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

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September 1, 2012

Abstract

We persuaded entities conducting a civil *Gideon* pilot program in summary eviction cases to allow us to randomize which potential clients would receive offers of traditional attorney-client relationships from oversubscribed legal aid staff attorneys and which would be referred to a lawyer for the day program. We examine outcomes related to whether matters not yet in litigation reached court, possession of the unit, monetary consequences of non-payment of rent cases, and court burden. We find no statistically significant evidence that the Provider's offer of full, as opposed to limited, representation had a large (or any) effect on any outcome of substantive import. We explore several possible interpretations of our results, and we caution against both over-interpretation and under-interpretation.

*The authors thank Jim Breslauer, Sheila Casey, Glenn Cohen, Russell Engler, Adriaan Lanni, Ben Sachs, Matthew Stephenson, Jed Shugerman, Holger Spamann, Mark Wu, and Richard Zorza for comments and suggestions on earlier drafts of this piece. They also thank Tim Taylor for truly outstanding research assistance. This paper benefited from suggestions received during presentations at the University of Chicago Law School Law and Economics Workshop, the Columbia Law School Law and Economics Workshop, the Rothberger Conference on Access to Justice, and the Conference on Empirical Legal Studies. The usual caveats apply.

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1 Introduction

This article reports the findings of a randomized control trial ("RCT") comparing the effectiveness of two alternative programs of legal representation for occupants[1] of housing units in parts of the Massachusetts North Shore. In this study, each study-eligible occupant facing eviction from her housing unit who contacted a particular legal aid Service Provider participated in a one- to two-hour instructional clinic on the summary eviction process, which included help in filling out answer and discovery forms for cases in litigation, or in a meeting with a Service Provider staff attorney. Subsequently, each consenting occupant 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 unbundled assistance consisted of court-hearing-day-only representation in hallway settlement negotiations and mediation sessions (but not in court appearances or in filing motions) through a lawyer for the day ("LFTD") program. The assignment to treatment or control group resulted in real differences in the level of legal assistance provided: occupants assigned to the treatment condition received an average of about 12.4 hours of lawyer time, versus an estimated 1.7 hours of lawyer time for control group occupants. We compare results for the treated and control groups.[2]

This study stands at the intersection of two trends or movements, one somewhat nascent, the other in full bloom. The first trend is an effort to make the fields of access to justice and court administration more evidence-based. Several (*e.g.*, Abel (2010), Zorza (2009))

[1]Ordinarily, we refer to the persons subject to eviction, our study subjects, as "occupants." As we explain, some (but not all) occupants' matters reached litigation. We use the term "defendants" to refer to the set of occupants whose cases reached litigation. We refer to the persons or entities attempting to evict occupants as 'evictors" or, if we wish to refer more narrowly to the set of evictors whose cases had reached litigation, "plaintiffs."

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

have called for such a change and for a corresponding focus on the use of RCTs. This study is the third of six RCTs we have in various stages of development; articles reporting the results of the first two will be published soon (Greiner and Pattanayak (2012) Greiner et al. (forthcoming 2013)). The second trend is strong movement among legal services providers towards unbundled or limited assistance programs in the place of traditional, full representation. We discuss this trend further below.

With respect to the first trend, 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[3] 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[4] provided sufficient money for two of the nine pilots, both in summary eviction proceedings. We agreed to serve as evaluators for both; our services were gratis 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. (forthcoming 2013)), although we use results from it to inform the present study. In the present pilot, most of the cases that reached litigation were heard in one of the five Massachusetts specialty courts, called Housing Courts, whose caseload consisted primarily of residential summary eviction cases. We report the results of this "Housing Court Study" in this article.

[3]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).

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

With respect to the second trend, 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 (American Bar Association Standing Committee on the Delivery of Legal Services (2012)). By "unbundled" or "limited assistance representation" we mean when a licensed attorney provides a set of legal services short of those that would be provided in a traditional attorney-client relationship accompanied by the expectation among all concerned that the client will proceed pro se on all other aspects of the matter. Any coherent definition of the term "unbundled" representation would include advocacy in a 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. 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 se litigants. Despite this overwhelming trend, 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 related to the asserted reason for the eviction; or (iii) the occupant was in danger of suffering a substantial injustice unless counsel were offered.[5] In part, the Task Force considered its approach as a way to address to cost and administrability concerns raised by civil *Gideon* opponents (*e.g.*, Barton and Bibas (2012)). As explained below, however, it remains unclear how closely the Task Force's vision of offering full representation only 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 means that we ware 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 full representation at a programmatic level. This is not to say that studies capable of detecting smaller effects are unnecessary; to the contrary, we invite those who believe that RCTs capable of measuring modest effects are necessary to join our efforts to find existing legal assistance programs willing to engage with us in larger n research efforts.

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

[5]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).

involvement in or attention to litigation cases; or on any other outcome. To the contrary, the treated and control group point estimates (meaning averages and the medians) on almost all measures were 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 possible interpretations of our findings below.

2 Literature Review

Greiner and Pattanayak (2012) 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 and discusses why RCTs are essential in this context. 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. (forthcoming 2013)), 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). None involved limited assistance programs.

Because the findings of Stapleton and Teitelbaum (1972) presage a potential explanation for the results we report here, we pause to summarize briefly the results of this study. Stapleton and Teitelbaum conducted RCTs comparing offers of traditional representation to juveniles facing delinquency proceedings to no assistance in two courts. In the "Gotham" court, the offer of representation had no discernible effect on case outcomes, while in the "Zenith" court, the offer of representation causes a statistically significant drop in delinquency rates (from 56% in the no-offer group to 40% in the offer group). Using rich sources of non-quantitative evidence, and after carefully considering and rejecting alternative explanations, Stapleton & Teitelbaum attributed the difference in results to the lack of formality of the Gotham court procedures and the judge-centric nature of the adjudicatory process there. These two features allowed the judges to chill attorneys from formally asserting their clients' rights against self-incrimination, to confrontation, to the exclusion of hearsay, etc. Thus, circumstances prevented the Gotham attorneys from deploying an aggressive or confrontational lawyering style. We return to this point below.

Regarding summary eviction and unbundled representation, Engler (2010) provides a brief literature review of the felt need for legal assistance in residential eviction matters, Sandefur and Smyth (2011) documents the extensive deployment of unbundled representation programs across the United States, and Jennings and Greiner (2012) provides a bibliography of publications focussing on adjudicatory proceedings. As these references discuss, legal aid providers have for decades constituted unbundled assistance efforts as a way to attempt to provide some help to persons in need when resources did not allow for an offer of a traditional attorney client relationship to all eligible potential clients. These unbundled programs ranged from LFTD programs to hotlines to instructional clinics to informal telephone advice to regularized ghostwriting of pleadings. Summary eviction proceedings and family law have been principal loci for the focus of these efforts (American Bar Assocaition Section of Litigation Modest Means Task Force (2003)). For example, in Massachusetts, early LFTD programs in summary eviction began in the late 1980s and early 1990s. These programs began by offering limited advice and counseling to pro se litigants, gradually expanded to include representation in mediation sessions, and moved from there to having attorneys argue motions or engage in court colloquies. These and other limited programs predated the Massachusetts Supreme Judicial Court's authorization of unbundled legal assistance in court proceedings by several years (Massachusetts Supreme Judicial Court (2009)).

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 are designed to adjudicate quickly who has the right to possess a housing unit. Summary eviction courts also have 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.

To enter into the study, study-eligible occupant made initial contact with the Provider at one of 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 demanding that the occupant vacate the unit within a specified time period;[6] or (b) when the evictor, now a plaintiff, had filed a summary eviction lawsuit. We refer to cases in which the occupant first contacted the provider after the evictor had issued a notice to quit as "notice to quit" or "NTQ" cases; we refer to cases in which the evictor contacted the Provider after the evictor had filed a lawsuit (*i.e.*, later in the lifetime of the case) as "complaint" cases. 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 expires, the evictor may file a summary eviction action. Special rules of procedure govern summary eviction cases. These rules are designed to expedite proceedings so as to adjudicate the right to possession of the unit quickly. For example, once a would-be evictor, now a plaintiff, files and serves a summary eviction complaint, the clerk sets the trial for the first available date no fewer than ten days later. If a plaintiff succeeds in obtaining a judgment for possession of the unit, the plaintiff ordinarily must wait at least ten days for a writ of execution to issue. At that point, the plaintiff may cause the issuance of a 48-hour constable's notice, and, only upon expiration of this notice, may 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.

Both Massachusetts and federal law provide a variety of defenses, counterclaims, and procedural devices that an occupant, now a defendant, may use to resist a plaintiff's action for possession. For example, if a defendant files and serves discovery requests upon a summary eviction plaintiff within a week of the entry of the complaint, the trial is automatically continued for two weeks, thus buying the defendant valuable time to prepare her defense,

[6]The date stated and the corresponding length of time from the date of the notice to the vacate date depend on the reason for the eviction. Fourteen-day notices to quit correspond to evictions for nonpayment of rent. Thirty-day notices to quit correspond to most other circumstances, including eviction for conduct in violation of leases.

to bargain with the plaintiff, and possibly to search for alternative living arrangements. In addition, substantive defenses can arise from Massachusetts statutes, Massachusetts common law, Massachusetts regulations, federal statutes, and federal regulations, depending on the type of housing unit involved. Few would characterize applicable substantive summary eviction law as simple or easily accessible.

3.2 The Workings of the Housing Court

In our study, evictors had two choices of where to file: the Northeast Housing Court or one of the several Massachusetts District Courts with geographic jurisdiction over the housing unit. Although there are several Massachusetts District Courts with geographic jurisdiction overlapping the Northeast Housing Court, due in part to rules allowing easy transfer of cases from District to Housing Court, about 90% of the litigated cases in the present study were resolved in Housing Court, so we focus our attention in this descriptive section there.[7] We describe here the mechanisms behind a 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.

In our dataset, evidentiary hearings of any kind (including trials) were rare, occurring in only seven of the 137[8] cases in our dataset that went to litigation. Defendant defaults of any kind were also rare.[9] Moreover, even though each litigated case in our dataset was ordinarily called before the Housing Court multiple times, each case saw an average of only .24 contested rulings. Thus, the overwhelming majority of litigated cases, and the overwhelming majority

[7]To clarify, all cases no matter where litigated were included in our dataset.

[8]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 102 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.

[9]Default events, by which we mean any kind of court ruling premised upon the defendant's failure to appear, occurred in only 19 of the 137 litigated cases.. A ruling based on a defendant's failure to appear could be (and often was) overturned when the defendant made a motion for relief from default. In other cases, an occupant's failure to appear, and a court ruling in favor of the evictor, would be followed by a settlement that effectively removed the default (although not always on terms favorable to the defendant). In only one case in our dataset did a judgment issue due a defendant's failure to appear in court at any time.

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, more often in the hallway outside the courtroom on the day of a court hearing.[10] 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 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 (evictors and occupants) 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[11] 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.

3.3 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 clin-

[10]In contrast to the practice of the District Court as described in Greiner et al. (forthcoming 2013), the Housing Court did not routinely insist that parties engage in hallway settlement negotiations.

[11]The same judge presided over almost all hearings in almost all cases in our dataset.

ics lasting 60-90 minutes on housing law and Housing Court procedures,[12] and the LFTD program. The instructional clinics were held at least weekly, typically for small groups of occupants. Each attendee upon whom a summons and complaint had been served received 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 the study, 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.[13] The Provider administered the LFTD program, but it 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 issued that brought the case to court that day. Sessions before the Housing Specialist typically lasted 20-30 minutes, after there might be further negotiations or some time needed to document a settlement agreement.

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

[12]During the Housing Court Study, local landlord associations sponsored periodic educational sessions that included instruction on Housing Court procedures.

[13]For cases that were not part of 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 Housing Court Study.

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 summary eviction 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 (see the Appendix for details). We tracked case outcomes through a combination of telephone calls (roughly once every two months until the matter was resolved) and 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. For matters that went to litigation, each summons and complaint served upon an occupant included an attached flyer notifying the defendant of the availability of LFTD assistance and stating that the defendant could call NLS ahead of time to reduce waiting times and receive additional information. But nothing in the notice advertised the possibility of receiving full representation, and the notice further stated, "You can find out eligible [sic] when you arrive in court." Thus, there was no individualized outreach referencing the possibility of full representation to the occupant population. Second, as noted above, two Provider staff attorneys each dedicated half of their time to providing full representation; for each, the remaining time was dedicated to running the instructional clinics and to the LFTD program, *i.e.,* to unbundled representation. As discussed below, however, this does not mean that the treated and control groups experienced similar levels of representation. To the contrary, there was a large difference in terms of the number of attorney hours dedicated to treated and control group occupants' cases. 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 aggressively 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 may form part of a potential explanation for our results.

4 Hypotheses

In case there is doubt on the matter, we articulate the hypotheses we held prior to its implementation. First, we expected that occupants would find traditional, full representation both valuable and difficult to obtain from sources other the pilot project. For this reason, we expected that a high percentage of treated group occupants would accept the offer of full representation, while a low percentage of control group occupants would find full representation elsewhere. We hypothesized that attorneys would be able to negotiate settlements in notice to quit cases before evictors filed suit, so that an offer of representation would result in a large decrease in the number of lawsuits filed. We expected that, for control group occupants whose cases went to court, usage of the LFTD program would be high. On the latter point, we reasoned that we would be dealing with a population of occupants who had the wherewithal to contact the Provider by telephone prior to their first court dates and who would receive an express Provider invitation to make use of the LFTD program.

In terms of substantive results, we hypothesized that the additional time for factual investigation, case development, and legal research that full representation would provide would lead to superior case outcomes in the treated group. We expected that there would be statistically significant differences in possession rates (with treated group occupants retaining possession more often), monetary consequences, and orders to make repairs. We expected treated group cases to last longer (as per Seron et al. (2001)), but were uncertain as to whether an offer of full representation would otherwise cause an increase in court burden.

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[14] background variables before summarizing essential results. We provide a more detailed summary of the results in the Appendix.

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 (described in greater detail in the Appendix). We used this method to test for differences in means (weighted and unweighted, although the two rarely differed in a substantive way), in medians, and in .25 and .75 quantiles for both covariates and outcomes. We produced 95% intervals for important results using both permutation techniques (for continuous variables) and regression-based statistical modeling (for both discrete and continuous outcomes). The Appendix explains both methods. Speaking broadly, with respect to the regression-based methods, we explored three sets of models: (i) "expert" models, in which we chose covariates we deemed likely to affect the variable of interest based on informal conversations with housing attorneys; (ii) "covariate balance" models, in which we chose the covariates with the lowest p-values from permutation tests in treated versus control comparisons; and (iii) "backward selection" models, in which we ran separate backwards selection algorithms for treated and control groups. Below, we report the results of the model that

[14] 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.

produces the largest intervals, but in any event, the models all produce the same substantive conclusions.

As noted above, we provide intervals for each of our most important results; we do not provide power calculations because "post hoc power calculations are not helpful"(Piantadosi (2005)). As we will explain, there is little danger of Type I (false positive) error in our dataset.

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 assignment of treatment/control.

Table 5.1 provides treated and control group means and standard deviations for 29 background variables ("covariates"), along with permutation test p-values for a difference in the unweighted[15] means and in the medians. Only four variables have p-values for a difference in means below .10. 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 prior beliefs 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 intensity of the evictor's desire to obtain possession were probably different from that in nonpayment of rent cases.

[15] We also examine 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 has a difference in means p-value of less than .05, and balance is generally better than that reported in Table 5.1.

Covariate	Treated Mean (SD) (N = 85)	Control Mean (SD) (N = 99)	P-Val Mean	P-Val Median
Is NTQ Case	.55 (–)	.56 (–)	.98	–
Is NTQ Type 14 Day	.55 (–)	.61 (–)	.70	–
Is NTQ Type 30 Day	.24 (–)	.27 (–)	.45	–
NTQ Amount All	902 (1546)	1065 (1636)	.43	.86
NTQ Amount Positive	1785 (1779)	1918 (1787)	.44	.20
Complaint Amount All	1656 (2112)	1772 (1880)	.74	.33
Complaint Amount Positive	2245 (2175)	2132 (1867)	.99	.53
Is Post Foreclosure	.14 (–)	.10 (–)	.66	–
Is Homeowner	.06 (–)	.05 (–)	.94	–
****Rent All**	***684 (332)***	***733 (394)***	***.08***	.25
Security Deposit All	404 (432)	419 (518)	.74	.35
****Security Deposit Positive**	***664 (365)***	***819 (441)***	***.04***	.12
Last Month All	253 (398)	275 (460)	.29	1.00
Last Month Positive	767 (291)	866 (390)	.28	.42
Is Occupant Wants To Stay Unit	.68 (–)	.70 (–)	.72	–
Is Section 8	.16 (–)	.23 (–)	.64	–
Is Public Housing	.06 (–)	.06 (–)	.82	–
Is Female	.75 (–)	.72 (–)	.62	–
Age	40 (12)	42 (13)	.26	.27
Is Hispanic	.32 (–)	.24 (–)	.19	–
Is Black	.18 (–)	.19 (–)	.46	–
****Is White**	***.59 (–)***	***.68 (–)***	***.07***	–
Is Needs Interpreter	.14 (–)	.13 (–)	.43	–
Is Mentally Disabled	.45 (–)	.33 (–)	.10	–
Is Physically Disabled	.35 (–)	.45 (–)	.14	–
Number In Unit	3.0 (1.8)	2.7 (2.0)	.53	.76
Number < 18 In Unit	1.4 (1.5)	1.1 (1.4)	.32	1.00
****Days Intake To Complaint**	***23 (33)***	***13 (37)***	***.09***	.29
Is Filed District Court	.30 (–)	.34 (–)	.59	–

Table 1: *Covariate Balance, All Cases: This table shows unweighted means and standard deviations for the treated and control groups. Any variable beginning with "Is" is 0-1, so all information is in the rate and the median is not a useful summary statistic. Any variable with the word "All" in it codes cases in which the variable is undefined as "0." Thus, "Security Deposit All" refers to the amount of a security deposit in all cases, foreclosure cases in which the occupant is a former homeowner (which would obviously involve no security deposit) as 0, while "Security Deposit Positive" refers to the amount of a security deposit only for cases in which that amount existed and was greater than 0. 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. One variable shows a p-value for the difference in means of less than .05 and three others show a p-value for the difference in means of less than .10. Three of these variables are continuous, and for none of them is the p-value for the difference in medians 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 are not among those our prior beliefs would lead us to deem particularly troubling.*

Because some of our analysis focuses only on notice to quit cases or only on complaint cases, we also examine the analogs to Table 5.1 for these subsets of cases. Among the notice to quit cases, the only large imbalance[16] is that the treated group has a far higher fraction of Hispanics (.40) and a far lower fraction of whites (.49) than does the control group (.18 Hispanic, .75 white); p-values for differences in unweighted means both variables are less than .01. Yet this difference does not translate into a large disparity in whether the occupant needed an interpreter ("Is Needs Interp" treated mean of .21, control mean of .15, p = .19). Although race-based or ethnic-based disparate treatment is always a concern and a possibility, we perceived no evidence of such in our study.

Among complaint cases, two variables have p-values for differences in unweighted means just under than .10, but we find neither result concerning.[17]

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 do some modeling to address the observed disparities, but the modeling and permutation results are similar.

5.2 Attorney Usage, Evictors and Occupants

In this subsection, we discuss the usage of attorneys among evictors and the occupants.

First, for evictors: In 47 of 82 complaint cases (57%), the docket sheet shows that the

[16]The variable "NTQ Amount Positive" has a statistically significant difference in medians (p = .02) but not in means (p = .32). The variable "Number In Unit" has a notable difference in unweighted means (p = .07) but not in medians (.67).

[17]The variable "Is Physically Disabled" has a p-value of .09. For the variable "Days Intake to Complaint," the 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 figure for the treated group here, *i.e.*, that the intake occurred before the complaint, apparently stems 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. The disparity in Days Intake To Complaint is 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.

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, we report compliance information for our study subjects, meaning occupant usage of full representation and the LFTD program as between treated and control groups. The results are summarized in Figure 1.

[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 are virtually identical to those in the notice to quit cases that reached litigation.

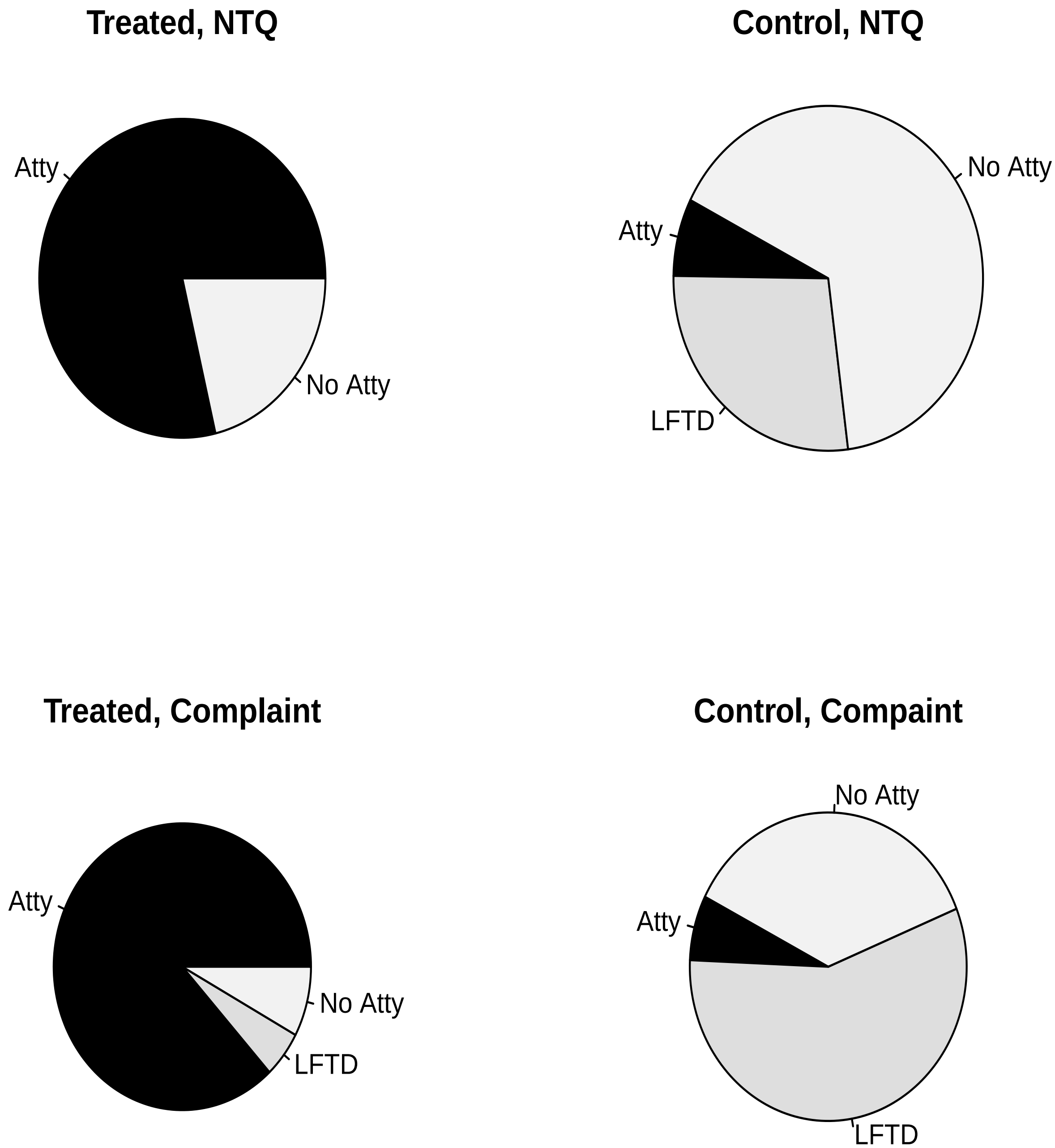

Figure 1: *Compliance: Full Representation and LFTD Usage by Occupants: The pie graphs indicate usage of full representation ("Atty"), usage of the LFTD program ("LFTD"), or self-representation ("No Atty") among the treated and control groups for NTQ cases (those in which the occupant contacted the Provider when an NTQ had been issued by before litigation had been filed) and complaint cases (those in which the occupant contacted the Provider after having been sued). The areas of the pies are proportional to the number of cases in each group. Full representation was highly valued (as evidenced by the high fraction of treated group cases, NTQ and complaint, that experienced it) but hard to obtain outside of the Study (as evidenced by the correspondingly low fraction among control group cases, NTQ and complaint). LFTD usage was lower than anticipated in the control group, although as explained in Note 20 and in the Appendix, the upper right pie chart suppresses a detail on this point.*

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.[19]

Figure 5.2 shows the compliance among the four groups of occupants in our study: those who called the Provider when they had received notices to quit but before being sued and who were offered full representation ("Treated, NTQ"); those who called the Provider when they had received notices to quit but before being sued and who were told to contact the LFTD program if their cases reach litigation ("Control, NTQ"); those who called the Provider after being sued and who were offered full representation ("Treated, Complaint"); and those who called the Provider after being sued and who were referred to the LFTD program (" Control, Complaint"). Over three quarters of treated group cases, both NTQ (79%) and complaint (87%), experienced full representation. In contrast, only 7% of control group cases (the same figure for both NTQ and complaint cases) experienced full representation. A negligible number of treated group occupants used the LFTD program. LFTD program usage among control group was approximately 40% overall, but this latter figure is somewhat distorting because NTQ cases were not eligible for the LFTD program unless they reached litigation.[20] Discounting NTQ cases that did not reach litigation, approximately 56% of control group occupants used the LFTD program.

Thus, Figure 5.2 suggests that almost half of the occupants who had the self-organization,

[19]The Provider felt strongly both during the present study and after the Study's results became public that occupants who had no further contact with the Provider after the initial office meeting or instructional clinic would experience less favorable outcomes than would occupants who took advantage of the Provider's services (principally the LFTD program). We did not examine the data to see whether this statement was correct. Even if it were true, it would have been impossible to attribute any improvement in outcomes to the Provider's services, as opposed to the distinct possibility that those occupants who used those services were comparatively more motivated, better able to bureaucratic systems and legal forms, more deeply invested in their cases, and/or possessing better factual arguments.

[20]The LFTD program was court-based, so it could only serve occupants whose cases reached court. Not all NTQ cases reached court. This fact matters primarily in the upper right pie graph. The "LFTD" portion of that graph represents 27% of all "Control, NTQ" cases, but it represents 47% of all "Control, NTQ" cases that reached court. Additional information appears in the Appendix.

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 litigation. 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, the latter strikes us as less plausible, given that (i) there were very few defaults 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 The Representation Provided to Occupants

Unsurprisingly, the assignment of an occupant to the treated or control group translated into real differences in the representation that occupant experienced. Provider attorneys recorded the post-randomization hours spent on each treated group case. They did not do so for cases that were part of the LFTD program, but we estimate the number of attorney hours expended in these cases by examining the court dockets and making the unrealistic assumption that the LFTD program expended two hours each time a case was called to court.[22]

[21] Ten of the 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.

[22] This assumption is unrealistic for two reasons. First, it is unlikely that occupants whose cases were called to court multiple times used the LFTD program each of those times. For example, one case was called to court seven times; it is unlikely that the occupant would have found the LFTD program had capacity to serve her each of those seven. Second, the assumption that a LFTD program attorney could dedicate two hours to an occupant each time her case reached court is unrealistic. We suspect the true figure was between 60 and 90 minutes.

In addition, we estimate the number of attorney hours expended in the seven control group cases that experienced full representation by using the Provider attorney hours in similar cases. This choice also likely resulted in an over-estimate of the number of hours expended for control group cases. In all but one of the seven control group cases experiencing full representation, the attorney involved was a local practitioner

Even with this unrealistic assumption, treated group occupants experienced approximately 12.4 hours of total attorney work per case, while control group occupants experienced only 1.7 hours per case, for 10.7 hour difference, or a ratio of over seven to one (p ¡ .01, based on a permutation test).

What were the lawyers doing with this additional time? From our conversations with those involved in the Pilot and LFTD programs, we conclude that attorneys pursued a risk-averse representation style designed to a facilitate settlement, as opposed to a high risk, aggressive, or confrontational style designed to put pressure an opposing party. The attorneys reported that they researched relevant law and facts, met with clients, requested inspections of units, and negotiated with opposing counsel. They did not, however, file numerous prejudgment[23] motions or frequently demand jury trials. Treated group complaint cases[24] saw an average of .18 prejudgment motions per case versus .16 for the control, p = .92. The corresponding jury trial demand rates were .18 versus .09, p = .47.

5.4 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 to 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 have surprisingly similar point estimates. The similarity in the point estimates lend further credence to the hypothesis that the offer of a traditional attorney client relationship from

working for a small firm. We think it likely that the Provider staff attorneys, who were salaried, would expend more hours per case than would private attorneys, regardless of the fee structure (if any) involved.

[23] We focus on prejudgment motions because motions filed before judgment, such as to dismiss or for summary judgment or to compel discovery responses, are more likely to put pressure on an evictor. Nevertheless, as discussed in Table 5.4.4, below, there is also little difference between treated and control groups in total motions filed by occupants.

[24] We report here the results of complaint cases, but the results are similar if we include NTQ cases that reached litigation.

a Provider staff attorney, as compared to a referral to the LFTD program, caused no large differences in important outcomes.

5.4.1 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 contacted the Provider after receiving a notice to quit but before litigation had been filed; and complaint cases, in which intake took place after the evictor had filed litigation. A principal reason for including the former type of case in this Study 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 matter decreased court caseloads could bolster the argument for additional legal aid funding.

No such strong evidence emerges, 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, means that we are likely to detect only very large effects. 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.[25]

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, lead us to be more

[25] Applying Horvitz-Thompson weights narrowed the difference to .52 in treated group versus .57 in the control group, p = .25.

cautious here than with other results. It does not seem likely, however, that Provider offers of full representation produced very large reductions in the probability that a notice to quit case reached litigation.

5.4.2 Possession Outcomes

There are several variables associated with possession. In our view, the most important of these is which party actually ended up in possession at the end of the dispute between the evictor and the occupant. We code this variable for both notice to quit and complaint cases. As Table 1 makes clear, 69% of occupants reported wanting to stay in their units. Moreover, for many of the 31% of occupants who said they wanted to leave their units, the desire may have been somewhat aspirational. We requested that Provider attorneys ask occupants at intake 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 is not the only possession-related outcome of interest. For complaint cases, we also code additional possession-related outcomes, including whether a judgment of possession entered for the plaintiff and whether a writ of execution for possession issued. We do not code these variables for notice to quit cases because both a judgment and an execution are contingent outcomes; that is, they are defined in notice to quit cases only if these cases went to litigation, and not all cases did so.[26]

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

[26]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, render these frameworks practically unavailable.

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. For example, on the key variable of actual possession, 1-.67 = 33% of treated occupants actually retained possession versus 1-.66 = 34% of control occupants. Unsurprisingly, this one percentage point difference is not statistically significant. We observed differences of five and six percentage points in the other two possession variables, but these are also not statistically significant, and in any event, the control group has more occupant-favorable results than the treated group.

	Treated Rate	Control Rate	P-value
Actual Possession, Evictor*	.67	.66	.93
Judgment Possession, Evictor	.32	.27	.84
Writ Execution Possession, Evictor	.29	.23	.47

Table 2: *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 are coded such that lower numbers would be more favorable to study subjects, the defendants/occupants. Thus, the 67% treated group evictor possession rate corresponds to a 33% occupant possession rate. The results with the "*" come from both notice to quit and complaint cases; the other results come from complaint cases only. There are virtually no differences between treated and control groups, and permutation-based p-values are high. Weighted figures are similar to those shown here.*

We also produce intervals for the critical outcome of actual possession using the sets of models described in subsection 5.4.1. Each set of models produce 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.

5.4.3 Financial Consequences

For reasons discussed in the Appendix, we are able to analyze financial consequences only for complaint cases, so our dataset is correspondingly smaller. As was true of possession, there are several possible outcomes associated with financial consequences, but we find most of these unappealing due to the fact that money judgments in this dataset represent 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 call "Evictor Months Rent Lost," 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, an Evictor Months Rent Lost 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 we effectively equate a judgment from the court that (say) the occupant pay $5000 with an actual $ 5000 payment by the occupant to the evictor.

We do not bother with a table, chart, or graph here. The unweighted treated (offer of a traditional attorney client relationship from a Provider staff attorney) group mean for Evictor Months Rent Lost is -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 produces 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 is .82, suggesting that this one-fifth of a month's rent, in addition to being of little substantive import, is far from statistically significant. A permutation based confidence interval for the difference in means was (-1.4, 1.1), while the regression-based interval is (-1.7, 1.1).[27]

We also examine other measures of financial consequences we prefer less than Evictor Months Rent Lost, but none show statistical or substantive significance. For example, we examine the amount of a money judgment and the amount of any monetary execution. For the money judgment, the treated group mean is \$903 in favor of the evictor versus a control group mean of \$486 for the evictor (higher numbers here are detrimental to the occupant). The permutation p-value is .40, and a permutation interval for the difference in means is (-\$815, \$915). For the amount of a monetary execution, the treated group mean is \$494, the control group mean is \$443 (higher numbers here are again bad for occupants), the permutation p-value for the \$51 difference is .99.

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) masks. To this end, Figures 2 and 3 plot the primary two outcomes in the same graphs. We look to see whether, for example, treated cases cluster in the top right (indicating that the occupant retained possession but with a less favorable financial arrangement) and bottom left (indicating that the occupant lost possession but obtained a more favorable financial arrangement) portions of these graphs, which might indicate that the treatment furthered

[27] Again, weighted figures are similar.

one or the other but not simultaneously both occupant goals of 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 draw from examining each outcome separately that no large treatment effects are likely.

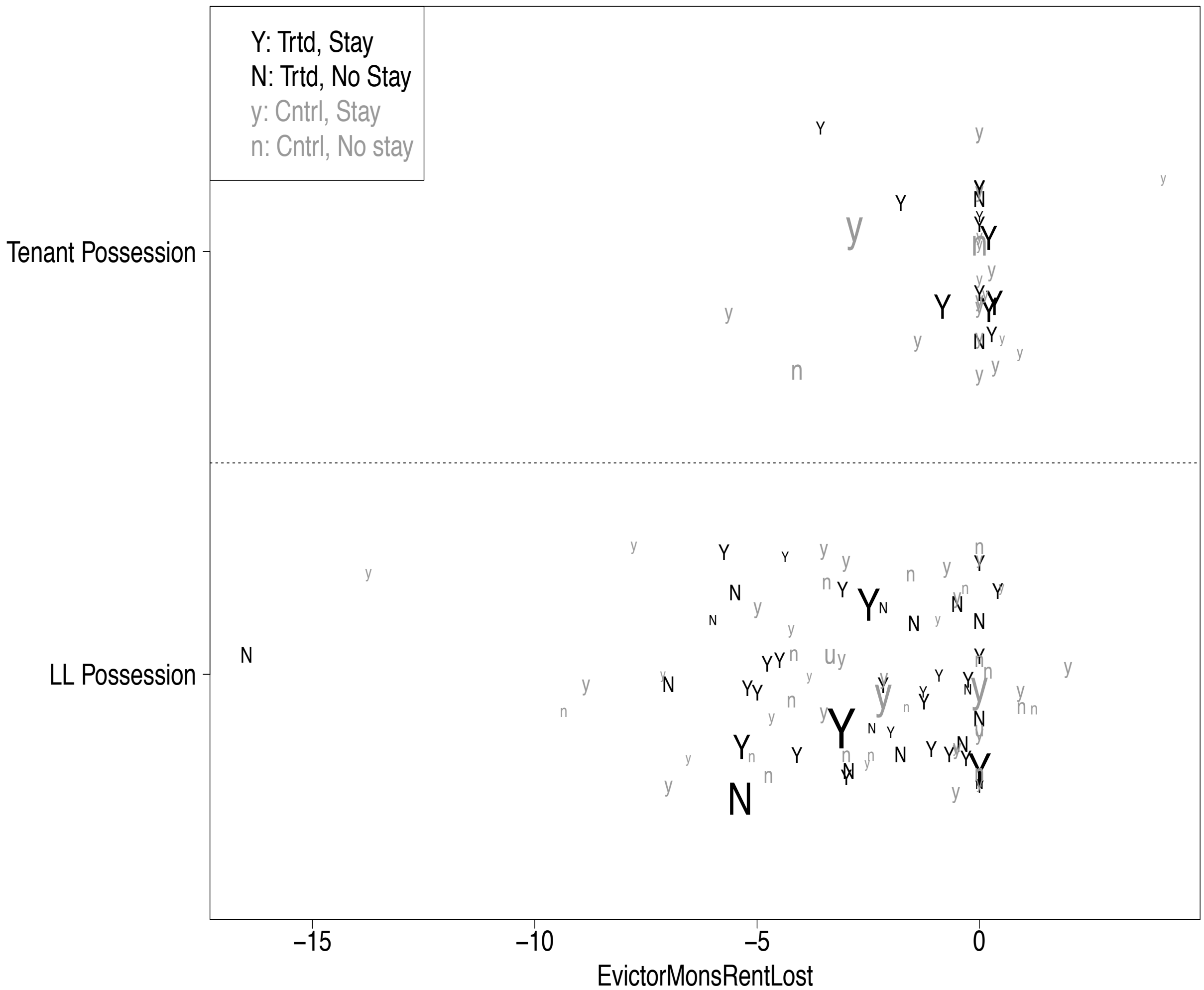

Figure 2: *Two Primary Outcomes: This graph plots the two most important outcome variables, possession (top versus bottom rectangle) and Evictor Months Rent Lost (on the x-axis). Lower case, grey letters represent control group cases. Capital, black letters represent treated group cases. The Y or y (N 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 jitter is solely to make it possible to see multiple datapoints with the same or similar Evictor Months Rent Lost. 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 supports the inferences of no large treatment effects drawn from testing each outcome variable separately.*

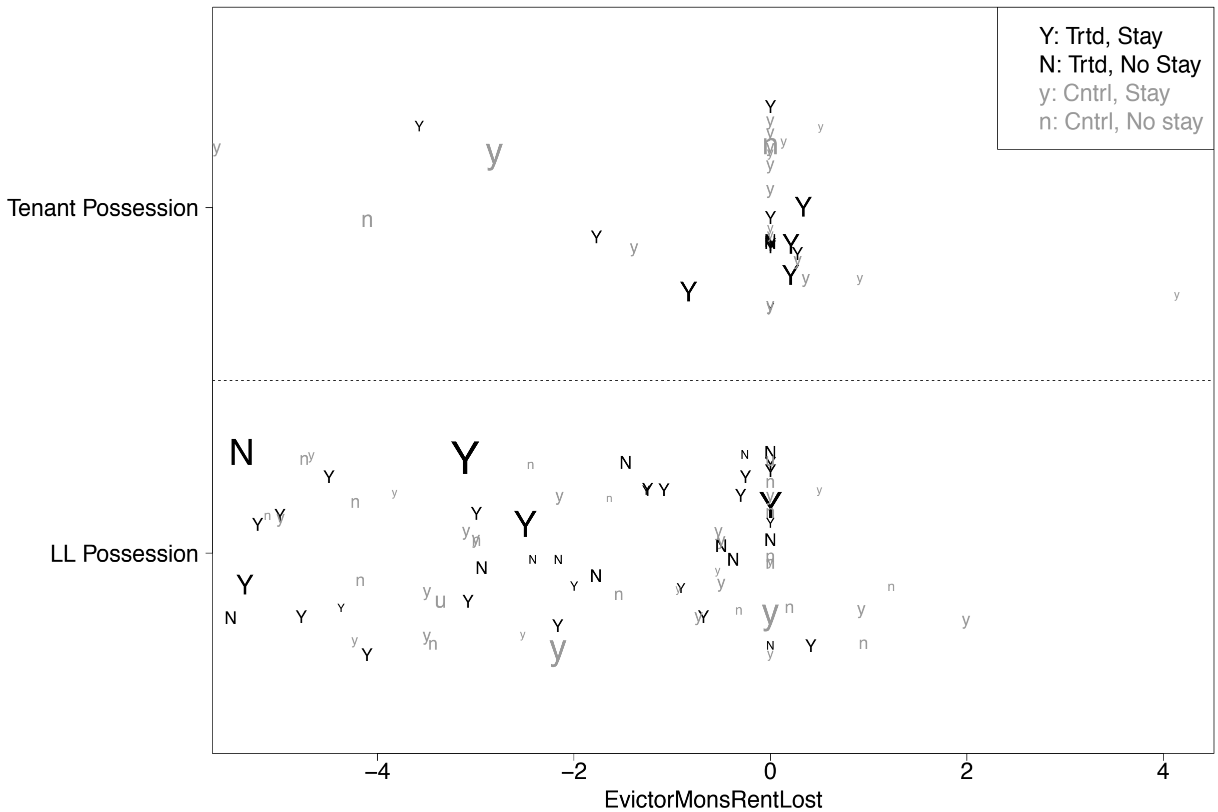

Figure 3: *Two Primary Outcomes: A Closer Look: This graph shows the same data as in Figure 2, except that the x-axis is truncated at -5 months of rent to ease visualization of the space that the vast majority of the data occupy. See the caption to Figure 2 for an explanation of the symbols used. Again, there is no apparent pattern in these outcome data, which supports the inferences of no large treatment effects drawn from testing each outcome variable separately.*

5.4.4 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 in days;[28] a variable we label "Number Judge Looks," a measurement based

[28]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 (2012)). More challenging is the presence of a point mass of control group cases reaching judgment at 24 days. Examination of these control group cases shows that the 24 days represent 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 explore normalizing transformations, including the logarithm and various roots. The transformations can reduce the influence of the right tail(s), but no monotonic transformation can 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.

on 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; "Number Judge Rulings," meaning the number of times the docket sheet discloses that the judge issued a contested ruling;[29] 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 4 summarizes the results, showing treated and control group (unweighted) means[30] 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
Number Judge Looks	2.1 (2.3)	2.1 (2.2)	.77
Number Judge Rulings	.18 (.46)	.32 (.98)	.23
Number PreJud Mots, Evi	.26 (1.0)	.14 (.63)	.69
Number PreJud Mots, Occ	.18 (.69)	.16 (.53)	.92
Number Tot Mots, Evi	.58 (1.6)	.57 (1.1)	.97
Number Tot Mots, Occ	.37 (.82)	.34 (.78)	.89

Table 3: *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. "Number Judge Looks" ("Number Judge Rulings") 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 is no evidence of a treatment effect with respect to any measure of court burden.*

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

[29]It is not 100% clear that all judge rulings would be noted on the docket sheet. In the District Court Study (Greiner et al. (forthcoming 2013)), 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.

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

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

A notable 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. Prejudgment motions, especially motions to compel responses to discovery, to dismiss, and/or for summary judgment, are among the principal tools that lawyers engaging in a combative litigation style would have used to pressure plaintiffs and to achieve a more favorable case posture for settlement negotiations. Defendants offered full representation by the service provider filed on average less than a fifth of a prejudgment motion per case, a figure statistically identical to the .16 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. This result confirms the observations above made with respect to the non-confrontational litigation style Provider Staff Attorneys used. We return to these figures in Section 6 when we discuss possible explanations for our results.

5.5 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 concerns 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 is a contingent outcome, with the contingency being the requirement that the occupant vacate. We risk a direct treated (offer of full representation from a Provider staff attorney) to control (referral to the LFTD program) group comparison here because, as discussed in subsection 5.4.2, 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.[31] We examine various measures of time to vacate,[32] and look at various sets of cases.[33] We also explore various normalizing transformations to these time measurements, which are right-skewed, and settled on a cube root, which performs somewhat well. All these variations in analysis produce the same conclusion: there is no statistically significant difference between treated and control groups.

Because all the variants of transformations and sets of cases analyzed produce 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.[34] On this measurement, the treated group mean (standard deviation) is 113 (99) days, versus 82 (80) days for the control group, and the permutation p-value for the difference in means is .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 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 is 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.[35] Thus, the treatment had no discernible effect with respect

[31] 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.

[32] Perhaps the ideal measurement would be the number of days from the notice to quit to the vacate date. Unless a case went to litigation, however, we are unlikely to have a copy of the actual notice to quit, so the notice to quit date is based on occupant recall at intake.

[33] We analyze all cases, only complaint cases, and only notice to quit cases.

[34] This validity of this measurement depends on the assumption that the offer of full representation had no effect on whether NTQ cases went to litigation. When we restrict our analysis to complaint cases only, however, we observe substantively similar results, but with higher p-values (reflecting the lower number of observations).

[35] Permutation tests for the .25, .50, and .75 quantiles confirm the impression given by the graphs. The

to assuring a decent minimum of a moveout time;[36] rather, it provided additional help to those already fortunate in this regard.

p-values for the difference in these quantiles between the treated and control groups are .36, .42, and .05, confirming that any treatment effect occurred on the longer side of the distributions.

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

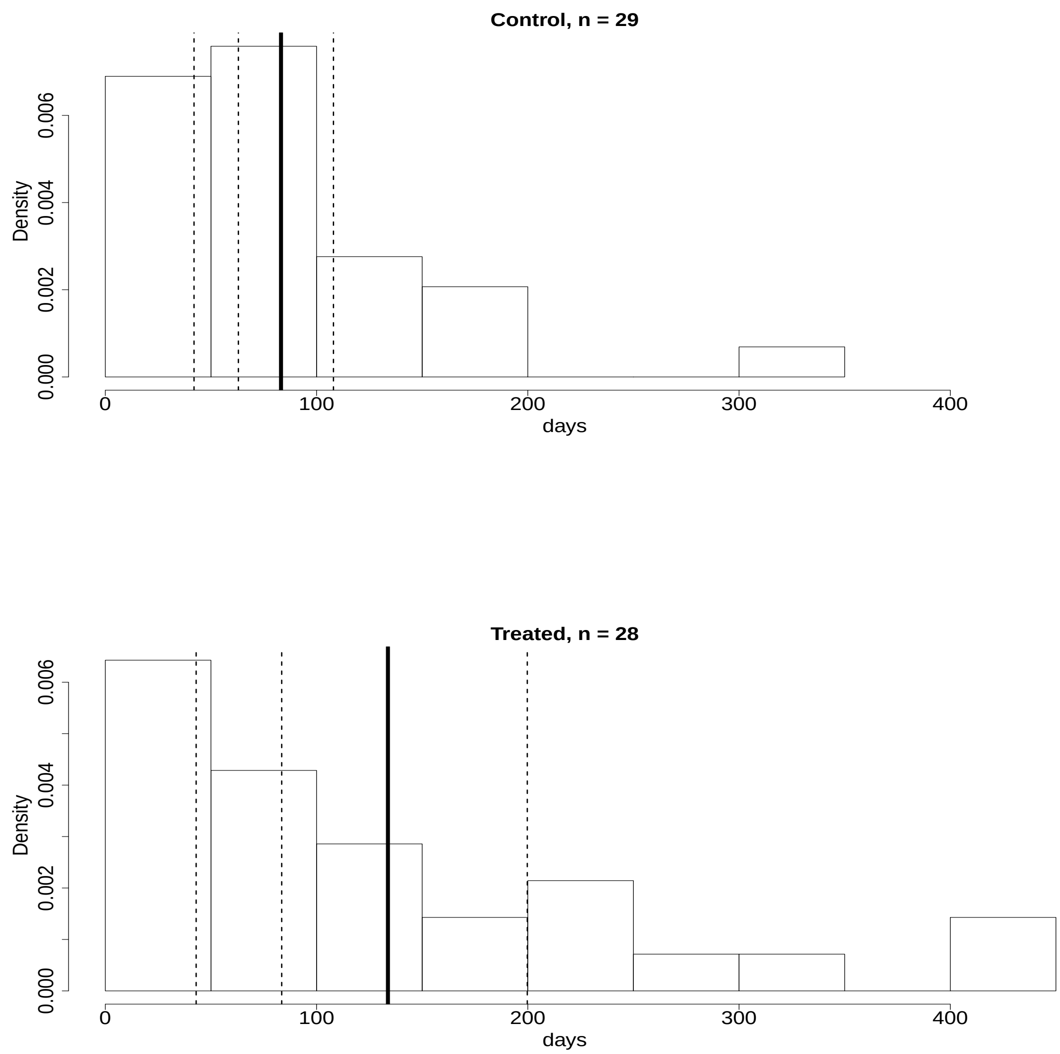

Figure 4: *Moveout Times for Those Required To Vacate: These two histograms show length of time in days from complaint to vacate dates for complaint case occupants who moved out. The dotted lines are .25, .50, and .75 quantiles, while solid lines represent means. Any treatment effect (there is actually no statistically significant evidence of any effect) occurred primarily with respect to cases in which the moveout time is 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 is no statistically significant difference between treated or control groups in the rates 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.[37] The difference, not statistically significant, is 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, at the risk of annoying the judge. These figures reinforce our comments regarding litigation style, and we return to them in Section 6, immediately below.

6 Possible Explanations of Our Results

The previous sections present 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 we measured. Moreover, for almost all outcomes, the treated and control group point estimates are 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 obtains 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 suggests that all is well with the Provider's limited assistance program. There 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

[37] 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, $p < .01$.

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 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 post-Study (meaning currently 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 is not large enough to allow us to rule out modest benefits due to treatment, but modest benefits may not be 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 is that the outcomes are 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 stems 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 potentially illuminating, 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 stems 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. (forthcoming 2013)). 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 the sort we offer here are unavoidable in most policy making.

6.2.1 Some Data

We can gain some leverage in understanding the results of the present Housing Court Study from a comparison of the results here to those from the District Court Study, reported in Greiner et al. (forthcoming 2013). 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 1, above, 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 other 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 filtered cases with a "can we help here" screen. That is, the District Court Study provider sent 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. The Housing Court Study provider employed no such screen.[38]

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

[38]The Housing Court Study provider pointed to this difference as having great power to explain the null result observed in Housing Court Study vis-à-vis the surprisingly large differences between treated and control groups in the District Court Study, discussed below. We have two thoughts in response. First, the explanation is in some, although not irreconcilable, tension with the the fact that the treated and control group point estimates in the Housing Court Study were so close to one another. In other words, the absence of a "can we help" screen/filter could have mattered only if the resulting population of occupants in the Housing Court Study was a mixture two case types: Group A, *i.e.*, those in which an offer of full representation (versus a referral to the LFTD program) could change the outcome; and Group B, *i.e.*, those in which an offer of full representation (versus a referral to the LFTD program) could not, as might happen if the occupant's case were so weak as to make it impossible for her to retain possession or to bargain for a favorable financial arrangement. If that were true, we would expect to see *some* difference in treated and control group case outcomes, corresponding to the fact the Group A cases existed. Instead, as noted above, treated and control group point estimates are very similar to one another. Second, if it were true that only a small fraction of the occupants were in Group A, meaning in only a small fraction of the cases could an offer of full representation make a difference, then the oversubscription problem in legal aid may not be as acute as has been previously reported. We remain uncertain about this whole line of argument.

even here, the story conflicts. For example, it is hard to see how needing an interpreter is helpful to an occupant, so all other things equal (which, of course, they are not), we might expect 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 would experience *more* favorable outcomes than their District Court Study counterparts.

Other factors listed above, such as the difference in the fraction of occupants with Section 8 vouchers, could cut either way. If an occupant has a Section 8 voucher, federal law imposes duties on the evictor that would not otherwise exist, which might provide additional leverage with which an attorney can work. On the other hand, housing attorneys have informally advised us that the consequences to an occupant of losing a Section 8 voucher (as can happen if an occupant suffers a judgment of eviction) are 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.[39]

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 Evictor Months Rent Lost. 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 are better for the occupant.[40] For "EvictorMonsRentLost," too, lower (meaning more

[39]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).

[40]For the actual possession variable, the Housing Court Study figures include both notice to quit and

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 litigation style the attorneys in the Housing Court and District Court studies applied: the number of occupant prejudgment motions, and 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 4: *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 come from both notice to quit and complaint cases; the other results come 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 is 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 is strikingly different in a way more favorable to occupants. The number of prejudgment motions filed by the defendants and the jury trial demand rates provide an indication of the difference litigating style the two sets of attorneys adopted.*

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 fares better: roughly two thirds of defendants retained possession, and on average (using an intensely conservative measurement, as explained in Greiner et al.

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.

(forthcoming 2013)) litigation saved them 9.4 months of rent. In the District Court Study, the differences in possession and months of rent saved are statistically significant.

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,[41] while defendant pretrial motions were filed roughly at eight times the rate in the former study.[42]

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 facilitative, non-confrontational style of the Housing Court Study Provider's staff attorneys poorly chosen? We lack sufficient information to answer these questions definitively. For the present, we offer comments in response to these questions.

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. In Greiner and Pattanayak (2012), two of us articulated the hypothesis that, in some

[41] The difference in the jury trial demand rate between the treated and control groups in the District Court Study is not statistically significant (a permutation p-value was .53). Thus, this variable alone does not explain the more favorable results by the District Court treated group. Rather, the idea here is that the jury trial demand created a setting in which an attorney with an aggressive or confrontational litigating style could apply pressure, perhaps because the threat of taking a case to a jury is more credible when issued by an attorney.

[42] The difference in the average number of defendant pretrial motions between the District Court Study treated and control groups is statistically significant at the conventional (but arbitrary) .05 cutoff, 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.

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." 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, are 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 is 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 many occupants not to demand juries. The low pretrial motion rate is also striking. Again, the LFTD program did not extend to assistance in filing motions: 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. As noted above, these figures reinforce impressions we formed from speaking to the two housing attorneys involved in the present Housing Court Study to the effect that these lawyers pursued a facilitative, non-confrontational litigation style. This involved not making matters too difficult for the opposing counsel (by filing pre-trial motions, such as to dismiss or to compel discovery responses or for summary judgment) or for the court (by demanding difficult-to-schedule jury trials).

To be clear, the issue is not, as some suggested when we presented this study to various audiences, that there was no difference in the attorney assistance experienced by the treated and control groups. The sizable difference in the number of hours of attorney assistance (an average of 12.4 in the treated group versus 1.7 in the control) members of each group received belies this contention.

Nor is the issue here that the attorneys in the Housing Court Study were inexperienced housing litigators, and therefore that the test of full versus unbundled representation was in some way not "fair." To begin, one of the two attorneys had over a decade of experience as a JAG litigator. Far more importantly, however, we question the view that a test of full versus unbundled representation is "fair" or, more importantly, relevant to any policy debate or decision, only if the attorneys involved in the full representation have decades of experience specializing in the relevant legal area. If policy makers were to implement a civil *Gideon* right, even in a limited class of matters involving litigation over basic human needs (American Bar Association (2006)), what fraction of the attorneys providing the representation would be specialists with decades of experience? What would these attorneys' caseloads look like? One need only consult the criminal *Gideon* experience (see Barton and Bibas (2012)) to suggest that it would be foolish to demand that all tests of the civil *Gideon* counterpart involve uber-litigators.

Nor is the issue that the attorneys in the Housing Court Study were in some way less competent than those in the District Court Study because of the lawyering they adopted. It is hard to see how anyone could know, a priori, whether a nonconfrontational lawyering style would produce better or worse results for represented occupants. A priori, one might have argued that an attorney can produce best results for her clients if she gets on a judge's good side over time, particularly if all relevant cases are before the same judge, and to get on that judge's good side, an attorney must avoid legal strategies that causes 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 Stapleton and Teitelbaum (1972) results described in the literature review, 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 point regarding lawyering style has merit, is it possible to train attorneys to adopt what could be the more desirable (in terms of achieve results for clients) style? In other words, if an attorney's natural inclination is to be facilitative, to be non-confrontational, and to achieve a settlement for her client, can that attorney be trained to adopt a higher-pressure, assertive, and combative posture? We cannot say, but we suggest that the question is worth investigating closely.

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