			[0.570]	[0.176]
Black	0.94	0.94	-0.00	-0.04
			(0.01)	(0.02)
			[0.951]	[0.085]
Female	0.80	0.88	0.12	-0.03
			(0.02)	(0.03)
			[0.000]	[0.372]
Household size	2.7	2.8	0.2	0.4
			(0.1)	(0.2)
			[0.008]	[0.012]
HS or less	0.65	0.61	-0.06	0.02
			(0.03)	(0.05)
			[0.047]	[0.627]
Single	0.90	0.88	-0.02	-0.03
			(0.02)	(0.03)
			[0.203]	[0.407]
Economic status:				
Monthly income	1,362	1,368	10	-9
			(80)	(120)
			[0.899]	[0.941]
Monthly rent	966	949	-27	13
			(28)	(40)
			[0.330]	[0.743]
Housing security:				
Applied after ERAP expiry	0.40	0.46	0.09	-0.02
			(0.03)	(0.03)
			[0.003]	[0.605]
Ever evicted before application	0.33	0.35	0.04	-0.01
			(0.03)	(0.05)
			[0.204]	[0.871]
Months in unit	24.7	27.6	4.7	-1.2
			(1.5)	(2.7)
			[0.002]	[0.660]
Previously took ERAP	0.35	0.40	0.09	-0.04
			(0.03)	(0.05)
			[0.003]	[0.434]
Total owed at application	2,968	2,835	-214	-237
			(156)	(250)
			[0.170]	[0.344]
Program outcomes:				
Selected in initial lottery	0.35	0.39	0.07	1.00
			(0.03)	(0.00)
			[0.018]	[0.000]
Has attorney	0.24	0.30	0.08	0.60
			(0.03)	(0.04)
			[0.002]	[0.000]
Judgment	0.58	0.51	-0.10	-0.18
			(0.03)	(0.05)
			[0.001]	[0.000]
N	435	435		
Joint p-value (non-program outcomes)			0.000	0.044

Note: This table shows the demographic composition of the main and endline samples. The third column shows a joint test of demographic balance. The fourth column shows the difference betweeen treatment and control within the endline sample.

Table A12: Fiscal Externality for MVPF During ERAP

	Treatment effect of eviction (1)	Implied effect on gov budget (2)	Budget effect scaled by IV estimate (3)
Effects mediated by judgments			
Emergency shelter use	3.4 p.p.	$103.65	-$37.63
	(1.7)		
Hospital visits	0.188 visits	$118.96	-$43.18
	(0.094)		
Earnings (over two years)	-$936	$120.74	-$43.83
Other effects			
Writs	50%	$67.5	-$15.59

Note: All dollar amounts are converted to 2022 dollars. The positive values in column (2) reflect that eviction increase government costs. The negative values in column (3) reflect that counsel decreases eviction rates, resulting in spending reductions. Estimates of the effect of eviction on shelter use, hospital visits, and earnings come from Collinson et al. (2024b). Costs of shelter use come from Hao et al. (2022), who report that a median emergency shelter visit is one month and costs $2,100 in 2006 dollars. Moore and Liang (2020) reports an average emergency department visit costs $530 in 2017 dollars. Reliable government costs for non-emergency visits were unavailable; Collinson et al. (2024b) found similar size (but statistically insignificant) effects on emergency hospital visits. We follow the literature and use a 12.9% tax and transfer rate for low income populations (Hendren and Sprung-Keyser, 2020). We find that about half of judgments result in writs, which are executed by detectives. King County estimates that fulfilling a writ takes no less than $135 of detective time. We scale effects by the following IV estimates on the effects of counsel over 90 days during the ERAP period: reduces the rate of eviction judgments by 36.3 pp, reduces the rate of writs by 23.1 pp

Table A13: Estimates of Willingness to Accept a Lawyer: Inattention Robustness

Scenario	**Inattention** α (1)	**Location** μ (2)	**Scale** σ (3)	**Mean WTA Lawyer** (if max WTP = $1,050) (4)	**Mean WTA Lawyer** (interval regression) (5)
1. Benchmark	·	·	·	691	845
				(25)	(53)
2. Match iPad max	.105	3.835	1.1	650	750
	(.033)	(.093)	(.191)	(27)	(49)
3. Match iPad max and min	.469	4.662	.613	662	720
	(.169)	(.774)	(.17)	(27)	(38)
4. Match iPad min	.33	4.946	.882	517	515
	(.07)	(.314)	(.089)	(28)	(37)

This table reports estimates of parameters (α, μ, σ) from Equation (6), estimated via Maximum Likelihood.

- Columns 4 and 5 show the Willingness to Accept a lawyer under two assumptions. In Column 4 we assume the maximum WTA is $1,050, just larger than the censoring point of $1,000. In Column 5, we fit a generalized Tobit regression ("interval regression," Cameron and Trivedi, 2010) and take the mean.
- Row 1 presents the benchmark specification, whose distribution is plotted in Figure 6. Row 2 estimates Equation (6). In Columns 4 and 5, we delete share α who report the maximum lawyer WTA exceeding $1,000, presuming they are inattentive, and re-estimate the means. In Row 3, we adjust the point-mass distribution at the max to be a point-mass at either the max or the min, with equal probability:

$$\text{iPad}_i \sim (1-\alpha)\text{Lognormal}(\mu, \sigma) + \frac{\alpha}{2}\delta_{\min} + \frac{\alpha}{2}\delta_{\max}. \tag{9}$$

  For the corresponding columns, we delete share $\alpha/2$ of those who report the minimum or maximum values for the lawyer WTA distribution, respectively. In Row 4 we return to Row 2 and adjust the point-mass distribution to be a point mass at the minimum, rather than the maximum.
- Standard errors in Columns 4 and 5 only account for sampling variation and not the two-step estimation process.

C Motivating Framework

Landlord–Tenant pairs i engage in asymmetric Nash Bargaining. Lawyers can change landlords' outside options, perhaps by increasing court costs. Lawyers can change tenants' outside options, perhaps by assisting with acquiring emergency rental assistance. Lawyers can also change bargaining power. We assume utility is linear in bargaining offers $b_i \in \mathbb{R}$ which are positive when the tenant pays the landlord. Bargaining is only possible when it renders both parties better off than their outside option.

The Nash Bargaining solution solves:

$$b_i^* = \arg\max_{b_i \in \mathcal{X}} \left(-b_i - \mu_{Ti} - \alpha_{Ti} A_i\right)^{\beta_i + \delta_i A_i} \left(b_i - \mu_{Li} - \alpha_{Li} A_i\right)^{(1-\beta_i) - \delta_i A_i}, \tag{10}$$

where A_i is an indicator for having an attorney; μ_{Ti} and μ_{Li} are the tenant and landlords' outside options without attorneys; α_{Ti} and α_{Li} are the changes in tenants' and landlords' outside options when the tenant has an attorney; β_i is the tenants' bargaining power; and δ_i is the change in bargaining power with an attorney. Let each parameter $\theta_i \in \Theta_i := \{\mu_{Ti}, \mu_{Li}, \alpha_{Ti}, \alpha_{Li}, \beta_i, \delta_i\}$ have CDF F_θ.

In this framework, bargaining is possible if and only if:

$$\mu_{Ti} + \alpha_{Ti} A_i + \mu_{Li} + \alpha_{Li} A_i \leq 0, \tag{11}$$

a modified version of which also appears in Rafkin and Soltas (2024). Equation (11) is a Coasean benchmark, as it says that in a frictionless environment, the parties are able to bargain to avoid court as long as court costs are positive. One way of interpreting Equation (11) is that bargaining occurs if and only if it is "efficient," in the sense that joint surplus from bargaining is less than joint surplus from going to court.

Meanwhile, if bargaining is possible, the Nash Bargaining solution to Equation (10) is:

$$b_i^* = \left(\beta_i + \delta_i A_i\right)\left(\mu_{Li} - \alpha_{Li} A_i\right) - \left((1-\beta_i) - \delta_i A_i\right)\left(\mu_{Ti} - \alpha_{Ti} A_i\right). \tag{12}$$

Inspecting Equations (11) and (12) yields several insights, which explain how we structure the remainder of the paper.

First, this framework shows why data on formal evictions and attorneys alone can provide useful information about mechanisms. Let E_i be an indicator that is 1 if Equation (11) is satisfied, and assume attorney assignment is random ($A_i \perp \Theta_i$). A corollary to Equation (11) is that lawyers affect court eviction rates only if they affect outside options:

$$\mathbb{E}[E_i | A_i = 1] \neq \mathbb{E}[E_i | A_i = 0] \implies \alpha_{Li} + \alpha_{Ti} \neq 0 \text{ for some } i. \tag{13}$$

Intuitively, Equation (11) shows that the probability of bargaining depends only on outside options and not bargaining power. Bargaining power exclusively affects the division of surplus, conditional on bargaining. Consequently, the administrative data on E_i and A_i alone are sufficient to test the hypothesis that lawyers do not affect outside options. If lawyers affect court outcomes on average, that is sufficient to reject that lawyers do not affect real bargaining constraints.

Second, this framework shows a limitation of having data only on E_i and A_i and no additional variation. Lawyers can affect court eviction only by affecting *either* landlord or tenant outside options. Equation (11) shows that if one only observes E_i and A_i, then without additional variation or structure, α_{Ti} and α_{Li} are not separately identified. Yet whether lawyers operate by hassling landlords or improving outside options for tenants is potentially important for welfare.

Happily, we can leverage the variation from ERAP's expiry. If the environment is truly identical before and after ERAP expires, then ERAP changes only tenants' outside options (Section 3). The difference between ERAP's effects before and after it concludes provides information about α_{Ti}. For instance, consider the parameterization:

$$\alpha_{Ti} = \tilde{\alpha}_{Ti}\text{ERAP}_i \tag{14}$$

where ERAP_i is an indicator that is 1 if ERAP is available. In this case, attorneys have no effect on tenant outside options unless they can obtain assistance via ERAP. If attorneys are more effective with ERAP, then they affect tenant outside options.

Third, the framework shows why data on b_i^* are useful. Our endline surveys capture informal bargaining outcomes. We use these outcomes to form a measure of b_i^*. Attorneys' effects on bargaining power δ_i only enter b_i^*. Thus, the effects of attorneys on b_i^* provides information about δ_i alone. However, if lawyers affect outside options, then additional structure is required to isolate δ_i from lawyers' other effects.

Finally, whether attorneys' effects come from changing outside options or bargaining power has important normative implications. Changing outside options may entail (i) direct externalities (e.g., because making court onerous for landlords requires filing socially costly motions that waste public resources), as well as (ii) indirect externalities via court evictions (e.g., because court evictions cause homelessness). The welfare effects of lawyers, in that case, requires adding up these externalities. On the other hand, if $\alpha_{Ti} = \alpha_{Li} = 0$ and $\delta_i > 0$, then lawyers would not change court eviction rates but would change bargained settlements if bargaining occurs. In that case, attorneys affect welfare only because they redistribute from landlords to tenants.

D Experiment Details

D.1 Lotteries

We implemented three different types of first lotteries. From March–September 2022, we assigned treatment based on discrete lotteries that took place with groups of 10–20 tenants every week. Assignment was determined with a random-number generator in these lotteries. From September 2022–February 2023, we randomized tenants into treatment or control live based on the number of seconds at which the tenant submitted their application (e.g., 10:01:01 versus 10:01:52 mapped to different selections). The exact assignment rule was not observed by or told to screeners or attorneys. Moreover, tenants did not receive assistance applying. It is valid as long as the seconds at which a tenant applied is not correlated with potential outcomes. We used this method because of logistical constraints. Beginning in February 2023, we assigned treatment using a live random-number generator, eschewing the seconds rule, since we found a logistical work-around. In addition to the first lotteries, we also conducted waitlist lotteries, as detailed in Section 3.1. These lotteries always employed a random-number generator.

D.2 Notification email

Tenants who are selected for the control group receive an email notification. The email includes some basic information about tenant rights and is reproduced below:

> Dear Tenant Name,
>
> Thank you for applying for free legal assistance via Home901.org. All available attorneys are currently assisting tenants, and we unfortunately do not have the capacity to provide you with legal representation at this time. This does not mean you were denied representation—we may be able to assist you in the future if an attorney or an eviction counselor becomes available. If an attorney or a counselor becomes available at a later date, we will reach out to you via email and text.
>
> Below are other resources available to tenants in Shelby County. General renters rights information can be found from the [Memphis Public Interest Law Center here](https://mpilc.org/renters-rights-qa/). Their hotline is 1-833-7RENTER (1-833-773-6837).
>
> ## What are my rights as a renter?
>
> You have the right to remain in your home until ten days following a judgement. This means your landlord legally cannot take action to remove you or your belongings from the property until they file a court case, go to court, obtain a judgement against you, and receive authorization from the judge, and provide you ten days to relocate.

How do I access information about my court date?

You can search for your name in the [public court record here (https://gscivildata.shelbycountytn.gov/pls/gnweb/ck_public_qry_cpty.cp_personcase_setup_idx) to access the most updated court information. If you court record currently says your case will be heard in 2023, this means your case will be reset. You will receive a reset notice with a new court date in the mail.

What other resources are available?

The following resources are available for tenants in Shelby County:

* The [Community Services Agency Rent and Mortgage Program](https://shelbycountycsa.org/services/rent-mortgage) offers rent and mortgage assistance. The program may be closed currently, but will likely continue in the future. Please check back on their website for further information.

* If you are seeking other County resources, please contact the Shelby County Community Services Agency at 901-222-4200.

* If you need utility assistance, you can apply [with MIFA here](https://www.mifa.org/applyonline).

* If you need emergency shelter assistance, call the 24-hour Homeless Hotline at (901) 529-4545 or visit [CAFTH's online resources](https://www.cafth.org/get-help/).

* If you are in need of general legal assistance please contact either: [Memphis Area Legal Services (MALS)](https://malsi.org/) - (901) 523-8822 - 200 Jefferson Avenue Suite 1075 Memphis, TN 38103 - or [Community Legal Center (CLC)](https://clcmemphis.org/) - (901) 543-3395 - 243 Adams Avenue, Memphis, TN 38103

If you believe you have received this email in error or have any other questions, please email evictionhelp@npimemphis.org.

Sincerely,

Eviction Legal Support
The Works, Inc.

D.3 Data Appendix

Merge Onto Court Data. We merge all but 20 applicants onto court records. These applicants have errors in court filing id numbers yet made it through tenant screenings and could not be reliably identified in court data. Of the 20, 14 receive attorney offers and 7 receive attorneys (both higher rates than the matched group).

Counseling Lotteries. More than 100 tenants were selected to receive eviction counseling. Eviction counselors were either law students in a legal clinic at University of Memphis Law School or part-time social workers. We exclude these from consideration because most tenants were contacted for eviction counseling long after their eviction case was concluded. Mechanically, these counselors could not affect eviction outcomes.

Tenants who received full legal assistance were not typically entered in lotteries. In three cases, due to processing errors, tenants got both legal assistance and counseling. We drop these three tenants.

E Empirical Appendix

E.1 Other potential mechanisms

Bundling. The main concern one might have about attributing the pre- and post-expiry impacts to ERAP is that other factors could have changed on January 1, 2023. Our mediation analysis mitigates this concern, since we test whether controlling for observed ERAP receipt in the pre-expiry period closes the pre/post gap. Still, one may be concerned that our main pre/post comparisons bundle ERAP expiry with other changes to the demographic composition of the sample or policy environment. In Below, we present results with finer time variation, disaggregating the differential effects on judgments by quarter. We test for changes in the demographic composition of RTC applicants or the eviction environment, using differences and regression discontinuity designs around January 1, 2023. Overall, our evidence suggests ERAP explains most of the pre- versus post-expiry gap.

Attorney Composition Or Tactics. One may worry that either the strategies or composition of the attorneys changed over the course of the study, and that these shifts drive the changes in effects we attribute to ERAP's expiration. However, the rate of continuance filings is one of the few outcomes that does not change through ERAP's expiration. Attorneys caused an approximately 35 p.p. increase in probability of filing a continuance within 180 days in *both* periods (p-value of difference: 0.603). An alternate explanation for shifts in the average effectiveness of attorneys is a change in the underlying composition of which attorneys represent clients. If anything, we find the opposite: TWI attorneys indeed appear to be directionally (though not statistically) more effective than low-bono attorneys, however, if anything they represent a *larger* share of tenants since January 2023.

E.2 Policy Bundling

Finer Time Variation. One may question whether the change in the program's effectiveness really coincides with ERAP expiry. If results attenuate sharply after ERAP expiry in January 2023, that would be evidence that ERAP is decisive. Indeed, the effects of attorneys on judgments at 30, 60, and 90 days by calendar quarter of application are consistent with a change at the date of ERAP expiry (Figure A9). Notably, the treatment effects of attorneys are largest in magnitude just before ERAP expiry, and then attenuate immediately after.

Demographics. By contrast, demographics do not change abruptly when ERAP concludes and typically exhibit small trends. We test for differences around January 1, 2023 in two ways. First, we examine demographics of applicants do not change in aggregate, estimating:

$$y_i = \text{After}_i\beta + \varepsilon_i \tag{15}$$

where After_i is an indicator for applying after January 1, 2023, and y_i is a demographic variable. Second, we estimate flexible RD specifications, where the date January 1, 2023 is the forcing variable and days are the running variable. We contrast these results to the estimate when y_i is an indicator for received an ERAP payment.

Reassuringly, we find the largest coefficients when the outcome is ERAP payment, rather than other demographic variables (Figure A8).

However, we do detect several variables that significantly change around ERAP expiry in the simple before-after specification (Equation 15), for instance, monthly income. These variables are only concerning if they correlate with the impact of an attorney offer on judgments. That is, even if the *levels* of eviction risk change (because monthly incomes differ), the differences in eviction risk must also change.

To quantify this concern, we estimate how important these demographics are for the treatment effects, based on their interaction coefficients in the pre-period. We aggregate demographics into vector D_i and include all the demographics with treatment in one fully interacted regression, using only the pre-period data:

$$y_i = \beta_1 \text{WinsFirstLottery}_i + \beta_2 \left(\text{WinsFirstLottery}_i \times D_i\right) + X_i\gamma + \varepsilon_i, \tag{16}$$

noting that vector D_i is included in controls X_i. For each demographic cell d, we form the difference in predicted eviction risk with and without an attorney offer: $\tilde{y}_i := \overline{y}^1_{d(i)} - \overline{y}^0_{d(i)}$, where these values are mean values of judgments with and without the offer). Then, we can regress $\tilde{y}_i$ as an outcome in Equation (15) and the corresponding RD.

Intuitively, this test tells us how much we expect the treatment effect to change after ERAP expiry, given observed changes in applicants' demographics and those demographics' heterogeneous treatment effect coefficients.

The result, plotted in bold in Figure A8, suggests we can rule out even minute changes in treatment effect coefficients. The reason is that monthly rents are correlated with improved (more negative) ITTs, whereas monthly incomes are correlated with worsened (more positive) ITTs in the pre-expiry period (Figure A8B). Thus the differences in those demographics cancel out.

In Panel A, we also detect a change in whether applicants had received ERAP before applying. This is natural, as more people had the chance to receive ERAP over time. However, it suggests one interpretation of the impact of ERAP expiry is that, with expiry, more people who apply received ERAP beforehand and do not have access to the program. Overall, given the tight null from predicting treatment effect estimates (bold series), we are not overly concerned.

Relatedly, we do not see important changes in the aggregate eviction filing or judgment numbers around ERAP expiry (Figure A7).

E.3 Demographic Reweighting

We also reweight the period after ERAP expiry to have similar demographics as the pre-expiry period (Hainmueller, 2012). Differences between the treatment and control *grow* modestly. This evidence suggests that any differences in demographics after ERAP expiry would only make the attorneys *more* effective; thus, if anything, the differences in Table 3, Panel C, are conservative (holding before/after demographics fixed). This evidence strongly implies that demographic changes in the applicant pool are not responsible for the difference in treatment effects in Table 3.[34]

F Endline Survey Details

F.1 Design Details

Four months after the date of application, tenants in both treatment and control groups are randomized to either be contacted via email or by professional phone surveyors. Tenants contacted via email are sent a link to the survey and informed they will be compensated with a $15 gift card for participating. They are sent reminder emails 10 and 20 days after the initial email. Tenants contacted by a phone surveyor are called weekly throughout the month unless they either participate or actively reject the invitation to participate. All tenants are told about the compensation immediately when asked whether they are willing to participate.

[34]ERAP could also influence attorneys' effectiveness by changing the demographic composition of applicants for the legal assistance program. This test also rules out a mechanism that ERAP's expiry caused a different type of applicant to apply. However, such a mechanism would not be inconsistent with our point that ERAP affected attorney efficacy.

After one month of the initial contact strategy (five months after applying), tenants who have not been reached are swapped and receive the alternate contact method, implemented as above. Thus, all tenants receive three emails and four phone calls (in randomly varying order) unless they either participate or actively reject the invitation to participate. After six months, tenants who have not yet been reached by either method are placed in a queue that phone surveyors can continue to call weekly once the surveyor has completed their other queues.

Outcomes. We collect the following outcomes data: whether the tenant was formally or informally evicted, whether any repairs were made, whether the tenant was represented by an attorney (either provided by TWI or from another source), whether the tenant moved, and whether the tenant stayed in a homeless shelter. We collect the following information about bargaining: how much the landlord initially claimed the tenant owed, how much the tenant eventually agreed to pay, the details of any payment plans agreed to, and the tenant's expectations about future payments. We collect the following information about the tenant's situation: their current address, whether they prefer their current or previous housing situation, and their current rent, employment status, income, and sources of income.

Causal Effects. We now turn to results of attorneys on informal outcomes (Table A8).[35] Inspecting the ITT first, we find that receiving an attorney reduces formal eviction at 60 days by 21 pp, which is reassuringly similar to the corresponding estimate in Table 2. However, we find no distinguishable effects on estimates on informal eviction, moves, attempts to bargain, and propensity to make an out-of-pocket payment to the landlord. We find a marginally significant effect on out-of-pocket payments, divided by a self-reported estimate of how much the landlords was asking for (Column 8, IV $p = 0.062$). This effect is sensitive to how we define the denominator, as the result vanishes if we divide it by the amount the tenant said they owed in back rents at baseline (Column 9).

Accounting for Attrition. First, we reweight the informal sample to balance on demographics (Table A9) and their outcomes in the administrative data (Table A10), using entropy-weight balancing (Hainmueller, 2012). In particular, we weight the tenants who participant in endline surveys to match the demographics or administrative outcomes of tenants who were contacted to participate in endline surveys. Figure notes provide the full list of covariates and outcomes. Weighting on either demographics or the administrative outcomes yields similar results. Results on the intensive-margin estimate for out-of-pocket payments attenuate slightly when reweighted on demographics and grow slightly when reweighted on administrative outcomes.

[35]These estimates use OLS without controls only and not Lasso to select controls, so that the attrition-reweighted estimates come from a comparable estimation procedure.

F.2 Discussion and Connection to Framework

Viewed through the lens of the Nash Bargaining framework, the results in this and the previous section imply several conclusions about how lawyers may operate. First, that lawyers have an effect on judgments with ERAP available (Table 3, Panel A) suggests that lawyers affect real constraints to bargaining. As noted in Appendix C, a sufficient condition to reject that lawyers have no effect on outside options is if random assignment of lawyers changes court evictions.

Second, that lawyers have no effect on judgments without ERAP available may suggest that lawyers are decisive when they affect tenants' outside options, rather than landlords'. ERAP mainly affected tenants' outside options. If the landlord rejected the offer to bargain (and get ERAP), tenants received direct ERAP payments. Suppose lawyers have small effects on tenants' outside options without ERAP available, as suggested by the null effects in Table 2, Panel B. In this case, the evidence is consistent with: $\alpha_{Li} = 0$ and $\alpha_{Ti} = \tilde{\alpha}_T \text{ERAP}_i$ for $\alpha_T > 0$ and an indicator ERAP_i that is 1 when ERAP is available.

Finally, that lawyers have small effects on informal bargaining implies that they have modest effects on bargaining power ($\delta_i \approx 0$). Observe that we observe null effects even in the pooled sample. In this sample, Equation (11) and the parameterization $\alpha_{Ti} = \tilde{\alpha}_T \text{ERAP}_i$ would suggest improvements in tenants' bargaining position. This pattern of results suggests that, if anything, $\delta_i < 0$ — that is, that lawyers *reduce* tenants' bargaining power.

Taken together, these results imply that the welfare effects of lawyers primarily operate through how they change tenants' outside options.

G Baseline Survey Details

G.1 Survey Design

Beginning in February 2023, we implemented a survey of tenant applicants, with the goal of explicitly measuring tenants' demand for legal assistance.

Survey Enrollment. After completing their application for legal assistance, tenants are redirected to the beginning of a Qualtrics survey and asked if they wish to participate. If they participate, we inform them that they will receive a $15 gift-card to Amazon or Starbucks. Participation is voluntary, and we receive informed consent. For ethical reasons, we choose not to directly embed the tenant demand questions in the application, since they are used for research only and require affirmative consent, whereas the application itself would occur independently of the research study.

Eliciting Demand: Overview. We conduct real-stakes, incentive-compatible survey elicitations using best practices in behavioral economics and lab-in-the-field elicitations. Before the elicitation, we inform tenants that their choices may be implemented. We ask several confirmation checks, which 90% of tenants get right. We use the strategy method and implement tenant choices for a small share of respondents.

Tenants make repeated choices over whether they would prefer to receive some value of cash or a lawyer, in a multiple price list design. We show tenants one cash value at a time and enforce monotonicity in their responses; as an example, if they state they prefer a lawyer to $200 in cash, we then ask whether they prefer a lawyer to $300 in cash, but we assume that they would prefer a lawyer to $100 in cash. We iterate over such questions until we obtain (a bound on) tenants' willingness to accept cash or a lawyer. We bound tenants' willingness to accept within $100 intervals. The maximum cash value we offer is $1,000. These are real cash valuations and not gift cards, to measure demand as well as possible.

Implementation Details. We randomly select one of the choices (across all possible choices, uniformly) and implement the one that the tenant would prefer. For instance, suppose a given tenant prefers a lawyer to $200 but $300 to a lawyer, and this tenant is chosen to have her choice implemented. We draw a value in $\{100, 200, \ldots, 1000\}$ (uniformly). If the value is less than or equal to 200, we give the tenant a lawyer. If the value is greater than or equal to 300, we give the tenant cash.

Tenants are informed that the lawyer that they would receive is exactly their attorney representation at The Works, Inc. As a result, we can directly interpret the elicitations as revealing their valuations of the attorney representation they receive in this study. To implement this, we tell tenants (truthfully) that the choice will be implemented only if they are selected for an attorney in the main experimental lottery. Thus, the choice of cash versus an attorney is equivalent to permitting the tenant to "sell" the lawyer they receive from TWI, if they are chosen for treatment.

We took care to address several ethical considerations. As long as they understand the elicitation, tenants can only be made better off by including it: We give tenants the right to sell the in-kind good that would be provided otherwise for cash. That said, a natural concern is that tenants may regret the choice they make in the elicitation. In particular, if it takes a day or two between the baseline survey and implementation, tenants' circumstances may have changed, and they may now prefer lawyer to cash but choose the opposite in the elicitation.

To address this issue, we allow tenants to renege on their choice of money versus cash when the choice is actually implemented (usually within a few days of the original choice being made). If tenants *do* renege, then we "fine" them $50. To implement this fine, we endow participants with a bonus of at least $50 if they are chosen to have their choice of cash versus a lawyer implemented. If the person reneges, they lose at least $50 of the bonus. Thus, it is always incentive-compatible for tenants to report their true valuation of attorneys versus cash at the point of the baseline survey, but they have the option to change their choice (thus ensuring the elicitation always benefits tenants).

Any tenant whose demand choice is implemented is dropped from the study. In practice, this is a small share of tenants, because we implement with probability 0.01.

We conduct three demand elicitations: for a lawyer, for a reference good (an iPad), and for a counselor. If chosen, we implement one of the elicitations at random.

For tenants who report that they prefer a lawyer to $1,000 in cash, we impute (fairly conservatively) their WTA as $1,050.

Other Survey Elicitations. We use the survey to conduct additional elicitations, intended to understand the drivers of tenants' demand for lawyers. First, we ask tenants their beliefs about the treatment effect of lawyers. To do this, we ask tenants about the number of eviction judgments they believe tenants with filings would receive with and without legal representation, which gives an implied IV estimate of counsel. We incentivize these questions with the true rates we observe in the program.

Second, we elicit a measure of trust in the legal system by having tenants play Trust Games (TGs) against TWI lawyers, landlords, doctors, police officers, and other tenants (Berg et al., 1995). In the TGs, the tenant applicant receives an endowment of $100. They can choose to share some amount x of the $100 with their opponent, which is then tripled to $3x$. The opponent can then choose to return some amount of the $3x$ back to the opponent. The subgame perfect equilibrium of this game under classical preferences is that the initial tenant applicant should pass $0 to the opponent, since that person would return $0. The games are real-stakes and implemented using the strategy method. We recruit opponents to play the games several times for implementation purposes.

Finally, in the original tenant demand elicitation, we randomize whether tenants make the choice in the state of the world where they are endowed with $50 or $500. Put another way, we inform tenants that if they are chosen to have the demand elicitation implemented, they will also earn a bonus of $50 or $500. The purpose of this randomization is to relax budget constraints.[36]

Naturally, eliciting demand, beliefs, and the TGs in our field setting poses a number of challenges, particularly since our sample has low levels of numeracy. We detail checks for these issues below.

[36]Tenants who are randomized into the $500 bonus but then renege on their original choice are still fined $50, so take home $450.