How Effective Are Limited Legal Assistance Programs? A Randomized Experiment in a Massachusetts Housing Court*

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Abstract

We persuaded entities conducting two civil Gideon pilot programs to randomize which potential clients would receive offers of traditional attorney-client relationships from legal aid staff attorneys and which would receive referrals to only limited ("unbundled") assistance. In both pilot programs potential clients were occupants in housing eviction disputes, and both programs were oversubscribed. In this article, we report the results of one of the two resulting randomized trials, which we label the "Housing Court Study," after the type of the court in which it took place. In the Housing Court Study, all study-eligible occupants could (and most did) receive assistance from a particular Service Provider's staff attorneys in filling out answer and discovery request forms. In addition, occupants not offered a traditional attorney-client relationship from the Provider's staff attorneys, *i.e.*, the control group, received a referral to that Provider's lawyer for the day ("LFTD") program. The LFTD program provided same-day-only representation to occupants in hallway settlement negotiations and mediation sessions but not in court appearances or in filing motions. We analyze the effect of the Provider's offer of full representation vis-à-vis the Provider's referring the potential client to the LFTD program. In doing so, we evaluate the relative effectiveness of two types of programs legal services providers might adopt.

We found no statistically significant evidence that the Provider's offer of full, as opposed to limited, representation had a large (or any) effect on the likelihood that the occupant would retain possession, on the financial consequences of the case, on

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judicial involvement in or attention to cases, or on any other litigation-related outcome of substantive import. To the contrary, treated and control group point estimates were close to one another. In addition, because about half of the potential clients in this program had contacted the Provider before litigation had been initiated, we were able to study whether the Provider's offer of full representation kept disputes out of court, although the limited size of the dataset made possible detection of only very large effects. We found no evidence of such very large effects.

We explore several possible interpretations of our results, and we caution against both over-interpretation and under-interpretation.

1 Introduction

This article reports the findings of a randomized trial comparing the effectiveness of two alternative programs of legal representation for occupants of housing units in the Massachusetts North Shore area. In this study, each study-eligible occupant facing eviction from her housing unit who contacted a particular Service Provider was randomized to one of two conditions. In the treatment condition, the occupant received an offer of a traditional attorney-client relationship from a Provider staff attorney. In the control condition, the Provider offered limited, or "unbundled," legal assistance if/when the occupant faced eviction litigation. The offered unbundled assistance consisted of instructional clinics that included help in filling out answer and discovery forms and of court-hearing-day-only representation in hallway settlement negotiations and mediation sessions (but not in court appearances or in filing motions). We compared results for the treated and control groups.[1]

The study we report here resulted from the efforts of the Boston Bar Association Task Force on Expanding the Civil Right to Counsel (the "Task Force"). In 2008, the Task Force issued a report (Boston Bar Association Task Force on Expanding the Civil Right to Counsel (2008)) identifying four substantive legal areas as those in which the need for a "civil Gideon" right[2] was thought most urgent: housing, family, juvenile, and immigration. The Task Force sought funding for nine pilot projects to illustrate how such a right might be implemented in practice and to demonstrate the effect that such a right might have on eligible populations. Three funders[3] provided sufficient money for two of the nine pilots,

[1]In statistical and econometric parlance, we estimated the "intention to treat" effect, meaning we compared the results for occupants offered treatment versus occupants offered control without regard to whether occupants in either set actually used the relevant services. We do, however, report so-called "compliance" information below.

[2]The term "civil Gideon" comes from the Supreme Court's decision in Gideon v. Wainwright, 372 U.S. 335 (1963), which held that indigent defendants have a right to counsel in criminal cases involving a substantial risk of incarceration. At present, no such blanket right to counsel exists in the civil context. See Lassiter v. Department of Social Services of Durham County, 452 U.S. 18 (1981), and Turner v. Rogers, 564 U.S. – (2011).

[3]The funders were the Boston Bar Foundation, the Massachusetts Bar Foundation, and the Boston Foundation. Their generosity in supporting this effort was extraordinary.

both in summary eviction proceedings. We agreed to serve as evaluators for both on the condition that we would be free to publish our findings. One of these pilots took place in a Massachusetts District Court; that study, which we label the "District Court Study," is reported in a separate article (Greiner et al. (2011)). In the present pilot, most of the cases that reached litigation were heard in one of the five Massachusetts specialty courts whose caseload consisted primarily of residential summary eviction cases. We report the results of this "Housing Court Study" in this article.

The Task Force's efforts took place against the backdrop of a larger movement, both nationally and in Massachusetts, toward "unbundled" or limited assistance representation. Jennings and Greiner (2012) defines these terms, briefly discusses the national and the state movements, and provides an extensive bibliography. For the purposes of this paper, it suffices to say the following: First, any coherent definition of unbundled representation would include advocacy in a lawyer for the day ("LFTD") program in which attorneys provide representation in hallway settlement negotiation sessions and in mediation sessions overseen by court personnel. Control group occupants in the present Housing Court Study were referred to such a program. Second, the overwhelming trend in the state courts of nearly every state is to amend ethical guidelines and civil rules, to provide appropriate training to judges and attorneys, and to otherwise encourage, unbundled legal assistance as a means both to promote access to justice and to manage the crush of pro set litigants. Third, prior to our studies, there had (to our knowledge) never been a rigorous quantitative evaluation of any form of unbundled legal assistance, either to determine how much is gained as compared to allowing the litigant to self-represent, or how much is lost as compared to providing the litigant with an offer of full representation.

As originally conceived, the Task Force's approach to both the District Court and Housing Court Studies was to test the viability of a civil Gideon strategy of identifying categories of cases as to which a full, traditional attorney-client relationship was thought to be particularly "needed." To identify the circumstances in which full representation might be needed, the Task Force informally surveyed experienced housing lawyers and judges, asking for their opinions on relevant and identifiable case characteristics. Based on this effort, the Task Force isolated three case categories: (i) the occupant had a disability related to the evictor's asserted reason for the eviction; (ii) the occupant allegedly committed criminal misconduct allegedly related to the asserted reason for the eviction; or (iii) the occupant was in danger of suffering a substantial injustice unless counsel were offered.[4] In part, the Task Force considered its approach as a way to reply to cost and administrability concerns raised by civil Gideon opponents (*e.g.*, Barton and Bibas (2011)). As explained below, however, it remains unclear how closely the Task Force's vision of offering full representation only (or even primarily) to cases in its three case categories was actually implemented in the present Housing Court Study.

In the Housing Court Study, the Task Force's funding was sufficient to produce a dataset consisting of 85 treated cases and 99 control cases. This modest size meant that we were likely to detect only large differences between treated and control groups. Given the massive oversubscription of legal aid programs (Legal Services Corporation (2009)), we believe that studies capable of detecting only large effects are valuable. The need in legal aid is such that if a program does not produce large effects, or if differences in outcomes between an expensive full-representation program and a less expensive (and more readily expandable) limited representation program are not large, providers and funders may wish to think carefully about whether to pursue the full representation model at a programmatic level.

A brief summary of our findings is as follows: We found no statistically significant evidence that the Provider's offer of a traditional attorney-client relationship, as compared to

[4]In making the substantial injustice determination, the Task Force referred to the following six factors: the occupant's potential vulnerability, the evictor's level of sophistication, whether the unit appeared to be affordable given the occupant's resources, the availability of defenses to an eviction action, the effect of an eviction on the occupant, and any power imbalance as between the evictor and the occupant (as might be induced, for example, if the evictor were represented).

a referral to the Provider's LFTD program,[5] had a large (or any) effect on the likelihood that the occupant would retain possession; on the financial consequences of the dispute; on the judicial involvement in or attention to litigation cases; or on any other outcome. To the contrary, the treated and control group point estimates on almost all measures were surprisingly close to one another. Finally, because about half of the potential clients contacted the Service Provider before litigation had been initiated, we could study whether the Provider's offer of full representation could keep disputes out of court, although the even more limited size of the relevant subset of the data made possible only detection of very large effects. No such very large effects were evident.

We discuss two possible interpretations of our findings below. In particular, we are uncomfortable with a definitive conclusion that the unbundled legal assistance program in place in the Housing Court Study duplicated the outcomes that would have been realized by a program of aggressive full representation.

2 Literature Review

Greiner and Pattanayak (2011) provides an extensive review of the quantitative literature evaluating the effect of an offer and/or the actual use of legal assistance of any form. Apart from the study reported in that paper, and apart from the Housing Court Study reported here and the companion District Court Study (Greiner et al. (2011)), there have been only three randomized assessments of any form of civil legal assistance in the United States: the two juvenile delinquency studies in different jurisdictions in Stapleton and Teitelbaum (1972), and the Manhattan eviction study reported in Seron et al. (2001). Greiner and Pattanayak (2011) summarizes the results of these studies, but as none involved limited assistance, we do not discuss them further here, except to say that they reach conflicting

[5] As we make clear below, all occupants (in both the treated and control groups) whose cases reached litigation received assistance in filling out answer and discovery request forms.

results regarding the effectiveness of offers of representation as compared to no such offer. Various observational studies have been attempted to measure the effect of differing levels and types of legal interventions. Again, Greiner and Pattanayak (2011) collects many such studies (which number in the dozens), but for reasons discussed there, we find none of these studies credible. The one observational study in this area we might credit is Monsma and Lempert (1992), which attempts to address selection effects by conditioning on some characteristics of the potential client, such as education. But this study, too, reports conflicting results regarding the effectiveness of actual use of representation in eviction proceedings, and there was no limited assistance component. Meanwhile, Sandefur (2009) attempts a meta-analysis using bounding methods of a few dozen observational studies plus Seron et al. (2001). We admire this effort, but note that (as Sandefur herself reports) the bounds end up being wide. Further, we are uncertain as to whether the data in the studies Sandefur analyzes correspond to a coherent intervention that a legal aid provider could implement.

Greiner et al. (2011) provides a brief literature review of the felt need for legal assistance in residential eviction matters, and Jennings and Greiner (2012) provides a bibliography of unbundled legal assistance programs in adjudicatory proceedings generally. We do not rehash this literature here. We note only that, in Massachusetts, limited legal assistance programs in housing date back to at least the late 1990s with the advent of a LFTD in the Boston Housing Court (Jennings and Greiner (2012)); these and other limited programs predated the Massachusetts Supreme Judicial Court's authorization of unbundled legal assistance in court proceedings by several years.[6]

[6] See Order, in re: Limited Assistance Representation (SJC April 10, 2009), available at http://www.mass.gov/courts/courtsandjudges/courts/probateandfamilycourt/documents/limitedrepresentationstandingorde

3 The Study Design

In this section, we provide a bare bones account of the legal and factual setting for our study as well as of its randomized design. We provide greater detail on all aspects of the study in a separate Appendix.

3.1 Massachusetts Law & the Case Types in Our Dataset

Massachusetts residential summary eviction proceedings were designed to adjudicate quickly who had the right to possess a housing unit. Summary eviction courts also had supplemental jurisdiction to adjudicate most monetary claims (such as for rent arrears) and counterclaims (such as for damages due to unlawful conditions in the unit) arising out of the parties' relationship. Although summary eviction could involve a wide variety of legal circumstances, cases in our dataset fell into one of the following three categories.

(i) About 12% of our cases: A purchaser at a foreclosure sale sought to evict either former homeowners who were still living in the unit (just over half of the 12%) or tenants who had been renting from the former owners and who remained in the unit after the foreclosure (the rest of the 12%).

(ii) About 27% of our cases: A landlord sought to evict a tenant who had allegedly committed some kind of misconduct, ranging from keeping an animal in violation of a lease's no-pet clause to committing a crime.

(iii) About 60% of our cases: A landlord sought to evict a tenant for allegedly failing to pay rent.

Study-eligible occupants contacted the Provider at two different stages of a dispute: (a) when the evictor had issued a "notice to quit," a legally required precursor to a lawsuit that the evictor issued to occupants of a housing unit; or (b) when the evictor, now a plaintiff, had filed a summary eviction lawsuit. We refer to cases in which the occupant contacted the provider after the evictor had issued a notice to quit as "notice to quit" cases; we refer to cases in which the evictor contacted the Provider after the evictor had filed a lawsuit as "complaint" cases.

Greiner et al. (2011) provides a more complete summary of the law applicable in Massachusetts summary eviction proceedings. We limit our discussion here to the concepts essential to understanding the results we report in this article.

First, and as suggested immediately above, Massachusetts law required an evictor seeking to evict an occupant from a housing unit to serve the occupant with a "notice to quit," essentially a demand that the occupant vacate the unit by a certain date. The date stated depended on the reason for the eviction. Fourteen-day notices to quit corresponded to evictions for nonpayment of rent. Thirty-day notices to quit corresponded to most other circumstances, including eviction for conduct in violation of leases. Slightly more than half (102 of 184, or 55%) of the cases in our dataset were notice to quit cases.

After the time period specified in the notice to quit, the evictor could file a summary eviction action in either the Northeast Housing Court or the Massachusetts District Court with geographic jurisdiction over the housing unit. 45% (82 out of 184) of the cases in our dataset were complaint cases.

Although there were several Massachusetts District Courts with geographic jurisdiction overlapping the Northeast Housing Court, about 90% of the litigated cases in the present study were resolved in Housing Court, so we confine our attention there. Special rules of procedure governed summary eviction cases. These rules were designed to expedite proceedings so as to adjudicate the right to possession of the unit quickly. For example, once a plaintiff filed and served a summary eviction complaint, the clerk set the trial for the first available date no fewer than ten days later. If a plaintiff succeeded in obtaining a judgment for possession of the unit, the plaintiff ordinarily had to wait at least ten days for a writ of execution to issue. At that point, the plaintiff could cause the issuance of a 48-hour constable's notice, and, only upon expiration of this notice, could forcibly reenter the housing unit, remove the occupant's belongings to a storage unit, and change the locks. In the cases in our dataset, bargaining could and did take place around these various deadlines.

Third, both Massachusetts and federal law provided a variety of defenses, counterclaims, and procedural devices that defendants could use to defend against a plaintiff's action for possession. For example, if a defendant filed and served discovery requests upon a summary eviction plaintiff within a week of the filing and service of the complaint, the trial was automatically continued for two weeks, thus buying the defendant valuable time to prepare her defense, to bargain with the plaintiff, and possibly to search for alternative living arrangements. In addition, substantive defenses could arise from Massachusetts statutes, Massachusetts common law, Massachusetts regulations, federal statutes, and federal regulations, depending on the type of housing unit involved. As suggested in Greiner et al. (2011), few would characterize applicable substantive summary eviction law as simple or easily accessible.

3.2 The Workings of the Housing Court

We describe here the mechanisms behind the most distinctive feature characterizing the administration and resolution of disputes in the Northeast Housing Court: the rareness of contested court rulings. Greater detail is available in the Appendix.

As might be expected, evidentiary hearings of any kind (including trials) were rare, occurring in only seven of the 137[7] cases in our dataset that went to litigation. Defendant defaults of any kind were also rare. Default events[8] occurred in only 19 of the 137 litigated

[7]Recall that occupants contacted the service provider at the notice to quit stage in 102 of the 184 cases in our dataset. 55 of those notice to quit cases ended up in litigation, along with the 82 cases in which the occupant contacted the service provider after a summons and complaint had been filed.

[8]By "default event" we mean any kind of court ruling premised upon the defendant's failure to appear. A ruling based on a defendant's failure to appear could be (and often was) overturned when the defendant made a motion for relief. In other cases, an occupant's failure to appear, and a court ruling in favor of

cases, and many of these were removed by subsequent motion. Moreover, despite that each litigated case was almost always called before the Housing Court more than once in its lifetime, each case saw an average of .24 contested rulings. Thus, the overwhelming majority of litigated cases, and the overwhelming majority of individual calls of each litigated case, were resolved by party agreement.

The parties reached these agreements in three primary ways. First, plaintiffs and defendants sometimes negotiated an agreement on their own, occasionally before a case was called for a particular day, but more often in the hallway outside the courtroom.[9] Second, and by far the most important, virtually no matter was allowed to come before the judge unless the parties first engaged in a "mediation" session run by a court-employed Housing Specialist. The reason for the quotation marks here is that we are familiar with few mediation programs in which the "mediator" (here, the Housing Specialist) wielded such forceful authority. The Housing Specialists were court employees who performed a variety of functions, including educating litigants by answering procedural and substantive questions, investigating the facts of cases, suggesting settlement terms to the parties, opining on settlement terms the parties proposed, predicting how the judge would rule if a matter came before him, and telephoning parties to remind them of their obligations. Unsurprisingly, a great many cases were resolved in these "mediations."

Third, if the Housing Specialist was unable to obtain a party agreement, the judge[10] often obtained one when the case came before him. Again, more detail is available in the Appendix, but essentially, the judge cajoled the parties to settlement from the bench.

These mechanisms, particularly the "mediation" sessions before the Housing Specialists, may explain some of the results we report below.

the evictor, would be followed by a settlement that effectively removed the default (although not always on terms favorable to the defendant).

[9] In contrast to the practice of the District Court as described in Greiner et al. (2011), the Housing Court did not routinely insist that parties engage in hallway settlement negotiations.

[10] In almost all Housing Court cases in our dataset, a single judge presided over all issues.

4 The Service Provider: Services, Outreach, Intake, & Screening

The Provider in our study was a medium-sized traditional legal aid organization. The Provider offered two forms of unbundled assistance in housing disputes: instructional clinics lasting 60-90 minutes on housing law and Housing Court procedures, and the LFTD program. The instructional clinics were held at least weekly, typically for small groups of occupants. Attendees who had received summons and complaints would ordinarily receive assistance in filling out answer and discovery forms; as noted above, the filing and service of discovery resulted in an automatic two-week postponement of the initially set trial date. Because (as discussed below) occupants had to attend either an instructional clinic or a meeting in one of the Provider's offices to be eligible for full representation, answers were filed in over 80% of the litigated cases in our dataset.

As relevant to the present Housing Court Study, the LFTD program provided same-day only representation in hallway settlement negotiations and in "mediation" sessions. LFTD services did not extend to assistance in filing motions or to court appearances.[11] The Provider administered the LFTD program, which was staffed by a combination of Provider attorneys and private attorneys working pro bono. Typically, a LFTD client met with an attorney in the hallway for between 10-25 minutes, during which time the attorney obtained basic facts and learned the issue that had brought the case to court that day. Representation ended after the matter bringing the case to court that day was resolved.

The Task Force's funding was sufficient to allow two Provider staff attorneys to dedicate half of each's time to providing full representation of Study subjects so randomized. Study subjects were recruited as follows: occupants called the Provider's intake line. Occupants

[11]For cases that were not in the Housing Court Study, LFTD attorneys also appeared before the judge to argue motions or to engage in colloquies (but LFTD attorneys would not examine witnesses or assist in filing motions). This service was not, however, offered to defendants who were randomized to the control group in the present Study.

provided basic information (enough to determine income eligibility, for example) to professional intake staff before they were referred to one of the two Provider staff attorneys. To be study-eligible, the occupant had to attend a meeting of some kind in the Provider's offices. If the occupant called after receiving a notice to quit but before litigation had been filed, she had to meet with one of the two Study attorneys to receive information about the process. If the occupant called after receiving a summons and complaint, she had to attend an instructional clinic. During these office meetings, Provider staff attorneys asked eligible occupants whether they wanted to be considered for a randomized chance at full representation. Those who assented received an explanation of the Study and gave informed consent. Provider attorneys periodically forwarded small batches of single-page sheets of information on occupants to us along with a report of the number of cases their caseloads allowed them to handle, and we randomized accordingly. We tracked case outcomes through a combination of telephone calls, roughly once every two months until the matter was resolved, and through examination of court records.

Three final items deserve note. First, the Provider depended on word of mouth, its reputation in the community, its website, and its engagement in community projects to generate the stream of occupants that it recruited to the Study. It did not conduct outreach specific to individual occupants, nor did it recruit Study participants from its LFTD program. Second, as noted above, two Provider staff attorneys each dedicated half of their time to providing full representation; for each, the other half was dedicated to running the instructional clinics and to the LFTD program, *i.e.,* to unbundled representation. Third, the Provider staff attorneys reported that they did not aggressively screen cases deemed otherwise eligible for full representation. In particular, the attorneys did not apply the criteria the Task Force identified to screen occupants away from the full representation opportunity the Study offered, nor did they limit the cases they forwarded to us to those for which representation was in some sense "needed" to produce a better result. Each of these aspects of the Provider's operations contrasts with that of those of its counterpart in the companion District Court Study (Greiner et al. (2011)).

5 Quantitative Results

We provide in this section the results of the analysis phase of this randomized trial. We begin by examining the balance between the treated (offered full representation from a Provider staff attorney) versus control (referred to the LFTD program) groups on observed[12] background variables. We then summarize the results for the four sets of outcomes we studied: whether notice to quit cases reached court; outcomes relating to possession of the unit, most importantly which party (the evictor or the occupant) obtained possession at the end of the transaction for which the occupant sought representation; outcomes relating to financial consequences of the dispute, most importantly the end-of-case financial situation of the landlords and tenants in disputes involving nonpayment of rent or serious financial counterclaims; and outcomes related to the burden on the court cases imposed, most importantly the number of times the judge had to look at (or issue a contested ruling in) a case. We then discuss some additional, miscellaneous outcomes before turning, in the next section, to possible explanations for what we saw.

Our primary analysis technique in this section was a simple permutation test, which allowed us to incorporate the non-standard randomization scheme circumstances forced us to employ. We used this method to test for differences in means (weighted and unweighted, although the two rarely differed in a substantive way), medians, and .25 and .75 quantiles for both covariates and outcomes.

We provided intervals for each of our most important results. As we will explain, there is little danger of Type I (false positive) error in our dataset. For continuous variables, we

[12] The background variables came from the single-page information sheets Provider attorneys forwarded to us prior to randomization, as well as from an examination of court case files for variables we believed to have been determined prior to randomization.

produced intervals using the permutation technique (assuming for this technique a constant average treatment effect) as well as with a straightforward adaptation of the Peters-Belson method to Bayesian regression (Peters (1941), Belson (1956)). For 0-1 outcome variables, we used only the latter method.

5.1 Balance Between Treated and Control Groups

In this subsection we examine, to the extent we can tell from observed covariates, whether the randomization scheme did what it was supposed to do, namely, produced treated and control groups that were the same (up to random variation) in all ways except for the treatment.

Table 5.1 provides treated and control group means and standard deviations for 29 background variables, along with permutation test p-values for a difference in the unweighted[13] means and in the medians. Only four variables had p-values for a difference in means below .10: "RentAll," the occupant's portion of monthly rent (coding cases in which no rent was paid, such as former homeowner cases, as 0); "SecDepPos," the amount of a security deposit paid by the occupant, considering only positive values (meaning ignoring cases with no security deposit);"IsWhite," whether the occupant who contacted the Provider self-identified as white; and "DaysIntakeToComp," the number of days from intake to a complaint (ignoring cases that did not reach litigation). Keeping in mind that randomization will produce some imbalances by chance, these disparities strike us as unremarkable. Perhaps more important than the absence of any serious disparity is that these four variables are not ones we would identify as particularly important for the outcomes we measured. Our priors would have led us to be more concerned had the imbalances occurred in, for example, whether the occupant was a former homeowner, in which case applicable law and the evictor's determination to

[13]We also examined the difference in weighted means (weighting each observation by inverse probability of selection to the condition it received, *i.e.*, a traditional Horvitz-Thompson weight). After weighting, no variable had a difference in means p-value of less than .05; only two variables, IsWhite (.07) and DaysIntakeToComp (.06), had p-values below .10.

obtain possession were probably different from that in nonpayment of rent cases.

Because some of our analysis focused only on notice to quit cases or only on complaint cases, we also examined the analogs to Table 5.1 for these subsets of cases. For the notice to quit cases, the only large imbalance[14] was that the treated group had a far higher fraction of Hispanics (.40) and a far lower fraction of whites (.49) than did the control group (.18 Hispanic, .75 white); p-values for both variables were less than .01. Yet this difference did not translate into a large disparity in whether the occupant needed an interpreter ("IsNeedsInterp" treated mean of .21, control mean of .15, p = .19). Although race-based or ethnic-based disparate treatment was always a concern and a possibility, we perceived no evidence of such in our study.

For complaint cases, two variables had p-values for differences in unweighted means less than .10,[15] "IsPhysDisab" (denoting whether any occupant had a physical disability[16]), and "DaysIntakeToComp" (denoting the number of days from the intake to the entry of the complaint.[17]) We find both results only mildly concerning. The disparity in IsPhysDisab had a p-value of .09, and the variable is not at the top of our list of substantively important covariates. The disparity in DaysIntakeToComp was such that the treated group cases contacted the service provider about a week earlier than did the control group cases. If anything, this difference should have given Provider's lawyers engaging in full representation more time to investigate the facts, conduct legal research, and otherwise alter the result. As we will explain, however, we found virtually no differences in outcomes between treated and

[14]The variable "NTQAmtPos" had a statistically significant difference in medians (p = .02) but not in means (p = .32). The variable "NumInUnit" had a notable difference in means (p = .07) but not in medians (.67).

[15]The weighted difference in means comparisons produced the same substantive results here.

[16]Treated mean = .26, control mean = .45, p = .09.

[17]Treated mean = intake occurring an average of 4.6 days *before* complaint, control mean = intake occurring an average of 3.9 dates *after* complaint. The p-value for the difference in means was .02, and for the difference in medians was .01. The counterintuitive result for the treated group here, *i.e.*, that the intake occurred before the complaint, apparently stemmed from the fact that in many cases the plaintiff would serve the defendant with the complaint (thus stimulating the call to the service provider) before filing it with the court and paying the filing fee.

Covariate	Treated Mean (SD) (N = 85)	Control Mean (SD) (N = 99)	P-Val Mean	P-Val Median
IsNTQCase	.55 (–)	.56 (–)	.98	–
IsNTQType14D	.55 (–)	.61 (–)	.70	–
IsNTQType30D	.24 (–)	.27 (–)	.45	–
NTQAmtAll	902 (1546)	1065 (1636)	.43	.86
NTQAmtPos	1785 (1779)	1918 (1787)	.44	.20
CompAmtAll	1656 (2112)	1772 (1880)	.74	.33
CompAmtPos	2245 (2175)	2132 (1867)	.99	.53
IsPostForecl	.14 (–)	.10 (–)	.66	–
IsHmOwn	.06 (–)	.05 (–)	.94	–
** ***RentAll***	***684 (332)***	***733 (394)***	***.08***	.25
SecDepAll	404 (432)	419 (518)	.74	.35
** ***SecDepPos***	***664 (365)***	***819 (441)***	***.04***	.12
LastMonAll	253 (398)	275 (460)	.29	1.00
LastMonPos	767 (291)	866 (390)	.28	.42
IsDefWantsStayUnit	.68 (–)	.70 (–)	.72	–
IsSec8	.16 (–)	.23 (–)	.64	–
IsPubHs	.06 (–)	.06 (–)	.82	–
IsFem	.75 (–)	.72 (–)	.62	–
Age	40 (12)	42 (13)	.26	.27
IsHisp	.32 (–)	.24 (–)	.19	–
IsBlack	.18 (–)	.19 (–)	.46	–
** ***IsWhite***	***.59 (–)***	***.68 (–)***	***.07***	–
IsNeedsInterp	.14 (–)	.13 (–)	.43	–
IsMentDisab	.45 (–)	.33 (–)	.10	–
IsPhysDisab	.35 (–)	.45 (–)	.14	–
NumInUnit	3.0 (1.8)	2.7 (2.0)	.53	.76
Num<18InUnit	1.4 (1.5)	1.1 (1.4)	.32	1.00
** ***DaysIntakeToComp***	***23 (33)***	***13 (37)***	***.09***	.29
IsFiledDistCt	.30 (–)	.34 (–)	.59	–

Table 1: *Covariate Balance, All Cases: This table shows means and standard deviations for the treated and control groups. A key for the variable names appears in Section 7. Any variable beginning with "Is" is 0-1, so all information is in the rate and the median is not a useful summary statistic. The final two columns report two-sided p-values for the unweighted mean and the median from the permutation test. There appear to be few large differences between treated and control groups. Only one variable showed a p-value for the mean of less than .05 and only three others showed a p-value for the mean of less than .10. Three of these variables were continuous, and for none of them was the p-value for the median particularly close to significant. In terms of substantive significance, all imbalances should be handled with care, but the four variables showing some lack of balance were not among those our prior beliefs would lead us to deem particularly troubling.*

control groups.

With 29 covariates, it is unsurprising that an occasional one had a statistically significant difference in means or medians. Overall, we view these results as confirming that the randomization achieved good balance between treated and control groups, at least on observed covariates. Out of an abundance of caution, we did some modeling to address the observed disparities, but the modeling and permutation results were similar.

5.2 Attorney Usage, Evictors and Occupants

In this subsection, we discuss the usage of attorneys among evictors (*i.e.*, plaintiffs/landlords) as well as the occupants (*i.e.*, defendants/tenants).

First, for evictors: In about 47 of 82 complaint cases (57%), the docket sheet showed that the evictor used an attorney. We suspect that the true figure of attorney usage by evictors might be slightly higher, perhaps as high as 63% or so.[18]

Next, for occupants: As a reminder, recall that the randomization was either to an offer of full representation by a Provider staff attorney or to a referral to the Provider's LFTD program, *i.e.*, to one of two conditions. Nevertheless, unsurprisingly, a substantial number of occupants experienced a third condition, self-representation or proceeding pro se.

Table 5.2 breaks down treatment assigned on the rows, by case type (notice to quit or complaint), versus treatment condition experienced on the columns. We draw the following conclusions. First, 82% ((22+15+33)/47+38)) of those randomized to an offer of full representation from a Provider staff attorney took advantage of the offer. This rate suggests that occupants strongly valued full representation. Refusals of offers of full representation appeared to be more common in notice to quit cases (10/47 = 21%) than in complaint

[18]Our suspicion on this point stems from defendant attorney figures: when we compared the Provider's records in the treated group complaint cases to the Housing Court's records, we found that of the 34 complaint cases in which the Provider actually represented, the Housing Court's records showed an attorney of record in only 31. This finding suggests that the Housing Court's records missed as many as 10% or so attorneys of record. Applying this 10% figure to the 57% rate led us to the 63% guess. Note that the figures we report here for complaint cases were virtually identical to those in the notice to quit cases that reached litigation.

cases ($5/38 = 13\%$); this is hardly surprising, given the attention-focusing fact of an extant court proceeding in complaint cases. Second, only 8% ($(5+3)/(55+44)$) of those randomized to no offer of Provider full representation ended up experiencing a full attorney-client relationship from some other source,[19] suggesting little availability of full representation for income-eligible persons so desiring. Third, occupants randomized to no Provider offer of representation used the LFTD program in just over half of the cases that reached litigation. For the complaint cases (which were in litigation at the time of randomization), this figure was 25 of 44 cases, or 57%.[20] This figure suggests that almost half of the occupants who had the self-organization, persistence, and motivation to contact the Provider prior to their answer due dates, to request legal assistance, and to follow up with a meeting at the Provider's offices nevertheless did not take advantage of the Provider's flagship program in summary eviction cases. We find this last result harder to explain,[21]

We speculate that occupants proceeding pro se either did not understand (despite the Provider's communications to this effect) that they could use the LFTD program or suffered from some form of intake fatigue (Sandefur and Smyth (2011)), meaning that having completed one intake process and being informed that full representation was unavailable, they lacked the energy to continue to reach out again to the Provider. Of the two explanations,

[19] To the extent we can tell from the records, occupants hired private attorneys in these cases.

[20] If we add in the control group notice to quit cases that reached litigation, the figure was $(14+25)/(14+13+25+16)$, or again, 57%. We remind readers that other forms of unbundled assistance were available, including the help in filling out answer and discovery forms noted earlier. Given the Provider's intake procedures, discussed above, we would not expect a differential between treated and control group in the usage of such assistance.

[21] Ten of the $(14+13+25+16) = 68$ control group cases reaching litigation were filed in District Courts and never transferred, despite the Provider's practice of offering occupants assistance in filling out the half-page transfer form. This fact might explain some of the failure to use the LFTD, which was available only in Housing Court. But that only begs the question of why these litigants did not take advantage of the Provider's offer of assistance in filling out transfer forms.

The Provider strongly believed that there would be a notable difference in the outcomes realized by occupants who took advantage of the LFTD program and those who did not; it also felt similarly about those notice to quit occupants whose cases reached litigation and who took advantage of its offer of an instructional clinic in its office versus those that did not. We did not conduct comparisons to test these assertions because we would not have been able to separate the causal effect of the LFTD program (or an instructional clinic) from a selection effect based on the difference in the kind of occupant who would take advantage of such services and the kind of occupant who would not.

	Full Rep Lit	Full Rep No Lit	LFTD Lit	No Rep Lit	No Rep No Lit	Total Total
Treated, NTQ	22	15	0	1	9	47
Treated, Complaint	33	–	2	3	–	38
Control, NTQ	5	0	14	13	23	55
Control, Complaint	3	–	25	16	–	44
Total	63	15	41	33	32	184

Table 2: *Compliance Table, All Cases: This table shows treatment assigned, by case type, on the rows versus treatment condition experienced on the columns. "LFTD" stands for lawyer for the day program, and "Lit" denotes whether the case reached litigation (automatic in "Complaint" cases, which were those in which the occupant contacted the Provider after receiving a summons and complaint). About 82% ((22+15+33)/47+38)) of persons offered full representation from the pilot service provider took advantage, while only 8% ((5+3)/(55+44)) of persons not so offered found full representation elsewhere. Thus, full representation was both highly valued and difficult to obtain otherwise. Just over half ((14+25)/(14+13+25+16)) of those referred to the LFTD program whose cases reached litigation took advantage of it.*

the latter strikes us as less plausible, given that (i) there were very few defaults (and almost no defaults that were not removed by motion or a subsequent bargain) in cases reaching litigation, and (ii) the LFTD desk and its accompanying signs were hard to miss when one approached the Housing Court's courtrooms.

5.3 Four Sets of Outcomes

In this subsection, we report the results for the four sets of outcome variables of primary interest in this Study. We find no statistically significant difference between treated and control groups with respect any outcome in any set. In fact, with the possible exceptions of the likelihood that notice to quit cases resulted in litigation and the length of time that occupants vacating their units were given to move out, the treated and control group had surprisingly similar point estimates. The similarity in the point estimates lent further credence to the hypothesis that the offer of a traditional attorney client relationship from a Provider staff attorney, as compared to a referral to the LFTD program, caused no large differences in important outcomes.

5.3.1 Whether Notice to Quit Cases Reached Court

Recall that the present Housing Court Study included cases at two different stages: notice to quit cases, in which the occupant completed intake after receiving a notice to quit but before litigation had been filed; and complaint cases, in which intake took place after the filing of a litigation. A principal reason for including the former type of case in this Study (it had not been the Provider's prior practice to assist occupants at this stage) was to ascertain whether an offer of full representation would facilitate pre-litigation settlement of disputes. The hope here was that strong evidence that a Provider offer early in the case decreased court caseloads could bolster the argument for additional legal aid funding.

No such strong evidence emerged, although we are less certain of conclusions on this point than we are with respect to the other results reported below. The limited number of notice to quit cases in the dataset, 45 treated and 55 control, meant that we were likely to detect only very large effects. As we suggested in Section 1, however, very large effects were probably the only ones that matter.

The results here are easily summarized. 50% of treated (offered full representation by a Provider attorney) group notice to quit cases reached litigation, versus 60% of control (no such offer) group cases. The permutation p-value for this difference was .15.[22]

To produce intervals, we explored three sets of models: (i) "expert" models, in which we chose covariates we deemed likely to affect the variable of interest (here, whether a notice to quit case was likely to go to litigation) based on informal conversations with housing attorneys; (ii) "covariate balance" models, in which we chose as covariates those with the lowest p-values from permutation tests run on only the notice to quit cases; and (iii) "backward selection" models, in which we ran separate backwards selection algorithms for treated and control groups. For all models, we fit separate Bayesian logistic regressions to the treated

[22] Applying Horvitz-Thompson weights narrowed the difference to .52 in treated group versus .57 in the control group.

and control groups; drew from the posterior distribution of the coefficients; married each treated group coefficient draw to each control group occupant's covariate vector to calculate a posterior predictive probability for each particular control group occupant's case that it would have gone to litigation had it been treated (and vice versa for control group coefficients and treated group covariates); compared this probability to a random uniform draw to fill in the missing potential outcome for whether the case went to litigation; and calculated an average treatment effect. Repeating this procedure several thousand times produced a posterior distribution for the average treatment effect (see Peters (1941) and Belson (1956) for precursors of the linear regression version of this technique). Unsurprisingly, for this outcome, the model-based intervals were wide. Of the expert, covariate balance, and backwards selection sets of models, the widest interval for the effect on litigation probability due to treatment came from the latter set, with an interval of (-.15, .09), meaning a -15 to 9 percentage point difference between treated and control groups. All three sets of models produced intervals that included 0.

The small size of the available dataset, along with the fact the point estimates for the treated and control groups show mild (but not striking) separation, led us to be more cautious here than with other results. It does not seem likely, however, that Provider offers of full representation produce very large reductions in the probability that a notice to quit case reached litigation, and very large reductions were probably necessary to support an argument for funding based on this outcome.

5.3.2 Possession Outcomes

There were several variables associated with possession. In our view, the most important of these was which party actually ended up in possession at the end of the dispute between the evictor and the occupant. We could and did code this variable for both notice to quit and complaint cases. Note that, as Table 5.1 makes clear, 69% of occupants reported wanting to stay in their units. Moreover, for the 31% of occupants who said they wanted to leave their units, the desire may have been somewhat aspirational. At intake, we had Provider attorneys ask occupants what they would do if they had to move out. Of the 57 occupants who reported wanting to leave their units, less than half (25) said that they would move to another unit. 17 did not know what they would do, 11 said they would go to homeless shelters, 3 said they would move in with family or friends, and 1 checked an "Other" box. We conclude that retaining possession was an important goal of the great majority of occupants.

Actual possession was not the only possession-related outcome of interest. For complaint cases, we also coded additional possession-related outcomes, including whether a judgment of possession entered for the plaintiff and whether a writ of execution for possession issued. We did not code these variables for notice to quit cases because both a judgment and an execution were contingent outcomes; that is, they were defined in notice to quit cases only if these cases went to litigation, and not all cases did so.[23]

In the Appendix, we provide information regarding how we coded the possession variables discussed in this section.

Table 3 summarizes the possession results. Within the limits of our modest-sized dataset, it is not easy to imagine a clearer indication of no treatment effect. Table 3 codes the outcomes in terms of occurrences favorable to the evictor, but as all three outcomes were 0-1 variables, one can transform them easily to occurrences favorable to the occupant. By way of example: on the key variable of actual possession, 1-.67 = 33% of treated occupants actually retained possession versus 1-.62 = 36% of control occupants. Unsurprisingly, this three percent difference was not statistically significant. Differences of five and six percent, also far from statistically significant (and in any event, the control group had more

[23]Statistical frameworks are available to address the problem of contingent outcomes (see Frangakis and Rubin (2002), Greenland and Robins (1986), and Robins (1986)), but the limited number of observations in the Housing Court Study, coupled with our inability to model well which notice to quit cases would result in litigation, rendered these frameworks practically unavailable.

occupant-favorable results than the treated group), were observed for the other two possession variables.

	Treated Rate	Control Rate	P-value
Actual Poss, Evictor*	.67	.66	.93
Judg Poss, Evictor	.32	.27	.84
Writ Exec Poss, Evictor	.29	.23	.47

Table 3: *Results for Possession Variables: Comparison of unweighted means for treated (offered Provider full representation) versus control (referred to the LFTD program) possession variables. All variables were coded such that lower numbers would be more favorable to study subjects, the defendants/occupants. Thus, the 67% treated group evictor possession rate corresponded to a 33% occupant possession rate. The results with the "*" came from both notice to quit and complaint cases; the other results came from complaint cases only. There were virtually no differences between treated and control groups, and permutation-based p-values were high. Weighted figures were similar to those shown here.*

We also produced intervals for the critical outcome of actual possession using the sets of models described in subsection 5.3.1. Each set of models produced virtually identical intervals, so we report the results for the "backward selection" set because it produced the widest interval for the average treatment effect: (-.09, .07).

In short, the data suggest it is unlikely that the Provider's offer of full representation, as compared to a referral to its LFTD program, had a large effect on possession variables, including the critical variable of whether the occupant lost possession at the end of the dispute.

5.3.3 Financial Consequences

For reasons discussed in the Appendix, we were able to analyze financial consequences only for complaint cases, so our dataset is correspondingly smaller. As was true of possession, there were several possible variables outcomes associated with financial consequences, but we find most of these unappealing, due to the fact that money judgments in this dataset represented fundamentally different things in the context of post-foreclosure cases, misconduct evictions, and nonpayment of rent cases.

We prefer looking only to cases in which a landlord alleged nonpayment of rent or in which a tenant alleged serious monetary counterclaims. For cases in this category, we calculate an outcome we called "EvictorMonsRentLost," short for "Evictors Months of Rent Lost." We provide the details of this variable in the Appendix. Essentially, the variable represents a calculation of flow of money between evictor and occupant, relative to the amount of money the former believed to be due and to have accrued during the pendency of the case. The unit of the measurement is months of rent, and negative values are pro-occupant. For example, a EvictorMonsRentLost value of -2.0 signifies that, relative to the amount of money the evictor alleged to be due and to have accrued during the litigation, the occupant "saved" two month of rent. A potential weakness of this measurement is that we could not observe actual payments between the parties, so this measurement equates a judgment from the court that (say) the occupant pay $5000 with an actual payment of $5000.

We do not bother with a table, chart, or graph here. The treated (offer of a traditional attorney client relationship from a Provider staff attorney) group mean for EvictorMonsRent-Lost was -1.8, indicating a savings to treated defendants of 1.8 months of rent on average, versus a control (referral to the LFTD program) group mean of -1.6. This finding produced an estimated difference in means of -.2, or one-fifth of a month of rent. The permutation-based p-value for the .2 difference in means was .82, suggesting that this one-fifth of a month's rent, in addition to being of little substantive import, was far from statistically significant. A permutation based confidence interval for the difference in means was (-1.4, 1.1).[24] Using an ordinary least squares versions of the Peters-Belson sets of models described in subsection 5.3.1, the "expert" set of models produced the widest interval of (-1.7, 1.1) for the difference in means (as before, the other two sets of models produced similar results).

We also examined other measures of financial consequences we prefer less than Evictor-MonsRentLost: the amount of a money judgment, the amount of any monetary execution,

[24]Again, weighted figures were similar.

and several others we do not report, but none of which showed a statistically significant difference between treated and control groups. For the money judgment, the treated group mean was $903 in favor of the evictor versus a control group mean of $486 for the evictor (higher numbers here were detrimental to the occupant). The $417 difference was not statistically significant: the permutation p-value was .40, and a permutation interval for the difference in means was (-$815, $915). For the amount of a monetary execution, the treated group mean was $494, the control group mean was $443 (higher numbers here were again bad for occupants), the permutation p-value for the $51 difference was .99, about as far from statistically significant as it is possible to imagine.

We recognize that there is a danger in looking only at marginal distributions of the two primary outcomes upon which summary eviction litigation tended to focus, namely, possession and financial consequences. The danger is that treated and control groups may have differed in the way that occupants traded the two outcomes against one another, so that the overall outcomes for one group were actually more favorable than those in the other group in a way that examining only marginal distributions (*i.e.*, looking at each variable separately) masked. To this end, Figures 1 and 2 plot the primary two outcomes in the same graphs. We would have been concerned if, for example, treated cases tended to cluster in the top right and bottom left portions of these graphs, which might have indicated that the treatment furthered one or the other but not simultaneously both occupant goals: retaining possession or achieving a good financial result. Again, such a pattern might not have been evident from testing the marginal distributions of these two outcomes separately. The graphs demonstrate, however, that nothing of this sort occurred. Instead, the graphs show no discernible pattern, reinforcing the inferences we drew from examining each outcome separately that no large treatment effects were likely.

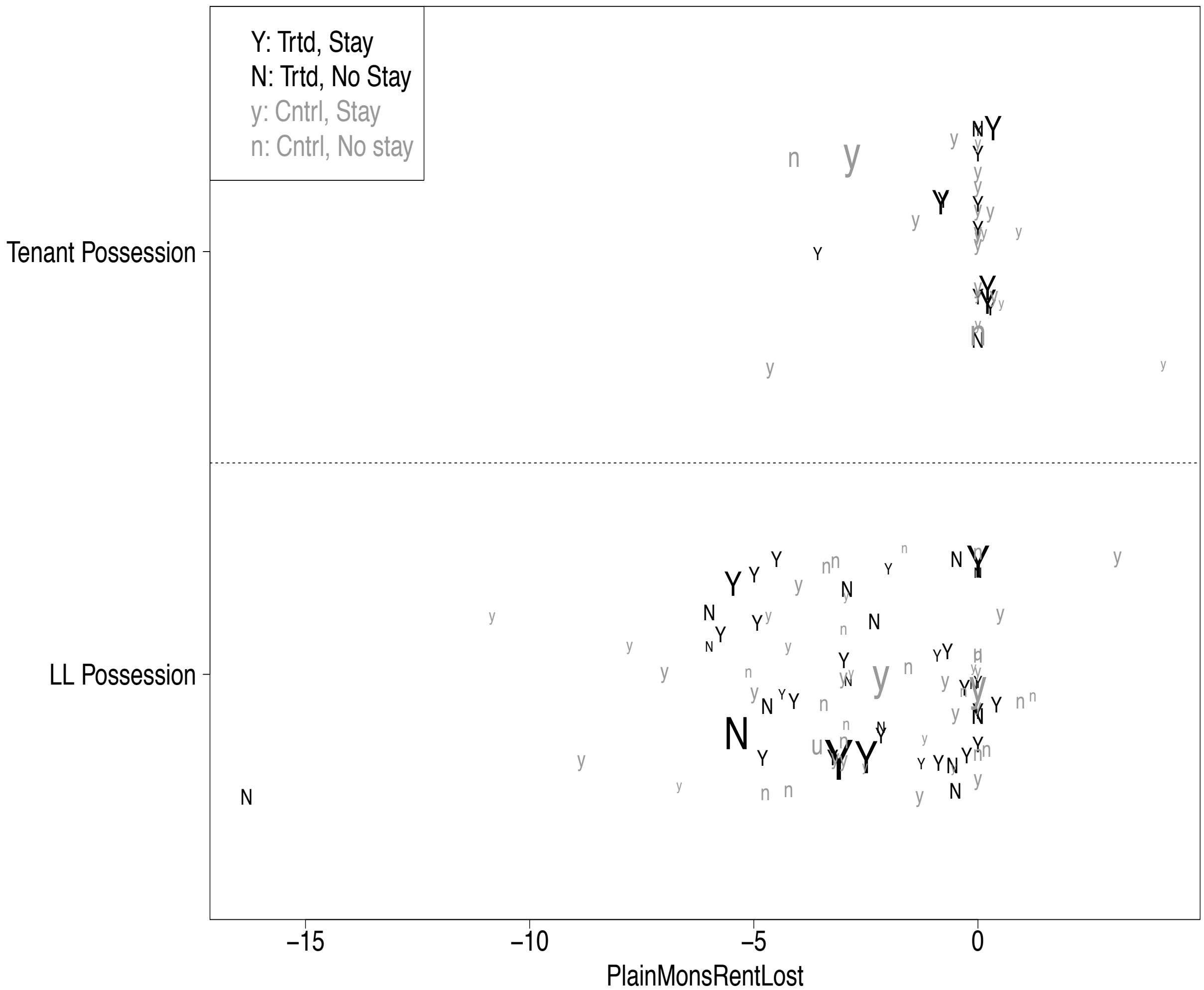


Figure 1: *Two Principal Outcomes: This graph plots the two most important outcome variables, possession (top versus bottom rectangle) and EvictorMonsRentLost (on the x-axis). Lower case, grey letters represent control group cases. Capital, black letters represent treated group cases. The Y or y (N or n) represents whether the tenant reported wanting to (not wanting to) stay in the unit at intake. The size of the letter represents a Horvitz-Thompson weight. The up/down location inside the "Tenant Possession" and "LL Possession" rectangles is irrelevant; the spread is solely to make it possible to see multiple datapoints with the same or similar EvictorMonsRentLost. For the tenant, the desireable outcome is ordinarily to retain possession and for the landlord to lose as many months of rent as possible. Thus, values on the top and to the left of the graph are generally better for the tenant. All complaint cases are shown in which a landlord sued for nonpayment of rent or in which the tenant asserted nontrivial monetary counterclaims. There is no apparent pattern in these outcome data, which tends to confirm the inferences of no large treatment effects drawn from testing each outcome variable separately.*

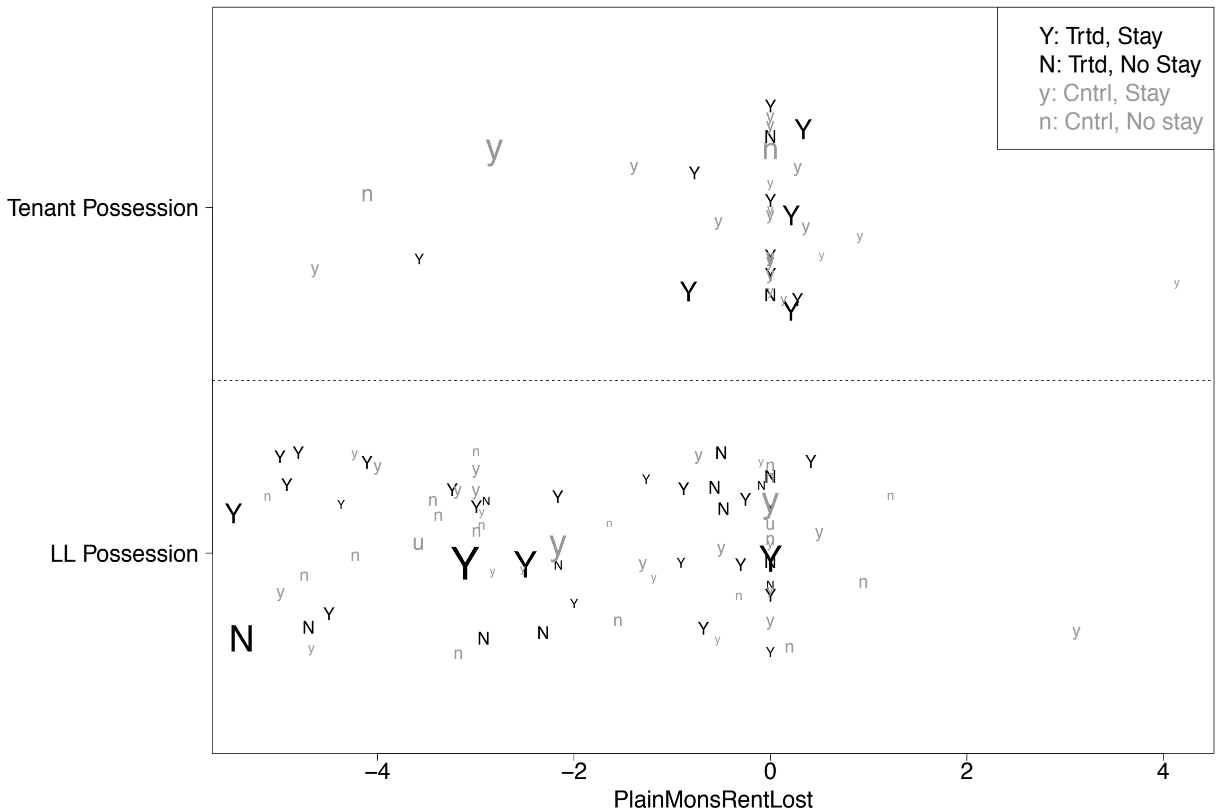


Figure 2: *Two Principal Outcomes: A Closer Look: This graph shows the same data as in Figure 1, except that the x-axis is truncated at -5 months of rent so as to ease visualization of the space that the vast majority of the data occupy. See the caption to Figure 1 for an explanation of the symbols used. Again, there is no apparent pattern in these outcome data, which tends to confirm the inferences of no large treatment effects drawn from testing each outcome variable separately.*

5.3.4 Court Burden Outcomes

As was the case for possession and financial consequences, there are several measures of court burden. We report the following outcomes here: case length in days and the logarithm of case length;[25] a variable we label "NumJudgeLooks," a measurement based on

[25]The logarithm measurement reflects that the distributions of this outcome, for both the treated and control group, had heavy right tails, as is often the case for time variables, particularly case length (see Greiner and Pattanayak (2011)). More challenging was the presence of a point mass of control group cases reaching judgment at 24 days. Examination of these control group cases showed that the 24 days represented the ten days from complaint to first-scheduled trial date standard in Massachusetts summary eviction actions, plus the two-week automatic delay accompanying the filing and service of discovery, then a settlement on day 24. We explored normalizing transformations, including the logarithm and various roots. The transformations could reduce the influence of the right tail(s), but no monotonic transformation could address the control group point mass at 24 days. For lack of a better idea, we report here analysis of the unadjusted figures as well as the logarithm.

the docket sheet of the number of times the judge had to interact with a case in a manner more substantial that a pro forma status conference; and "NumJudgeRulings," meaning the number of times the docket sheet disclosed that the judge issued a contested ruling;[26] the number of pre-judgment motions filed, separately analyzed by plaintiff and defendant; and the total number of motions filed, separately analyzed by plaintiff and defendant. All these outcomes are contingent on the existence of litigation, so we include only complaint cases in this analysis.

Table 5.3.4 summarizes the results, showing treated and control group (unweighted) means[27] and standard deviations as well as permutation-based p-values. The figures seem to tell a relatively clear story, as follows.

	Treated Mean (SD)	Control Mean (SD)	P-value
CaseLength, Days	92 (101)	71 (80)	.17
Log(CaseLength)	4.1 (.94)	3.8 (.88)	.19
NumJudgeLooks	2.1 (2.3)	2.1 (2.2)	.77
NumJudgeRulings	.18 (.46)	.32 (.98)	.23
NumPreJudMotsEvi	.26 (1.0)	.14 (.63)	.69
NumPreJudMotsOcc	.18 (.69)	.16 (.53)	.92
NumTotMotsEvi	.58 (1.6)	.57 (1.1)	.97
NumTotMotsOcc	.37 (.82)	.34 (.78)	.89

Table 4: *Measurements of Court Burden: This table shows unweighted means and standard deviations for the treated and control groups for various measures of court burden. P-values come from permutation tests. "NumJudgeLooks" ("NumJudgeRulings") represents the number of times the judge interacted with (issued a contested ruling in) the case. The last four variables measure motions activity, prejudgment and throughout the whole case. There was no evidence of a treatment effect with respect to any measure of court burden.*

There was no evidence of any treatment effect. To the contrary, point estimates are close

[26]It is not 100% clear that all judge rulings would be noted on the docket sheet. In the District Court Study (Greiner et al. (2011)), we saw evidence of bench rulings not captured in the docket sheet. This study occurred in a separate court with a different judge and a different clerk's office, so it is not clear how transferrable the lessons from that system are to the Housing Court we study here.

It was also not always clear from the docket sheet whether the ruling was contested. It is not, however, immediately evident why it would be harder (or easier) to discern whether a ruling on a motion was contested in treated group docket sheets as opposed to control group docket sheets.

[27]Weighted figures were similar to those shown, although the p-values for the two case length outcomes were slightly higher.

to one another, and the lowest p-value was .17.

In our view, a startling aspect of these results is the low number of motions filed, particularly the number of pre-judgment motions filed by the defendant in the treated group. Our priors suggest that prejudgment motions, especially motions to compel responses to discovery, to dismiss, and/or for summary judgment, would have been among the principal tools lawyers engaged in a traditional attorney-client relationship would have used to pressure plaintiffs and to achieve a more favorable case posture for settlement negotiations. Yet defendants offered full representation by the service provider filed on average less than a fifth of a motion per case, a figure statistically identical to the .11 motions filed by control group defendants. Recall that the LFTD program, to which control group cases were referred, did not extend to assistance in motions practice. We return to these figures in Section 6 when we discuss possible explanations for our results.

5.4 Additional Outcomes

The previous four subsections summarize the primary results of our analysis. In this subsection, we provide some additional results that we deem of lesser substantive importance.

The first result concerned only occupants who vacated their housing units, specifically, the amount of time those who had to move out were given to do so. This result was a contingent outcome, with the contingency being the requirement that the occupant vacate. We risk a direct treatment (offer of full representation from a Provider staff attorney) to control (referral to the LFTD program) group comparison here because, as discussed in subsection 5.3.2, we find no evidence that the treatment had any effect on the probability that the occupant would have to vacate, and the point estimates for treated and control groups are surprisingly similar.[28] We examined various measures of time to vacate,[29] and

[28]Thus, there is at least a reasonable chance that in comparing treated and control groups on this outcome, we are comparing cases that are alike in all ways except for the treatment, despite the contingent nature of the outcome.

[29]Perhaps the ideal measurement would be the number of days from the notice to quit to the vacate date.

looked at various sets of cases.[30] We also explored various normalizing transformations to these time measurements, which were right-skewed, and settled on a cube root, which performed somewhat well. All these variations in analysis produced the same conclusion: there was no statistically significant difference between treated and control groups.

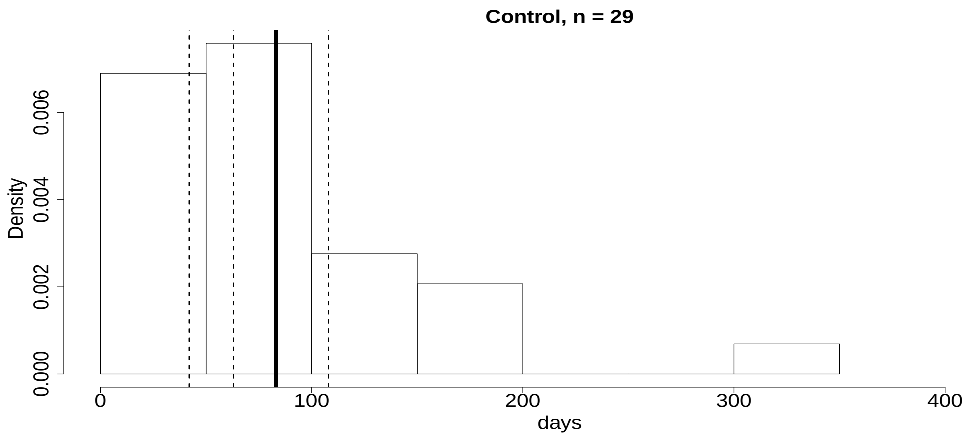
Because all the variants of transformations and sets of cases analyzed produced the same conclusion, we present the results for the simplest measurement to understand, namely, the number of days from complaint to vacate date for all cases reaching litigation and resulting in the occupant's having to move out. On this measurement, the treated group mean (standard deviation) was 113 (99) days, versus 82 (80) days for the control group, and the permutation p-value for the difference in means was .12. How could what appears to be a reasonably sized difference in means (31 days, or a month) yield such a high p-value? Figure 5.4 shows histograms of the moveout time lengths for the treated and control groups; the dotted lines show the .25, .50, and .75 quantiles, and the solid lines show the means. As these plots demonstrate, any treatment effect was limited to the right side of the distribution. In other words, it is unlikely that the treatment had a large effect on cases in which the underlying facts and law were such that the occupant would be given relatively little time to move out. Instead, for the cases in which the underlying facts and law were such that the occupants would already have been given a substantial time to vacate, the treatment might have extended this time.[31] Thus, the treatment had no discernible effect with respect to assuring a decent minimum of a moveout time;[32] rather, it provided additional help to those already fortunate in this regard.

Unless a case went to litigation, however, we were unlikely to have a copy of the actual notice to quit, so the notice to quit date was based on occupant recall at intake.

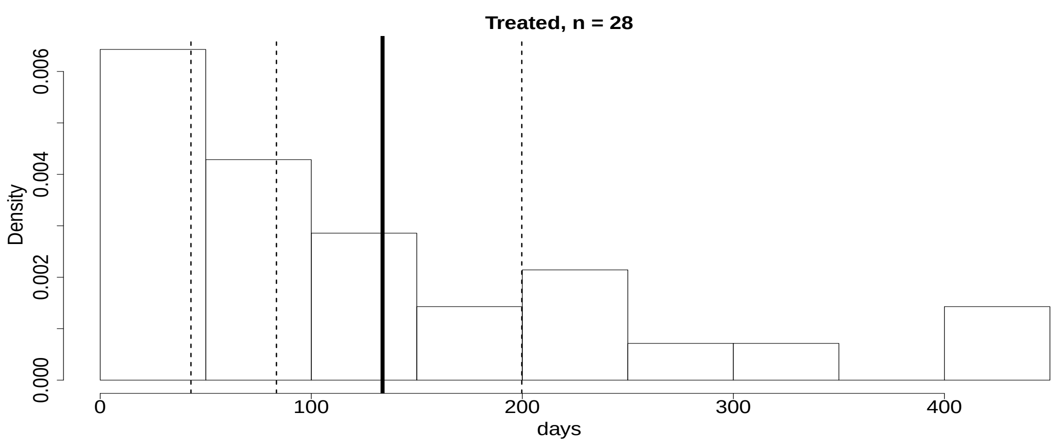
[30]We analyzed all cases, only complaint cases, and only notice to quit cases.

[31]Permutation tests for the .25, .50, and .75 quantiles confirm the impression given by the graphs. The p-values for the difference in these quantiles between the treated and control groups were .36, .42, and .05, confirming that any treatment effect occurred on the longer side of the distributions.

[32]To be clear, we take no position on whether moveout times in this dataset were too short, too long, or something inbetween. Our point is only that the treatment had little if any effect on the cases on the short side of the moveout time distribution.



(a)



(b)

Figure 3: *Moveout Times for Those Required To Vacate: These two histograms show length of time in days from complaint to vacate dates for complaint case occupants who moved out. The dotted lines are .25, .50, and .75 quantiles, while solid lines represent means. Any treatment effect (there was actually no statistically significant evidence of any effect) occurred primarily with respect to cases in which the moveout time was on the longer end of the distribution. There was little evidence of such an effect on the shorter end of the distribution, where an effect arguably would have had greater substantive significance.*

With respect to other miscellaneous outcomes: For complaint cases, there were no statistically significant differences between treated or control groups in the rate at which answers, counterclaims, discovery, and jury trial demands were filed. For the first three outcomes, the rates at which these documents were filed were around 80%. The more interesting figures were for jury trial demand rates: .18 on the treated group versus .09 in the control group.[33] The difference, not statistically significant, was less notable than was the low overall rate of the jury trial demands. Our understanding (from conversations with housing attorneys) is that jury trials were harder to schedule than bench trials, that scheduling difficulties could be good for occupants, and thus demanding juries could be useful for occupants. Obviously, strategic thinking could inform whether to take a case to a jury, but as noted above, evidentiary hearings of any kind were rare in this dataset, and a jury demand could ordinarily have been withdrawn later if case development (and/or the client's characteristics) counseled in favor of a bench trial. We return to these facts in Section 6, immediately below.

6 Possible Explanations of Our Results

The previous sections presented a complete null finding. There is no statistically significant difference (in means or medians) between treated and control groups in any of the outcome variables that we measured. Moreover, for almost all outcomes, the treated and control group point estimates were surprisingly close to one another, which decreases the always-present danger of Type II (false negative) error. We thus have considerable confidence in stating that it is unlikely that an offer of a traditional attorney-client relationship from a Provider staff attorney, versus a referral to the LFTD program, produced large alterations in outcomes. This result obtained despite the fact that only 57% of the control group occupants whose cases were in litigation took advantage of the LFTD program, while over 80% of the treated group occupants were fully represented.

The data are consistent with at least two different interpretations. The first interpretation would suggest that all is well with the Provider's limited assistance program. There

[33] If we risk a comparison between notice to quit treated and control cases, despite the contingent nature the outcome, we did see a striking difference: .22 of treated notice to quit cases reaching litigation saw a jury trial demand versus .00 of control cases, $p < .01$.

are no discernible differences in outcomes between (a) the most any legal aid program can realistically do for a summary eviction potential client, *i.e.*, providing an offer of full representation by a staff attorney devoted exclusively to housing litigation, versus (b) a referral to a LFTD program after an instructional clinic that included assistance in filling out answer and discovery forms. Under this first interpretation, the Provider's instructional clinics and LFTD program were effective in the sense that they duplicated the results of a maximal intervention. On this interpretation, the Provider properly calibrated the intervention level.

The second interpretation is more troubling, and it raises questions regarding the effectiveness of the Provider's offer of full representation, the litigating style of its staff attorneys, and perhaps even its LFTD program.

We discuss each interpretation in turn.

6.1 First Possible Explanation: All Is Well

If one accepts that a relevant baseline for access to justice is the outcomes a potential client would realize if offered a traditional attorney-client relationship from a competent attorney, and if one remembers that randomized studies can provide no gold-standard statistical evidence beyond a comparison between treated and control groups, then a reasonable conclusion from these data is that all is well. The referral to the Provider's limited intervention, the LFTD program,, when added to the instructional sessions made available to all study subjects, duplicated the outcomes of an offer of a full attorney-client relationship. In fact, under this interpretation, there is even evidence to suggest that the service provider might consider rolling back its current version of the LFTD program to the level that existed during this Housing Court Study. That is, in the now-extant version of the LFTD program, attorneys make same-day court appearances before the judge to represent defendants in colloquies with the court (but not to engage in evidentiary hearings). Yet during the present Housing Court study, court appearances were not offered to control group occupants; rather, representation was limited to (i) hallway negotiations with landlords and/or their attorneys, and (ii) "mediation" sessions run by Housing Specialists. Yet a referral to this more limited form of assistance, a referral only 57% of those whose cases went to court accepted, duplicated the outcomes realized under an offer of the most aggressive form of assistance a legal aid organization can realistically provide.

Under this interpretation, the underlying facts, law, and judicial setting in this Housing Court and the population the Provider's outreach program reached were such that about two thirds of occupants should have lost possession of their units, despite the fact that 70% of them reported at intake that they would prefer to stay put (and that the majority of the remaining 30% had no identified fall-back option). The available defenses in cases involving nonpayment of rent and substantial monetary counterclaims were such that the landlords should have lost (and thus that tenants should have saved approximately) about 1.8 or so months of rent. Median vacate periods in litigation resulting in possession for the plaintiff should have been about 66 or so days. We are not housing litigators, but none of these results strike us as inherently unreasonable. They certainly are a far cry from the outcomes reported in the exposè-style analyses of eviction courts collected in Engler (2010).

As implied in the previous paragraph, this interpretation of the data takes as given all aspects of the adjudicatory setting in which the Provider operates. To the extent that the Housing Court and its personnel operated in a manner reasonably accessible to defendants receiving limited or no assistance, this accessibility may be due in part to the legal and cultural influence of the Provider's work over the years. There may be other aspects of the system worthy of study and possible imitation. The aggressive "mediation" style, backed by the unusual investigative, enforcement, and predictive authority exercised by the Housing Specialists, may be one candidate for further study. The judge's on-the-bench cajoling towards settlement may be another.

Like all other forms of gaining knowledge, randomized control trials such as the study reported here have limits. They provide no definitive statistical information beyond a comparison of treated and control groups. Our dataset was not large enough to allow us to rule out modest benefits due to treatment, but as we have repeatedly argued in this paper, modest benefits are probably not worth the candle in the present legal aid setting.

Under this view of the results, there is cause for considerable celebration. Under this view, this is the first study to assess quantitatively and in a credible manner the effectiveness of referrals to a limited assistance program in any United States adjudicatory setting, and the result was that the outcomes were essentially indistinguishable from those that would have been achieved via a more expensive program offering full representation. Surely this is good news.

6.2 Second Possible Explanation: Questions

We find ourselves unable to credit completely the interpretation of the data offered in the previous subsection. One source of our discomfort stemmed solely from the numbers and data in the Housing Court Study we report here. We have already hinted at aspects of the data we find unsettling, such as the number of defendant pretrial motions per case and the fraction of cases featuring a jury trial demand. A second, perhaps more substantial, source of our discomfort stemmed from an analysis that we concede to be risky: a comparison of the results we report here to those we observe in the District Court Study (Greiner et al. (2011)). In the District Court Study, we randomized offers of full representation from a different service provider's staff attorneys in Massachusetts summary eviction proceedings. In the District Court Study, however, there was no LFTD program to which to refer study subjects. As a result 96% of the control group proceeded with no legal assistance beyond an initial instructional clinic, which did include help in filling out answer and discovery forms. The District Court Study took place in a separate geographic area, and as its name suggests, the court involved was a more generalized District Court, not a specialized Housing Court.

While space does not allow a complete report of the other study here, we provide some basic figures, then attempt to draw admittedly risky inferences.

To be 100% clear: in this subsection, we are speculating. We could not randomize across geographic area, court, and study. We take what comfort we feel from the fact that guesses of this sort are unavoidable in most policy making.

6.2.1 Some Figures

First, we report some gut feelings in response to figures we observed in this Housing Court study. The number of prejudgment defense motions per case in treated group complaint cases, .18, strikes us as troubling, a feeling reinforced by the fact that it was close to the .16 per case in control group complaint cases. Recall that although over half of the control group complaint case occupants had limited legal assistance in the form of the LFTD, this program provided no assistance with the filing of motions. Thus, the number of motions filed by treated group defendants, 82% of whom experienced full representation from a provider staff attorney, was almost identical to the number filed by control group defendants, only 8%[34] of whom had any lawyer help in filing motions. Recall further that in the Housing Court, discovery materials were typically filed with the court, and that 83% of complaint cases (82% of treated and 84% of control) featured discovery requests from the occupant. We saw very few discovery responses from evictors but also very few motions to compel. In some cases, it may have been good strategy to bargain away the right to discovery responses in an overall settlement, but it was not immediately apparent why the occupant's posture in negotiations would not have been improved by having filed a motion to compel against a unresponsive evictor.

Similarly, we are not certain why occupants would not want to make greater use of a jury trial demand. Recall that jury trials were harder to schedule than bench trials, but that

[34] These 8% obtained full representation.

18% of treated group complaint cases versus 11% of control group complaint cases featured jury demands. Delay would generally seem to have been favorable for summary eviction defendants, meaning such defendants should not have been adverse to the scheduling issues jury trials presented. And the demand could ordinarily have be withdrawn if, in the unlikely event that the case appeared as though it would go to trial, the facts and law suggested that a bench trial would be more favorable.

We can perhaps gain some additional leverage from a comparison of the results in the present pilot study to the outcomes from the District Court Study, reported in Greiner et al. (2011). Like the present Housing Court Study, the District Court study featured a traditional legal aid provider who offered instructional clinics (these lasting 2-3) hours at which provider attorneys assisted occupants in filling out answer and discovery forms. As was true in the Housing Court Study, in the District Court Study some occupants were randomized to an offer of full representation, and some to no such offer.

There were other similarities. For example, in both studies the fraction of treated group occupants who accepted the offer of full representation was high (82% in the Housing Court Study, 93% in the District Court Study), while the fraction of control group occupants who found full representation despite not receiving a provider offer was low (8% in the Housing Court Study, 4% in the District Court Study). Table 1 in the Appendix reproduces the covariate information in the Housing Court Study from Table 5.1 and also shows the corresponding covariate information from the District Court Study. We cannot discuss all covariates, but we note rough similarities in many of them, such as the fraction of occupants who received 14-day notices to quit (alleging nonpayment of rent), average rent, fraction of postforeclosure units, and the fraction of occupants who suffered from physical disabilities.

There were also differences between the two studies. First, structurally, the District Court Study did not include notice to quit cases. Second, in the District Court Study there was no LFTD program to which control group occupants were referred. Third, the providers in the two studies used different outreach, intake, and case selection mechanisms. The District Court Study provider conducted individualized outreach to eligible occupants (in the form of letters sent shortly after summons and complaints were filed), and it aggressively screened cases, sending to us for randomization only those cases for which it thought full representation would transform the outcome of the case from unfavorable to the occupant to favorable for the occupant.

Fourth, there were some differences in background characteristics of the occupants in the two studies. Although we discuss these differences briefly in this paragraph, we note that they could have been a function of the different outreach, intake, and screening mechanisms the providers in the two studies used. Turning to the differences themselves, again, Table 1 in the Appendix has the relevant figures. There were notable differences in the fraction of occupants on Section 8 or in public housing (the fraction was lower in the Housing Court Study), the fraction of occupants who needed interpreters (higher in the Housing Court Study), and the amount of time provider attorneys were given to litigate cases (higher in the Housing Court Study).

Fifth, the fraction of evictors who used attorneys in the District Court Study (90%) was higher than the corresponding fraction in the Housing Court Study (57-63%).

We have no way to assess the combined effect of these similarities and differences. For some of the factors listed above, we might hypothesize the sort of effect they could have, but for these factors as to which we might have strong prior beliefs, the story conflicts. For example, it is hard to see how needing an interpreter would have been helpful to an occupant, so all other things equal (which, of course, they were not), we might have expected control group occupants in the Housing Court Study to experience *less* favorable outcomes than the control group occupants in the District Court Study. On the other hand, the lower fraction of evictors with lawyers in the Housing Court Study might lead one to surmise that control group occupants there experienced *more* favorable outcomes than their District Court Study counterparts.

Meanwhile, other factors listed above, such as the difference in the fraction of occupants with Section 8 vouchers, could have cut either way. If an occupant had a Section 8 voucher, federal law imposed duties on the evictor that would not otherwise have existed, which might have provided additional leverage with which an attorney could work. On the other hand, housing attorneys have informally advised us that the consequences to an occupant of losing a Section 8 voucher (as could have happened if an occupant had suffered a judgment of eviction) were so great to the client as, perhaps, to deprive an attorney's threat to take an eviction case to trial of any credibility.

We do not have information sufficient to hazard a guess regarding the cumulative effect of these differences. We have not attempted an across-study statistical analysis, fearing that doing so might lend greater credence to this cross-study comparison than it deserves.[35]

With all this in mind, we now compare outcomes across the two studies. Table 5 provides means and standard deviations for the treated and control groups across both studies for what we believe to be the two most important outcome variables: actual possession and EvictorMonsRentLost. Actual possession is shown in terms of the fraction of cases in which the evictor obtained possession, so as to be consistent with Table 3; thus, lower values were better for the occupant.[36] For "EvictorMonsRentLost," too, lower (meaning more negative) numbers are better for tenants. Also included in Table 5 are means and standard deviations for two outcomes that might in part capture the amount of legal pressure the occupant was placing on the evictor in the litigation: the number of occupant prejudgment motions, and

[35]Nor have we applied observational study techniques to this cross-study comparison. It is not easy to conceptualize a hypothetical intervention or counterfactual that would allow us to analogize a cross-study comparison here to a randomized experiment. Regarding the desirability of a hypothetical intervention and the essential nature of a counterfactual, see Neyman (1990 reprint and translation of 1923 original), Rubin (1973), Rubin (1974), and the primary paper and discussion of Holland (1986).

[36]For the actual possession variable, the Housing Court Study figures include both notice to quit and complaint cases. If we limit the analysis to complaint cases only, the results are even less favorable to the occupants: in .74 of treated group complaint cases, versus .66 of the control group, the evictor obtained possession.

the rate at which jury trials were demanded.

Outcome	Housing Ct Trtd Mean (SD) (N = 85)	Housing Ct Cntrl Mean (SD) (N = 99)	District Ct Trtd Mean (SD) (N = 76)	District Ct Cntrl Mean (SD) N = 53
ActualPossEvictor*	.67 (–)	.66 (–)	.34 (–)	.62 (–)
EvictorMonsRentLost	-1.8 (2.1)	-1.6 (3.2)	-9.4 (20)	-1.9 (4.6)
JuryTrialDemand	.18 (–)	.09 (–)	.81 (–)	.74 (–)
NumPreJudMotsOcc	.18 (.69)	.16 (.53)	1.4 (1.6)	.81 (1.5)

Table 5: *Side-by-Side Comparison of Outcomes, Housing Court & District Court Studies: This table shows outcome (unweighted) means and standard deviations for the treated and control groups for the present, Housing Court Study and the other pilot study we evaluated, which was conducted in a geographically separate District Court. In the rows with a "*", the Housing Study results came from both notice to quit and complaint cases; the other results came from only complaint cases. In the two key outcome variables of possession and number of months rent lost by plaintiffs (and thus probably saved by defendants), there was a striking similarity between the treated and control groups in the Housing Court Study and the control group in the District Court Study. But for these two critical outcomes, the treated group in the District Court Study was strikingly different in a way more favorable to occupants. The number of prejudgment motions filed by the defendants and the jury trial demand rates might provide one possible explanation of why this was the case.*

To the extent that cross-study comparisons are relevant, the results are striking. On the two primary outcomes, the Housing Court Study treated group, the Housing Court Study control group, and the District Court Study control group look similar. For each of these groups, roughly one third of occupants retained possession of their units, and on average litigation saved them around 1.5-2.0 months of rent. The treated group in the District Court Study fared better: roughly two thirds of defendants retained possession, and on average (using an intensely conservative measurement, as explained in Greiner et al. (2011)) litigation saved them 9.4 months of rent.

There may be some suggestion of a possible explanation of these figures in the next two rows of Table 5. Comparing the two treated groups, jury trials were demanded at roughly four times the rate in the District Court Study than in the Housing Court Study,[37] while

[37]The difference in the jury trial demand rate between the treated and control groups in the District Court Study was not statistically significant (a permutation p-value was .53). Thus, this variable alone would not

defendant pretrial motions were filed roughly at eight times the rate in the former study.[38]

6.2.2 Questions

The statistics reported in the previous subsection raise questions about the "all is well" explanation articulated in subsection 6.1. First, in the Housing Court Study, did the Provider's intake and screening mechanisms cull the set of potential clients so as to isolate those less in need of assistance? Second, what role does the forceful brand of "mediation" practiced by the Housing Specialists play in explaining the results we observe? Third, and closely related to the second, was the litigating style of the Housing Court Study Provider's staff attorneys insufficiently assertive (or, to use a more loaded term, confrontational) to protect their clients' rights? We lack sufficient information to answer these questions definitively.[39] For the present, we offer suggestions about the these three questions.

explain the more favorable results by the District Court treated group. Rather, the idea here would be that the jury trial demand created a setting in which an attorney could apply pressure, perhaps because the threat of taking a case to a jury was more credible when issued by an attorney.

[38]The difference in the average number of defendant pretrial motions between the District Court Study treated and control groups was probably statistically significant, although not hugely so (p = .04). We note that the District Court Study service provider staff attorneys did provide occasional assistance to control group occupants in filing motions, although not in arguing them to the court or in negotiating with the landlord.

[39] Stapleton and Teitelbaum (1972) conducted two simultaneous studies on the effect of offers of counsel in the juvenile delinquency setting. When the two studies produced different results, the authors were able to draw on thorough, non-statistical information sources, including case reports written by the attorneys the authors had hired to conduct the representation. With this information available, the authors were able to hypothesize with considerable credibility that the difference in the results of the two studies was best explained by the courtroom setting in which the attorneys operated, particularly the judicial attitude towards the lawyers' attempts to assert their clients' procedural and substantive rights. In the study in which the offer of representation had no apparent effect, the study lawyers felt as though the judges would punish their clients if they sought to make objections to assert their clients' rights to confrontation and against self-incrimination. The case reports the lawyers wrote in this study reflected the attorneys' chariness toward making what they feared would be perceived as obstructionist legal objections and motions. In the study in which the offer of representation had a statistically significant effect, the juvenile system more closely resembled an adult criminal court system. The attorneys' case reports stated that they felt few constraints on their ability to assert their clients' rights.

Our point here is decidedly not to suggest that the Housing Court in our study suppressed or even disfavored defendant assertions of their rights. We received no report to this effect from service provider attorneys, nor do our personal observations suggest that such was the case. Our purpose in summarizing the conclusions reached in Stapleton and Teitelbaum (1972) is to highlight the importance of an overall information-gathering effort. The rich information reported in Stapleton and Teitelbaum (1972) is a lesson in the usefulness of early planning for, and some minimal funding of, an evaluation when attempting to assess the effect of legal interventions, particularly in legal services.

First, as noted above, the occupants who became part of this Housing Court study did so because they, not the Provider, initiated contact by calling the Provider to request assistance. Further, they were required to attend a meeting in the Provider's offices to be study-eligible. In contrast, the District Court Study service provider did individualized outreach to potential clients, and it recruited approximately 30% of study occupants from chance meetings (or on-the-spot judge referrals) in court. In Greiner and Pattanayak (2011), two of us articulated the hypothesis that, in some settings, relying on a client-initiated intake system may cull potential clients in the sense that only (or primarily) those with stronger organizational and motivational skills, and greater socioeconomic connections, end up requesting assistance. If so, then in some settings a client-initiated intake system may narrow the pool of potential clients to those who need assistance less. Here, the requirements in the Housing Court Study that the occupant initiate contact and attend a meeting may have done just that.

Second, the Housing Specialists in this Housing Court practiced an unusually forceful brand of "mediation.' Further, in the unusual cases in which 'mediation" failed to produce an agreement and the case went to the judge, the judge in this Housing Court further cajoled the parties to settle. Perhaps the combination of these two practices, particularly the first (which affected the greater number of cases), left little room for attorneys to be effective. Under this hypothesis, the question becomes whether this result was a good thing. If one at least partially credits (again, at substantial peril) the District Court versus Housing Court cross-study comparison, then there is reason to question whether the fraction of occupants remaining in possession was lower than it should have been, and whether the financial consequences should have been more favorable to the tenants. If so, were the Housing Specialists' forceful mediations, partially justified on access-to-justice grounds, in fact desirable?

Third, and closely related to the second, the fraction of cases featuring a jury trial demand and the average number of pretrial motions per case in the Housing Court Study were both (a) low, and (b) similar between treated and control groups. The low jury trial demand rate was striking because in most cases, the decision on whether to demand a jury was made in the Provider's instructional clinics, *i.e.*, before randomization. The point is that the Provider's staff attorneys had to have been advising occupants not to demand juries. The low pretrial motion rate was also striking. Again, the LFTD program did not extend to assistance in filing motions, so on average the motion activity of a treated group of occupants, 82% of whom enjoyed full representation, was the same as a control group of occupants, 8% of whom enjoyed full representation. The combined effect of all this was to suggest that, in contrast to the District Court Study, the Housing Court Study featured staff attorneys who used a non-confrontational style of representation. At least on the basis of these figures, the Housing Court Study Provider staff attorneys chose not to make things unduly difficult for either the Housing Court (by demanding hard-to-schedule jury trials) or the evictors (by filing, say, motions to compel when discovery responses went unanswered).

We do not suggest that, a priori, we knew or would know whether a nonconfrontational lawyering style would produce better or worse results for represented occupants. A priori, one might have argued that an attorney could produce best results for her clients if she got on a judge's good side, particularly if all relevant cases were before the same judge, and to get on that judge's good side, an attorney had to avoid legal strategies that caused scheduling problems. Similarly, a priori, one might have argued that a facilitative approach to adversaries might produce better results for one's clients. Of course, a priori, one might have argued the opposite. Our point is that the contrast in results between the District Court Study and the Housing Court Study, particularly when combined with the results of prior research,[40] begins to build an empirical case that an assertive style of lawyering may pay dividends.

We close this paper with a final question: if our points regarding lawyering style have

[40]See the comparison between the two studies conducted in Stapleton and Teitelbaum (1972), as summarized in note 39.

merit, what was it about the institutional setting in which the Housing Court Study Provider staff attorneys operated that led them to adopt a less desirable modus operendi? We note two facts. First, two staff attorneys spent half of their time fully representing study treated group clients, but they spent the other half of their time implementing the LFTD program. Second, neither was a long-time housing attorney, although one was an experienced litigator. Thus, it is possible that the institutional setting in which these attorneys operated made it more difficult for them to differentiate full representation from LFTD duties. If this explanation has even some merit, it substantially complicates the picture of the legal aid setting that suggests that simply funneling more funding to legal aid providers will necessarily result in better outcomes for indigent clients. The institutional setting may matter greatly in determining how effective such funding might be.

If any or all of these aspects of the setting in which this study took place are salient, they complicate the "all is well" hypothesis articulated in subsection 6.1, and they suggest greater attention to the socioeconomic and institutional environment in which access to justice measures operate, a point two of us have made before (Greiner and Pattanayak (2011)).

7 Variable Name Key

We provide here a key for the variable names used in Section 5. In general, any variable name beginning in "Is" signifies an indicator for whether the condition described in the rest of the variable name is true. Any variable name ending in "All" means that all observations were used in the calculations; "All" should be contrasted with "Pos," which indicates that only variables with positive values are included in the calculations.

Name	Explanation
IsNTQCase	At intake, case at notice to quit ("NTQ") (1) or complaint (0) stage
IsNTQType14D	Whether NTQ type was 14 days
IsNTQType30D	Whether NTQ type was 30 days
NTQAmtAll	Amount arrears alleged in NTQ, all cases, no arrears coded as 0
NTQAmtPos	Amount arrears alleged in NTQ, only cases where arrears > 0
CompAmtAll	Amount arrears alleged in complaint, all cases, no arrears coded as 0
CompAmtPos	Amount arrears alleged in complaint, only cases where arrears > 0
IsPostForecl	Whether housing unit was post-foreclosure
IsHmOwn	Whether occupant was former homeowner who suffered foreclosure
RentAll	Amount occupant share of monthly rent, all cases, no rent coded as 0
SecDepAll	Amount occupant security deposit ("SecDep"), all cases, no SecDep coded as 0
SecDepPos	Amount occupant security deposit, only cases where SecDep > 0
LastMonAll	Amount occupant last month's rent ("LMR"), all cases, no LMR coded as 0
LastMonPos	Amount occupant LMR, only cases where LMR > 0
IsDefWantsStayUnit	Whether occupant at intake reports wanting to stay in the uint
IsSec8	Whether occupant has a Section 8 voucher
IsPubHs	Whether the unit is public housing
IsFem	Whether occupant who contacted service provider was female
Age	Age of occupant who contacted service provider
IsHisp	Whether occupant who contacted service provider was Hispanic
IsBlack	Whether occupant who contacted service provider was black
IsWhite	Whether occupant who contacted service provider was white
IsNeedsInterp	Whether occupant who contacted service provider needed an interpreter
IsMentDisab	Whether someone in the unit had a mental disability
IsPhysDisab	Whether someone in the unit had a physical disability
NumInUnit	Total number of persons living in unit at time of intake
Num<18InUnit	Number of persons under 18 years old living in unit at time of intake
DaysIntakeToComp	Number of days from intake to complaint (may be negative)
IsFiledDistCt	Whether litigation in the matter was filed in a District Court, all cases

Table 6: *Key for Table 1*

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