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

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This draft reports early results from an ongoing trial. Please see latest version here.

Abstract

We randomize provision of attorneys to tenants facing eviction in Memphis, Tennessee ($N =$ 265 treatment, 753 control), who otherwise seldom have legal representation. Despite landlord-friendly eviction law, providing an attorney reduces tenant eviction judgment rates within 90 days by 27 percentage points (50%). 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 90 days shrink by about 70% and are indistinguishable from zero. Attorneys have little effect on informal outcomes and bargaining. Incentivized surveys suggest tenants' willingness to pay for an attorney is double attorneys' price, and seven 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 = 0.4).

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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., 2023b), 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.

Moreover, the mechanisms through which lawyers operate have different positive and normative implications. First, if lawyers affect outcomes, they may do so by (i) changing parties' outside options and hence affecting "real" constraints to bargaining out of court or (ii) redistributing resources from landlords to tenants by changing tenant bargaining power. Second, attorneys may affect outcomes like tenant stress that are difficult to measure and monetize. Yet a careful cost-benefit accounting is critical, as attorneys are expensive, so there may be more efficient ways to assist the poor or redistribute.[2] Given the momentum of policy expansion, evidence on lawyers' effects and mechanisms is urgently needed.

How does free provision of tenant eviction attorneys affect evictions, and is it worth the fiscal cost? We answer this question with a field experiment that randomizes provision of attorneys to tenants facing eviction. 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 for lawyers.

We start with a simple Nash Bargaining framework to interpret lawyers' effects and propose normative implications (Section 2). 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 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

[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] Eviction attorneys in New York City cost $3,200 per household served (Cassidy and Currie, 2023).

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) or control.[3]

This draft presents early results from an ongoing trial. Through mid-August 2023, we have randomized 265 of 1,018 tenants to receive attorneys. Randomization will continue until the end of 2023, when we project having approximately 325 treated households.

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 27.6 pp reduction in eviction judgments over 60 days (standard error: 5.1) off a control mean of 45.3%. Lawyers reduce the amount that tenants owe in a judgment by $1,341 (s.e.: 314), a 59% reduction from the control mean of $2,277. Attorneys increase nonsuits by 14.4 pp (s.e.: 4.3) over a control mean of 13%.

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 treatmen t 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 block fast evictions. Yet the effect on judgments attenuates by 90 days and becomes indistinguishable from zero, although splitting the sample reduces power (coefficient at 90 days: -11.2 pp, s.e.: 8.8). Differences between pre- and post-expiry are large and statistically significant. Once ERAP expires, lawyers are 24.8 pp (69%; s.e.: 11.1 pp) less effective in reducing judgments over the 90 days after a filing. They are 15.8 pp (s.e.: 8.5) less effective in increasing nonsuits. These

[3]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.

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.[4]

While we lack randomized evidence on this mechanism, several tests all point to the ERAP environment, rather than other forces, as decisive. For this analysis to be confounded, changes to the policy or economic environment must have occurred around ERAP's expiry and differentially affect treatment versus control. We see little movement on overall eviction outcomes in Shelby County around the period of ERAP expiry. If anything, 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 29% of contacted tenants for a sample of $N = 267$, which is high relative to contemporary response rates in financially distressed populations in the United States but low in absolute terms.[5]

The surveys provide rich descriptive evidence about the prevalence of landlord–tenant bargaining. About 60% of tenant respondents are ultimately evicted, of which 13 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, 18% pay immediately, 42% pay in installments, and about 40% 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.

Attorneys have small effects on informal outcomes. 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. When we conduct attrition adjustments, including balancing based on either demographics of respondents or their administrative outcomes, results are stable.[7] We suggest caution for interpreting these results in particular, as we

[4] The fact that lawyers have large effects when combined with ERAP does not imply that ERAP alone has large effects on eviction. Rafkin and Soltas (2023) find ERAP alone also has small effects.

[5] The sample size will grow as we collect endline data from tenants who applied later in the program. So far, we find only modest differences in response rates among treatment versus control ($p = 0.16$) and no selection on whether the case got a judgment ($p = 0.24$). We do detect treatment–control imbalance on demographics among the participating sample ($p = 0.014$), which motivates reweighting adjustments. Given the paucity of existing evidence and the potential importance of informal evictions, the surveys make a useful contribution but should be interpreted cautiously in light of reasonable concerns about response biases and attrition.

[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.

[7] Some specifications suggest that lawyers may reduce intensive-margin measures of the share paid out-of-pocket, but results attenuate with attrition adjustments.

anticipate collecting substantially more endline surveys by the final draft.

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 the welfare effects of attorney provision using a Marginal Value of Public Funds (MVPF) framework (Hendren and Sprung-Keyser, 2020). We focus on the effects of lawyers after ERAP expires, since that is likely most generalizable. We ignore general-equilibrium effects or potential negative effects on landlords. We estimate the MVPF fiscal costs (denominator) by netting the treatment effects of attorneys on eviction multiplied with the downstream effects of eviction on the government budget from the direct costs of providing a lawyer.

Estimating tenants' willingness-to-pay for the in-kind transfer of lawyers (the MVPF numerator) is challenging for two reasons. First, attorneys may have direct impacts on unmeasured yet welfare-relevant outcomes. For instance, attorneys could directly reduce tenant stress about engaging in the legal system. Second, even the outcomes that we measure well (e.g., attorneys' effects on eviction judgments) could have knock-on effects on outcomes like time to find another unit which are challenging to monetize. Such forces should enter the MVPF numerator in principle, but in practice, analysts often calibrate demand by considering the observed impact of a policy and monetizing channels that are measured well.

We start with a traditional calibrated approach, using the only treatment effect estimate that we observe and credibly monetize: the impact of attorneys on the following two years' wages operating through eviction judgments. This approach gives a willingness to pay estimate of \$91. Meanwhile, attorneys cost between \$250–325 per lawyer. Fiscal externalities reduce the net cost to \$253. The calibrated MVPF is then 0.4 for the period after ERAP expires. However, the standard approach likely underestimates tenant willingness to pay for many welfare-relevant factors.

We therefore elicit tenant demand directly in a real-stakes, incentivized baseline survey embedded into the application for the legal assistance program ($N = 185$ completes so far). This method gives an average tenant willingness to accept (WTA) cash versus a lawyer of \$679 (s.e.: 28), all in the period after ERAP expires. 42% of tenants prefer a lawyer to receiving \$1,000 in cash. 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 2.7 — about seven times above the traditional approach, and implying that providing lawyers is more efficient than providing equivalent cash to the same population.

Such high valuations for lawyers are perhaps surprising, yet several tests suggest elicitation errors, binding budget constraints, or misperceptions do not explain the results. On elicitation errors, demand for a placebo good does not exhibit high valuations.[8] On budget constraints, we randomize endowing tenants with $450 extra cash, which has no detectable effect on valuations. On misperceptions, we elicit incentivized beliefs about lawyers' efficacy in court and find similarly high valuations even among tenants with accurate beliefs.

Then what does explain tenants' high valuations? 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) 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 this point, we elicit incentivized beliefs about lawyers' effectiveness in court and find that they are uncorrelated with demand. While this explanation for tenants' high valuations likely remains incomplete, it highlights how direct elicitation — if done credibly — can account for additional factors that analysts often neglect in MVPF calculations.

Contribution. First, we contribute to the nascent literature on Right to Counsel (Table A1). Four previous RCTs, of which only one took place in the past decade, show mixed effects of legal representation from relatively small samples. 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 more relevant comparison for policymakers deciding whether to implement Right to Counsel.[9] 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.

No one study is definitive, but 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 and is more relevant to today's housing markets than studies from over a decade ago.[10] Second, we collect detailed survey outcomes on informal outcomes for a meaningful share of the sample, which lets

[8] WTA for the placebo good, an iPad, is $108 and is not left-skewed. Dropping people who have WTA estimates that are seemingly high or low for the iPad ($0 or $300 respectively) has only a small effect on lawyer WTA.

[9] 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).

[10] 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.

us study lawyers' effects on bargaining and whether lawyers just shift tenants from formal to informal evictions. 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. Fourth, we study the effects of lawyers on tenant welfare, contrasting a standard approach that calibrates demand with direct, incentivized elicitations at baseline.

Our second contribution is to the broader literature on housing insecurity and eviction (Collinson et al., 2015, 2023b). Related studies examine emergency rental assistance (Collinson et al., 2023a; Rafkin and Soltas, 2023), 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 (e.g., Trounstine, 2020; Soltas, 2022). We study a proposal for addressing housing insecurity that has gained favor (e.g., Desmond, 2016) and which has driven policy change despite thin evidence. Providing lawyers to tenants conceptually differs from other policies, as it can directly affect the informal negotiations that are key to low-income housing markets.

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 study how lawyers can shift clients' outside options or bargaining power, and how policy influences lawyers' effectiveness.

Fourth, we conduct behavioral welfare analysis by eliciting tenants' incentivized demand for legal assistance among informed and potentially biased tenants (Bernheim and Taubinsky, 2018). Relative to previous studies, we consider the costs and benefits of calibrating versus directly measuring the numerator of the MVPF in a setting where welfare calculations are fraught.

2 Framework

We notate lawyers' effects in a simple Nash Bargaining framework which lets lawyers operate through several distinct mechanisms. 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.

<u>L</u>andlord–<u>T</u>enant 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:

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

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:

$$\mu_{Ti} + \alpha_{Ti} A_i + \mu_{Li} + \alpha_{Li} A_i \leq 0, \tag{2}$$

a modified version of which also appears in Rafkin and Soltas (2023). Equation (2) 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 (2) 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 (1) is:

$$b_i^* = \left(\beta_i + \delta_i A_i\right) \left(\mu_{Li} - \alpha_{Li} A_i\right) - \left((1-\beta_i) - \delta_i A_i\right) \left(\mu_{Ti} - \alpha_{Ti} A_i\right). \tag{3}$$

Inspecting Equations (2) and (3) 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 (2) is satisfied, and assume attorney assignment is random ($A_i \perp \Theta_i$). A corollary to Equation (2) is that lawyers affect court eviction rates only if they affect outside options:

$$\mathbb{E}[E_i | A_i = 1] \neq \mathbb{E}[E_i | A_i = 0] \implies \alpha_{Li} + \alpha_{Ti} \neq 0 \text{ for some } i. \tag{4}$$

Intuitively, Equation (2) 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 (2) 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:

$$\alpha_{Ti} = \tilde{\alpha}_{Ti}\mathrm{ERAP}_i \tag{5}$$

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.

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.

Tenants apply for a TWI program that is advertised as providing legal services to tenants facing eviction. The application takes less than 10 minutes and is conducted entirely online. Tenants are informed in the application that the program has limited resources and hence 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 a social worker. The size of the social worker treatment is small (about 100 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

[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.

[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.

light-touch (see Appendix C.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.

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 full control group, including 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 ap-

[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.

plications 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 C. 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.

ITT estimates result from:

$$y_i = \beta \text{WinsFirstLottery}_i + \gamma X_i + \varepsilon_i, \tag{6}$$

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:

$$y_i = \beta \text{Lawyer}_i + \gamma X_i + \varepsilon_i, \tag{7}$$

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 select controls separately for each IV or ITT specification and outcome. We always impose that the propensity control enters the regression and allow Lasso to select from a vector of demographic controls.[15]

3.5 Data

We collect data on tenant outcomes from two sources: (i) administrative court data, and (ii) endline surveys. We collect additional information in baseline surveys. The administrative court

[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.

outcomes are observed for more than 98% of the sample (Appendix C), since program eligibility depends in part on being confirmed to have an eviction filing in the court records. In addition to the endline surveys, 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 much more subject to concerns about potentially important attrition.

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. We group all such cases as "nonsuits." Nonsuits and judgments do not partition all possible resolutions for court cases: Landlords who settle out of court frequently leave cases open without nonsuiting or pursuing a judgment. By doing so, they retain the option to pursue a judgment later without filing a new eviction. For this reason, nonsuits typically represent a better outcome for tenants than leaving a case idle.

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.

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.

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 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.[16] If representation helps tenants negotiate more favorable informal arrangements, this limitation is a first-order concern. We thus field endline surveys 3-6 months after tenants apply to track informal outcomes regarding tenant moves and payment plans that are unobservable in court. 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 Section 5.

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

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

outreach, which allows us to implement additional attrition tests (Dutz et al., 2022). Section 5 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, if one grants that survey participation is independent of treatment, the surveys provide novel information about the effects of legal counsel, especially relative to the existing empirical literature on Right to Counsel.

We stake a compromise position. 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 information on the effects of counsel on this margin is very valuable. However, concerns about attrition bias are reasonable, so we present the causal effects on survey outcomes with caution and clearly demarcate where our results have more uncertainty.

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. (2023b) 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 (Section 2) 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 (Table 2). We focus on a time horizon of 60 days, as Figure 1 suggests most court cases in the control group conclude by that point, and present both ITT and IV estimates. We find a strong first-stage: receiving an offer for representation causes a 60 p.p. increase in representation by the program (t-stat of the first offer > 20). Consistent with Figure 1, the ITT on judgments at 60 days is -16.3 pp (Column 3; $p < 0.001$), and is mirrored by the effect on nonsuits (Column 6), but not one-to-one. Attorney offers reduce the amount owed in judgments by about \$815 off a control mean of \$2,277. The IV estimates suggest attorneys reduce judgments by nearly 28 pp, cause a 14 pp increase in nonsuits, and reduce writs by 16 pp.

We visualize the time path of the effect on judgments and show that the effects persist through 120 days (Figure A1). The effects at 30, 60, and 90 days are not anomalous: at 120 days, lawyers reduce judgment rates by 27 pp.

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 ($p = 0.35$). 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 3A).[17] However, we find that TWI attorneys are significantly more likely to file continuances for their clients, buying them potentially valuable time (Figure A4).[18] In our setting, the returns to experience are present but subtle.

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 because the relevant data are not recorded in administrative court filings and because coincidental policy variation in available resources is rare. Yet each attorney role may have different policy implications: attorneys are expensive and there are cheaper ways to provide tenants with more time to move or easier access to social services.

As discussed in Section 3, during the first half of our study, attorneys could help tenants apply for emergency rental assistance through the federally-funded, locally-administered Emergency Rental Assistance Program (ERAP) that was implemented as part of the response to the COVID-19 pandemic. The Memphis ERAP expired in January 2023, so we study treatment effects when ERAP is and is not available.[19]

We begin by plotting the raw data for judgments and non-suits, split by before versus after ERAP expires (Figure 2). 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 no discernible effect on non-suits without ERAP being available (Panel D).

Formal tests confirm ERAP's quantitative relevance (Table 3). Effects on judgments before ERAP expiry (Panel A) are large and highly significant. For instance, with ERAP available, lawyers cause a 36 pp reduction in eviction judgments at 90 days (s.e.: 6.8 pp). However, without ERAP, effects on judgments attenuate to 11.2 pp at 90 days (s.e.: 8.8, Panel B). Naturally, cutting the sample reduces power, and we cannot reject a meaningful 27 pp reduction in the post-ERAP expiry period. Yet the difference in treatment effects at 90 days is economically large and signifi-

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

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

[19] The ERAP stopped making payments in late December 2023, but tenants needed to have an initial ERAP application before September 2023. As we see in lawyers' records that their clients often got ERAP in December, we use January 1 as the cutoff date.

cant (Panel C). Lawyers are 24.8 pp more effective in stopping judgments at 90 days when ERAP is available (s.e.: 11.1; *p*-value: 0.025). Thus, lawyers in the post-ERAP expiry period are about 69% less effective in stopping judgments at 90 days.[20]

Without ERAP, lawyers still stop fast judgments. They reduce judgments at 30 days by an economically meaningful 19 pp (s.e.: 5.5). These effects at 30 days without ERAP are indistinguishable from the with-ERAP effects, and the point estimate is larger in magnitude.

We plot the ITT and IV effects on judgments at varying horizons, before and after ERAP expiry (Figure 3). 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.

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 17 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 impacts of providing legal assistance on our primary court outcomes. Of course, when we split the sample, power naturally drops; for instance, we cannot reject a 27 pp reduction in eviction rates at 90 days. At a minimum, these results suggest that the combination of ERAP and attorneys has larger effects than when ERAP is not available (differences in Panel C).[21]

4.3 Other Potential Explanations

We do not have randomized variation in whether ERAP is available. It is reasonable to question whether the time-varying heterogeneity in Table 3 reflects ERAP expiry versus other forces. We now present the evidence in favor of ERAP–attorneys' joint effectiveness and consider alternative explanations.

Positive Evidence for ERAP. First, while some skepticism is appropriate, we argue that ERAP is a natural candidate explanation. As discussed in Section 3, ERAP was a $100-million program in

[20]The marginally significant coefficient on judgments at 60 days is insignificant in alternate specifications (Section 4.4).

[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.

Memphis that fully extinguished tenants' back rents and provided additional months of rental support. Attorneys encouraged landlords to nonsuit eviction cases and apply for ERAP funds. While ERAP was available for control-group tenants, it was often challenging to access and receiving payment had substantial delays (Rafkin and Soltas, 2023). TWI explicitly coached attorneys in the program to bargain with landlords by raising the possibility of receiving ERAP. There was no other meaningful change to the eviction policy environment in Memphis coincident with ERAP expiry.[22]

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 3a). An indicator for ERAP availability has among the largest coefficients, relative to other forces that one might believe would affect attorneys' efficacy.

Finer Time Variation. One may question whether the change in the program's effectiveness really coincides with ERAP expiry. If the 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 A8). Notably, the treatment effects of attorneys are largest in magnitude just before ERAP expiry, and then attenuate immediately after.

In a related exercise, we consider the difference-in-differences between the program's effects at 60 or 90 versus 30 days, by before or after ERAP expiry (Figure A9). This exercise demonstrates that the plateauing in Figure A9 truly begins at ERAP expiry. To the extent that lawyers seem to be able to influence the speed of judgments even without ERAP, it illustrates that their (in)effectiveness at later horizons begins then.

We acknowledge that this exercise loses power by disaggregating the results. At the very least, we do not find evidence that the encouraging results before ERAP expiry persist after ERAP expiry.

Differences in Demographics Or Eviction Composition. By contrast, demographics do not change abruptly when ERAP concludes and typically exhibit small trends (Figure A7). This suggests that ERAP expiry, and not other differences in the applicant pool, is the most important mechanism. Relatedly, we do not see important changes in the aggregate eviction filing or judgment numbers around ERAP expiry (Figure A6).

To further explore this point, we 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

[22]Because we anticipated ERAP could have a large effect on outcomes, we registered that we would examine this heterogeneity in our initial AEA registration on December 21, 2022 (before to ERAP expiry).

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.[23]

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. One natural change in strategy embedded in the expiration of ERAP is the inability to pressure landlords to accept ERAP in exchange for nonsuits. We consider other possible changes in strategy or composition in turn.

We find no evidence that attorney strategies unrelated to ERAP changed. In fact, the most natural measure of attorney strategy in administrative data — the rate of continuance filings — is one of the few outcomes that does not change through ERAP's expiration. Attorneys caused a 34 p.p. increase in probability of filing a continuance within 60 days in *both* periods (p-value of difference: 0.955).

An alternate explanation for shifts in the average effectiveness of attorneys is a change in the underlying composition of which attorneys represent clients. For example, if TWI attorneys are more effective but started representing a smaller share of tenants than low-bono attorneys in January 2023, we may misattribute the change in average outcomes to ERAP's expiration. 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. Again, this would appear to make our estimates of the effects of ERAP conservative since outcomes in the post-ERAP era may be slightly buoyed by a higher likelihood of TWI representation.

4.4 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 A4 and A5).

[23]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.

5 Informal outcomes

5.1 Design

Timing and Collection Details. We survey 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. 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. Tenants are told about the compensation immediately when asked whether they are willing to participate.

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 Collected. The online and phone surveys ask identical questions.[24] 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. 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.

Demographics, Response Rates, and Attrition. Table 4 shows response rates and attrition. We have attempted to reach 87% of participants across both modes. Altogether, we have a conditional participation rate of 29% (267 respondents reached so far).

Two main factors contribute to low response rates in our setting. First, low-income groups

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

who are on the verge of eviction routinely lose access to traditional means of communication when they stop paying bills. Second, we must rely on cell phones because landlines are particularly biased when studying possible evictions.[25] 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 will allow us to test for differences in the mode of survey when we have a larger sample in the future.

We find only modest reasons for concern about differential attrition. We test whether either treatment of judgments predict higher response rates. There is no difference both between treatment and control (coefficient: 5 pp, $p = 0.117$) and between tenants who have judgments in administrative data (coefficient: -4 pp, $p = 0.237$).

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, is more likely to have ever been evicted before applying, 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 some 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.

Altogether, we find some cause for concern about differential participation in the endline samples. Attrition that yields biased estimates of treatment effects is potentially present but unlikely to be large (one out of three tests points to differential attrition at $p < 0.05$, a second with $p \approx 0.1$). In the following, we first report naive results on informal outcomes, positing no differential attrition. We then perform reweighting exercises to account for the attrition that we observe.

5.2 Results

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

Descriptive Evidence. We first document several novel descriptive facts separately for tenants who were formally evicted, informally evicted, and not evicted (Table 5). All tenants in our sample had an initial eviction filing, perhaps explaining the higher share of all evicted tenants who received formal court judgments (80%) 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 (62%

[25] Response rates among cell phone calls are significantly lower than landlines: Kahneman and Deaton (2010) reports that a Gallup poll of 450,000 US residents had 28% response rates among cell-phone contacts.

versus 53% and 46% for formally and informally evicted tenants, respectively) and agreed to pay a higher share of the amount they owed (89% versus 69% and 66% 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 evicted were most likely to stay in a homeless shelter (31% versus 22% and 5% 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 (26% versus 15% and 20% 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 80% avoid making out of pocket payments — and reinforces the downstream effects of eviction on tenants' economic outcomes.

Causal Effects. We now turn to results of attorneys on informal outcomes (Table 6).[28] 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 A6) and their outcomes in the administrative data (Table A7), 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

[26]Reassuringly, we find similar rates of tenants who did not receive formal judgments report having moved ($\sim 40\%$) as other studies (Collinson et al., 2023b).

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

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.

5.3 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 Section 2, 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 ($\delta_i \approx 0$). If anything, the fact that we observe null effects even in the pooled sample — where Equation (2) and $\alpha_{Ti} = \tilde{\alpha}_T \text{ERAP}_i$ would suggest improvements in tenants' bargaining position — suggests that $\delta_i < 0$, that is, that lawyers *reduce* tenants' bargaining power.

Taken together, these results imply that the welfare effects of lawyers primarily operates through how they change tenants' outside options. In the next section, we apply the Marginal Value of Public Funds framework to compute the welfare effects of giving lawyers as in-kind transfers.

6 Welfare Analysis

6.1 Set-Up

We use the Marginal Value of Public Funds (MVPF) to summarize and evaluate the various effects of counsel. The MVPF is the value of the program to recipients generated by every dollar spent by the government. Hendren and Sprung-Keyser (2020) define the MVPF as: $\text{MVPF}^j = \frac{\sum_i \text{WTP}_i^j}{G^j}$, where $\sum_i \text{WTP}_i^j$ is the sum of 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). We first calculate the net costs and then discuss WTP.

We summarize MVPF results from the post-ERAP period. We aim to measure the welfare effects of providing an attorney without the additional effects of rental assistance.

We consider the impacts on tenant welfare. Landlords likely have a negative WTP for tenants they are evicting to be represented by an attorney. We exclude this from consideration.[29] Excluding this force is an imperfect abstraction, particularly because many landlords in our setting are small or middle-income themselves.

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 both in terms of deterring eviction filings in the first place and 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. (2023b) to convert our IV estimates on decreased evictions into the fiscal externalities for other government programs. Table 7 summarizes our calculation of the fiscal externality generated by these and other effects in the post-ERAP period. We find that providing an attorney generates a fiscal externality of -\$35, implying it is socially beneficial and recovers $\sim$ 12% of the direct costs, which reduces the net cost of an attorney to \$253. 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).

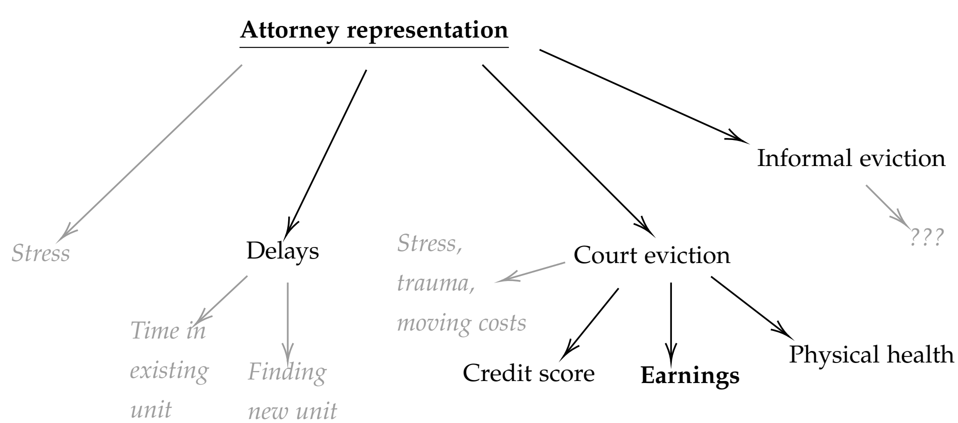
Such WTP calibrations require *measuring* all downstream effects of treatment and *monetizing* (i.e., generating a tenant willingness-to-pay for) each in turn. In particular, attorneys have many direct effects; for instance, on formal eviction, informal eviction, delays, and tenant

[29] In the MVPF framework, this is done by setting $\hat{\eta}_L$ — the average welfare weight on landlords relative to tenants — equal to 0.

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. While 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) a 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. These two conditions can conflict, especially when elicitations before receiving a lawyer can 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. (2023b)'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 \$91. This WTP estimate implies an MVPF of 0.4 ($\approx 91 \div 253$). 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 $N = 185$ tenants who have completed the survey so far, and anticipate collecting it for more than $N = 250$ tenants before the study closes.[30] We survey tenants only after ERAP expiry, meaning that elicited WTAs do not reflect beliefs about accessing ERAP. Appendix D presents survey and elicitation details.

Our belief elicitation method draws on standard techniques from experimental economics. We repeatedly 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 $\{100, \ldots, 1{,}000\}$. We use the strategy method and implement tenants' choices with a small probability. 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. Tenants appear to have high comprehension; for instance, 90% get a confirmation question about WTA experimental procedures correct.

[30] 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.

Tenant applicants exhibit very high demand for lawyers. The average tenant demand for an eviction attorney from NPI is \$679 (s.e. 28), assuming conservatively that tenants who prefer a lawyer to \$1,000 in cash have a willingness to accept of \$1,050. Approximately 40% of tenant applicants prefer a lawyer to receiving \$1,000 (Figure 5). It costs about \$350 to hire an eviction defense attorney in Memphis.

Despite our conservative elicitation choices, these results are high if lawyers do not stop court judgments, which naturally raises skepticism. Appendix D presents a number of tests that rule out that (i) these results reflect confusion, elicitation error, or lack of numeracy; (ii) these results reflect belief biases about lawyers' treatment effects or are otherwise not normatively respectable (Bernheim and Taubinsky, 2018). For instance, WTAs for a placebo good (iPads) are not left-skewed (Figure A11), which rules out that the elicitation procedure or lack of comprehension of the incentives somehow pushes tenants toward high valuations. Moreover, tenants with optimistic and pessimistic beliefs about lawyers' effects have similar WTAs for lawyers, suggesting that high demand is not driven by misperceptions (Figure 6 and Figure A12). In a sub-experiment, we experimentally relax tenants' budget constraints when eliciting WTAs and find that doing so has little effect on tenants' high demand for lawyers.[31] While mechanical elicitation errors are likely not responsible for high valuations, we cannot rule out that unmeasured misperceptions or alternative nonclassical explanations play a role.

One potential explanation for high WTA for lawyers is that tenants 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 A13). 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. Still, this evidence is not decisive, so we see the question of why tenants' have such high WTA estimates as ultimately unresolved. Nevertheless, that tenants often report valuing difficult-to-monetize aspects of lawyers highlights one advantage of WTA estimates versus calibration.

In light of the above evidence, we use the estimate of \$679 directly for the MVPF numerator. Our elicited WTA implies an MVPF of 2.7 ($\approx 679 \div 253$) 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.

[31] We relax budget constraints by telling tenants that we will provide them with \$500 in cash in the event their WTA is implemented. Thus, if they are in the state of the world where they receive a lawyer, they will get a lawyer plus cash. See Appendix D for details.

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 interaction of legal representation and access to the Emergency Rental and Utilities Assistance Program. We contrast two approaches to compute the Marginal Value of Public Funds for attorney provision without access to ERAP, ignoring impacts on landlords or general equilibrium. Taking tenants' high valuations for attorneys at face value, direct elicitation yields an seven-times larger MVPF than a standard calibrated approach (2.7 versus 0.4). Tenants' elicited willingness to pay might remain overstated, perhaps due to unmeasured misperceptions or alternative behavioral biases. Future research could contrast revealed willingness to pay for other in-kind transfers against calibrated demand, and further explore why these estimates do not coincide.

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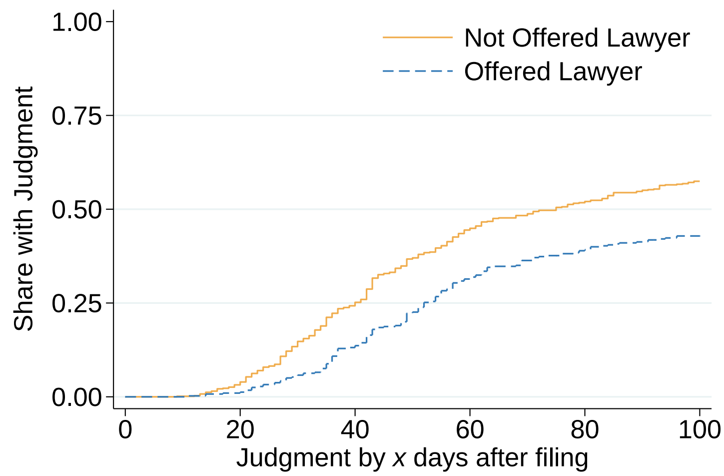
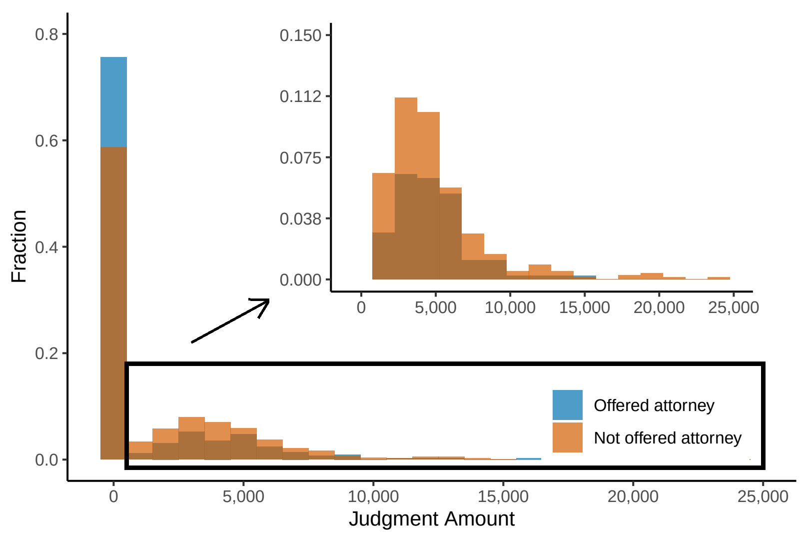
8 Figures

Figure 1: Effect of Legal Offer on Eviction Judgments

(a) Judgments by Attorney-Offer



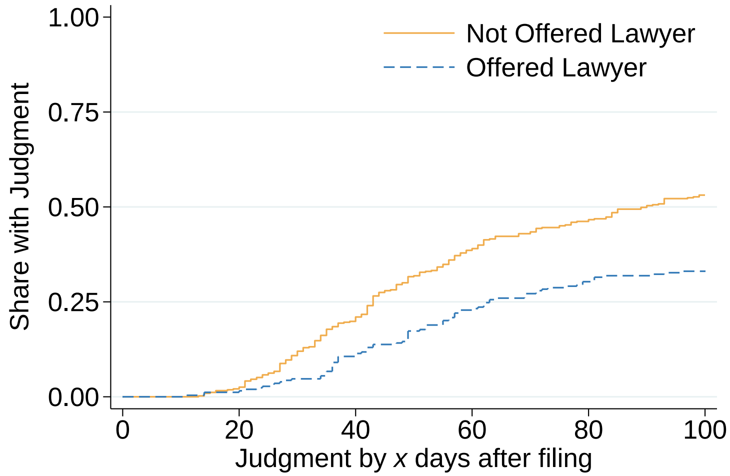
(b) Judgment Amounts by Attorney-Offer



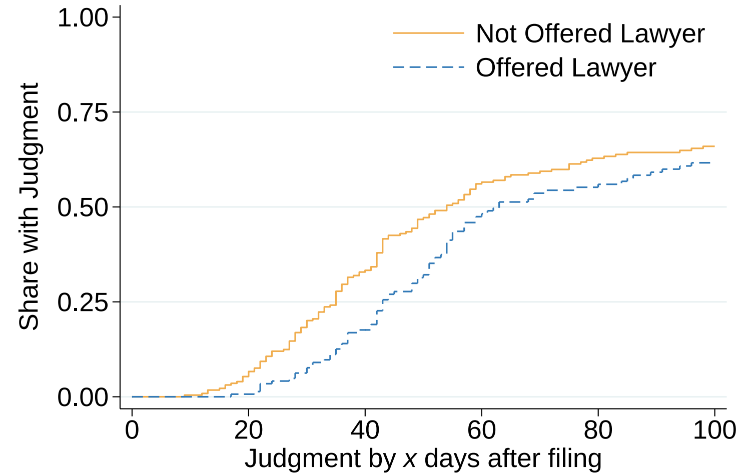
Note: This figure shows the difference in eviction judgment rates (Panel A) and judgment dollar amounts (Panel B) among those offered and not-offered a lawyer in the first lottery in which they are entered. Panel A plots Kaplan-Meier failure curves, which present the rate at which a group has achieved 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: Judgments and Nonsuits by Attorney Offer Status, Pre- and Post-ERAP Expiry

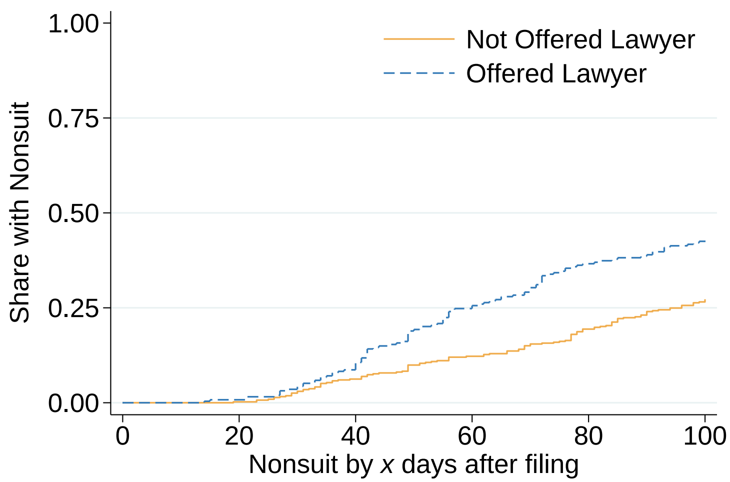
(a) Judgments, pre-ERAP expiry



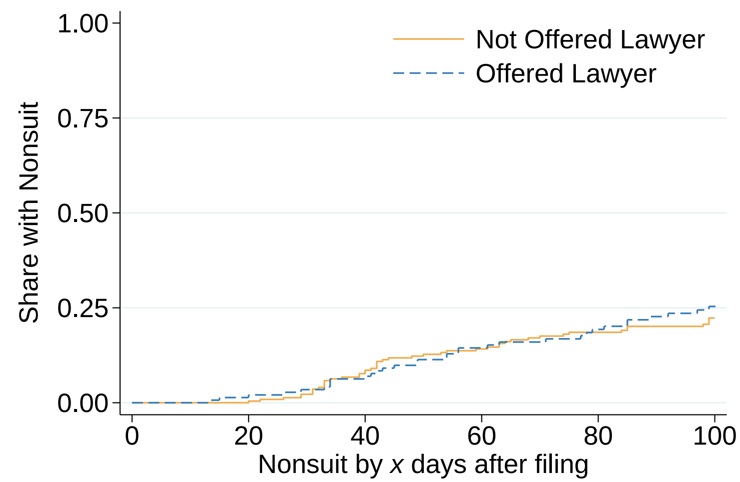
(b) Judgments, post-ERAP expiry



(c) Nonsuits, pre-ERAP expiry



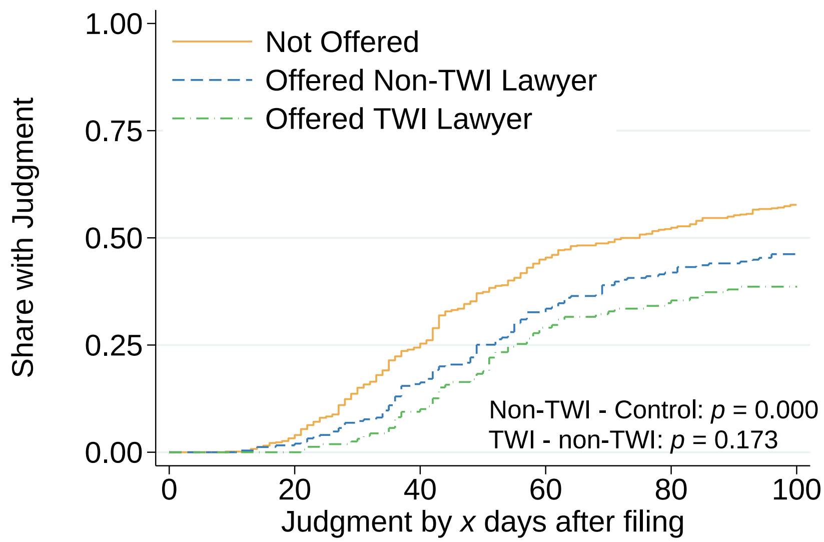
(d) Nonsuits, post-ERAP expiry



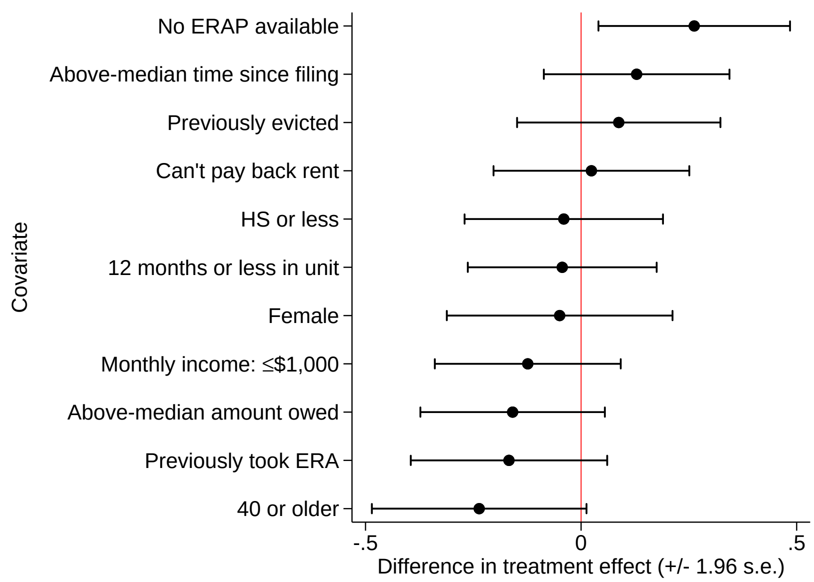
Note: These figures present Kaplan-Meier curves for judgments and nonsuits before and after ERAP expired. An individual is assigned to being post-ERAP expiry if they applied for assistance from TWI after January 1, 2023.

Figure 3: Heterogeneity in Treatment Effects

(a) Judgments by TWI vs. other attorneys



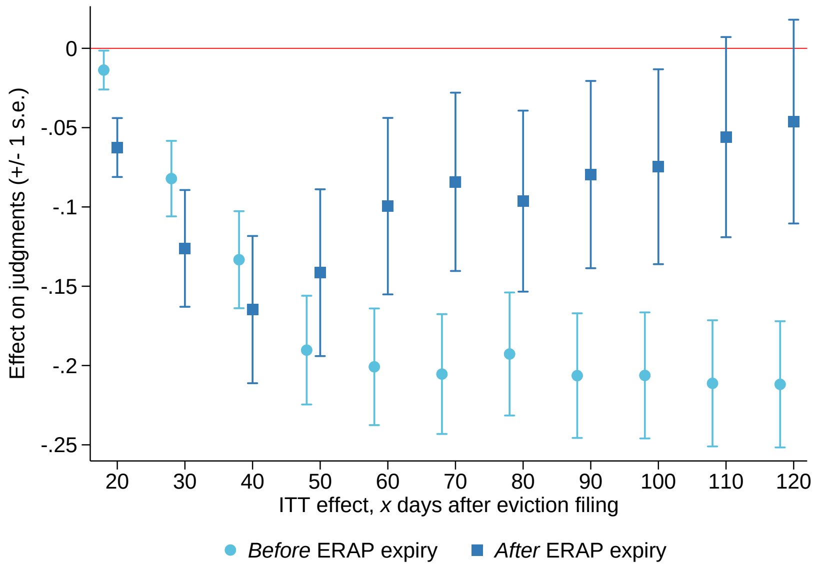
(b) Heterogeneity in IV estimates on judgments within 60 days



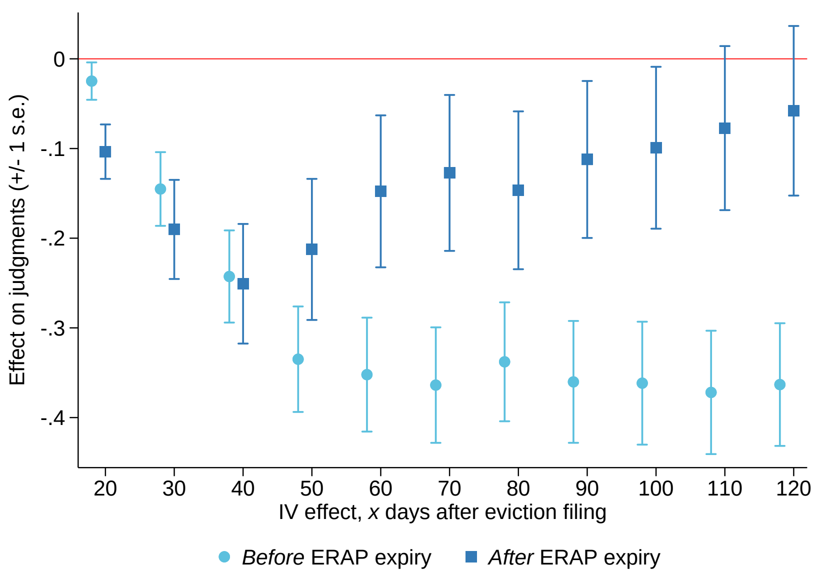
Note: Panel A separates the treatment effect on judgment between those who receive TWI attorneys versus those who receive contract attorneys. The p-values should be interpreted cautiously, since we do not reweight or adjust for time-varying assignment propensities. Panel B interacts the main specification (Equation 7) 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 4: Heterogeneity in Treatment Effects: Before and After ERAP Expiry

(a) ITT Estimates on Judgments

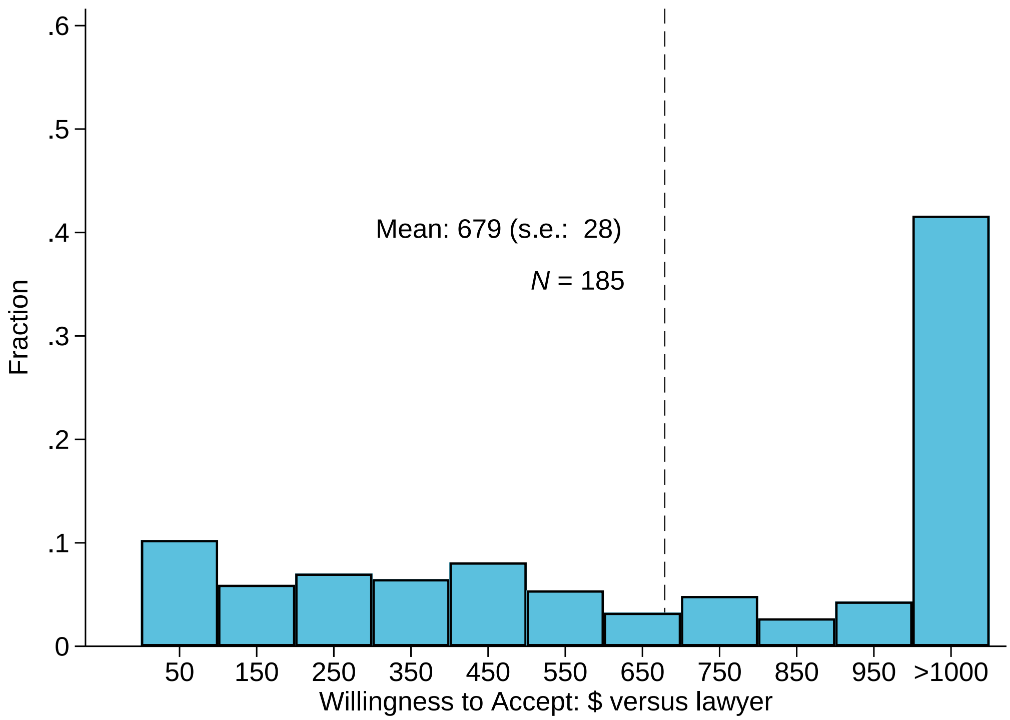


(b) IV Estimates on Judgments



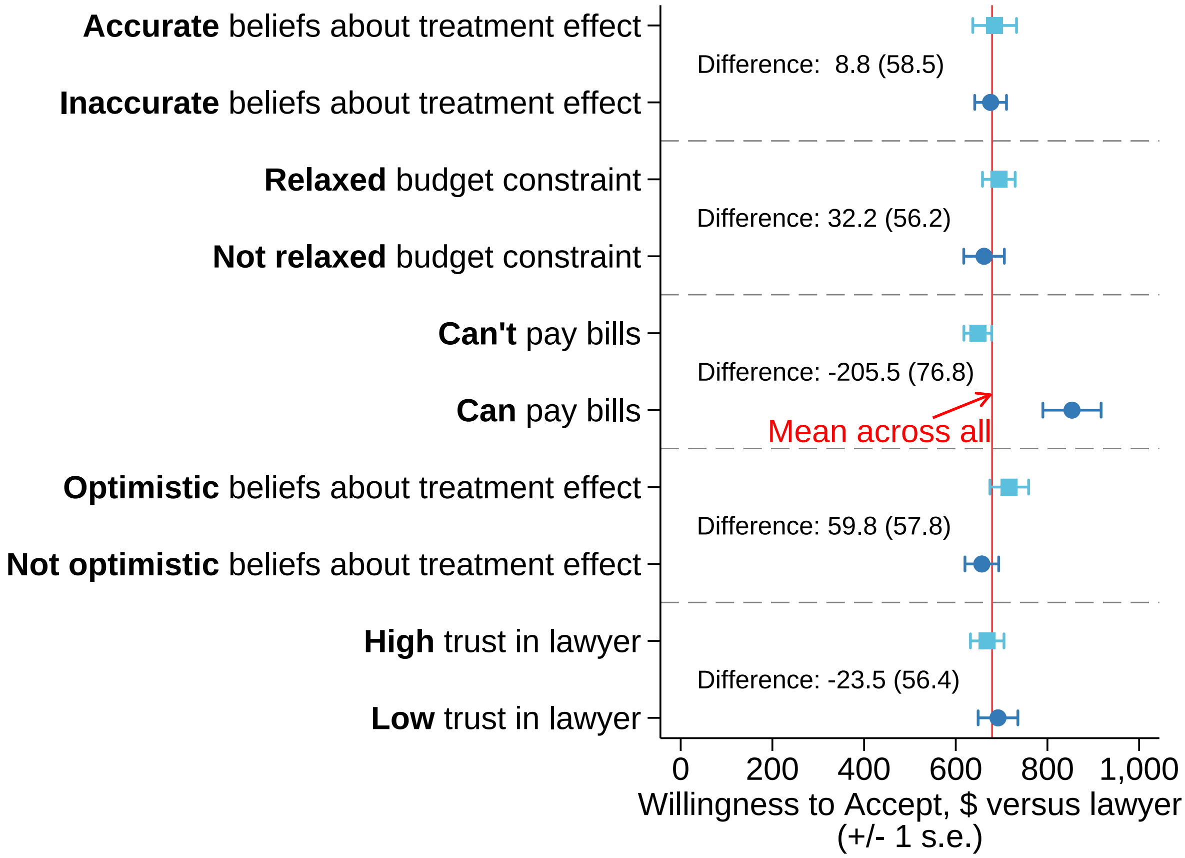
Note: These figures present the ITT and IV effect on judgments, split by before or after ERAP expired. Individuals are assigned to being in the post-ERAP expiry period if they applied for assistance from TWI after January 1, 2023. Each plotted coefficient comes from a different post-double-selection Lasso procedure to select controls.

Figure 5: 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 line indicates the mean. See Appendix D for details on the elicitation method and checks used to ensure data quality.

Figure 6: Willingness to Accept Cash for Lawyer, Across Normatively Relevant Groups



Note: This figure plots tenant willingness to accept for lawyers across various groups. All tenants who took this survey applied after ERAP expired. The red line marks the mean valuation across all responses. We label beliefs about treatment effect as accurate if they report that lawyers have an effect of -0 to -25 p.p. on judgments. We relax budget constraints experimentally by endowing a random subset of participants with an additional $500 in the event that their choice is implemented. Next, we separate participants by whether they report the ability to pay all their bills, then by whether they overestimate treatment effects (i.e., believe lawyers reduce effects judgments by 30 pp or more). Lastly, we group participants by whether they play above-median trust strategies in trust games against TWI attorneys. We find homogeneous valuations on average across all groups, with the exception that tenants who report that they are able to pay all their bills have a higher willingness to accept. The text shows differences between the two coefficient and standard errors (in parentheses).

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	34.0	-0.1
	[17.0]	[18.0]	[9.9]	(0.7)
Black	0.53	0.72	0.94	-0.01
				(0.02)
Female	0.53	0.61	0.79	-0.02
				(0.03)
Household size	2.0	2.0	2.6	-0.0
	[1.0]	[1.0]	[1.5]	(0.1)
HS or less	0.45	0.69	0.66	0.02
				(0.03)
Single	0.40	0.56	0.89	0.00
				(0.02)
Economic status:				
Monthly income	5,408†	1,100†	1,375	-58
	[115,769]	[631]	[1,957]	(122)
Monthly rent	834	682	955	-23
	[376]	[333]	[517]	(32)
Housing security:				
Applied after ERAP expiry			0.32	0.01
				(0.02)
Ever evicted			0.33	-0.03
				(0.03)
Months in unit			24.6	0.1
			[23.6]	(1.6)
Previously took ERAP			0.36	-0.08
				(0.03)
Total owed			3,079	-477
			[3,012]	(198)
F-statistic				3.41
p-value				0.323
N	5,586	814	1,018	

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 attorney (1)	Judgment within 30 days (2)	Judgment within 60 days (3)	Judgment within 90 days (4)	Amount owed in judgment (5)	Nonsuit within 60 days (6)	Writ within 60 days (7)	Continuance within 60 days (8)	Days left in unit (9)
ITT: first offer	0.603	-0.097	-0.163	-0.165	-815	0.084	-0.092	0.207	23.9
	(0.026)	(0.020)	(0.031)	(0.033)	(189)	(0.026)	(0.020)	(0.031)	(4.2)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]
IV: has lawyer		-0.162	-0.276	-0.274	-1,341	0.144	-0.156	0.344	39.6
		(0.033)	(0.051)	(0.054)	(314)	(0.043)	(0.033)	(0.051)	(7.0)
		[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]
Control mean	0.000	0.153	0.453	0.547	2,277	0.130	0.144	0.398	100.5
N total	1,045	1,045	1,018	977	1,045	1,018	1,018	1,018	1,045
N assigned attorneys	275	275	265	257	275	265	265	265	275

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 attorney (1)	Judgment within 30 days (2)	Judgment within 60 days (3)	Judgment within 90 days (4)	Amount owed in judgment (5)	Nonsuit within 60 days (6)	Writ within 60 days (7)	Continuance within 60 days (8)	Days left in unit (9)
Panel A. With ERAP Available (March–December 2022)									
ITT: first offer	0.575	-0.082	-0.203	-0.207	-1,106	0.117	-0.137	0.196	31.0
	(0.032)	(0.024)	(0.037)	(0.039)	(207)	(0.032)	(0.022)	(0.039)	(5.3)
	[0.000]	[0.001]	[0.000]	(0.000)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
IV: has lawyer		-0.145	-0.352	-0.360	-1,895	0.206	-0.238	0.339	54.1
		(0.041)	(0.064)	(0.068)	(365)	(0.056)	(0.040)	(0.066)	(9.1)
		[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Control mean	0.000	0.123	0.400	0.513	2,141	0.123	0.144	0.385	107.0
N total	687	687	687	687	687	687	687	687	687
N assigned attorneys	163	163	163	163	163	163	163	163	163
Panel B. Without ERAP Available (January–July 2023)									
ITT: first offer	0.651	-0.124	-0.089	-0.076	-306	0.024	-0.002	0.226	11.2
	(0.042)	(0.036)	(0.056)	(0.059)	(370)	(0.041)	(0.041)	(0.054)	(6.9)
	[0.000]	[0.001]	[0.110]	[0.198]	[0.409]	[0.561]	[0.965]	[0.000]	[0.107]
IV: has lawyer		-0.190	-0.148	-0.112	-476	0.048	-0.021	0.349	16.7
		(0.055)	(0.085)	(0.088)	(580)	(0.064)	(0.061)	(0.080)	(10.6)
		[0.001]	[0.081]	[0.200]	[0.412]	[0.458]	[0.730]	[0.000]	[0.117]
Control mean	0.000	0.214	0.570	0.635	2,558	0.145	0.145	0.425	87.0
N total	358	358	331	290	358	331	331	331	358
N assigned attorneys	112	112	102	94	112	102	102	102	112
Panel C. Difference in Treatment Effects: After Minus Before									
ITT: first offer	0.075	-0.042	0.113	0.132	800	-0.093	0.135	0.030	-19.8
	(0.053)	(0.044)	(0.067)	(0.071)	(421)	(0.052)	(0.046)	(0.066)	(8.7)
	[0.158]	[0.338]	[0.089]	[0.062]	[0.058]	[0.075]	[0.004]	[0.650]	[0.023]
IV: has lawyer		-0.045	0.204	0.248	1,419	-0.158	0.217	0.010	-37.4
		(0.069)	(0.106)	(0.111)	(678)	(0.085)	(0.073)	(0.103)	(14.0)
		[0.514]	[0.054]	[0.025]	[0.036]	[0.063]	[0.003]	[0.926]	[0.008]

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: Survey Participation Rate and Attrition

	Survey (all) (1)	Survey (phone) (2)	Survey (web) (3)
N completes	267	199	68
Share attempted to reach	0.87	0.70	0.69
Conditional participation rate:	0.29	0.26	0.09
Offer − No Offer	0.05	0.06	0.00
	(0.03)	(0.04)	(0.03)
	[0.117]	[0.081]	[0.928]
Judgment − No Judgment	-0.04	-0.03	-0.02
	(0.03)	(0.04)	(0.03)
	[0.237]	[0.345]	[0.617]

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 5: Survey Descriptive Results

	Formally evicted	Informally evicted	Not evicted
Share of total evicted	0.80	0.20	
Panel A. Agreement details			
Agreed to pay as share of owed	0.69	0.66	0.89
	(0.04)	(0.09)	(0.06)
Tried to bargain	0.53	0.46	0.62
	(0.04)	(0.09)	(0.05)
Out of pocket (extensive)	0.21	0.14	0.22
	(0.04)	(0.06)	(0.04)
Out of pocket (amount)	281	140	253
	(75)	(76)	(82)
Panel B. Outcomes			
Stayed in homeless shelter	0.22	0.31	0.05
	(0.04)	(0.08)	(0.02)
Moved	0.65	0.63	0.33
	(0.04)	(0.08)	(0.05)
Current rent	682	792	855
	(45)	(84)	(51)
Employed	0.62	0.71	0.65
	(0.04)	(0.08)	(0.05)
Current income	1837	2154	1940
	(103)	(227)	(158)
Went to court	0.88	0.83	0.70
	(0.03)	(0.06)	(0.05)
Panel C. Landlord relationship			
Never saw landlord	0.42	0.26	0.31
	(0.04)	(0.07)	(0.05)
Good landlord relationship	0.15	0.20	0.26
	(0.03)	(0.07)	(0.04)
N	136	35	96

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 6: 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	0.685	-0.144	-0.217	0.009	-0.052	-0.039	0.029	-0.057	-0.031
	(0.047)	(0.061)	(0.063)	(0.043)	(0.064)	(0.067)	(0.054)	(0.031)	(0.040)
	[0.000]	[0.018]	[0.001]	[0.832]	[0.416]	[0.560]	[0.594]	[0.068]	[0.437]
IV: has lawyer		-0.211	-0.317	0.013	-0.076	-0.057	0.042	-0.079	-0.044
		(0.085)	(0.091)	(0.061)	(0.092)	(0.096)	(0.077)	(0.042)	(0.055)
		[0.013]	[0.000]	[0.828]	[0.405]	[0.551]	[0.585]	[0.062]	[0.424]
Control mean	0.000	0.397	0.603	0.126	0.556	0.556	0.781	0.117	0.106
N total	267	267	267	267	267	267	267	267	267
N assigned attorneys	85	85	85	85	85	85	85	85	85

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 7: 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	-$10.57
	(1.7)		
Hospital visits	0.188 visits	$118.96	-$12.13
	(0.094)		
Earnings (over two years)	-$936	$120.74	-$12.32
Other effects			
Writs	50%	$67.5	-$0.27

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. (2023b). 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. (2023b) 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 in the post-ERAP period: reduces the rate of eviction judgments by 10.2 p.p.s, reduces the rate of writs by 0.4 p.p.s.

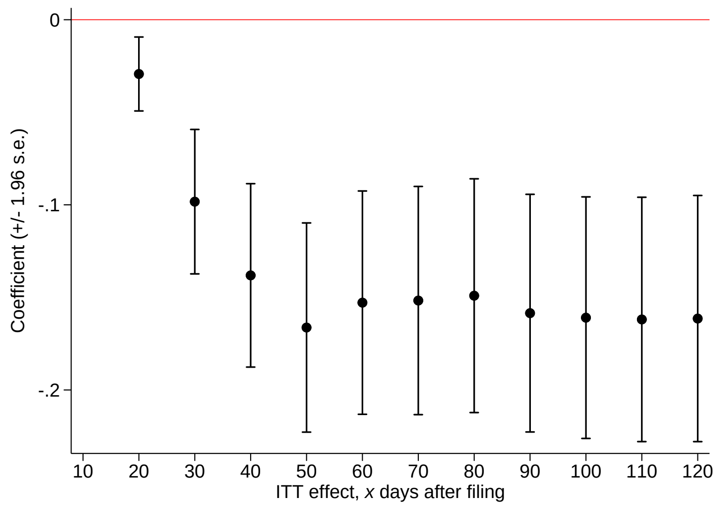
Appendices for Online Publication

Contents

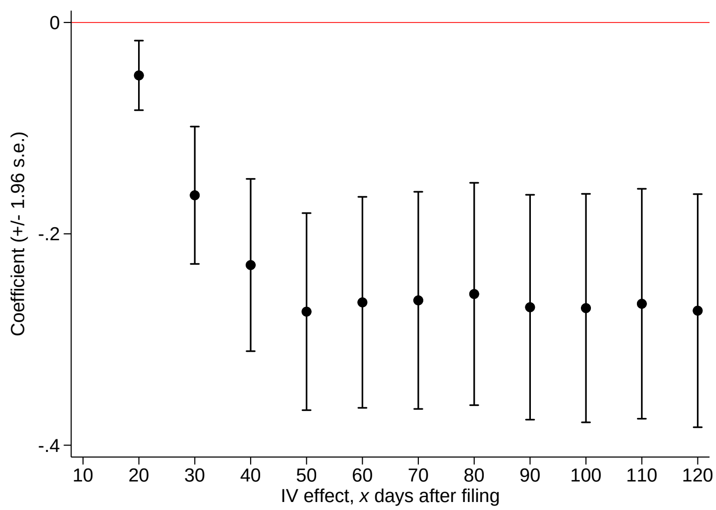
A Additional Figures

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

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



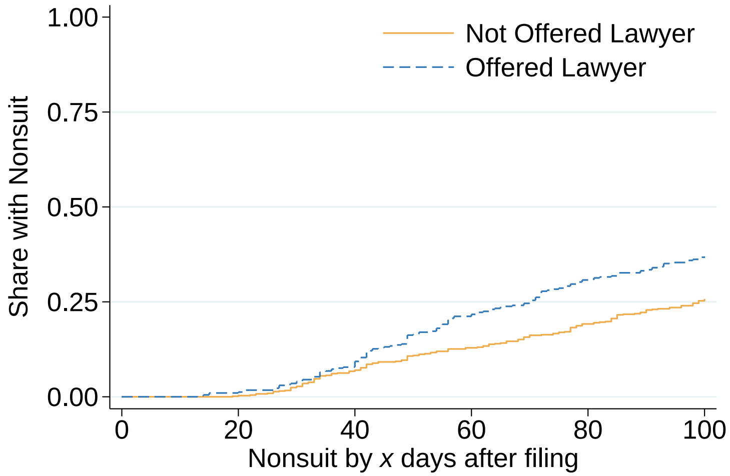
(b) IV on Judgments at 10, 20, . . . , 120 days



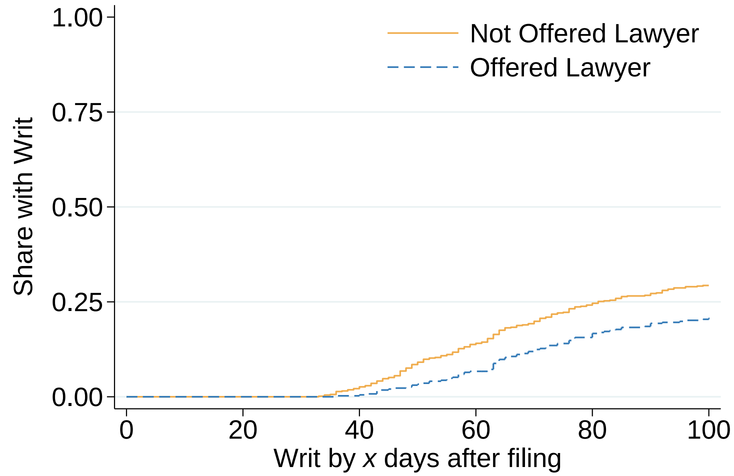
Note: This figure shows the estimates on judgments at various windows of time since eviction filing. Panel A plots ITT estimates and Panel B plots IV estimates for different window lengths. We use the specification in Table 2.

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

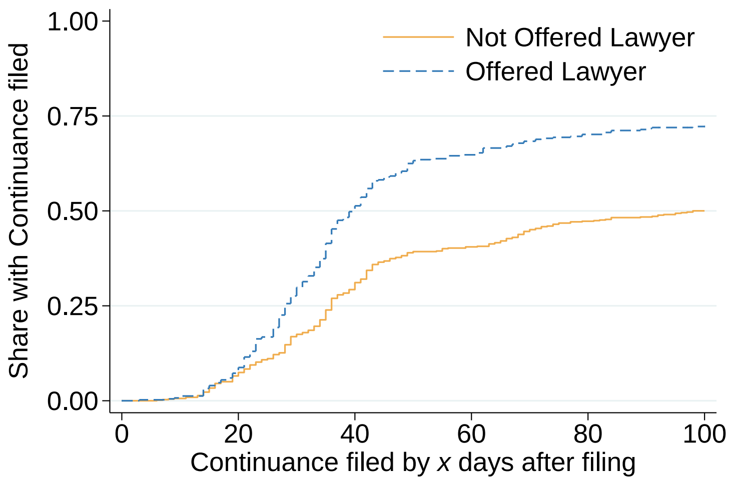
(a) Nonsuits



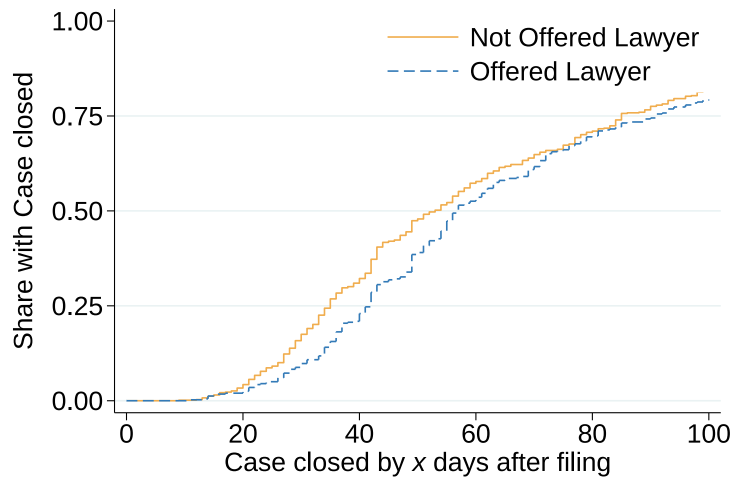
(b) Writs



(c) Continuances



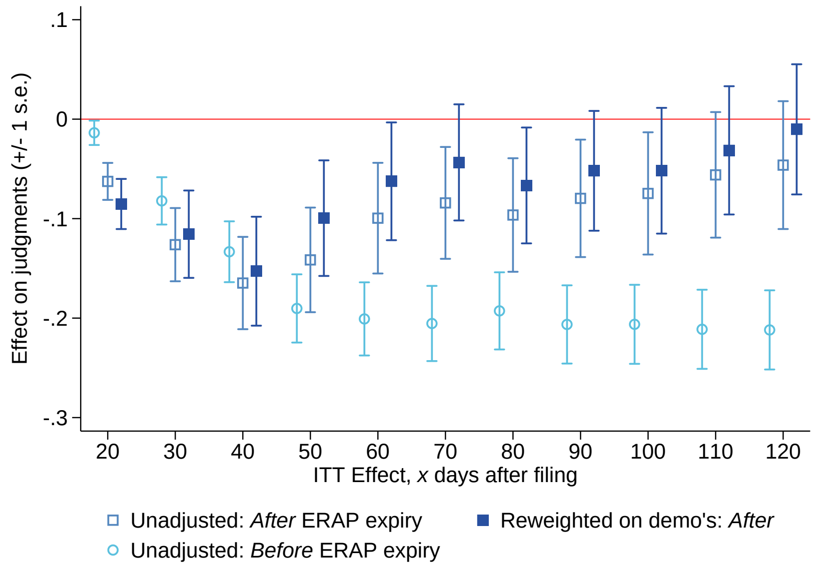
(d) Time in court



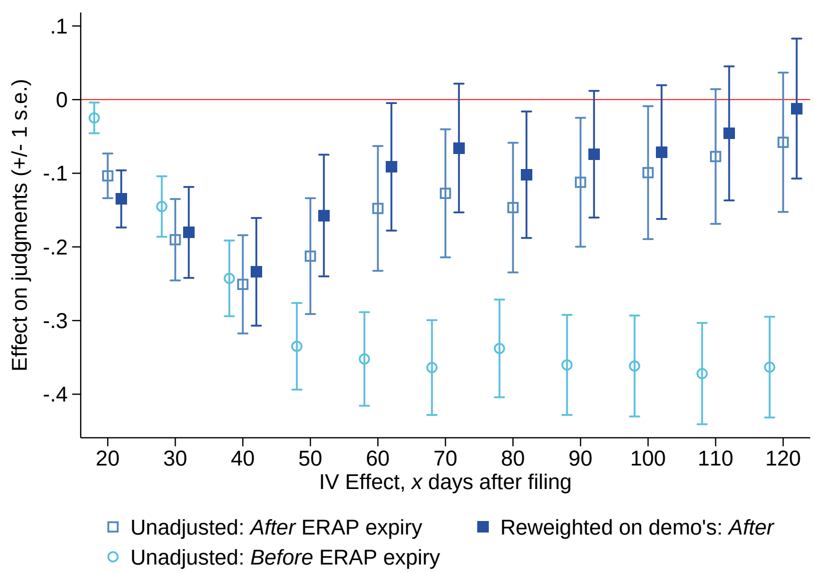
Note: This figure plots secondary outcomes using Kaplan-Meier failure curves, split by whether the individual was or was not offered at attorney. Panel A shows tenants offered an attorney (blue, dashed line in all plots) are significantly more likely to resolve their case with a nonsuit than those not 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

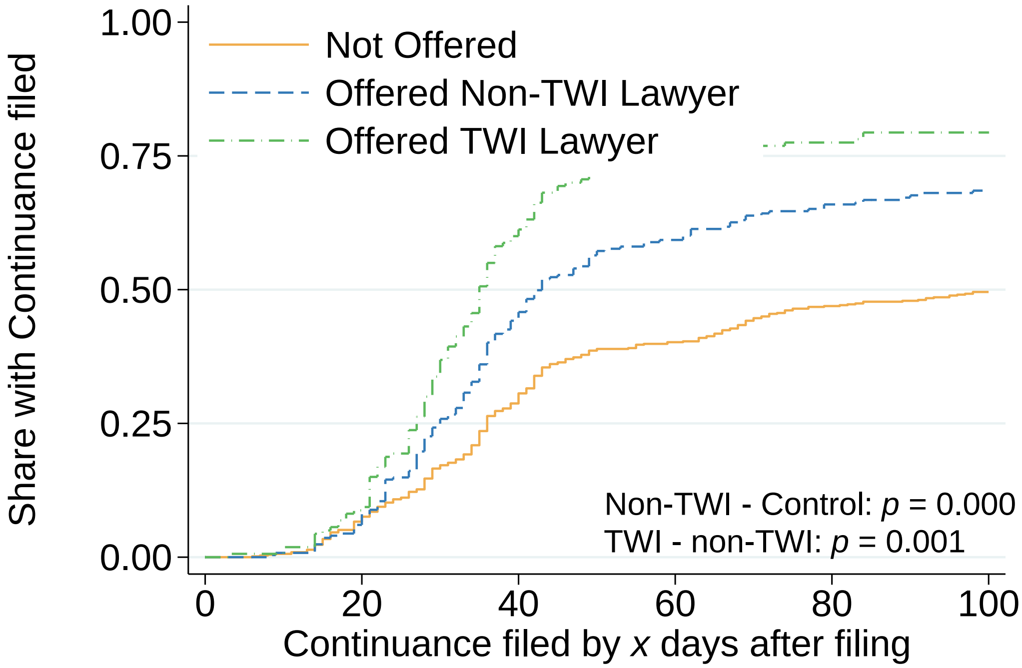


(b) Judgments by TWI vs. Other Attorneys



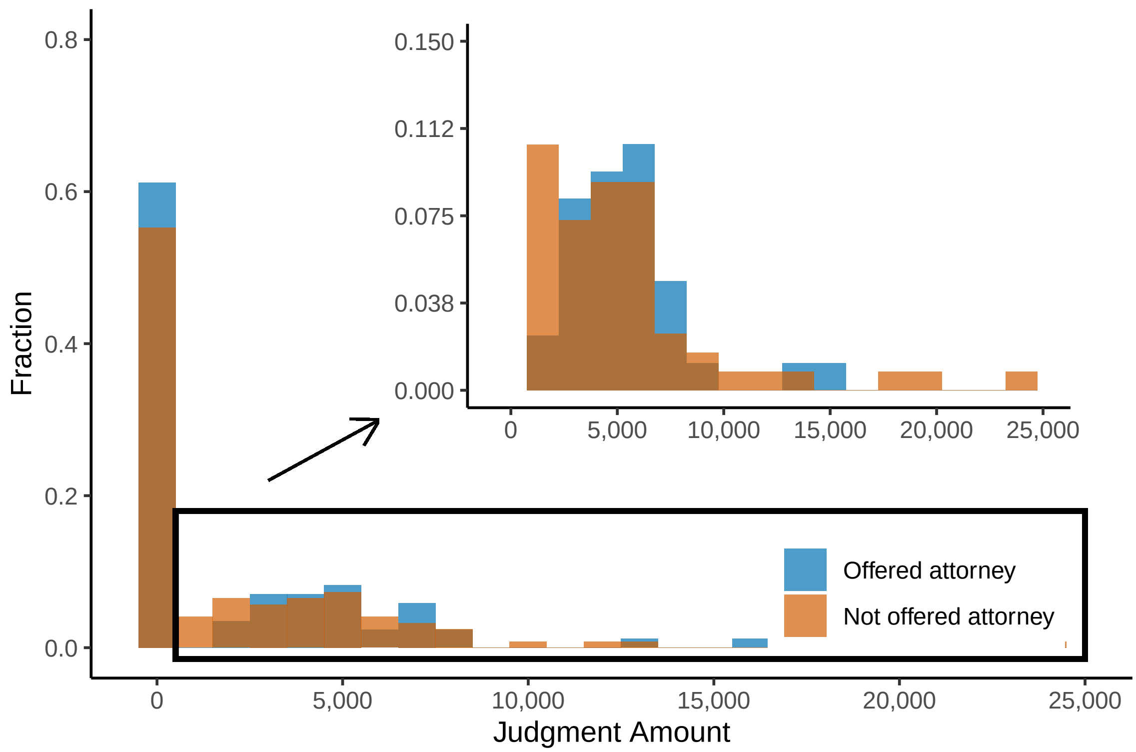
Note: The unadjusted estimates in this figure are identical estimates to Figure 4. The adjusted estimates use entropy rebalancing to reweight individuals in the post-ERAP expiry period to match those in the pre-expiry period (Hainmueller, 2012). The rebalancing demographics are the same as those selected for Lasso (see Section 3 for the list).

Figure A4: Continuances by The Works vs. Other Attorneys



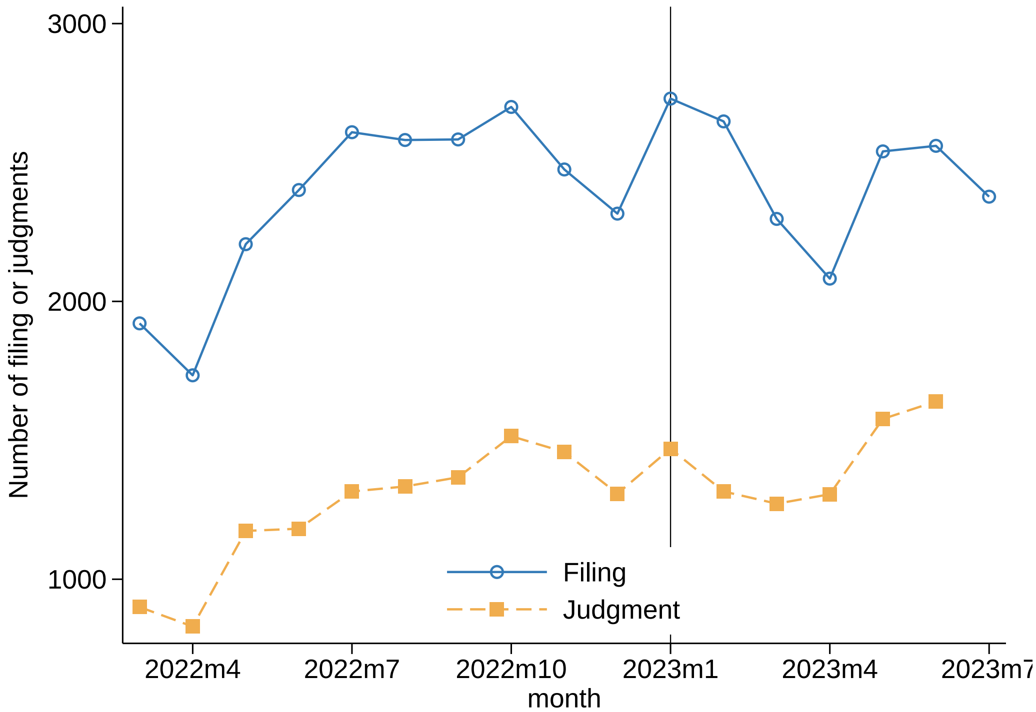
Note: This figure plots Kaplan-Meier failure curves separately for the likelihood of filing a continuance for TWI (green, dashed line) and non-specialized, ”low bono” attorneys recruited by TWI (blue, dotted line) relative to the control group (orange, solid line). p-values should be interpreted cautiously, 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: Monthly Evictions: March 2022–July 2023

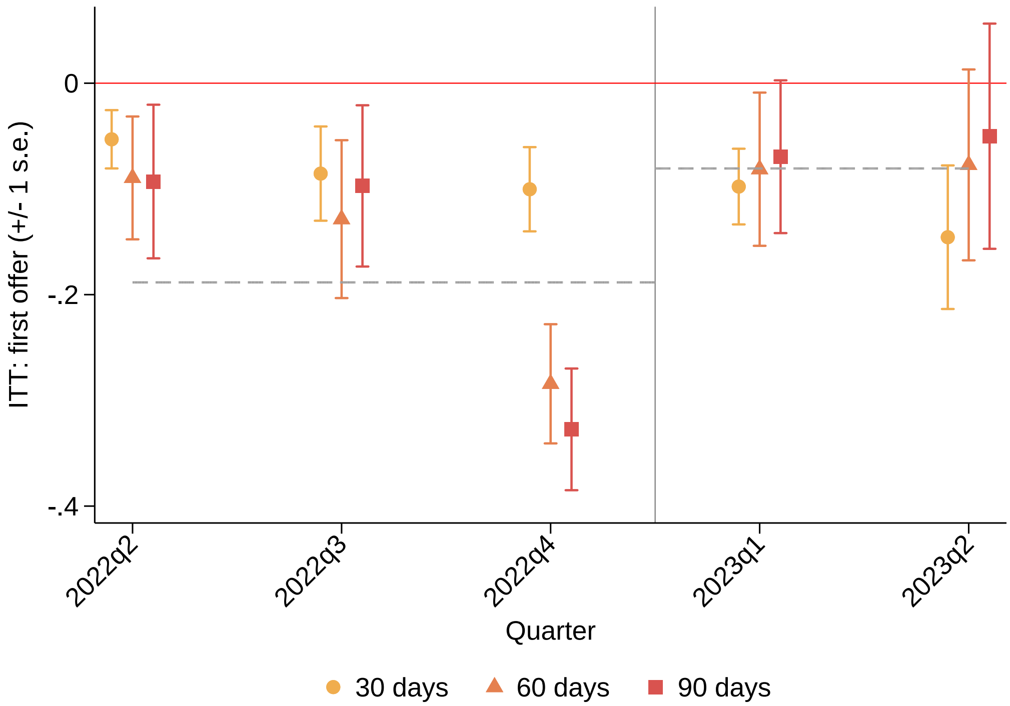


Note: This figure plots the monthly eviction filings and judgments during the listed time period. We list judgments only through June, as they would not have time to resolve by July.

Figure A7: Demographics: Before and After ERAP Expiry

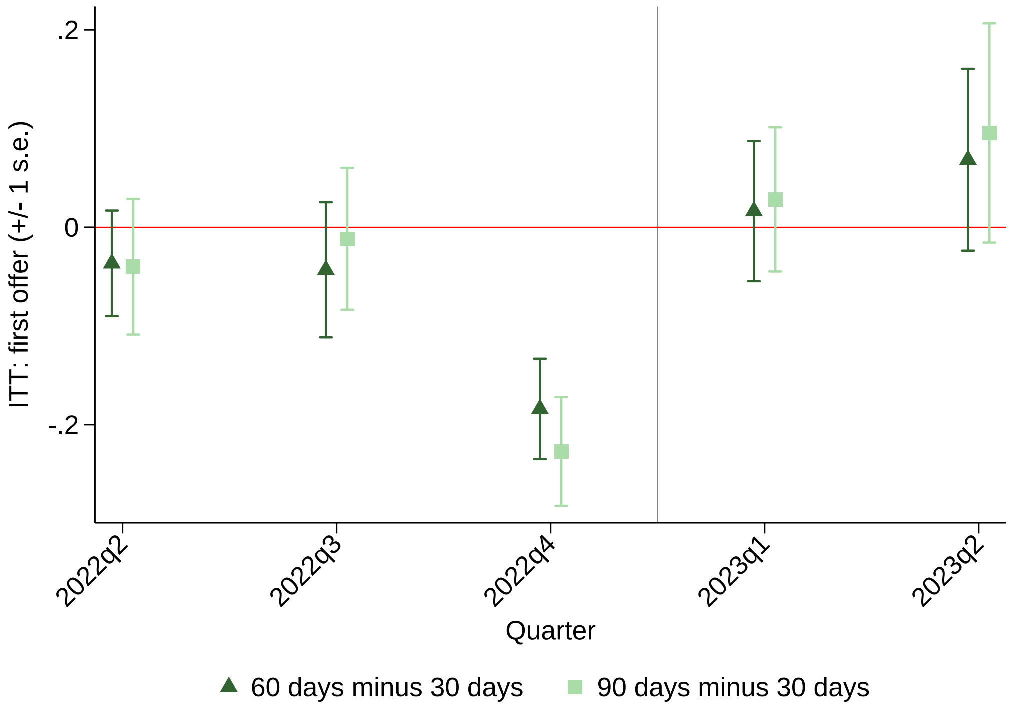
Note: This figure plots how demographics evolve for cohorts that apply in each quarter before and after ERAP expiry. Applicants in Feburary 2022 and July 2023 are aggregated at the endpoints. The vertical line indicates the beginning of ERAP.

Figure A8: Finer Time Variation: Before and After ERAP



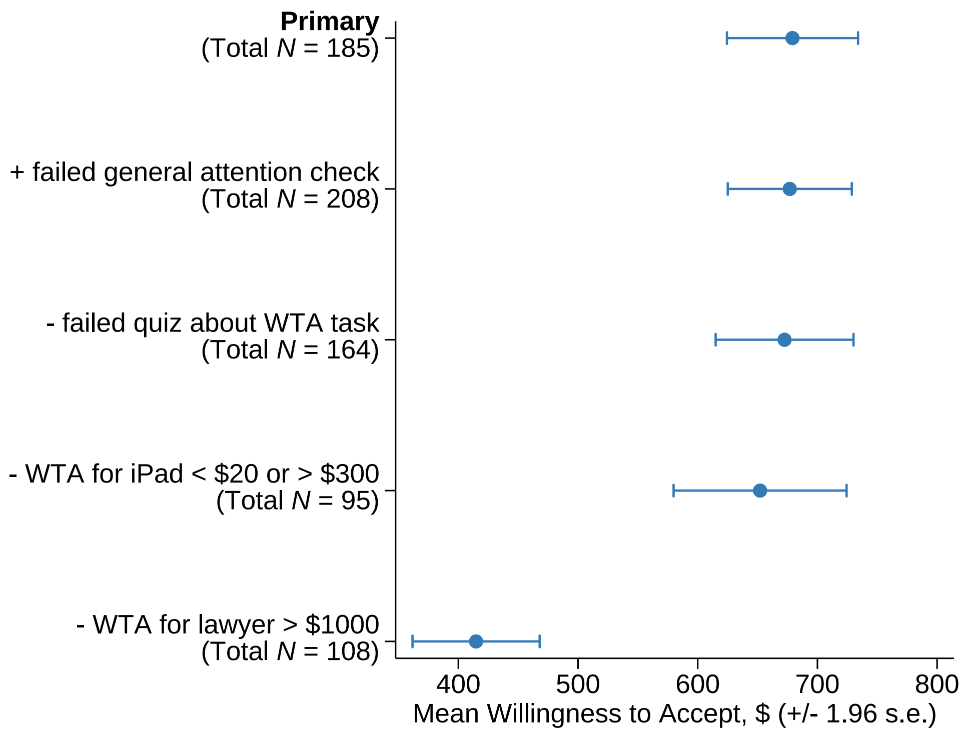
Note: This figure plots treatment effects on judgments at 30, 60, and 90 days using the main specification (Table 2). We reweight to adjust for dropping people selected in counseling lotteries but do not use the Lasso procedure to select controls. Applicants in Feburary 2022 and July 2023 are aggregated 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 A9: Finer Time Variation: Before and After ERAP



Note: This figure plots treatment effects on judgments at 60 and 90 days, relative to the treatment effect at 30 days, using the main specification (Table 2). These effects are the differences relative to the yellow bands in Figure A8. 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.

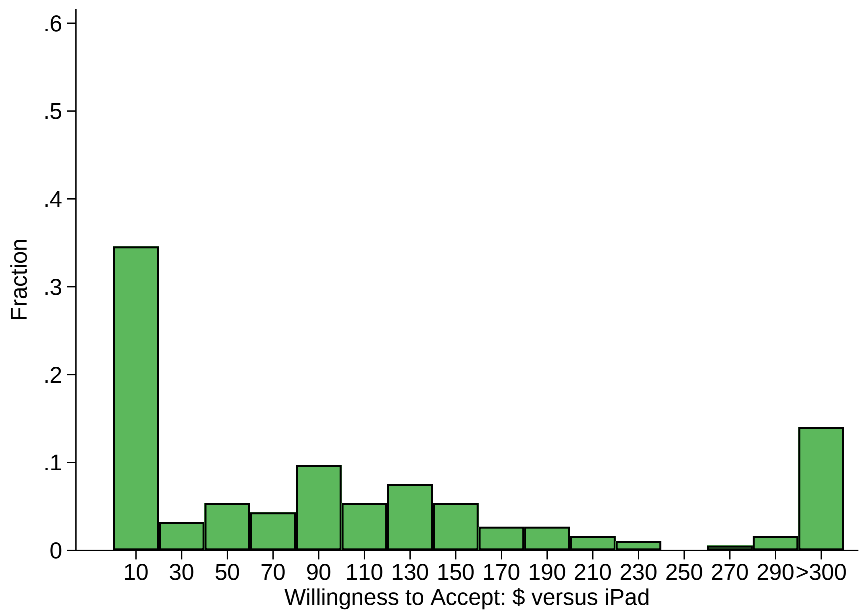
Figure A10: Robustness to Elicitation Error



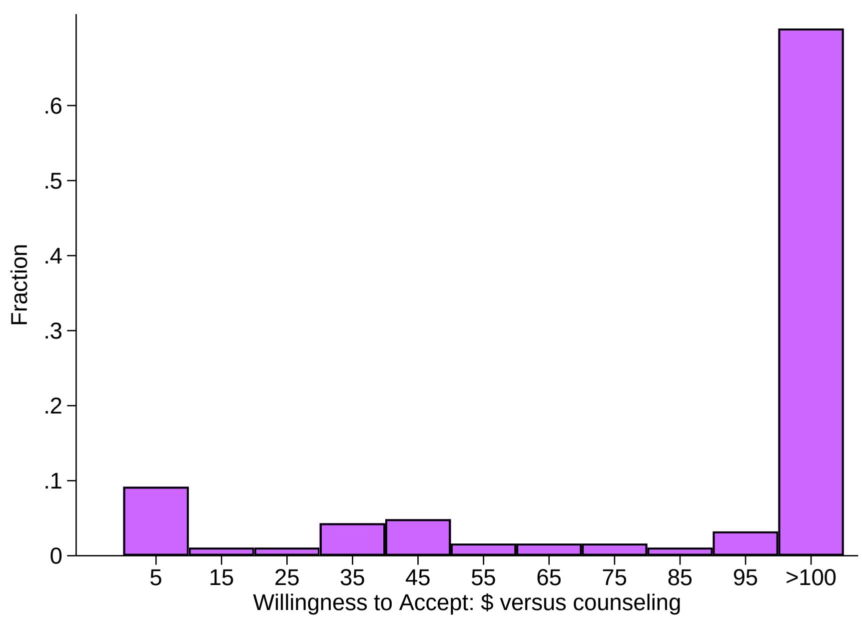
Note: This figure presents tests for the validity of the Willingness to Accept (WTA) elicitation. The first row shows our main estimate. The second row includes participants who failed a general attention check. The third row includes people who failed a confirmation check, prior to the elicitation, 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

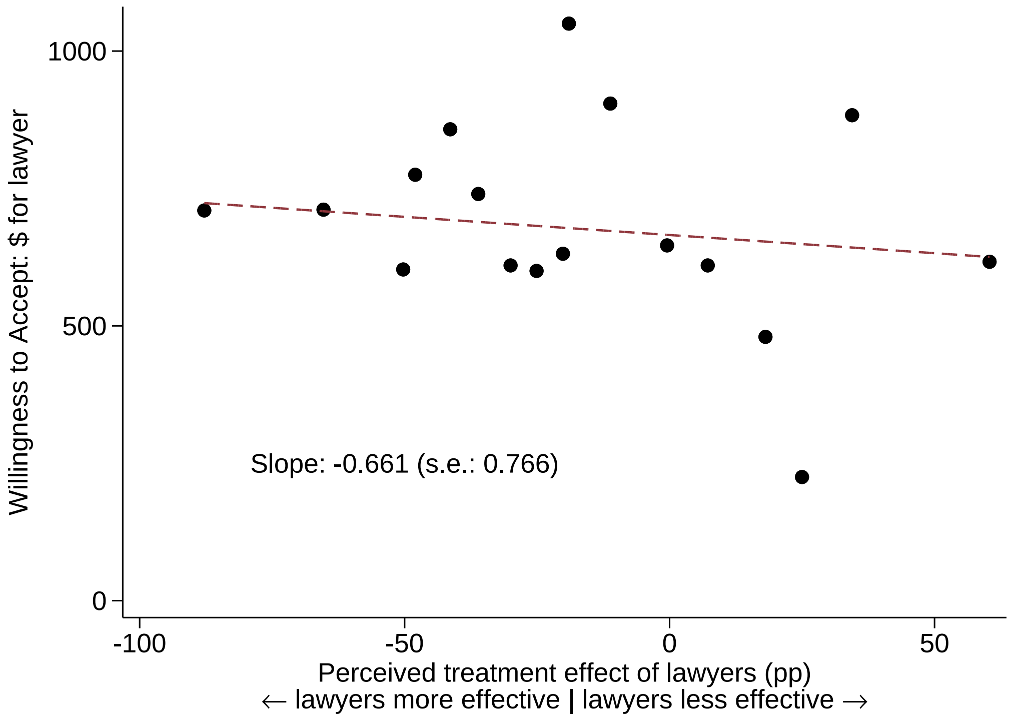


(b) Counselor



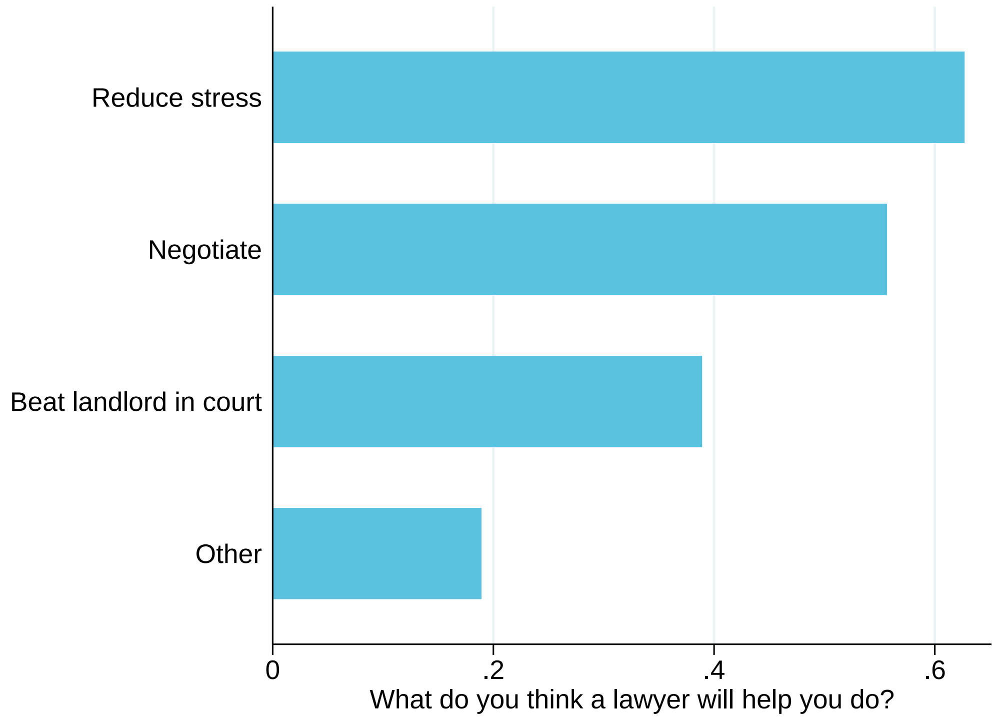
Note: This figure presents the distributions of Willingness to Accept for a reference good, an iPad, and for a counselor (social worker). Appendix D provides details about eliciting these measures.

Figure A12: Willingness to Accept a Lawyer versus Beliefs about Lawyers' Efficacy



Note: This figure reports the correlation between beliefs and Willingness to Accept cash versus a lawyer. The perceived treatment effect of lawyers is formed by computing perceptions about the average share with judgments with and without lawyers.

Figure A13: 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.

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 attorney (1)	Judgment within 30 days (2)	Judgment within 60 days (3)	Judgment within 90 days (4)	Amount owed in judgment (5)	Nonsuit within 60 days (6)	Writ within 60 days (7)	Continuance within 60 days (8)	Days left in unit (9)
ITT: first offer	0.607	-0.095	-0.156	-0.157	-740	0.085	-0.091	0.206	22.5
	(0.025)	(0.018)	(0.029)	(0.032)	(175)	(0.025)	(0.019)	(0.030)	(4.0)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]
IV: has lawyer		-0.156	-0.259	-0.261	-1,283	0.144	-0.151	0.340	37.5
		(0.030)	(0.048)	(0.052)	(290)	(0.041)	(0.030)	(0.048)	(6.6)
		[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]
Control mean	0.000	0.152	0.443	0.538	2,276	0.134	0.142	0.401	100.9
N total	1,165	1,165	1,134	1,082	1,165	1,134	1,134	1,134	1,165
N assigned attorneys	278	278	268	260	278	268	268	268	278

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 attorney (1)	Judgment within 30 days (2)	Judgment within 60 days (3)	Judgment within 90 days (4)	Amount owed in judgment (5)	Nonsuit within 60 days (6)	Writ within 60 days (7)	Continuance within 60 days (8)	Days left in unit (9)
Panel A. With ERAP Available (March–December 2022)									
ITT: first offer	0.578	-0.087	-0.190	-0.202	-1,064	0.116	-0.128	0.200	30.2
	(0.032)	(0.022)	(0.034)	(0.037)	(193)	(0.031)	(0.020)	(0.036)	(4.9)
	[0.000]	[0.000]	[0.000]	(0.000)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
IV: has lawyer		-0.150	-0.330	-0.346	-1,854	0.203	-0.223	0.346	51.9
		(0.037)	(0.059)	(0.065)	(340)	(0.054)	(0.036)	(0.062)	(8.5)
		[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Control mean	0.000	0.126	0.387	0.503	2,181	0.124	0.138	0.385	107.6
N total	776	776	776	776	776	776	776	776	776
N assigned attorneys	163	163	163	163	163	163	163	163	163
Panel B. Without ERAP Available (January–July 2023)									
ITT: first offer	0.662	-0.108	-0.086	-0.064	-191	0.029	-0.014	0.215	9.3
	(0.041)	(0.033)	(0.054)	(0.058)	(349)	(0.040)	(0.038)	(0.052)	(6.7)
	[0.000]	[0.001]	[0.114]	[0.271]	[0.585]	[0.468]	[0.722]	[0.000]	[0.166]
IV: has lawyer		-0.164	-0.128	-0.096	-314	0.045	-0.023	0.328	13.8
		(0.050)	(0.082)	(0.086)	(523)	(0.062)	(0.056)	(0.076)	(10.1)
		[0.001]	[0.116]	[0.267]	[0.548]	[0.469]	[0.687]	[0.000]	[0.171]
Control mean	0.000	0.210	0.574	0.637	2,483	0.157	0.153	0.440	86.5
N total	389	389	358	306	389	358	358	358	389
N assigned attorneys	115	115	105	97	115	105	105	105	115
Panel C. Difference in Treatment Effects: After Minus Before									
ITT: first offer	0.084	-0.020	0.104	0.138	873	-0.087	0.114	0.015	-20.9
	(0.051)	(0.040)	(0.064)	(0.069)	(399)	(0.051)	(0.043)	(0.063)	(8.3)
	[0.104]	[0.614]	[0.104]	[0.046]	[0.029]	[0.086]	[0.008]	[0.809]	[0.012]
IV: has lawyer		-0.014	0.201	0.250	1,539	-0.158	0.201	-0.018	-38.1
		(0.063)	(0.101)	(0.108)	(620)	(0.082)	(0.067)	(0.097)	(13.2)
		[0.823]	[0.046]	[0.021]	[0.013]	[0.053]	[0.003]	[0.849]	[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 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: Formal Outcomes, Robustness: No Controls

	Has attorney (1)	Judgment within 30 days (2)	Judgment within 60 days (3)	Judgment within 90 days (4)	Amount owed in judgment (5)	Nonsuit within 60 days (6)	Writ within 60 days (7)	Continuance within 60 days (8)	Days left in unit (9)
ITT: first offer	0.599	-0.095	-0.152	-0.158	-922	0.086	-0.086	0.198	23.1
	(0.026)	(0.020)	(0.031)	(0.033)	(194)	(0.025)	(0.020)	(0.032)	(4.2)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]
IV: has lawyer		-0.159	-0.254	-0.261	-1,539	0.144	-0.144	0.331	38.5
		(0.033)	(0.052)	(0.055)	(327)	(0.043)	(0.034)	(0.052)	(7.1)
		[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]
Control mean	0.000	0.153	0.453	0.547	2,277	0.130	0.144	0.398	100.5
N total	1,045	1,045	1,018	977	1,045	1,018	1,018	1,018	1,045
N assigned attorneys	275	275	265	257	275	265	265	265	275

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 A5: Formal Outcomes, Before and After ERAP Expiry: No Controls

	Has attorney (1)	Judgment within 30 days (2)	Judgment within 60 days (3)	Judgment within 90 days (4)	Amount owed in judgment (5)	Nonsuit within 60 days (6)	Writ within 60 days (7)	Continuance within 60 days (8)	Days left in unit (9)
Panel A. With ERAP Available (March–December 2022)									
ITT: first offer	0.575	-0.082	-0.188	-0.200	-1,172	0.118	-0.129	0.186	29.7
	(0.032)	(0.023)	(0.036)	(0.039)	(205)	(0.032)	(0.022)	(0.038)	(5.2)
	[0.000]	[0.000]	[0.000]	(0.000)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
IV: has lawyer		-0.142	-0.328	-0.348	-2,038	0.204	-0.224	0.323	51.6
		(0.040)	(0.063)	(0.068)	(364)	(0.055)	(0.039)	(0.066)	(9.1)
		[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Control mean	0.000	0.123	0.400	0.513	2,141	0.123	0.144	0.385	107.0
N total	687	687	687	687	687	687	687	687	687
N assigned attorneys	163	163	163	163	163	163	163	163	163
Panel B. Without ERAP Available (January–July 2023)									
ITT: first offer	0.640	-0.119	-0.081	-0.064	-493	0.025	0.000	0.222	11.6
	(0.043)	(0.037)	(0.057)	(0.060)	(396)	(0.042)	(0.041)	(0.055)	(7.1)
	[0.000]	[0.001]	[0.155]	[0.279]	[0.213]	[0.551]	[0.993]	[0.000]	[0.099]
IV: has lawyer		-0.186	-0.126	-0.097	-771	0.039	0.001	0.347	18.2
		(0.058)	(0.088)	(0.089)	(614)	(0.065)	(0.063)	(0.082)	(11.0)
		[0.001]	[0.151]	[0.279]	[0.210]	[0.550]	[0.993]	[0.000]	[0.098]
Control mean	0.000	0.214	0.570	0.635	2,558	0.145	0.145	0.425	87.0
N total	358	358	331	290	358	331	331	331	358
N assigned attorneys	112	112	102	94	112	102	102	102	112
Panel C. Difference in Treatment Effects: After Minus Before									
ITT: first offer	0.065	-0.037	0.108	0.136	678	-0.093	0.129	0.036	-18.0
	(0.054)	(0.044)	(0.067)	(0.071)	(446)	(0.052)	(0.046)	(0.067)	(8.8)
	[0.226]	[0.397]	[0.110]	[0.057]	[0.129]	[0.076]	[0.005]	[0.591]	[0.040]
IV: has lawyer		-0.044	0.202	0.251	1,267	-0.166	0.224	0.024	-33.4
		(0.070)	(0.108)	(0.112)	(714)	(0.085)	(0.074)	(0.105)	(14.3)
		[0.533]	[0.062]	[0.025]	[0.076]	[0.052]	[0.002]	[0.822]	[0.019]

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 A6: 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	0.686	-0.127	-0.189	0.011	-0.038	-0.043	0.029	-0.044	-0.010
	(0.050)	(0.064)	(0.067)	(0.049)	(0.068)	(0.071)	(0.057)	(0.032)	(0.037)
	[0.000]	[0.047]	[0.005]	[0.823]	[0.578]	[0.551]	[0.609]	[0.170]	[0.792]
IV: has lawyer		-0.186	-0.275	0.016	-0.055	-0.062	0.043	-0.061	-0.014
		(0.088)	(0.093)	(0.069)	(0.096)	(0.102)	(0.081)	(0.043)	(0.051)
		[0.036]	[0.003]	[0.818]	[0.568]	[0.543]	[0.601]	[0.159]	[0.786]
Control mean	0.000	0.397	0.603	0.126	0.556	0.556	0.781	0.117	0.106
N total	267	267	267	267	267	267	267	267	267
N assigned attorneys	85	85	85	85	85	85	85	85	85

Note: Parentheses show robust standard errors. Brackets show p-values. Relative to Table 6, 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 A7: 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	0.687	-0.183	-0.251	0.017	-0.075	-0.041	0.033	-0.073	-0.041
	(0.048)	(0.064)	(0.064)	(0.045)	(0.066)	(0.069)	(0.057)	(0.034)	(0.047)
	[0.000]	[0.005]	[0.000]	[0.708]	[0.262]	[0.549]	[0.559]	[0.031]	[0.375]
IV: has lawyer		-0.267	-0.365	0.025	-0.109	-0.060	0.049	-0.100	-0.058
		(0.090)	(0.091)	(0.064)	(0.095)	(0.098)	(0.081)	(0.045)	(0.063)
		[0.003]	[0.000]	[0.701]	[0.253]	[0.540]	[0.549]	[0.027]	[0.360]
Control mean	0.000	0.397	0.603	0.126	0.556	0.556	0.781	0.117	0.106
N total	267	267	267	267	267	267	267	267	267
N assigned attorneys	85	85	85	85	85	85	85	85	85

Note: Parentheses show robust standard errors. Brackets show p-values. Relative to Table 6, 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 A8: Demographics of Survey Sample and Additional Attrition Tests

	Sample (1)	Endline (2)	Endline − sample (3)	Endline: Treatment − Control (4)
Demographics:				
Age	34.1	34.5	0.7 (0.7) [0.356]	-0.7 (1.2) [0.525]
Black	0.94	0.93	-0.01 (0.02) [0.785]	-0.01 (0.03) [0.760]
Female	0.79	0.87	0.12 (0.03) [0.000]	-0.05 (0.04) [0.225]
Household size	2.6	2.8	0.2 (0.1) [0.113]	0.3 (0.2) [0.254]
HS or less	0.66	0.64	-0.02 (0.04) [0.503]	-0.06 (0.06) [0.346]
Single	0.89	0.87	-0.03 (0.02) [0.246]	-0.01 (0.05) [0.786]
Economic status:				
Monthly income	1,312	1,282	-42 (87) [0.630]	32 (139) [0.818]
Monthly rent	940	921	-27 (28) [0.329]	10 (53) [0.854]
Housing security:				
Applied after ERAP expiry	0.23	0.28	0.07 (0.03) [0.018]	0.02 (0.05) [0.759]
Ever evicted before application	0.33	0.39	0.07 (0.04) [0.036]	-0.05 (0.06) [0.419]
Months in unit	24.6	26.8	3.1 (1.7) [0.077]	-3.9 (3.2) [0.220]
Previously took ERAP	0.37	0.44	0.10 (0.04) [0.007]	-0.08 (0.07) [0.233]
Total owed at application	3,144	2,975	-239 (251) [0.342]	-1,028 (445) [0.022]
Program outcomes:				
Selected in initial lottery	0.34	0.38	0.06 (0.03) [0.087]	1.00 (0.00) [0.000]
Has attorney	0.23	0.30	0.09 (0.03) [0.004]	0.68 (0.05) [0.000]
Judgment	0.55	0.53	-0.03 (0.04) [0.398]	-0.24 (0.06) [0.000]
N	262	262		
Joint p-value (non-program outcomes)			0.000	0.014

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 A9: 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. (2023b). 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. (2023b) 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

C Experiment Details

C.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.

C.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.

C.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.

D Baseline Survey Details

D.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 NPI, 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 NPI 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.[32]

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.

D.2 Elicitation Checks and Normative Tests

Elicitation Checks. Why are valuations so high? The market value for an eviction defense lawyer is about $350, according to NPI attorneys, so the valuations here suggest tenants may be leaving money on the table. Tenant valuations could be high for a combination of reasons: for instance, tenants may not know the market value; may not know how to find lawyers; or may value or trust NPI lawyers more than other lawyers.

A natural concern when seeing such high valuations is poor data quality due to comprehension issues or expressive responses. We took several steps within the survey to achieve high-quality responses. First, we randomize the order of whether cash or lawyers are presented first in the multiple price list, across participant. Thus, participants do not seem to select lawyers first because they simply click the button to the right, for instance. Second, we included many comprehension checks and bolded wording indicating that the choice could be implemented.

To additionally examine the role of confusion or elicitation issues, we conducted a WTA elicitation for a benchmark good, an iPad, using a nearly identical elicitation strategy (Figure A11A). In the survey, we elicited valuations of this good using a nearly identical elicitation strategy. The distribution of valuations for iPads are not left-skewed, as they are for attorneys,

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

and appear reasonable. This valuation rules out that mechanical elicitation issues or confusion cause participants to select the largest valuation. Moreover, in Figure A11B, we elicit the value of eviction counselors who could assist tenants with finding resources. This valuation is highly left-skewed, as for lawyers. The combination of these panels suggests that tenants indeed have high demand for eviction resources, rather than elicitation issues.

We quantify how large confusion might be in Figure A10. Our primary estimates drop participants who failed an attention check. When we include the 10 participants who failed the check, we find nearly identical results. Before doing the WTA task, we include a confirmation check. Our primary estimates include people who fail this check, because we inform participants of the right answer if they fail. Dropping these tenants gives nearly identical responses. We also drop people who report very low or high values for the iPad elicitation, since they may be people who just click the left or right button on the multiple-price-list to speed to the end of the survey. This depresses the valuation only modestly, to about $650, and the difference is not significant. Together, these tests suggest that inattention or numeracy issues that we can observe is not correlated with demand and thus casts doubt on the hypothesis that confusion that we do not observe is correlated with demand. As a final test, we drop anyone who reports the highest valuation of the WTA and arrive at a valuation around $400, which is still higher than the market price of a lawyer.

Extracting a Normative Benchmark. Should we respect tenants' demand when computing the MVPF? If tenants' demand is driven by behavioral bias like misinformation, then what they report in a demand survey may not be normatively respectable (Bernheim and Taubinsky, 2018). We now show that the high valuation we find is indeed normatively respectable, meaning it does not emerge from behavioral biases introduce a wedge between decision and experienced utility.

We begin by testing the role of beliefs. We categorize people as having 'accurate' beliefs if their implied treatment effect of lawyers is that attorneys reduce the effect of judgments by between 10 and 50 pp.[33] People with accurate beliefs have almost identical demand as people with inaccurate beliefs (Figure 6). Indeed, beliefs are only weakly correlated with demand for lawyers (Figure A12).

Next, if tenants are highly budget constrained, their demand for lawyers may be difficult to interpret and might, for instance, reflect present-focused demand for cash now. This concern would have more bite if valuations were low. Nevertheless, to test this concern, we embedded a subexperiment into the WTA elicitations. Half of participants' WTA are elicited in a state of the world where, if the tenant's choice is implemented, they are additionally endowed with $500, regardless of what they choose. The other half are elicited in a state of the world where they are first endowed with $50.

[33] All endline surveys occur after ERAP expiry.

The second row in Figure 6 shows that relaxing the budget constraint by endowing people with $500 has no material effects on WTA. If anything, relaxing the budget constraint reduces demand.

Drivers of Demand. We next study what *does* correlate with demand for lawyers among applicants (subsequent tests in Figure 6). Neither optimistic beliefs, nor trust in lawyers relative to others in the Trust Game, correlate with beliefs. However, tenants who apply are selected: They trust lawyers enough, and have high enough beliefs in their efficacy, to apply in the first place. We do find a role for tenants' self-reports about whether they can pay their bills.

We conclude by asking tenants why they value lawyers (Figure A13). More than 60% of tenants report that they believe lawyers will help them negotiate and that lawyers will reduce stress. These results exceed the 39% who believe that lawyers will beat landlords in court. Calibrations that exclude unmeasured forces like negotiation and stress reduction risk underestimating tenants' WTP since those forces appear to matter. This provides a reason to prefer direct elicitation.