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THE LIMITS OF UNBUNDLED LEGAL ASSISTANCE: A RANDOMIZED STUDY IN A MASSACHUSETTS DISTRICT COURT AND PROSPECTS FOR THE FUTURE

D. James Greiner, Cassandra Wolos Pattanayak, and Jonathan Hennessy

CONTENTS

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*D. James Greiner,*** *Cassandra Wolos Pattanayak,**** *and Jonathan Hennessy*****

We persuaded entities conducting two legal aid programs designed to provide evidence regarding a civil right to counsel to allow us to randomize which potential clients would receive offers of traditional attorney-client relationships from legal aid provider staff attorneys and which would receive only limited ("unbundled") assistance. In both pilot programs, potential clients were occupants facing eviction from their housing units, and in neither pilot program did the legal aid provider have capacity sufficient to offer full representation to all occupants who sought it. In this Article, we report the results of one of the two randomized trials, which we label the "District Court Study" after the type of court in which it took place. In this District Court Study, most occupants who became part of the study population received limited assistance in how-to sessions, which included instruction on the summary eviction process as well as help in filling out answer and discovery request forms. After receiving this "unbundled" assistance, members of a randomly selected treated group were offered a traditional attorney-client relationship from a legal aid provider staff attorney; members of the remaining randomly selected control group received no such offer. We compared outcomes for the treated group versus the control group on a variety of dimensions, focusing primarily on possession of the unit, financial consequences of the litigation, and measures of court burden.

At least for the clientele involved in this District Court Study — a clientele recruited and chosen by the legal aid provider's proactive, timely, specific, and selective outreach and intake system — an offer of full representation mattered. Approximately two-thirds of occupants in the treated group, versus about one-third of occupants in the control group, retained possession of their units at the end of litigation. Using a conservative proxy for financial consequences, and based on a subset of cases in which financial issues were at the forefront, treated-group occupants received payments or rent waivers worth on average a net of 9.4 months of rent per case, versus 1.9 months of rent per case in the control group. Both results were statistically significant. Meanwhile, although treated cases did take longer to reach judgment, the offer of representation caused no

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increase in court burden as measured by other, more salient metrics, such as the number of party motions or the quantity of judicial rulings.

We discuss possible reasons for the magnitude of the differences between the outcomes experienced by the treated and control groups. For example, following previous work, we discuss the possible importance of the legal aid provider's process for client recruitment and selection. Here, the provider invested substantial resources into a system designed to recruit and identify clients for whom unbundled legal assistance would be inadequate, suggesting that identifying such cases can be done but that doing so may be expensive. We conclude by discussing future directions for a movement, growing in momentum, toward an evidence-based approach for access to, and administration of, justice.

INTRODUCTION

In the past decade, state bar associations, state courts, bodies that compose and administer ethical codes, and others have accelerated a trend begun much earlier by legal aid providers[1] toward the legitimization and promotion of "limited" or "unbundled" forms of legal assistance. By "limited" or "unbundled" assistance,[2] we mean some form of legal service or information provision short of a traditional, matter-specific attorney-client relationship.[3] Advocates of unbundling argue that it has the potential to address, in part, two related crises afflicting the U.S. legal system over the past two decades: the access-to-justice challenges that have arisen as the legal system has become more complicated, and the influx of pro se litigants that have flooded the nation's courts, particularly the state courts.[4] In response to these challenges, almost every state in the nation has amended its ethical code or

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[1] *See* Forrest S. Mosten, *Unbundling of Legal Services and the Family Lawyer*, 28 FAM. L.Q. 421, 425 (1994) (discussing unbundled programs that existed in the 1970s).

[2] The experiment we report in this Article deals with unbundled assistance provided by practicing lawyers. The provision of assistance by nonlawyers has also received substantial attention in the literature, *see, e.g.*, Alex J. Hurder, *Nonlawyer Legal Assistance and Access to Justice*, 67 FORDHAM L. REV. 2241 (1999); Deborah L. Rhode, *The Delivery of Legal Services by Non-Lawyers*, 4 GEO. J. LEGAL ETHICS 209 (1990), although (to our knowledge) there has been no credible, quantitative empirical evaluation.

[3] We recognize that this definition is broad — broader than that adopted by many state bodies in their considerations of unbundling. For a discussion of definitional issues, see Molly M. Jennings & D. James Greiner, *The Evolution of Unbundling in Litigation Matters: Three Case Studies and a Literature Review*, DENV. U. L. REV. (forthcoming) (manuscript at 4–7), *available at* http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2038616.

[4] *Id.* (manuscript at 7–8). Note that we consider here the empirical effect of limited representation; we do not discuss in this Article the various ethical issues posed when lawyers deliver unbundled legal services. For a discussion of these issues, see generally, for example, David A. Hyman & Charles Silver, *And Such Small Portions: Limited Performance Agreements and the Cost/Quality/Access Trade-Off*, 11 GEO. J. LEGAL ETHICS 959 (1988); Fred C. Zacharias, *Limited Performance Agreements: Should Clients Get What They Pay for?*, 11 GEO. J. LEGAL ETHICS 915 (1988); and Fred C. Zacharias, *Reply to Hyman and Silver: Clients Should Not Get Less than They Deserve*, 11 GEO. J. LEGAL ETHICS 981 (1988).

adopted rules of civil procedure to promote unbundled services as a way for the private bar to do business.[5] Further, almost every state in the United States currently has at least one formal program in which legal aid staff attorneys or private attorneys litigating pro bono offer unbundled assistance.[6] The promise of such efforts is that they might allow service providers to reach a greater number of those in need than they would reach if they offered only traditional attorney-client relationships. The following question is, however, implicit: how much does a potential client gain or lose as a result of an offer of limited assistance, an offer that, in our study, 97% of potential clients accepted?

To our knowledge, there has never been a rigorous quantitative evaluation of any form of limited legal assistance in the United States.[7] In reporting the results of a randomized evaluation of a legal assistance program in unemployment benefits proceedings — a program that offered full representation by a law student clinic — two of us recently reviewed several dozen publications.[8] We found only three other randomized assessments of any form of legal assistance in U.S. civil litigation: a study of a full-representation program in Manhattan Housing Court in 1993–94,[9] and two studies simultaneously conducted in the late 1960s of offers of representation in juvenile detention pro-

[5] Jennings & Greiner, *supra* note 3 (manuscript at 1).

[6] Rebecca L. Sandefur & Aaron C. Smyth, Access Across America: First Report of the Civil Justice Infrastructure Mapping Project 11, 13 (Oct. 7, 2011) (unpublished report), *available at* http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1962790.

[7] Some studies purport to measure outcomes but use no control group at all, much less a randomly selected control group. *See, e.g.*, OFFICE OF THE DEPUTY CHIEF ADMIN. JUDGE FOR JUSTICE INITIATIVES, VOLUNTEER LAWYER FOR A DAY PROJECT REPORT: A TEST OF UNBUNDLED LEGAL SERVICES IN THE NEW YORK CITY HOUSING COURT (2008), *available at* http://www.courts.state.ny.us/courts/nyc/housing/pdfs/vlfdreport_0208.pdf; JESSICA PEARSON & LANAE DAVIS, THE HOTLINE OUTCOMES ASSESSMENT STUDY, FINAL REPORT — PHASE III: FULL-SCALE TELEPHONE SURVEY (2002), *available at* http://www.nlada.org/DMS/Documents/1037903536.22/finalhlreport.pdf; Michael Millemann et al., *Limited-Service Representation and Access to Justice: An Experiment*, 11 AM. J. FAM. L. 1 (1997). Other assessments are observational studies, meaning they use nonrandomly selected control groups. *See, e.g.*, Jessica K. Steinberg, *In Pursuit of Justice? Case Outcomes and the Delivery of Unbundled Legal Services*, 18 GEO. J. ON POVERTY L. & POL'Y 453, 457–58 (2011) (explicitly acknowledging the limits of the observational study design employed). For a multimode evaluation that did not include a randomized design, see THE EMPIRICAL RESEARCH GRP., UCLA SCH. OF LAW, EVALUATION OF THE VAN NUYS LEGAL SELF-HELP CENTER: FINAL REPORT (2001), *available at* http://www.courts.ca.gov/partners/documents/Final_Evaluation_Van_Nuys_SHC2001.doc.

[8] *See* D. James Greiner & Cassandra Wolos Pattanayak, *Randomized Evaluation in Legal Assistance: What Difference Does Representation (Offer and Actual Use) Make?*, 121 YALE L.J. 2118, 2125 (2012).

[9] Carroll Seron et al., *The Impact of Legal Counsel on Outcomes for Poor Tenants in New York City's Housing Court: Results of a Randomized Experiment*, 35 LAW & SOC'Y REV. 419 (2001). The pro bono attorneys providing much of the representation received substantial support from a legal aid provider's attorneys and paralegals. *Id.* at 422 n.1.

ceedings.[10] The design for the Manhattan Housing Court Study initially included an element of limited assistance, specifically a triage component for the randomly selected treated group.[11] This triage component was abandoned,[12] and none of the other randomized studies, including the unemployment study, incorporated any aspect of limited representation.

The effectiveness of unbundled legal assistance might be assessed from at least two perspectives. First, how much benefit does a potential client receive by being offered limited legal assistance as compared to being compelled (for lack of an alternative) to pursue unassisted self-representation? Colloquially, how much does a potential client gain from limited assistance as compared to a baseline of nothing? Second, what does a potential client "lose"[13] when referred to a limited assistance program as compared to receiving an offer of a traditional attorney-client relationship with a competent lawyer? Colloquially, how does limited assistance compare to full representation? Both questions are interesting. In certain circumstances, when providing at least some (perhaps minimal) form of assistance costs little, and there is only a small chance that the assistance could have harmful side effects (such as delay in a public benefits proceeding[14]), there may be ethical concerns in studying the first question. Here, we address the second question, using an offer of a traditional attorney-client relationship as the baseline by which to compare the effectiveness of limited assistance received by most[15] study-eligible potential clients.

Specifically, in this Article, we provide gold-standard evidence in the area of summary eviction on the question of how closely a limited assistance program can approximate the outcomes realized if a potential client had received an offer of a full attorney-client relationship.

[10] W. VAUGHAN STAPLETON & LEE E. TEITELBAUM, IN DEFENSE OF YOUTH (1972).

[11] Specifically, prior to randomization an attorney chose which of three levels of legal assistance a potential client would receive if randomized to treatment. The three levels were a traditional attorney-client relationship, assistance from a paralegal, and limited legal advice from a lawyer. Seron et al., *supra* note 9, at 423–24.

[12] After the triage component in the treatment group was abandoned midstudy, all treated-group potential clients who could be contacted received an offer of a full attorney-client relationship. *Id.* at 425.

[13] We place "lose" in quotation marks because the idea of a potential client's "losing" something assumes that there was a realistic possibility that he or she would actually receive an offer of a full attorney-client relationship. Given the level of the unmet need for legal assistance, this assumption is not currently realistic for broad classes of persons in need.

[14] *See* Greiner & Pattanayak, *supra* note 8, at 2153–58.

[15] As we explain below, at least 70% of occupants in our study received substantial assistance in the form of two- to three-hour instructional clinics. These clinics included assistance in filling out crucial answer and discovery forms. In the other 30% of cases, occupants did not attend clinics, but an undetermined percentage received assistance in filling out forms. Thus, a strong majority of occupants in this District Court Study received some form of unbundled legal assistance.

Bar associations,[16] state legislatures,[17] and academics,[18] among others, have identified housing or shelter as among the "basic" or "critical" human needs that, if subject to direct threat in an adversarial proceeding, should ideally give rise to a "civil *Gideon*"[19] right to a traditional attorney-client relationship at state expense for persons unable to afford an attorney. But if for certain case types, limited and full representation produce similar outcomes,[20] or outcomes similar enough given the differences in expense, then limited representation offers the obvious advantages of potentially lower costs and wider availability. Accordingly, we contribute evidence on the question of whether limited legal assistance is sufficient to approximate a traditional attorney-client relationship in summary eviction proceedings.

The genesis of this project was years of work by a Massachusetts civil *Gideon* task force, the Boston Bar Association Task Force on Expanding a Civil Right to Counsel. Working with the Task Force, we designed and implemented[21] two randomized control trials testing the effectiveness of limited legal assistance vis-à-vis an offer of a full attorney-client relationship, one study for each of the two pilot programs the Task Force organized. For convenience, we refer to each

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[16] *See, e.g.*, AM. BAR ASS'N, REPORT TO THE HOUSE OF DELEGATES, RESOLUTION 112A (2006), *available at* http://www.americanbar.org/content/dam/aba/administrative/legal_aid_indigent_defendants/ls_sclaid_06A112A.authcheckdam.pdf.

[17] *See, e.g.*, Sargent Shriver Civil Counsel Act, CAL. GOV'T CODE § 68651(b)(1) (West 2009) (highlighting "housing-related matters").

[18] *See, e.g.*, Russell Engler, *Connecting Self-Representation to Civil* Gideon*: What Existing Data Reveal About When Counsel Is Most Needed*, 37 FORDHAM URB. L.J. 37, 46–51 (2010).

[19] *Gideon v. Wainwright*, 372 U.S. 335, 344–45 (1963), held that indigent defendants have a right to counsel at state expense in criminal cases. The Supreme Court later clarified *Gideon* by holding that no constitutional violation occurs unless a defendant tried without counsel (or a knowing waiver of counsel) is actually incarcerated. *See* Scott v. Illinois, 440 U.S. 367, 373–74 (1979). No blanket right to counsel exists in the civil context. *See* Turner v. Rogers, 131 S. Ct. 2507, 2512 (2011); Lassiter v. Dep't of Soc. Servs., 452 U.S. 18, 31 (1981).

[20] As we have discussed in Greiner & Pattanayak, *supra* note 8, at 2205–06, and as we discuss further in Part III of this Article, it will not do to limit the definition of "outcomes" to adjudicatory outputs.

[21] To be clear, we asked to be allowed to participate in the Task Force's efforts after it and others had selected or decided the courts in which the two studies would be sited, the amount of money that would be provided, the legal aid providers who would provide the representation, the amount and nature of unbundled assistance that the legal aid providers would offer, the attorneys involved, and many other specific aspects of the field operation. Thus, we "designed" the District Court and Housing Court Studies primarily in the sense that we persuaded those involved to engage in a randomized evaluation. Our late arrival to the overall process of design and implementation placed limits on our ability to affect how the two Studies evolved. For example, because we had no funding and no field support, we could not survey study subjects to assess whether an offer of full representation (versus unbundled assistance) affected whether they thought they had been treated fairly, whether they felt they had an opportunity to have their cases presented, or whether they would be likely to comply with the results. As another example, statistical power calculations were pointless (and we did not bother with them) because we had no ability to affect the amount of data we would generate.

study by the nature of the court in which it took place: the District Court Study,[22] which we describe in this Article, and the Housing Court Study, the results of which appear in a separate paper.[23]

In the District Court Study, our design consisted of a comparison of outcomes realized by randomly assigned treated and control groups of summary eviction defendants,[24] most of whom received limited legal assistance in the form of how-to clinics run by a Greater Boston Legal Services (GBLS) staff attorney.[25] The instructional clinics included assistance in filling out answer and discovery forms. The treated group received an additional benefit, an offer of a traditional attorney-client relationship from a GBLS staff attorney; the control group received no such offer. The offers were highly valued, and crossover between treated and control groups was minimal: 97% of treated-group occupants took advantage of GBLS's offer of representation, while 89% of the control group occupants were forced to self-represent.[26] A review of court records, supplemented by a small number of telephone contacts, allowed us to discern both legal outcomes (for example, whether a judgment for possession was entered against an occupant) and some socioeconomic outcomes (for example, whether an occupant actually lost possession).

In this District Court Study, the differences between the outcomes realized by the treated and control groups were large. Approximately two-thirds of treated-group occupants retained possession of their housing units at the end of summary eviction proceedings, as compared with about one-third of control group occupants. In cases involving nonpayment of rent or serious monetary counterclaims, the net financial effect of the litigation was such that those in the treated group were not obligated to pay an average net of 9.4 months of rent per case (relative to what the evictor alleged to be due), while the corresponding figure for control group occupants was 1.9 months of rent

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[22] This study took place in the Quincy District Court. Quincy is a city in Norfolk County, Massachusetts, located southeast of Boston. For more information on Quincy, see CITY OF QUINCY, http://www.quincyma.gov (last visited Dec. 1, 2012).

[23] D. James Greiner et al., How Effective Are Limited Legal Assistance Programs? A Randomized Experiment in a Massachusetts Housing Court (Sept. 1, 2012) (unpublished manuscript), *available at* http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1880078.

[24] To ease comparability with the Housing Court Study and to avoid confusion, we refer to the potential Greater Boston Legal Services clients (who were the study subjects) as "occupants," and to the parties who sought to obtain possession of the units as "evictors." For the latter group of parties, "would-be evictors" is probably a more accurate term, but it is cumbersome.

[25] *See Housing Direct Client Services*, GREATER BOS. LEGAL SERVICES, http://www.gbls.dbdes.info/our-work/housing/housing-direct-client-services (last visited Dec. 1, 2012).

[26] Moreover, 90% (86% in the treated group, 96% in the control group) of the evictors in study cases were represented by counsel. As we discuss below, this difference was of borderline statistical significance ($p = 0.07$ under a permutation test). We adjust for this difference in various ways; adjustment does not affect the results we report here.

per case.[27] Both results were statistically significant, despite the small size of our study (76 treated and 53 control cases). Meanwhile, there was no evidence to suggest that treated-group cases imposed a greater burden on the judge or the court than did control group cases,[28] apart from the fact that treated-group cases took longer to reach final judgment. We discuss additional results below.

We proceed as follows: In Part I, we briefly discuss the existing literature on summary eviction proceedings and on unbundled legal assistance. In Part II, we summarize the factual and legal setting for our study. In Appendix I, we provide greater detail, focusing in turn on the substantive and procedural law; the court's personnel and practices; GBLS; the two staff attorneys who provided almost all the representation in the pilot project as well as the limited assistance in the how-to clinics; the outreach, intake, and case-selection system GBLS used; and the randomized design of the District Court Study. Part III provides the quantitative results and possible explanations for them. In Part IV, we use the recent Supreme Court decision in *Turner v. Rogers*[29] as a springboard to discuss future prospects for rigorous, empirical legal research into facts implicating due process values.

One final point: This Article and the paper reporting the results of the Housing Court Study[30] express the views of only the authors. Neither represents the views of the Task Force, the funders, the legal services providers, or any other person involved or not involved with these pilot programs.[31]

I. LITERATURE REVIEW

In this Part, we briefly discuss the literature regarding summary eviction proceedings and limited legal representation.

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[27] As we discuss below, these figures come from a proxy measurement that probably understates the effect of an offer of representation from a GBLS staff attorney, but that measurement does count legal obligations to pay (such as might be imposed by a court judgment) as equal to cash payments.

[28] As we explain below, this statement is not to suggest that there was no burden on the court from the presence of GBLS attorneys in its system. Our finding is only that, given that the GBLS attorneys were already offering unbundled assistance to all occupants faced with eviction proceedings in Quincy District Court, there was no additional burden imposed by having these same attorneys offer full representation to a selected subset of potential clients.

[29] 131 S. Ct. 2507 (2011).

[30] Greiner et al., *supra* note 23.

[31] The Task Force has issued its own report detailing its views. *See* BOS. BAR ASS'N TASK FORCE ON THE CIVIL RIGHT TO COUNSEL, THE IMPORTANCE OF REPRESENTATION IN EVICTION CASES AND HOMELESSNESS PREVENTION (2012), *available at* http://www.bostonbar.org/docs/default-document-library/bba-crtc-final-3-1-12.pdf.

A. Summary Eviction Proceedings

Summary eviction proceedings have long been a primary area of emphasis for proponents of a civil *Gideon* right.[32] Because shelter (a basic human need) is at stake, because eviction proceedings are adversarial,[33] because such proceedings ordinarily occur in an adjudicatory body labeled a "court" (sometimes a specialized court) as opposed to within an administrative body using informal procedures, and because housing law in most states is thought to have some degree of complexity,[34] these adjudications are at the core of the set of cases in which it is thought that the self-represented occupant is at her most vulnerable, especially when facing a represented evictor.

The pilot study we discuss here concerned offers of differing levels of legal assistance in summary eviction proceedings in a Massachusetts district court. Looking back over several decades, eviction proceedings across the United States, including those occurring in specialized housing courts, have been the subject of intensely critical academic articles and exposé-style reports. In a recent article, Professor Russell Engler collects many of these reports[35] and notes the information available from their titles, which include "Injustice in No Time,"[36] "Judgment Landlord,"[37] "Time to Move: The Denial of Tenants' Rights in Chicago's Eviction Court,"[38] and "A Tenants' Court of No Resort."[39]

We do not in any way suggest that the district court in our study shared characteristics of the courts described in these colorfully titled publications. In fact, our personal observations suggest otherwise (and studies of other eviction courts have been less critical[40]). With respect to certain settings, however, these publications suggest that some

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32 Engler, *supra* note 18, at 46–51 (summarizing the literature on summary eviction in an article attempting to pinpoint when counsel is most necessary).

33 *See* AM. BAR ASS'N, *supra* note 16, at 13.

34 *See, e.g.*, Andrew Scherer, Gideon*'s Shelter: The Need to Recognize a Right to Counsel for Indigent Defendants in Eviction Proceedings*, 23 HARV. C.R.-C.L. L. REV. 557, 569–76 (1988) (arguing that the complexity of housing law strongly disadvantages pro se litigants); Rachel Kleinman, Comment, *Housing* Gideon*: The Right to Counsel in Eviction Cases*, 31 FORDHAM URB. L.J. 1507, 1516 (2004) (same).

35 Engler, *supra* note 18, at 46–47.

36 WILLIAM E. MORRIS INST. FOR JUSTICE, INJUSTICE IN NO TIME: THE EXPERIENCE OF TENANTS IN MARICOPA COUNTY JUSTICE COURTS (2005), *available at* http://morrisinstituteforjustice.org/docs/254961Finalevictionreport-P063.06.05.pdf.

37 JULIAN R. BIRNBAUM ET AL., JUDGMENT LANDLORD: A STUDY OF EVICTION COURT IN CHICAGO (1978).

38 LISA PARSONS CHADHA, LAWYERS' COMM. FOR BETTER HOUS., INC., TIME TO MOVE: THE DENIAL OF TENANTS' RIGHTS IN CHICAGO'S EVICTION COURT (1996).

39 Anthony J. Fusco, Jr. et al., *Chicago's Eviction Court: A Tenants' Court of No Resort*, 17 URB. L. ANN. 93 (1979).

40 *See, e.g.*, Raphael L. Podolsky with Steven O'Brien, *A Study of Eviction Cases in Hartford: A Follow-Up Review of the Hartford Housing Court*, *in* EMPIRICAL JUDICIAL STUDIES SERIES (Legal Assistance Res. Ctr. of Conn., Inc., Empirical Judicial Studies Series Report No. 11, 1995).

courts may be failing to achieve even a rough approximation of justice in an area where the stakes are high. Further, the above reports imply that the unmet need for legal assistance in this area is substantial, suggesting an imperative to structure legal assistance programs in order to achieve a decent minimum of access to justice for as many persons as possible. In other words, efficiency matters greatly here.

B. Limited or Unbundled Representation

The idea of an attorney and a client agreeing that the attorney will perform only one or a subset of all the legal tasks potentially required in a matter is a familiar one to lawyers engaged in certain practices, including, for example, financial transactions, estate planning, and insurance litigation. Until relatively recently, however, influential portions of the bench and bar gave a chilly response to proposals to allow (or even to encourage) lawyers to perform discrete tasks within a piece of litigation, such as drafting a particular court document,[41] providing a client with information or strategic advice that would facilitate self-representation, or representing the client in negotiations, mediation sessions, or court colloquies, but not at trial.

Facts on the ground overwhelmed the bench and bar's squeamishness toward the idea of unbundled legal assistance in litigation. Commentators have referred to the "pro se crisis,"[42] as though the emergence of higher and higher percentages of self-represented litigants, together with an insufficient amount of resources to provide legal services to such litigants, was or is a recently occurring and temporary matter. In fact, legal aid providers and pro bono groups recognized the emergence of a new normal in terms of the prevalence of pro se litigation decades ago. They began by making arrangements with local courts — arrangements of somewhat questionable status under then-existing ethical codes and civil procedure rules[43] — to allow unbundled legal assistance in discrete litigation areas. In the area of family law, some unbundled programs were in place in the early 1970s in the

[41] For example, courts have addressed the practice of "ghostwriting" litigation documents. *See, e.g.*, Laremont-Lopez v. Se. Tidewater Opportunity Ctr., 968 F. Supp. 1075, 1077 n.2 (E.D. Va. 1997) (suggesting that an attorney file a complaint she has ghostwritten and simultaneously file a motion to withdraw, despite uncertain prospects that such a motion would be granted); Johnson v. Bd. of Cnty. Comm'rs, 868 F. Supp. 1226, 1231–32 (D. Colo. 1994) (strongly disapproving of ghostwriting), *aff'd on other grounds*, 85 F.3d 489 (10th Cir. 1996).

[42] *See, e.g.*, James M. McCauley, *Current Ethical and Unauthorized Practice Issues Relating to Endeavors to Assist Pro Se Litigants*, VA. LAW., Dec. 2002, at 43, 43, *available at* http://www.vsb.org/docs/valawyermagazine/dec02access.pdf.

[43] *See, e.g.*, Interview with Jonathan Asher, Dir., Colo. Legal Servs. (Dec. 19, 2011) (describing how a legal aid provider and a pro bono organization arranged with a local Colorado court to provide ghostwriting, information, and strategic advice to family law litigants several years before changes in ethical and civil procedure rules made such arrangements formally ethical and legal).

form of "private legal clinics" that provided "legal consultations of a specified time and price without any further obligation on the part of the client or the lawyer."[44] For example, in 1974 in King County, Washington, a young lawyers' group created a program in which its members reviewed legal forms and provided explanations and advice.[45] And in Massachusetts, eviction defense clinics have been in place for decades,[46] at least twenty years before the Massachusetts Supreme Judicial Court's 2006 Pilot Project on Limited Assistance Representation.[47]

State courts, ethical authorities, and bar associations eventually responded as well by providing formal recognition for the unbundled programs already in existence and by changing rules and standards to allow new programs to emerge. Motivating factors for change included the burdens pro se litigants placed on the judicial system, the hypothesis that offering unbundled services might constitute a coherent business model for the private bar, and a desire to promote access to justice for persons unable to afford traditional full representation in a litigation matter.[48] States began to make changes that tackled a variety of ethical issues, including the obligation to investigate potential conflicts, the propriety of contacts with parties receiving unbundled representation, and the duty of candor to the court. A milestone in this process of addressing ethical issues was the 2002 amendment to the American Bar Association's (ABA) Model Rules of Professional Conduct explicitly authorizing unbundled legal services.[49] Also indicative of the general trend was the fact that, in 2007, the ABA Standing Committee on Ethics and Responsibility issued a formal opinion stating that an attorney's ghostwriting of legal documents (i) is permissible and (ii) need not be disclosed to a tribunal.[50] This formal opinion expressly superseded an informal opinion issued almost two decades ear-

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[44] Mosten, *supra* note 1, at 425.

[45] MARK H. TUOHEY III ET AL., HANDBOOK ON LIMITED SCOPE LEGAL ASSISTANCE 121 (2003), *available at* http://apps.americanbar.org/litigation/taskforces/modest/report.pdf.

[46] Email from Stefanie Balandis, Senior Attorney, Greater Bos. Legal Servs., to James Greiner (Oct. 21, 2011, 11:19 PM) (on file with the Harvard Law School Library).

[47] *See In re* Ltd. Assistance Representation (Mass. 2009), *available at* http://www.mass.gov/courts/courtsandjudges/courts/probateandfamilycourt/documents/limitedrepresentationstandingorder.pdf (discussing the 2006 initiation of the Pilot Project and providing procedures to be followed when attorneys engage in limited assistance).

[48] Jennings & Greiner, *supra* note 3 (manuscript at 7–8).

[49] *See* MODEL RULES OF PROF'L CONDUCT R. 1.2(c) (2011).

[50] ABA Comm. on Ethics & Prof'l Responsibility, Formal Op. 07-446, at 4 (2007) [hereinafter Formal Op. 07-446], *available at* http://www.nlada.org/DMS/Documents/1185213796.98/ABA%20ghostwriting%20opinion%206-07.pdf.

lier that had held that ghostwriting, though permissible, had to be disclosed to the tribunal.[51]

Currently, almost every state in the nation has at least one legal aid or assistance program that explicitly offers unbundled legal assistance,[52] while many states either have already amended or are in the process of amending their ethical codes and court rules in order to allow both legal assistance providers and private attorneys to offer limited services.[53] Despite the prevalence of such programs, to our knowledge there has been no rigorous evaluation of the effect limited assistance has on the clients or the court systems those programs are intended to serve.

II. FACTUAL BACKGROUND, LEGAL SETTING, STUDY DESIGN, AND FIELD OPERATION: ABBREVIATED DESCRIPTION

In this Part, we provide a stripped-down description, with minimal citations, of the factual and legal setting for the District Court Study. This highly abbreviated discussion is designed to make this Article accessible to readers interested in a top-level summary of the results. In Appendix I, we provide the detail we deem essential to good scholarship.

The study we report here is one of two stemming from our work with several entities, including a Massachusetts district court; a Massachusetts housing court; GBLS, one of New England's largest legal aid providers; Neighborhood Legal Services, Inc., which covers the North Shore area of Massachusetts; the Task Force; and three generous funders: the Boston Bar Foundation, the Massachusetts Bar Foundation, and the Boston Foundation. As noted in the Introduction, we designed two studies on behalf of the Task Force: the District Court Study reported in this Article and the Housing Court Study reported in a separate paper.[54] The two studies were the culmination of a multiyear effort by the Task Force to provide credible evidence regarding the necessity of a traditional attorney-client relationship to protect certain types of indigent persons facing threats to basic human needs in adversarial adjudications.[55]

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[51] ABA Comm. on Ethics & Prof'l Responsibility, Informal Op. 1414 (1978), *superseded by* Formal Op. 07-446, *supra* note 50; *see also* Mass. Bar Ass'n, Op. 98-1 (1998), *available at* http://www.massbar.org/publications/ethics-opinions/1990-1999/1998/opinion-no-98-1 (prohibiting ghostwriting).

[52] *See* Sandefur & Smyth, *supra* note 6, at 13 fig.1.

[53] *See, e.g.*, ABA Standing Comm. on the Delivery of Legal Servs., *Court Rules*, A.B.A., http://www.americanbar.org/groups/delivery_legal_services/resources/pro_se_unbundling_resource_center/court_rules.html (last visited Dec. 1, 2012).

[54] Greiner et al., *supra* note 23.

[55] BOS. BAR ASS'N TASK FORCE ON THE CIVIL RIGHT TO COUNSEL, *supra* note 31, at 12–13.

A key part of the Task Force's project was to assess the effectiveness of an offer of full representation in a subset of summary eviction cases that, it was hoped, could be identified at the time of their initiation as requiring a full attorney-client relationship in order to realize a minimum of justice. As a result, a condition of the funding was that the legal services providers offer representation only in cases that fit into one of three categories: (i) the occupant had a disability related to the evictor's asserted reason for the eviction; (ii) the occupant had 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.[56] The Task Force considered its focus on this subclass of cases — in which an offer of full representation was, by hypothesis, required to produce justice — to be a unique feature of its effort. This access-to-justice strategy was different from a focus on providing full representation (at state expense for persons unable to pay on their own) in certain types of adjudicatory proceedings, as proposed in a recent ABA civil *Gideon* pronouncement.[57]

The Task Force focused on summary eviction, as opposed to another area of law, because the consequences of a court-ordered eviction can be serious and can extend beyond the obvious need to find (or the inability to find) a new place to live. For example, under federal law a public housing authority may choose to promulgate regulations denying admission and assistance to any tenant who has suffered a court-ordered eviction from federally assisted housing in the prior five years.[58] A court-ordered eviction, particularly one for nonpayment of rent, can affect a tenant's credit rating and thereby the tenant's access to the credit and rental markets after the eviction.[59] Finally, a court-

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[56] In making the substantial injustice determination, the terms of the grant required that GBLS (the provider in this study) consider 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 the eviction action, the effect of an eviction on the occupant, and any power imbalance between the evictor and the occupant (as might be induced, for example, if the evictor were represented). These criteria and subcriteria came from the Task Force's research.

[57] *See* AM. BAR ASS'N, *supra* note 16, at 1 (recommending that an attorney be provided to indigent litigants in adversarial proceedings involving direct threats to the provisions of basic human needs, including shelter).

[58] 24 C.F.R. § 982.552(c)(1)(ii) (2012).

[59] This consequence is one that popular sources of legal and more general advice highlight. *See, e.g.*, Heather Leigh Landon, *The Consequences of Eviction*, EHOW MONEY, http://www.ehow.com/info_8012467_consequences-eviction.html (last visited Dec. 1, 2012) ("[Y]ou[r] credit history will be affected if you do not pay rent and are eventually evicted. An eviction on your credit history will affect you longer than you may think. Future landlords may not rent to you, and if they do they may ask you to pay higher rent and/or ask for more money as a deposit."); *see also Credit Advice*, EXPERIAN (May 27, 2009), http://www.experian.com/ask-experian/20090527-judgment-for-eviction-could-appear-on-your-credit-report.html (similar).

ordered eviction, or certain kinds of agreement for judgment, can make a household ineligible for some forms of emergency shelter assistance.[60]

As noted above, the Task Force decided to field two separate studies because Massachusetts summary eviction litigation takes place both in specialized housing courts and in district courts. In the remainder of this Part, we describe summary eviction actions in district courts in general and in Quincy District Court in particular, as well as the way the District Court Study operated.

Massachusetts district courts have jurisdiction over small- to medium-sized civil and criminal matters. Special, simplified rules of procedure govern eviction litigation in all Massachusetts courts,[61] and standardized forms (some created by the courts, some created by noncourt entities) are frequently used. By law, trial dates are set for no sooner than ten days after the filing of a complaint,[62] but as we discuss below, we rarely saw a trial in our dataset. The substantive law applicable in summary eviction cases bears notable complexity. Sources of relevant law include federal statutes, federal regulations, state statutes, state regulations, and state common law. Content includes, for example, nonwaivable warranties, allocations of duties that can be shifted only by means of written agreements, dependent covenants, and procedural requirements regarding the service and content of the "notice to quit," the initial document the would-be evictor must serve on the occupant as a precursor to a formal court action.

Post-judgment practice under Massachusetts summary eviction law also bears notable complexity, and the details proved important to the District Court Study. In Massachusetts, a judgment in favor of an evictor is insufficient to allow the evictor to remove forcibly an occupant from a unit. Instead, the evictor must induce the court to issue a writ of execution for possession, and afterward, to issue a forty-eight-hours constable's notice. The judgment, writ, and constable's notice procedure were important to the study; we could see in the court records the results of parties' bargaining around these various stages. For example, a settlement in an eviction lawsuit in which the evictor alleged nonpayment of rent might provide that the case would be stayed for two months, during which time the occupant would attempt to pay the arrearage; or the settlement might provide that judgment and a writ of execution would issue in favor of the evictor, and that the writ would be held unless the occupant failed to pay the arrearage.

[60] *See, e.g.*, 106 MASS. CODE REGS. 309.040(B)(3)–(6) (2009).

[61] *See* MASS. UNIF. SUMM. PROCESS R., *available at* http://www.lawlib.state.ma.us/source/mass/rules/tc/summaryprocessrules.html.

[62] *See id.* 2(c).

The former scenario requires further court involvement before a forcible eviction could occur, but the latter does not.

To complicate matters further, much may depend on how functionally identical transactions are documented in court records. To illustrate, imagine that a tenant falls behind on the rent, and the landlord files a summary eviction action in court. In settlement negotiations, the tenant concedes that she cannot pay the arrearage and offers to move out in two weeks if the landlord will forgive it. The landlord agrees. Now comes the potential for a difference, as illustrated by the following two scenarios. In one scenario, the parties agree to a three-week stay of the litigation, with the understanding that if the tenant vacates during that time, the landlord will dismiss the case. The tenant moves out within the two weeks, and the case is dismissed. In the other scenario, the parties execute a side agreement (perhaps drafted by the landlord's attorney) providing that judgment in the landlord's favor will issue immediately for possession and for the full amount of the arrears, but that the landlord will not seek to levy on the judgment or ask for a writ of execution for possession if the tenant departs within two weeks. Judgment in the landlord's favor enters for possession and for the arrears, the tenant vacates two weeks later, and the landlord never seeks to collect or to execute. To a layman, the two transactions may appear identical; in both, the landlord obtained possession within two weeks but did not receive the arrears. But to an external observer (such as a credit reporting agency), who cannot see the parties' side agreement, the two scenarios are night and day; the first involves a dismissal of a summary eviction action, while the second involves a court-ordered eviction. And as noted above, there can be other important consequences to a judgment of eviction (as opposed to, again, an agreement to move out followed by a dismissal), such as ineligibility for certain types of homelessness-prevention emergency shelter assistance.[63]

The Quincy District Court, the site of the present District Court Study, handled approximately 1280 summary eviction cases in fiscal year 2010, the year when most of the cases in our study began. During the study, the Quincy District Court devoted one judge to the summary eviction docket every Thursday morning; this judge's two-plus-decade career on the bench was preceded by a law practice that included extensive public service as well as three years as a legal aid attorney.

[63] *See* 106 MASS. CODE REGS. 309.040(B)(3)–(6) (declaring that a household is ineligible for assistance under a Massachusetts emergency shelter program if the household was evicted for nonpayment of rent, criminal activity, destruction of the unit, or "because it lost its housing under an agreement *for judgment*" in an eviction proceeding based on these reasons (emphasis added)).

The district court handled its Thursday morning summary eviction docket by following a pattern. The judge or the clerk called cases to ascertain whether both parties were present. If both were present,[64] the judge required the parties to engage in a hallway settlement negotiation in an attempt to settle whatever had brought them to court. If the hallway session failed to produce an agreement, the judge referred as many cases as he could to mediation run by volunteer mediator teams. These mediation sessions were facilitative, party centered, and party driven, with mediators striving to avoid suggesting possible resolutions, evaluating facts, or predicting how the court might rule. If neither of these steps succeeded in producing a settlement, the judge heard briefly from each party, then engaged in a back-and-forth colloquy that essentially pushed the parties toward settlement. Contested rulings were uncommon. Although jury trials were available (and, as discussed below, frequently demanded in the cases in our study), evidentiary hearings of any kind, including trials, were rare. Nevertheless, for obvious reasons, jury trials posed scheduling difficulties for the court, and thus a jury trial demand could be a powerful tactical weapon.

The legal aid provider in the District Court Study, GBLS, recruited study participants through two different methods. Approximately 70% of study participants were recruited through a proactive, timely, individualized, and selective system. This system required that for thirteen of the seventeen months that the study lasted, GBLS assigned half of two staff attorneys' time to study work; these two attorneys were housing specialists with fifteen and thirty years of experience, respectively. From our limited observation, both attorneys appeared to possess a high degree of dedication, skill, and zeal. A third attorney, with two years of experience in housing law, worked on the project for four months.

Every week, one of these three GBLS attorneys, a paralegal, or a volunteer went to Quincy District Court, examined the files in recently initiated summary eviction actions, and mailed letters to occupants likely to be study eligible[65] inviting them to two- to three-hour instructional sessions (called "clinics") held in Quincy or in GBLS's offices in downtown Boston. These instructional sessions included overviews of

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[64] If one party was absent, the court ordinarily took appropriate action in favor of the party who was present. If both parties were absent, the court ordinarily took appropriate action against the party who had caused the case to be on the court's calendar for that day.

[65] To be study eligible, an occupant had to live within the geographic jurisdiction of the Quincy District Court and had to meet GBLS's income-eligibility guidelines. *See Can GBLS Help Me?*, GREATER BOS. LEGAL SERVICES, http://www.gbls.org/get-legal-help/can-gbls-help-me#Eligibility%20Table (last visited Dec. 1, 2012). As discussed below, the GBLS attorneys applied additional criteria in selecting the cases forwarded to us for randomization.

the summary eviction process and individualized assistance in filling out pleading and other forms. One such form was a lengthy summary process eviction answer form[66] that was available online and used by lawyers and pro se litigants alike to assert, in a checkbox format, any number of defenses to the eviction action that occupants could raise. Checkbox discovery forms were also frequently used.[67]

During these instructional sessions, GBLS attorneys evaluated the circumstances of individual cases. Formally, the attorneys were looking for cases that fit within the Task Force's stated criteria for inclusion in the study;[68] in practice, a major factor in applying these criteria was the attorneys' judgment regarding whether they thought they could alter the outcome of the case. In most instances, the staff attorneys made this judgment on the basis of the information gained in the instructional sessions. In a few cases, the staff attorneys remained in contact with the occupant during the process of propounding and obtaining answers to discovery requests. The attorneys then used the information gained in discovery to decide whether to include these cases in the study.

The three previous paragraphs describe how 70% of study subjects were recruited. GBLS staff attorneys present in the Quincy District Court on other matters recruited the remaining 30% on the basis of on-the-spot interviews conducted either when the judge referred litigants to the staff attorneys or when the litigants approached the staff attorneys on their own. GBLS staff attorneys assisted an undetermined number of these litigants with filling out answer and discovery forms, although none of this 30% attended an instructional clinic.

Upon selecting a case for the study, GBLS staff attorneys forwarded to us single-page information sheets about the occupants, which included verification of consent to participate in the study. We randomized cases to treatment, meaning an offer of full representation by a GBLS staff attorney, or to control, meaning no further assistance.

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[66] *Representing Yourself in an Eviction Case: The Answer*, MASS. LAW REFORM INST. (Sept. 9, 2011), http://www.masslegalhelp.org/housing/legaltactics1/answer-how-to-defend-your-eviction-case.pdf; *cf.* Turner v. Rogers, 131 S. Ct. 2507, 2520 (2011) (highlighting the lack of an easy-to-use form that would elicit critical information in holding that a civil contemnor's incarceration violated due process).

[67] *Representing Yourself in an Eviction Case: Discovery*, MASS. LAW REFORM INST. (July 2008), http://www.masslegalhelp.org/housing/legaltactics1/discovery-get-information-prepare-for-trial.pdf. The filing and service of discovery resulted in an automatic continuance of the summary eviction trial date, ordinarily set for ten days after the filing of a complaint, for two additional weeks. Thus, discovery requests (and the corresponding motions to compel when, as was usually the case, evictors failed to respond), along with jury trial demands, were tools for occupants to gain bargaining leverage with would-be evictors.

[68] *See supra* note 56 and accompanying text.

We were able to gain a picture of the nature of the representation the GBLS attorneys provided from our conversations with them as well as from our personal review of the case records. Three aspects of the attorneys' representation practices appear particularly important. First, the representation extended beyond courtroom advocacy and legal preparation into advocacy before programs and agencies that could provide services and assistance to the tenants. In one case, for example, the GBLS attorneys engineered a transaction to keep a homeowner in her home by helping to structure a foreclosure sale house repurchase, an endeavor that required some funding by an entity set up for this purpose. In several other cases, the attorneys successfully sought bridge funding from entities with money to help tenants temporarily behind on their rents. Second, the attorneys were aggressive in their investigation of the underlying facts and circumstances of their clients' units and financial conditions. In at least one case, the attorneys discovered that the relevant housing authority had (badly) miscalculated a client's income for purposes of figuring out the amount of her individual contribution toward rent for her Section 8 unit. In other cases, GBLS attorneys documented conditions in the unit that constituted breaches of landlord duties and covenants. Third, the GBLS attorneys employed what one might label an "assertive" or "confrontational" style of litigation. As discussed in greater detail below, they made frequent use of jury trial demands, motions to compel responses to discovery, and motions for preliminary relief (such as for attachments to prevent sales of property and for injunctions to require landlords to pay for utilities). To make their jury trial demands credible, they prepared fully for trial on several occasions (with the cases ordinarily settling on the proverbial courthouse steps on the morning of trial). This "assertive" or "confrontational" style of litigation is in contrast to that employed by the attorneys in the Housing Court Study, which one might label "facilitative" or "nonconfrontational," and this difference in litigating styles may provide a partial explanation for the contrast in the results between the Housing Court Study and the present District Court Study, which we discuss elsewhere.[69]

III. QUANTITATIVE RESULTS

In this Part, we provide the results of this randomized trial. We begin by examining the balance between the treated group (offered full representation from a GBLS staff attorney) versus the control group

[69] *See* Greiner et al., *supra* note 23 (manuscript at 45–46).

(not offered full representation) on observed background variables.[70] We do so to check the effectiveness of the randomization in providing comparable treated and control groups, and relatedly to assess whether it might be worthwhile to risk some modeling to adjust for any variables that might be somewhat unbalanced. We follow this check of randomization with the results for attorney usage by evictors and by the treated and control group occupants. We then summarize the results for the three primary sets of outcomes we studied: possession-related outcomes, financial consequences of the dispute, and the burden on the court that the case imposed.[71] We discuss some additional outcomes before turning to possible explanations for the results.

One word of caution: the small number of observations in our study, 129, means that we were likely to detect only large differences between treated and control groups.[72] Given that demand for legal aid far outstrips supply, studies likely to detect only large effects are worthwhile. If a legal intervention is not producing large changes in outcomes, it may be wise to consider either changing the nature of the legal intervention (so as to produce substantial differences in outcomes) or devoting resources to other areas of need, of which there would appear to be no shortage. Large differences in outcomes were indeed present in the District Court Study. We provide intervals for each of our primary results, which incorporate uncertainty from the variation of outcomes in the data while reflecting the relatively small number of observations.[73]

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[70] The background variables came from the single-page information sheets GBLS 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.

[71] Our primary analysis technique in this section is a "Fisher" or permutation test, which allows us to incorporate the nonstandard randomization scheme described in Appendix I. Essentially, a Fisher or permutation test relies solely on the randomization scheme to make inferences about the causal effect of a treatment. We use this method to test for differences in averages (calculated both with no weights and with each observation as weighted by the inverse of the probability of being assigned to the treatment it received, although the two were rarely different), medians, and 0.25 and 0.75 quantiles for covariates as well as outcomes. See Greiner & Pattanayak, *supra* note 8, at 2149–50, for a brief explanation of the Fisher/permutation test. Specifically, as we did in that article, we use a Monte Carlo version of the test, as was apparently first proposed in Meyer Dwass, *Modified Randomization Tests for Nonparametric Hypotheses*, 28 ANNALS MATHEMATICAL STAT. 181 (1957).

[72] We are still waiting for one of the 129 cases to conclude. This case is from the treated group and is excluded from all covariate and outcome calculations, except those related to attorney usage and actual possession (it is included there because we know these values for this observation).

[73] For continuous outcomes, we produce intervals with the Fisher/permutation technique as well as with a straightforward adaptation of the Peters-Belson method to Bayesian regression. *See generally* William A. Belson, *A Technique for Studying the Effects of a Television Broadcast*, 5 APPLIED STAT. 195 (1956); Charles C. Peters, *A Method of Matching Groups for Experiment with No Loss of Population*, 34 J. EDUC. RES. 606 (1941). If the outcome variable was binary (0–1), we used a Bayesian logistic regression variant of Peters-Belson regression. If the outcome was continuous, we used Bayesian ordinary least squares regression.

A. The Balance Between Treated and Control Groups

In this section we examine whether the randomization scheme did what it was supposed to do, namely, whether it produced treated and control groups that were roughly the same in all ways except for the treatment.

To the extent we can tell from observed covariates, the randomization performed well but not perfectly. Table 1 provides treated and control group means and standard deviations for twenty-seven background variables, along with permutation test *p*-values for differences in the unweighted means and medians.[74] In this context, the *p*-value represents a measure of bad luck. Due merely to bad luck, the randomization might have put more of one kind of case (say, cases in which the potential client needed an interpreter) in the control group rather than in the treated group. This kind of bad luck does not matter unless two things are true: (i) the difference between the treated and control groups is of a sufficient magnitude, and (ii) the difference occurs in a variable that is associated in some way with the outcomes about which we care. Regarding the first point, whether the difference between the treated and control groups is of a decent magnitude, we can ask the following question: how likely is it for a difference (in averages or medians or other metrics) this big or bigger to occur simply because of random chance? That is the *p*-value. Thus, small *p*-values are cause for further investigation and for considering statistical adjustment, such as with a model.[75]

[74] Missing values, while infrequent, were handled via a form of multiple imputation. *P*-values were combined following C. Licht et al., Combining One-Sided *P*-Values from Multiply-Imputed Data (2011) (unpublished manuscript) (on file with the Harvard Law School Library). The method generates one-sided *p*-values, which we double and report as two-sided *p*-values. For more explanation of the multiple imputation methods, please contact the authors directly.

[75] How do we get quantitative leverage on the second question, whether the variable is one associated in some manner with the outcomes about which we care? There are some ways, but none is very reliable. For this study, we use the judgments of attorneys and other participants in the summary eviction process, as reported to us in informal conversations.

TABLE 1: BACKGROUND VARIABLES COMPARISON: TREATED GROUP VERSUS CONTROL GROUP

(low *p*-values are cause for concern)

Covariate	Treated Mean (SD) N = 76	Control Mean (SD) N = 53	*P*-Value, Mean	*P*-Value, Median
Is NTQ Type 14 Days?	0.72 (--)	0.63 (--)	0.71	--
Is NTQ Type 30 Days?	0.18 (--)	0.17 (--)	0.87	--
NTQ Amount, All Cases	1327 (1349)	1297 (1692)	0.84	0.83
NTQ Amount, Positive Only	1853 (1249)	2050 (1727)	0.55	0.41
Complaint Amount, All Cases	1888 (1878)	1910 (1897)	0.82	0.85
Complaint Amount, Positive Only	2284 (1833)	2596 (1760)	0.34	0.46
Is Post-Foreclosure?	0.11 (--)	0.19 (--)	0.31	--
Is Occupant Homeowner?	0.03 *(--)*	0.13 *(--)*	0.07	--
Rent All Cases	707 (459)	676 (439)	0.63	0.46
Security Deposit All Cases	517 (565)	358 (438)	0.38	0.71
Security Deposit Positive Only	803 (514)	658 (393)	0.38	0.17
Last Month Rent All Cases	321 (513)	236 (450)	0.40	1.00
Last Month Rent Positive Only	867 (486)	952 (364)	0.43	0.75
Does Occupant Want To Stay In Unit?	0.85 (--)	0.77 (--)	0.23	--
Is Unit Section 8?	0.39 (--)	0.40 (--)	0.47	--
Is Unit Public Housing?	0.11 (--)	0.13 (--)	0.69	--
Is Occupant Female?	0.69 (--)	0.68 (--)	0.67	--
Occupant Age	41 (9.8)	42 (11)	0.49	1.00
Is Occupant Hispanic?	0.08 (--)	0.02 (--)	0.26	--
Is Occupant Black?	0.44 (--)	0.30 (--)	0.14	--
Is Occupant White?	0.44 (--)	0.53 (--)	0.45	--
Occupant Needed Interpreter	0.03 *(--)*	0.09 *(--)*	0.09	--
Is Occupant Mentally Disabled?	0.31 (--)	0.32 (--)	0.68	--
Is Occupant Physically Disabled?	0.25 (--)	0.40 (--)	0.30	--
Number In Unit All Persons	2.9 (1.5)	2.7 (1.6)	0.90	1.00
Number In Unit Less Than 18	1.4 (1.3)	1.2 (1.4)	0.99	1.00
Days From Complaint To Intake[76]	8.0 (7.3)	8.3 (9.3)	0.98	1.00

Table 1: Background Variables Comparison: Treated Group Versus Control Group: This table shows means and standard deviations for the treated and control groups. "NTQ" refers to "notice to quit," the document the evictor served upon the occupant to initiate the eviction

[76] Readers familiar with the corresponding table in Greiner et al., *supra* note 23 (manuscript at 17), should note that, in that table, we reported the number of days from intake to complaint, which is the negative of what we report here.

process. Any variable beginning with "Is" is binary (0–1), so all information is in the average/rate, and neither the median nor the standard deviation is a useful statistic. The final two columns report two-sided p*-values for the difference in unweighted averages and the difference in medians from the Fisher/permutation test. There appear to be few large differences between treated and control groups. No variable has a* p*-value less than* 0.05*, and only two (italicized) have median or mean* p*-values less than* 0.10*. Nevertheless, the two variables that do have* p*-values of less than* 0.10 *— whether the occupant was a former homeowner and whether the occupant needed an interpreter — could be related to the principal outcomes in our analysis.*

Table 1 shows that the randomization produced few large differences between treated and control groups. Of the twenty-seven background variables we measured, none had permutation p-values of less than 0.05 for the mean or the median, and only two had p-values for either the mean or the median of less than 0.10.[77] That result suggests that the randomization produced treated and control groups that resembled one another, which was our goal. When one examines twenty-seven background variables, one would expect a few variables to end up with the sort of small p-values Table 1 demonstrates.

Yet a few rows in Table 1 do cause us discomfort. The discomfort stems not from the number of background variables showing smallish p-values but rather from which variables had those p-values. Our intuition is that two of these variables — whether the occupant was a former homeowner and whether the occupant needed an interpreter — could have been related to the outcomes we examined. When a homeowner lost title to a housing unit in a foreclosure, many of the legal protections that might have applied in a landlord-tenant situation were unavailable. Similarly, an occupant who needed an interpreter might have had more difficulty in understanding district court forms and procedures and in accessing any available social, economic, or legal assistance. If forced to pick two variables in which we could tolerate imbalance between treated and control groups, we would not have chosen these two. Moreover, the imbalance in both cases is in favor of the treated group; that is, the treated group has the lower fraction of homeowners and of occupants who need interpreters.

We should not overstate our discomfort here; this is indigestion, not dysentery. The imbalances are not extraordinarily large. Nevertheless, we report results from statistical modeling that (at some risk) adjusts for these imbalances, along with results that rest solely on the randomization scheme. As it turns out, the substantive conclusions from the various techniques are the same. The difference in outcomes between

[77] We also compared weighted means, treated versus control, where the weighting was by inverse probability of assignment. The results were qualitatively similar: two of the variables highlighted in the discussion above, whether the occupant was a homeowner and whether the occupant needed an interpreter, had p-values from the permutation test of 0.04.

treated and control groups is so large that it obliterates background variable imbalance, dataset size, and other statistical worries.

B. Attorney Usage

In this section, we discuss the usage of attorneys among evictors before turning to the usage of attorneys among our treated and control groups.

Usage of attorneys among evictors in our dataset was high. Ninety percent (116 of 129) of evictors in our dataset were represented. Eighty-six percent of evictors litigating against treated group occupants were represented, versus 96% of evictors litigating against the control group, and this difference was borderline statistically significant (p = 0.05). We also address this difference with statistical modeling.[78]

Based on conversations with attorneys and court personnel, as well as informal observations by an intern, we concluded that this 90% attorney usage rate among District Court Study evictors was higher than the overall rate for the district court's summary eviction caseload.[79] This fact is hardly surprising, given that the study's inclusion criteria explicitly instructed GBLS staff attorneys to consider whether the evictor was represented. These figures, along with the outcome results we report below, suggest that GBLS attorneys chose their cases well. The high evictor representation rate does, however, affect whether the results we report here can be generalized to all cases on the District Court Study's calendar.

With respect to attorney usage by occupants, Table 2 provides the relevant results in simple form. Ninety-seven percent (74 of 76) of treated-group occupants accepted the offer of representation, suggesting that representation was greatly valued. Eighty-nine percent (47 of

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[78] In a different setting, we would not necessarily recommend adjusting for whether the opposing party has an attorney. It seems plausible to us that whether an opposing party is represented might be an outcome, not a covariate, in the sense that if an occupant becomes represented, the evictor may then obtain counsel. But here, the disparity takes the form of the *control* group's having a higher rate of evictors represented than the treated group. If evictors decided whether to retain attorneys depending on whether occupants retained counsel, we would expect to see exactly the opposite. So we feel that adjustment may be warranted.

[79] The latter rate was not easy to estimate without a sample of case records, and as discussed below, the Task Force that organized the District Court and Housing Court Studies dedicated no funding for an evaluation. An intern observed the District Court's case call for four consecutive weeks during one month in the summer of 2011, when the study was almost completed. She found that in somewhere between 51% and 83% of cases called, evictors had counsel, but some cases were probably called more than once in these four weeks, and the sample was not representative.

53) of control group occupants were self-represented,[80] suggesting that representation was largely unavailable from other sources.[81]

TABLE 2: OFFER VERSUS ACTUAL USE OF REPRESENTATION FOR OCCUPANTS

		Actual Use of Representation		
		Represented	Pro Se	Total
Randomly Assigned Group	Offer of GBLS Representation	74	2	76
	No Such Offer	6	47	53
	Total	80	49	129

Table 2: Offer Versus Actual Use of Representation for Occupants: Treated occupants received an offer of a traditional attorney-client relationship from a GBLS staff attorney. Control occupants received no such offer. Ninety-seven percent (74/76) of treated-group occupants were ultimately represented, while 89% (47/53) of control group occupants received no legal help beyond the substantial but limited assistance provided at intake. These figures suggest that representation was highly valued but, apart from this pilot program, largely unavailable to study-eligible clientele.

C. *Three Critical Sets of Outcomes, Plus Some Others*

In this section, we discuss the three critical sets of outcomes we studied, plus a few others. The three critical sets of results concern possession, monetary consequences of litigation, and court burden. We refer to "sets" of results because we measured multiple variables relat-

[80] That is, they were self-represented except for the limited (but substantial) assistance these occupants received from GBLS's instructional clinics.

[81] In the two treated-group cases in which the occupant proceeded pro se, the occupants did not allow GBLS staff attorneys to represent them: one made a deal with her evictor before the attorneys could intervene, and the other did not respond to the attorneys' efforts to contact him.

With respect to the six control group cases in which the occupant was represented, in the majority of these cases, GBLS attorneys violated the study's protocol, to great effect. For example, in one case, a GBLS attorney handed a ghostwritten motion to dismiss (for failure to serve a notice to quit) to the occupant and explained what the motion argued and the basics of the applicable law. The occupant filed the motion, and the case was dismissed. Had the case not been dismissed, we would have had to decide whether to code the case as represented or unrepresented, but the fact that the GBLS attorney's intervention had an immediate effect left us little choice. In another case, a GBLS staff attorney, upon finding out that a case had been randomized to control, referred the occupant to a local private attorney with over forty years' experience in summary eviction litigation who operated a small law firm dedicated to housing matters. This attorney obtained a series of orders and a settlement in which the occupant retained possession and obtained a waiver of almost a year's rent. The attorney then obtained a series of fee awards of over $40,000; at last check, the fee awards were on appeal.

We provide the facts in the previous paragraph in an effort to describe what happened, not to suggest that GBLS staff attorneys acted in any way inappropriately. In randomized studies of the kind designed here, protocol must occasionally bend to circumstances on the ground. The randomized study can nevertheless provide useful information so long as protocol violations are not too frequent, as the present study illustrates.

ed to each issue.[82] We conclude this section by discussing some other results potentially of interest.

1. Possession Outcomes. — We coded three outcomes related to possession of the unit. By far the most important was actual possession, meaning which party was in possession of the unit at the end of the litigation. Actual possession refers to whether the evictor ended up in possession, not whether any loss of possession by the occupant was voluntary or otherwise. In some cases occupants no doubt did choose to move voluntarily; but as the Table 1 variable "Does Occupant Want To Stay In Unit?" demonstrates, approximately 80% of study-eligible occupants reported wanting to stay in their units at intake. Moreover, it would appear that for at least some of the 20% who reported either that they were undecided regarding a desire to stay or that they affirmatively wanted to vacate, their desires were somewhat aspirational. We asked occupants where they would live if they were evicted: the majority of this 20% (16 out of 23 occupants) reported that they did not know, with smaller fractions reporting that they would live in a shelter (3 of 23), in another unit (2 of 23), or with family or friends (2 of 23). For the overwhelming majority of occupants in this dataset, then, it would appear that retaining possession was important to socioeconomic well-being. We also coded two other possession-related outcomes: whether a judgment of possession entered for the evictor and whether a writ of execution for possession issued.[83]

To preview a distinction mentioned above and discussed in further detail below: whether the evictor obtained actual possession of the unit at the end of the summary eviction action was a socioeconomic outcome in the sense that it was an on-the-ground fact that described the occupant's life. Whether the evictor obtained a judgment of eviction, or whether a writ of execution for possession was issued, was a legal outcome that constituted one of the outputs the district court produced as a result of the litigation. Both socioeconomic and legal outcomes are interesting. Most studies of the effects of offers of representation measure only the second type of effect.[84] As we discuss in greater de-

[82] We discuss multiple-testing penalties, which are statistical safeguards that counter the increased likelihood of false positives inherent in testing multiple variables, in Appendix II.E.

[83] The right to actual possession at the end of litigation might not have corresponded to which party obtained a judgment of possession. An example of how the disparity between the two could arise is as follows: The parties settled a nonpayment-of-rent lawsuit by agreeing that a judgment of possession would enter for the evictor, but that no execution would issue pending the occupant's keeping current with future rent plus complying with a schedule for repayment of the arrears. The occupant complied with the repayment schedule, and the evictor never sought to effectuate the judgment for possession.

[84] This was the case in every one of the studies listed in Greiner & Pattanayak, *supra* note 8, at 2175 n.154.

tail in Part IV, legal outcomes are critical, but studies of representation and, indeed, access-to-justice efforts more generally must expand to incorporate socioeconomic outcomes.

Table 3 shows the results for these three possession outcomes. The differences between the treated and control groups were large. On the critical variable of actual possession, approximately one-third of treated-group occupants did not retain possession, versus approximately two-thirds of control group occupants. *P*-values[85] based on permutation tests for all three variables were 0.01 or below. The disparities between treated and control group outcomes were much larger than those reported in the only other randomized study of representation in housing.[86] Using statistical modeling that (at the risk of some mathematical assumptions) adjusted for the possible imbalances in the homeowner, interpreter, and evictor-represented variables noted in section III.A, we produced a 95% interval for the change in the probability that the evictor would gain possession (-0.39, -0.17). In other words, we believe that a GBLS staff attorney's offer of representation reduces the probability that the occupant vacates the unit by an amount that is 95% likely to be between 0.17 and 0.39, with values in the middle of this range (between, say, 0.25 and 0.35) most likely.[87]

TABLE 3: RESULTS FOR POSSESSION VARIABLES, LOWER VALUES ARE PRO-OCCUPANT

(low *p*-values suggest a significant treatment effect)

	Treated Rate	Control Rate	*P*-Value
Actual Possession, Evictor	0.34	0.62	0.01
Judgment of Possession, Evictor	0.17	0.75	< 0.01

[85] Here, *p*-values represent the possibility that the differences we observe are entirely due to chance, as opposed to the offer of representation by a GBLS staff attorney. A low *p*-value suggests that there is little possibility that chance alone explains the outcome differences between treated and control groups, so we infer that the observed differences are due to the treatment, that is, the offer of representation.

[86] *See* Seron et al., *supra* note 9, at 426. That study did not track actual possession. Its figures for what we label "Judgment of Possession, Evictor" were 0.32 for the treated group versus 0.52 for the control group. Its figures for what we label "Execution Writ for Possession Issued, Evictor's Favor" were 0.24 for the treatment group versus 0.44 for the control group. *Id.*

[87] 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 as covariates those with the lowest *p*-values from permutation tests; and (iii) "backward selection" models, in which we ran separate backward-selection algorithms for treated and control groups. Due to the potential imbalances noted in Table 1 and discussed in the accompanying text, we report the results for the "covariate balance" models. The results for the other two sets were, however, surprisingly similar. For each set of models, we used a Bayesian analog to the Peters-Belson method. *See supra* note 73.

Execution Writ for Possession Issued, Evictor's Favor	0.12	0.60	< 0.01

Table 3: Results for Possession Variables: Treated versus control group comparisons for possession variables. All variables are coded from the point of view of the evictor, so lower values are better for the occupant. P*-values are derived from the Fisher/permutation test for the difference in unweighted means.*[88] *The differences between treated and control groups are substantively large and statistically significant.*

In conclusion, the data demonstrate that an offer of representation from a GBLS staff attorney caused substantively large and statistically significant alterations in possession outcomes that favor occupants.[89]

2. *Financial Consequences.* — As is true of possession, there are several possible outcomes associated with financial consequences. We find most of these measurements unappealing because, as explained in detail in Appendix I, 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 to look only at cases in which the evictor alleged nonpayment of rent or in which the occupant alleged serious monetary counterclaims. For cases in this category, we calculate an outcome we call "EvictorMonthsRentLost," short for "Evictor's Months of Rent Lost." The details of this calculation are mildly complicated and appear in Appendix III, but the aim here is to produce a proxy for the number of months of rent an occupant did not have to pay relative to the amount the evictor alleged to be due. In very rough terms, this is the number of months of rent the occupant "saved" in the litigation.

We place a great deal of stock in this EvictorMonthsRentLost measurement and find the results (discussed below) revealing, persuasive, and important. Nevertheless, the measurement has at least three drawbacks. First, we could not observe payments to landlords on tenants' behalf from charity groups and tenant preservation organiza-

[88] Weighted figures are substantively similar although less extreme. For example, the weighted figures for the "Actual Possession, Evictor" variable are 0.38 treated versus 0.60 control, with a *p*-value of 0.05.

[89] A 2005 Massachusetts Law Reform Institute survey of summary eviction cases statewide found that "landlords" were awarded possession in 78% of cases; "tenants" were awarded possession in 2% of cases; and in 20% of cases, the case was dismissed in a way that made it difficult to know whether the tenant had already moved out or whether the tenant retained possession. MASS. LAW REFORM INST., 2005 SUMMARY PROCESS SURVEY NUMBER 4, at 8 (2005), *available at* http://www.masslegalservices.org/system/files/2005_summary_process_survey.pdf. However, in contrast to the current study, the data in that survey are statewide, not specific to the Quincy District Court's geographic jurisdiction; they are from a time period that predated the 2008 recession, unlike the present study; the use of the terms "landlord" and "tenant" make it difficult to know whether the survey includes foreclosure cases; the data concern all cases, not just those from occupants who responded to a provider's outreach letters; and so forth.

tions — particularly the Quincy Community Action Program,[90] which was active in the district court area in our study — because these payments were not evident from the case records.[91] In the District Court Study, however, our inability to measure external funding sources almost certainly causes us to understate the financial effect of an offer of representation upon potential clients. GBLS staff attorneys in the District Court Study informed us that they aggressively pursued external funding sources on behalf of their clients and provided several examples of cases in which they secured such funding.[92] We speculate that control group occupants, lacking the assistance of these aggressive efforts, may have had less success in obtaining external funding, but we do not know. If our speculation is correct, the results we report for EvictorMonthsRentLost understate the true effect on occupant finances.

Second, EvictorMonthsRentLost measures the payments that were legally required, not the payments that were actually made (which we could not determine).[93] Thus, for the purposes of this variable, a court order that an occupant pay an evictor $1000 was counted the same as a recitation in a settlement document that an occupant had in fact paid an evictor $1000.

Third, EvictorMonthsRentLost is based on a subset of cases in which, in our judgment, monetary considerations were at the forefront of the parties' disputes. We based this judgment primarily on the complaint and the answer. We note, however, that we also calculated the EvictorMonthsRentLost measurement with respect to several subsets of cases that were defined by completely objective criteria, such as whether the evictor issued a fourteen-day notice to quit. In these alternative calculations, discussed in Appendix III, we obtained different but nevertheless highly significant results. We also note that not all of these subsets included the three cases with outcomes most favorable to the occupant; the results in these cases were so pro-occupant that some of our statistical analyses would not run properly if we kept them in the dataset. We discuss this "outlier effect" in Appendix II.

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[90] *See* QUINCY COMMUNITY ACTION PROGRAMS, http://www.qcap.org/ (last visited Dec. 1, 2012).

[91] This is one reason that we have measured the number of months of rent lost by evictors as opposed to the number of months' rent saved by occupants, although as explained, our interest was obviously in the latter. We do believe that the former constitutes a reasonable proxy for the latter.

[92] In contrast, the attorneys in the Housing Court Study did not see pursuing funds from charitable groups or other sources as part of their role, although they did provide clients with contact information for these groups and suggest that clients pursue this funding.

[93] Obtaining this information would have required a complex field operation, which we would have liked to have implemented had we possessed sufficient funds. We return to this issue below. *See infra* section III.E.2, pp. 949–50.

With these caveats, the results are as follows[94]: while there were several ways to estimate EvictorMonthsRentLost, our preferred method provides an estimate of -9.4 for the treated group versus -1.9 for the control group. Negative numbers here are good for the occupant, so these figures indicate that occupants who received offers of representation saved an average of over three-quarters of a year's rent while tenants who received no such offer saved an average of less than two months of rent. The Fisher/permutation p-value for this result is 0.01, which is statistically significant under most measures. We thus have a point estimate of a difference between treated and control groups' averages — the treatment effect — of seven and one-half months of rent saved. We generated 95% intervals for this treatment effect using permutation and modeling methods. For the permutation method, the interval is (-12.0, -1.9), meaning (again) that we think that the true average number months of rent saved due to treatment is 95% likely to be between -12.0 and -1.9, with values near the middle of the interval being more probable than values at the extremes. For the modeling method, the interval is (-10.8, -3.6).[95]

For reasons in addition to the external funding issue discussed above, the results we report here likely represent sizable underestimates of the treatment effect. We discuss these reasons — outliers, an unresolved case, and ambiguous control group cases — in Appendix III.

We also examined other measures of financial consequences.[96] For instance, if we lump all case types together rather than analyzing only cases with nonpayment of rent or serious monetary counterclaims, we obtain a median judgment amount[97] of $0 for the treated group versus $617 for the control group (p < 0.01). This measurement is important

[94] All the figures reported in this section are unweighted. If we weight by probability of assignment, we generally get similar figures with slightly higher p-values. For example, for the primary result we report in this paragraph, EvictorMonthsRentLost with the three outlier cases removed from the treated group, the treated-group mean is -9.4, the control group mean is -2.1, and the permutation p-value is 0.06.

[95] For this EvictorMonthsRentLost interval, we used a Bayesian ordinary least squares regression analog of the logistic regression models described *supra* note 73.

[96] The discussion *supra* p. 926 applies to some extent here. That is, EvictorMonthsRentLost represents an attempt to measure the real-world flow of money from one party to the other as a result of the summary eviction litigation, a socioeconomic consequence. Variables discussed in this paragraph, such as the amount of a money judgment, represent the legal products of the summary eviction process. As discussed *supra* p. 926, both types of outcomes are interesting, but thus far, only the legal outcomes have been examined in other studies. *See* Greiner & Pattanayak, *supra* note 8, at 2175 n.154 (collecting studies). In Part IV, we suggest that this exclusive focus on legal outcomes must change.

[97] We report medians because a comparison of average judgments in the treated and control group suffers from outliers, an effect explained in Appendix II. The treated-group average judgment amount was -$1652 (meaning $1652 in favor of the occupant), versus $373.15 for the control group (meaning $373.15 in favor of the evictor), but the outliers induce a permutation p-value for the difference in averages of 0.28.

and interesting because it represents the most visible legal outcome of the summary eviction process concerning the financial aspect of cases. As a measurement of the actual financial outcomes for the parties (particularly the occupant), however, judgment amount has two drawbacks. First, as noted above, it groups all case types together, despite the fact that a payment to or from a former homeowner in a post-foreclosure eviction means something different than does a payment to or from a tenant in a nonpayment-of-rent case. Second, the amount of a judgment often does not reflect the financial consequences of a case. For example, suppose that a landlord sued a tenant for nonpayment of rent, claiming that four months are in arrears; the tenant counterclaimed, pointing to bad conditions in the unit that violated the implied warranty of habitability. After three months of litigation, the parties settled, with the landlord's agreeing to waive all past-due rent, make repairs, and allow the tenant to keep possession. Here, seven months of rent were waived, but the judgment was for $0. Now imagine the same lawsuit with the same arrears and three months of litigation, but the tenant paid the landlord the full amount of the claim in order to keep possession and dismiss the case. This judgment would also be for $0. Judgment amounts are legal outputs of summary eviction proceedings; as measurements of actual financial consequences, they are approximations.

We also studied execution writs for monetary payments.[98] Such writs were issued in 7% of treated-group cases versus 38% of control group cases ($p < 0.01$). Regarding the monetary amount of execution writs, recording $0 for cases in which no writ was issued (to avoid the problem of contingent outcomes[99]), the treated-group average was $147 versus $1124 in the control group ($p < 0.01$).[100] Other monetary results followed this pattern of statistically significant and substantively large differences between treated and control groups, all in the occupant's favor.[101]

In conclusion, the data demonstrated that an offer of representation from a GBLS staff attorney caused substantively large and statistically significant alterations in monetary outcomes in favor of occupants.

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98 *See* MASS. GEN. LAWS ch. 239, § 3 (2010).

99 Appendix II, *infra* pp. 977–85, discusses the problem of contingent outcomes.

100 These are all unweighted results, with p-values stemming from permutation tests. Weighted figures are qualitatively similar.

101 For example, we examined a version of EvictorMonthsRentLost in which we did not divide by one plus the occupant's version of rent amount (see Appendix III for an explanation of the denominator used in the calculations reported in the text). In other words, this measurement captured the raw, not scaled, money flowing between evictor and occupant as a result of the summary eviction litigation. The treated-group average and median were -$4849 and -$3200, respectively, versus a control group average and median of -$1308 and $0 ($p < 0.01$ for both the average and the median). Recall that negative numbers are pro-occupant.

3. Court Burden. — As was the case for possession and financial consequences, there were several measures of how litigation burdened courts. We report the following outcomes here: case length (complaint to final judgment) in days;[102] the number of prejudgment motions filed by the evictor; the number of prejudgment motions filed by the occupant; the number of total motions filed by the evictor and by the occupant; a variable we label "NumberJudgeLooks," a measurement based on the docket sheet of the number of times the judge had to interact with a case;[103] a variable we label "NumberJudgeRulings," meaning the number of times the docket sheet disclosed that the judge issued a contested ruling;[104] and whether the case had an evidentiary hearing of any kind, trial or otherwise.

We pause to clarify one point: we measure here the difference in court burden imposed by treated versus control group cases. Recall that almost all cases, treated and control alike, received substantial legal assistance in the form of instructional sessions and help in filling out answer and discovery forms. We received informal information that the injection of the GBLS staff attorneys into the Quincy District Court's summary eviction calendar nearly doubled the time it took the court on Thurs-

[102] The distribution of this outcome, for both the treated and control groups, had a heavy right tail, meaning a large number of cases that took substantially longer than the average. This is often true of time variables, particularly case length. *See, e.g.*, Greiner & Pattanayak, *supra* note 8, at 2154. We explored various transformations of the case lengths to see if any would produce a bell-shaped curve, and found that taking a fifth root worked reasonably well. But the conclusions from analysis of the untransformed case lengths were the same as those from the analysis of the fifth root, and as it is not easy to interpret results in terms of a fifth root of "case length," we do not report the transformed results.

[103] This variable is roughly analogous to the "mean number of court appearances" measurement in Seron et al., *supra* note 9, at 427.

[104] Two caveats: First, the NumberJudgeRulings figures should be interpreted with caution. GBLS staff attorneys told us that there were a handful of cases in which court rulings (typically grants of motions to compel or of motions for preliminary injunctions) were never memorialized in writing and thus were not reflected in the docket sheet. Asked how often this happened, the attorneys replied that it was not frequent but was common enough to be memorable. They did think this phenomenon of unrecorded rulings was more likely to have occurred in cases in which the occupant was represented. We do not view this problem of undocumented rulings as serious, however, because as Table 4 demonstrates, the data come close to showing that the offer of representation caused a statistically significant *decrease* in NumberJudgeRulings. All we conclude here is the absence of any evidence that the treatment caused an *increase* in this variable, so we have a wide margin for error.

Second, court records do not allow us to capture several ways in which cases consume judicial resources. For example, we could not tell how long the judge spent discussing a case each time it was called. This issue might be important because, as discussed in Appendix I, the judge who adjudicated almost all of the District Court Study cases often (perhaps even ordinarily) did not make rulings so much as he made "suggestions" that he expected the parties to implement via settlement. On matters that were not case dispositive, such as discovery disputes or the issue of access to a housing unit for inspection purposes, the judge might have spent substantial time cajoling the parties into settlement, which the parties would have then implemented via a letter agreement or some other mechanism not filed with the court.

day mornings to process its housing docket. Our data did not allow us to assess this claim or any other regarding the possible burden on the court imposed by the unbundled assistance the staff attorneys provided. Our data did, however, allow us to assess whether providing an offer of representation to occupants who had already received substantial unbundled legal assistance causes an increase in the burden on the court.

The results appear in Table 4.

TABLE 4: MEASUREMENTS OF COURT BURDEN

(low *p*-values suggest a significant treatment effect)

	Treated Average (SD)	Control Average (SD)	*P*-Value
Case Length, Days Complaint to Judgment	117 (128)	69 (67)	0.01
Number Prejudgment Motions, Evictor	0.36 (0.92)	0.42 (0.82)	0.38
Number Prejudgment Motions, Occupant	1.39 (1.55)	0.81 (1.53)	0.03
Number Total Motions, Evictor	0.43 (1.09)	0.66 (1.00)	0.12
Number Total Motions, Occupant	1.43 (1.67)	1.06 (1.96)	0.29
Number Judge Looks	1.41 (1.77)	2.02 (1.55)	0.03
Number Judge Rulings	0.27 (0.89)	0.51 (0.87)	0.07
Case Had Evidentiary Hearing	0.04 (--)	0.00 (--)	0.28

Table 4: Measurements of Court Burden: This table shows unweighted[105] *averages and standard deviations for the treated and control groups for various measures of court burden.* P*-values come from Fisher/permutation tests. "Number Judge Looks" and "Number Judge Rulings" refer, respectively, to the number of times that the docket sheet indicates that the judge interacted with and issued a contested ruling in a case. "Case Had Evidentiary Hearing" is a binary (0–1) variable measuring whether there was an evidentiary hearing of any kind, including a trial, evident from the court records. We conclude that the service provider's offer of representation increased both case length from complaint to final judgment and (probably) the frequency of occupant prejudgment motions filed. Yet we find these results of little importance given that no more salient variable shows any evidence of a treatment effect. In particular, there was no statistically significant increase in the average number of total motions filed (by the evictor or the occupant), nor in the burden imposed on the judge. These results thus suggest that GBLS staff attorneys filed additional motions before judgment, but that the judge did not ordinarily rule on them (or even interact with them). Instead, the staff attorneys ordinarily used the motions as leverage to achieve favorable settlements, although there were exceptions in individual cases.*

In our view, these figures tell a fairly clear story. An offer of representation caused cases to take longer to reach judgment, and case length is always a concern. We place limited value on case length as a

[105] Weighted figures were qualitatively similar, although they generally possessed higher *p*-values.

measure of court burden,[106] however, because our review of the docket sheets suggests that the increased length stemmed from treated cases' being extended (sometimes over and over again) as the lawyers investigated facts and negotiated settlements. And the remaining variables confirm this story of the lawyers' settling matters without the court's involvement. Treated cases featured more prejudgment motions by occupants; from a review of the dockets, we know that these motions were most often to compel discovery responses, with some dispositive motions (that is, to dismiss or for summary judgment) as well. But the judge infrequently ruled on these motions, as demonstrated by the lack of a difference in the NumberJudgeRulings variable. Evidentiary hearings of any kind (including trials) were rare, as demonstrated by the CaseHadEvidentiaryHearing variable. Thus, most often, GBLS attorneys used the motions to compel (and other pretrial motions) not to obtain favorable rulings on those motions or to prevail at trial, but rather to obtain information and bargaining leverage to induce more pro-occupant settlements. There were certainly exceptions; for example, the GBLS attorneys occasionally filed, and obtained favorable rulings on, motions to attach property and for a preliminary injunction. But the majority of motions were never adjudicated.

In conclusion, as far as one can tell from court records, the data demonstrate that an offer of representation from a GBLS staff attorney caused the more favorable possession and financial results summarized in the two previous subsections without increasing the burden on the court, beyond an increase in the time needed to reach judgment, which on its own has limited substantive significance.

4. Additional Outcomes. — In this section, we discuss the frequency with which answers, counterclaims, and discovery requests were filed, as well as with which jury trials were demanded. We conclude with an explanation of why we do not show results for an outcome that many persons interested in housing law and summary eviction processes find of intense interest — namely, the time for moving out provided to those who did not retain the right of possession.

Table 5 shows the results for the answers, counterclaims, discovery, and jury trial demands.

[106] Again, delay typically advantages the occupant, because absent a court order to the contrary, the occupant retains the right of possession but can almost always vacate the unit without consequences.

TABLE 5: ADDITIONAL RESULTS

(low *p*-values suggest a significant treatment effect)

	Treated Rate	Control Rate	*P*-Value
Answers	0.93	0.91	0.62
Counterclaims	0.87	0.83	0.78
Discovery	0.89	0.91	0.79
Jury Trial Demanded	0.81	0.74	0.51

Table 5: Additional Results: This table shows the rates at which answers, counterclaims, and discovery requests were filed, and at which jury trial demands were made. Each of these four outcomes is binary (0–1). For example, we recorded whether any answer was filed in each case. P*-values come from the Fisher/permutation test. There was little difference between treated and control groups, likely due to the limited legal assistance provided to virtually all study-eligible occupants. Nevertheless, usage rates for all four procedural devices were very high.*

Again, the data tell a clear story. The message is not one of differences between treated and control groups, but rather one of answers, counterclaims, discovery requests, and jury trial demands' being filed in almost every case, treated or control. We cannot say with certainty (because we lacked resources to examine cases that were not study eligible), but we strongly suspect that the high usage rates of these procedural devices were not the norm in the district court but rather were due to the limited (prerandomization) assistance GBLS attorneys gave to almost all study-eligible occupants. Yet large differences in possession and financial consequences between treated and control groups persisted, despite the fact that both groups benefited from assistance sufficient to allow them to use the procedural weapons listed in Table 5. It is hard to imagine clearer evidence that the level of limited intervention available to both treated and control groups in this District Court Study — that is, assistance in using these procedural devices plus an instructional clinic — was not enough to assure outcomes a competent attorney could produce. An important caveat here is that, as we discuss below, all our results obtain with respect to the class of clients recruited by GBLS's outreach, intake, and case-selection mechanisms.

We briefly discuss two other results: repairs and vacate periods. Regarding repairs, we examined in each case whether court records demonstrated that the evictor was required to make any repairs of any kind to the unit. Sixteen percent of treated cases versus 6% of control group cases involved repairs, and the difference was not statistically significant ($p = 0.18$).[107]

With respect to vacate lengths, meaning the time for moving out given to occupants who lost the right of possession, a direct comparison between treated and control groups would be meaningless, as explained in detail in Appendix II. To summarize Appendix II's discussion, the problem here is that vacate length was defined only for cases in which the occupant vacated. In the control group, about two-thirds of occupants did not remain in possession at the end of the litigation, while in the treated group, only about one-third did not do so. This distinction means that if we compare the treated occupants who vacated to the control occupants who vacated, we would be comparing one-third of the treated group to two-thirds of the control group. This disparity, one-third versus two-thirds, is not problematic in itself,[108] but it is a signal that something else is amiss. That something else is that the one-third of occupants in the treated group who did not retain possession probably had, as a group, background case characteristics different from the two-thirds of occupants in the control group who did not retain possession. One such difference might be that treated-group cases with the strongest facts in favor of occupants were not included in the one-third in which the occupants lost possession, because in those strongest cases, GBLS staff attorneys prevented their clients from having to vacate at all. This explanation assumes occupants wanted to stay, as we think reasonable in most cases given the data described above regarding whether occupants desired to stay in their units. We attempted to address this problem via statistical modeling, but we did not succeed.[109] We have no useful results with respect to this variable.

D. Possible Explanations of Our Results

The treatment effects measured in this District Court Study are large. To repeat, the two most important results in this District Court Study are as follows: 34% of treated-group occupants, versus 62% of control group occupants, lost possession of their units, and treated-group occupants saved on average 9.4 months of rent (using a measurement that is conservative but equates cash and legal obligations),

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[107] GBLS staff attorneys objected to this finding, stating that repairs were effectively required more often than was reflected in the District Court records. We found no evidence to support or contradict this assertion, but of course the GBLS attorneys may be correct.

[108] The comparison would still make sense if, for example, the one-third and two-thirds were randomly selected from the treated and control groups.

[109] We did not have a sufficient number of observations to fit the kind of models suggested in Junni L. Zhang & Donald B. Rubin, *Estimation of Causal Effects via Principal Stratification When Some Outcomes Are Truncated by "Death,"* 28 J. EDUC. & BEHAV. STAT. 353 (2003).

versus an average of 1.9 months of rent saved in the control group. The next question is why. In other words, what was it about the circumstances in which the District Court Study took place that allowed the offer of representation to have the extraordinary effect that it had? At present we can only hazard guesses. We are aware of only six randomized studies (including the present District Court Study and its Housing Court Study companion)[110] regarding the effect of offers of representation completed in any U.S. civil litigation system in the past half century, and so cannot yet conduct the kind of cross-study comparisons that might shed light on what combination of factors tends to produce sizeable offer-of-representation effects. In this section, we limit our observations to six aspects of the District Court Study setting that might have enabled the offer of representation to produce the large effects we observed. These possible explanations are neither mutually exclusive nor exhaustive.

1. Outreach, Intake, and Screening. — A first potential explanation is GBLS's proactive, specific, selective, and timely system for outreach, intake, and case selection. Because of this system, GBLS did not select a random sample of district court summary eviction occupants for the study; rather, two forms of screening took place. The first form of screening depended on the occupants themselves: to enter the study, an occupant had either to respond to GBLS's outreach letter offering legal assistance or to attend a district court hearing on her case (in order to come into contact with GBLS staff attorneys while the latter were present on court call days). The second form of screening was provider centered: GBLS considered whether it thought it could make a difference in the case outcome when deciding whether to send the case to us for randomization, and it did so on the basis of a face-to-face interaction with the occupant. In other words, GBLS had a stronger informational basis upon which to predict whether it could affect case outcomes than it would have had if, for example, it had limited its intake process to a telephone interaction.

In another setting, two of us had previously hypothesized that the absence of a large effect due to an offer of representation was at least partially attributable to a service provider's nonspecific and client-initiated intake system, by which we meant that the client had to find the service provider and initiate the conversation between them.[111] We further conveyed our uncertainty regarding whether a legal services provider that attempts to isolate cases whose outcome will be al-

[110] The six to which we refer are the present District Court Study, the Housing Court Study, the unemployment study reported in Greiner & Pattanayak, *supra* note 8, the Manhattan Housing Court Study reported in Seron et al., *supra* note 9, and the two studies in juvenile delinquency proceedings reported in STAPLETON & TEITELBAUM, *supra* note 10.

[111] *See* Greiner & Pattanayak, *supra* note 8, at 2188–93.

tered by an offer of representation can successfully do so if its case-selection decisions depend on telephone conversations (especially short, meaning twenty- to thirty-minute, telephone conversations) to screen cases.[112] The District Court Study constitutes a logical inversion. As discussed above, GBLS's outreach system was provider initiated, specific to each individual client, and timely in that an initial letter was sent to potential clients shortly after they received court summonses. Further, GBLS chose cases[113] for randomization based on in-person interactions (in the form of the instructional clinics GBLS ran), some lasting an hour or more, sometimes supplemented with information available from discovery responses. This system was costly. It required GBLS staff (i) to go to the district court courthouse (located a substantial distance from GBLS's offices) one afternoon per week to pull and examine files, then address and send outreach letters; (ii) to conduct instructional sessions in the community, instead of exclusively in its offices; and (iii) to juggle intake for potential new clients along with advocacy for existing ones on days in which cases were called for court. If one believes that the proactive, specific, selective, and timely system for outreach, intake, and case selection was an important factor in explaining the results we observe here, then a clear hypothesis emerges: legal services providers can screen cases, or at least some kinds of cases, to isolate those in which an intervention will likely produce an improved outcome, but they may not be able to do so on the cheap.

Note that the previous discussion does *not* mean that we recommend that legal aid providers implement an outreach and intake system that requires no effort on the part of the potential client. It may be that if a provider makes it too easy for a potential client to begin a relationship, a great many potential clients may initiate that process but then fail to follow through, meaning that they will fail to respond to offers of representation. If outreach, intake, and screening were all costless, this possibility would not matter, but as suggested immediately above, each of these activities costs time and resources. A cautionary tale here: in a separate and still-ongoing randomized study measuring the effect of an offer of representation, we funded advertisements on a couple of occasions by the relevant service provider in local newspapers that the target population of potential clients was likely to read. These advertisements invited potentially eligible persons to come to instructional clinics or to call the provider for an appointment. After two or three rounds of ads, the service provider involved decided

[112] *See id.* at 2147 n.102.

[113] To clarify: the fact that the provider chose cases for randomization does *not* raise the kind of selection effects discussed in *id.* at 2188–95. *Within* the set of cases the provider chose and sent to us, the randomization assured that selection effects could not arise.

not to pursue this method of generating cases. The provider felt that this method brought in too many potential clients who would complete the intake process, be randomized to an offer of a full attorney-client relationship, but then fail to respond to efforts to proceed with representation. In other words, making the process of contacting the service provider too easy resulted in too many time-wasting clients.

The relevance of this first explanation for policy makers depends on what sort of legal assistance program one contemplates implementing (or advocating for) on a more general basis. In the criminal arena, for example, the model of "legal aid" for cases carrying a serious possibility of eventual incarceration is a default to appointed counsel for all indigent persons charged.[114] Suppose one contemplates implementing an imperative to appoint counsel for all persons facing eviction from their housing units who lack resources to hire lawyers (a civil *Gideon*[115] right in cases of summary eviction), and that one important argument for such an imperative is to produce outcomes (possession-related, financial, and so forth) that are closer to the legally correct outcomes. How much information does the District Court Study provide to support the argument that such an imperative should run to *all* summary eviction occupants? The answer to this question depends in part on how much of a role GBLS's outreach, intake, and screening process played in producing the startlingly large treatment effects observed here. Either of the two selection processes identified above, the client-centered process or the GBLS-centered process, might have been sufficient on its own to produce a client base for this study that was not representative of all low- or moderate-income occupants in the district court's summary eviction calendar. If so, it is hard to know how much the results observed here would generalize to a program that offered representation to all summary eviction occupants, per the criminal law model.

But is implementation of the criminal law model, which would amount to a nearly universal system of traditional attorney-client relationships in summary eviction cases, a serious possibility? We do not want to argue that the answer is a definitive no, but we have some doubts based on our experience with the District Court Study. Putting aside both the problem of potentially formidable cost concerns in an era of shrinking budgets and the issue of why one would choose summary eviction over various competitor-adversarial adjudicatory set-

[114] *See* Gideon v. Wainwright, 372 U.S. 335, 344–45 (1963).

[115] On a proposed civil *Gideon* right, see generally Jeanne Charn, *Legal Services for All: Is the Profession Ready?*, 42 LOY. L.A. L. REV. 1021 (2009); and Thomas D. Rowe, Jr., *If We Don't Get Civil* Gideon*: Trying to Make the Best of the Civil-Justice Market*, 37 FORDHAM URB. L.J. 347 (2010).

tings involving fundamental human needs,[116] how exactly would such a system work? For example, would such a system assure that a substantial percentage of indigent summary eviction occupants would actually use the lawyers the state would provide?[117] In the criminal context, establishment of an attorney-client relationship with an indigent occupant is made easier by several facts, including: (i) the arrest and the temporary incarceration that precedes a bond hearing, which means that in at least one point in time, a lawyer can usually find her client to engage in a face-to-face interaction and begin an ongoing relationship; (ii) the threat of separate and independent criminal penalties for failing to appear at a court hearing;[118] and (iii) the attention-grabbing possibility of loss of physical liberty. In contrast, in the summary eviction context, nonuse of attorneys by persons offered representation unquestionably occurs on a more frequent basis. Although such nonuse was low in the present District Court Study,[119] this fact may have been due to the client-centered and provider-centered selection processes used, and nonuse might be much higher in a program in which all summary eviction occupants are offered counsel.[120]

Alternatively, one might (at least in the short term) contemplate a legal assistance program possibly more attractive to policy makers — that is, one that mixes limited assistance for most with full representa-

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[116] *See* AM. BAR ASS'N, *supra* note 16, at 1, 13.

[117] This discussion assumes, of course, that one cares about whether an indigent person actually uses counsel, as opposed to whether the state makes counsel available for use. That assumption is debatable. If one cares about producing accurate results under the law, and about conducting a proceeding that is fair, the assumption may make sense. It would appear that such concerns are active in the criminal context in that a criminal defendant must overcome procedural obstacles if he wants to proceed pro se. *See Right to Counsel*, 37 GEO. L.J. ANN. REV. CRIM. PROC. 477, 489 (2008). An alternative conceptualization of due process and procedural fairness might focus less on producing accurate results and more on promoting personal responsibility, in which case one might devote resources to making counsel (or legal assistance more generally) available to a summary eviction occupant, then leave it to the occupant to follow up if she so chooses.

[118] By this factor, we mean that in the criminal context, if an occupant fails to appear at a court hearing, he will typically face a separate process of contempt or criminal charge for failing to appear in addition to the underlying offense for which he still must answer. In the summary eviction context, as in most civil proceedings, failing to appear results in default that, unless cured, may cause the occupant to lose the case but does not result in independent legal consequences.

[119] The aggressive screen that GBLS used held noncompliance by treated-group occupants to a minimum, around 3%. In the companion Housing Court Study, however, even though the provider there required would-be clients to attend a meeting in its office (thus screening out those who lacked the organizational and planning skills, resources, and so forth to reach its offices), noncompliance in the treated group was higher, around 18%. Greiner et al., *supra* note 23 (manuscript at 18).

[120] In Professor W. Vaughan Stapleton and Professor Lee Teitelbaum's "Zenith" study, 17.6% of the youths randomly offered representation by the study attorneys in delinquency proceedings ended up unrepresented; the figure for "Gotham" was 17.3%. STAPLETON & TEITELBAUM, *supra* note 10, at 52. Thus, even with the attention-grabbing possibility of incarceration, close to one in five juveniles did not follow up on an offer of representation.

tion for some.[121] If so, the results we report here constitute a credible assessment of a critical aspect of such a program — specifically, offers of full representation to occupants specifically chosen for such treatment.

2. Confrontational Litigation Style. — As discussed at the end of Part II, the GBLS attorneys in the present District Court Study made frequent use of jury trial demands, filed frequent motions to compel responses to discovery, aggressively sought preliminary relief, and fully prepared cases for trial (thus making their jury trial demand threats credible). They did seek settlement, and in fact the overwhelming majority of cases in the treated group settled. But the GBLS attorneys were aggressive in using the legal and tactical tools available to alter the bargaining landscapes of cases before engaging in settlement talks. It appeared to us that their style was confrontational and assertive, though not a form of hardball. By way of contrast, and as discussed elsewhere,[122] the attorneys in the Housing Court Study did not use such confrontational or assertive tactics.

In 1972, Professor W. Vaughan Stapleton and Professor Lee Teitelbaum published a book reporting the results of two randomized control trials in which offers of representation were randomly provided to a treated group of juveniles in delinquency proceedings in two different courts.[123] Randomly selected control groups of juveniles received no such offers. In one of the courts, the offer of representation changed outcomes significantly; in the other, it did not.[124] After an exhaustive investigation into the factual setting for the two studies, Stapleton and Teitelbaum concluded that the explanation for the contrasting results lay in the different litigating styles that the different sets of lawyers were able to deploy. In the court in which the offer of representation made a difference, the attorneys had adopted a litigation style that included frequent assertion of rights against self-incrimination, rights to confrontation, and rights to attendance of favorable witnesses. In the court in which the offer had no effect, the attorneys were unwilling or unable to assert these rights.[125] One explanation for the results of the present District Court Study (particularly in contrast with the Housing Court Study results) is that the confrontational litigation style successfully deployed by Stapleton and

[121] *See generally* Russell Engler, *Reflections on a Civil Right to Counsel and Drawing Lines: When Does Access to Justice Mean Full Representation, and When Might Less Assistance Suffice?*, 9 SEATTLE J. SOC. JUST. 97 (2010).

[122] *See* Greiner et al., *supra* note 23 (manuscript at 37–38). We caution that there are several possible explanations for the contrasting results of the two studies. *See id.*

[123] STAPLETON & TEITELBAUM, *supra* note 10, at 50.

[124] *Id.* at 66–67.

[125] *Id.* at 156–59.

Teitelbaum's first group was used by the district court attorneys and was effective.

3. Complication in the Applicable Law. — A third potential explanation concerns the level of complication in the applicable law. For the set of cases we saw in this District Court Study, the law applicable in the run-of-the-mill case appeared to our inexpert eyes to be complicated,[126] or at least more complicated than that, for example, governing the run-of-the-mill case in the study concerning unemployment benefits that two of us had conducted.[127] This difference does not mean that the field of housing law as a whole is more complicated than the field of unemployment law; we speak here only in terms of the set of cases we saw in our studies. The cases in the District Court Study implicated multiple sources of law, including state statutes, state common law, state regulations, federal statutes, and federal regulations;[128] they involved multiple provisions or doctrines within each source of law; and they required evidence from third parties such as housing inspectors, contractors, public utilities, and financial institutions. To the best of our knowledge, none of these three features characterized the set of cases in our unemployment study, and some of these features are missing in other areas of law.[129]

4. The Adjudicatory System. — A fourth potential explanation concerns the District Court Study's adjudicatory system. For the caseload to remain manageable, the overwhelming majority of the disputes in cases on call on any particular day had to be resolved without the judge's personal intervention, meaning they had to be decided via default or settlement. On a typical call day, the judge had to hear thirty to sixty summary eviction matters in the morning in order to leave time for a full criminal docket, portions of which might be subject to constitutionally or statutorily mandated timelines.[130] Demanding that the parties engage in hallway negotiations before the judge would hear a description of the dispute (much less the parties' arguments), referring as many cases as possible to mediation, cajoling the parties into a

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[126] There are definitional problems here: what is "complicated," and how does one measure it? For a discussion of these issues, see Rebecca L. Sandefur, Elements of Expertise: Lawyers' Impact on Civil Trial and Hearing Outcomes (Oct. 15, 2012) (unpublished manuscript) (on file with the Harvard Law School Library).

[127] *Cf.* Greiner & Pattanayak, *supra* note 8, at 2134–39.

[128] Some of the best results GBLS staff attorneys achieved concerned tenants in federally funded Section 8 or public housing programs, where either the landlord or the relevant housing authority violated regulations governing these programs, such as by miscalculating the amount of money the tenant was supposed to contribute toward the rent.

[129] For example, unemployment benefits appeals (at least those we saw in our study) are often resolvable without evidence from third parties. *See* Greiner & Pattanayak, *supra* note 8, at 2134–37.

[130] *See, e.g.*, Gerstein v. Pugh, 420 U.S. 103, 114 (1975).

settlement, and conducting infrequent and short trials all might serve many purposes, but we speculate that preserving the judge's time was an important purpose.

A cost, however, was that no matter how good the judge's intentions, and no matter how fairly he dealt with the parties that did end up before him, this modus operandi did not allow implementation of many of a long list of "best practices" developed over the past decade that might allow a proactive judge to obtain the information he needs to reach a legally correct judgment on the facts and law.[131] An example of such practices might be a prehearing explanation by the judge of the meaning and importance of procedural posture, the nature of the issue to be decided, the procedure to be followed at the hearing, the facts (in a general sense) that might be relevant, and the applicable substantive law. A possible implication is that because the district court was unable to follow some of these best practices, there was plenty of room for a skilled advocate with knowledge of the law and facts to make a difference in hallway settlement negotiations, in mediation sessions, in (perhaps resisting) the judge's cajoling-to-settlement process, and (far less often) in evidentiary hearings.

The complexity of the law governing summary eviction cases may resist judicial efforts to nudge self-represented litigants toward achieving full self-sufficiency by internalizing all possibly relevant law. Any litigator knows that part of her job in a case involving complicated law is to educate the judge regarding what the law is. By way of example, in at least two of the cases in the District Court Study dataset, GBLS staff attorneys achieved large, favorable monetary settlements after discovering that the relevant housing authority administering applicable Section 8 vouchers had miscalculated the amount of rent the tenant-clients should have been contributing from their own funds. This amount was based on the tenants' incomes. The regulations governing the calculation of rent contributions are not straightforward, and not as widely known as the common law warranties of habitability and fitness. Speaking more generally, the Massachusetts Law Reform Institute's answer form[132] might be viewed as including an acceptable minimum of subjects and information that should be discussed and elicited in a summary eviction proceeding. But if so, it is not easy for us to envision a setting in which the judge in the District Court Study would have time to review all the information on this

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[131] *See generally* Richard Zorza, *A New Day for Judges and the Self-Represented: The Implications of* Turner v. Rogers, JUDGES' J., Fall 2011, at 16. Although the principles outlined in Richard Zorza's article appear constructive and useful, we suggest that the pros and cons of many of them can and should be quantitatively evaluated, a suggestion with which we suspect Zorza would agree. We return to these issues in Part IV.

[132] *See Representing Yourself in an Eviction Case: The Answer*, *supra* note 66.

form with each self-represented summary eviction occupant who appears before him, unless the nature of the district court were fundamentally changed to lighten or spread its caseload by orders of magnitude.

We pause to note here that three of the four possible explanations for our results articulated thus far — that is, the provider outreach and intake system, the complication in the area of law, and the adjudicatory setting — are the flip sides of the three possible explanations that two of us offered in earlier work in a different setting to explain why it might be that an offer of representation probably did *not* have a large effect on case outcomes.[133] In our previous study of unemployment benefits, the service provider did no individualized outreach, and the potential client had to make the first contact; here, GBLS's outreach and intake program was proactive, individualized, timely, and selective. In the cases handled by the service provider from our previous study, the most important issue was typically why a former employee quit or was discharged, and the law might be characterized as somewhat simpler than that in the District Court Study cases. And in our previous study, each case was scheduled to occupy an hour of decisionmaker time at a minimum. By contrast, consider a back-of-the-envelope calculation for the District Court Study context: in fiscal year 2010, the district court handled approximately 1280 summary eviction cases.[134] If each took an hour of the judge's time, the judge would need 1280 (1280 × 1) hours per year to handle a set of cases that were scheduled for roughly half of a Thursday each week.

5. The Need for Prehearing Factual Development. — A fifth possible explanation concerns the need for prehearing factual investigation and documentation in order to develop the evidence necessary to support certain defenses. In the unemployment study, it appeared that, at least with respect to the set of cases in the service provider's practice, the primary issue was the reason for (and circumstances surrounding) an employee's quit or discharge.[135] These circumstances were typically known to the employee and the employer and susceptible to resolution based on testimony from the parties. To the extent that documents were needed, a claims adjuster (an employee of the adjudicator and an initial decisionmaker) had already attempted to do initial document gathering, often contacting the employer and requesting the employee's file. For these reasons, there was less in the way of prehearing factual development that was (i) necessary to allow the adjudicators in the un-

[133] Greiner & Pattanayak, *supra* note 8, at 2173–74.
[134] This estimate was provided to us by district court personnel.
[135] Greiner & Pattanayak, *supra* note 8, at 2133–34.

employment study to render their decisions, and (ii) not already accomplished by an employee of the adjudicatory body.

By contrast, in the present District Court Study, it appeared that at least some treated-group cases benefited from substantial prehearing factual development that GBLS staff attorneys initiated. As noted above, in our conversations with these attorneys and in our review of case files, it became apparent that the attorneys were engaging in substantial investigation of the circumstances of the units involved and of their clients' lives. In doing so, they uncovered facts that would not have been easy to unearth in the context of, for example, a short client interview pursuant to a lawyer-for-the-day program, or pursuant to a discussion of form pleadings in an instructional clinic. We have already discussed GBLS attorneys' investigation into Section 8 income calculations and into conditions in the units, so we limit our discussion here to a listing of a few other ways in which the attorneys engaged in factual investigation outside of court: they requested inspections of housing units by the inspectional division of the city's health department; they investigated and successfully asserted so-called "cross-metering" defenses;[136] and they discovered that landlords had been charging tenants for utilities without a written lease provision to this effect. All of these defenses depended on some prehearing investigation into facts, on some development of documentary evidence, or on some inducement of third-party activity. Thus, the nature of the issues that arise in at least some summary eviction actions may provide more of an opportunity for a well-organized and diligent legal representative to lay the foundation for a case.

If we are right that the need for prehearing factual development plays an important role in explaining the results of this District Court Study, then there is again reason for some pessimism regarding the effectiveness of adjudicator-based best practices and interventions, such as active questioning from a judge preceded by explanations of the issues to be decided and the nature of the evidence that might be relevant. If prehearing investigation and factual development are critical, then obviously no amount of adjudicator explanation at a hearing can fill in the gap left by the absence of competent legal assistance.[137]

6. Model of Service Delivery. — A sixth possible explanation for our results turns on the model of service delivery involved in this

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[136] Cross-metering occurs when a landlord charges to the tenant amounts for utilities that do not correspond to a tenant's unit, such as electricity charges for the lighting of another unit or of common areas.

[137] *Cf. In re* Gault, 387 U.S. 1, 33, 34 n.54 (1967) (recognizing, in a decision that also required counsel to be provided in juvenile delinquency proceedings leading to the juvenile's confinement, that to be constitutionally adequate, notice must be given "sufficiently in advance of the hearing to permit preparation," *id.* at 33).

study. GBLS is a legal aid organization specializing in poverty law. The two staff attorneys it assigned to the District Court Study were housing specialists with fifteen and thirty years' experience litigating housing cases in a variety of contexts.[138] Importantly, these attorneys spent half of their time representing District Court Study clients, and the other half engaging in other housing-related individual and class litigation. Thus, the attorneys were not spending their time outside of the district court on limited representation through a lawyer-for-the-day or similar program.[139]

The relative effectiveness and efficiency of different service delivery models in legal assistance, a subject that appears to have received less attention in the civil context than in the criminal context, should be of intense interest to researchers. A hardly controversial hypothesis, and one that would help explain the results we observe here, is that specialists with long experience in an area of law, even those who lack intimate knowledge of a particular court's informal norms and procedures, might produce better case outcomes for potential clients than nonspecialists[140] or those with less experience. But more controversial questions might follow: If experienced specialists produce superior outcomes, is it efficient or useful to attempt to meet the need for civil legal assistance with, for example, law firm attorneys working pro bono in an area in which they do not specialize?[141] Or would the clien-

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[138] Attorneys' levels of experience have been shown to affect case outcomes. *See, e.g.*, David S. Abrams & Albert H. Yoon, *The Luck of the Draw: Using Random Case Assignment to Investigate Attorney Ability*, 74 U. CHI. L. REV. 1145, 1145 (2007) (using a public defender office's practice of randomized assignment of attorneys to demonstrate that "[a]ttorneys with longer tenure in the office achieve better outcomes for the client").

[139] In contrast, the two staff attorneys in the Housing Court Study in Greiner et al., *supra* note 23 (manuscript at 12), spent the other half of their time staffing a lawyer-for-the-day program.

[140] *See* James M. Anderson & Paul Heaton, *How Much Difference Does the Lawyer Make? The Effect of Defense Counsel on Murder Case Outcomes* 3 (RAND Infrastructure, Safety, and Env't Working Paper Series, Paper No. WR-870-NIJ, 2011), *available at* http://www.rand.org/pubs/working_papers/WR870.html (using randomized assignment in Philadelphia to demonstrate that public defenders produce outcomes more favorable to criminal defendants charged with murder than do private attorneys); Radha Iyengar, *An Analysis of the Performance of Federal Indigent Defense Counsel* 3 (Nat'l Bureau of Econ. Research, Working Paper No. 13187, 2007), *available at* http://www.nber.org/papers/w13187 (using same with respect to federal public defenders and private attorneys compensated on an hourly basis under the Criminal Justice Act).

[141] There are obviously a host of assumptions inherent in this question, not the least of which is that law firms do not specialize in the area of law in which they do pro bono work. There are some pro bono practices in law firms in which those involved invest the time and resources needed to build up institutional expertise in a particular practice area. *See generally* Scott L. Cummings & Deborah L. Rhode, *Managing Pro Bono: Doing Well by Doing Better*, 78 FORDHAM L. REV. 2357 (2010). But there are also some law firms in which some pro bono work functions essentially as a way to give less-seasoned associates a chance to gain non-office-based litigation experience in a setting in which mistakes do not affect the firm's bottom line. The results of Greiner & Pattanayak, *supra* note 8, as well as those of the present District Court Study, suggest that prac-

tele in need be better served if these law firm attorneys spent their time doing what they do best — that is, earning money — and then outsourcing their pro bono obligations to experienced poverty law specialists (by donating the additional funds earned to legal aid organizations)?[142] Answering these two questions requires addressing issues beyond the desire to produce results for a particular set of clients in need, including the role that various types of legal assistance (such as pro bono) play in mobilizing lawyers, law firms, and other persons or institutions who wield political power to support the effort to fund and lobby for civil legal assistance. All aspects of these questions deserve great attention.

One other point: as suggested in Part IV, no legal assistance entity, including GBLS, had operated any sort of aid program in the district court's summary eviction calendar for over a decade. Thus, at least at the beginning of the seventeen-plus-month study period, the two staff attorneys were not repeat players in the district court and did not have long-term familiarity with its informal procedures and norms. Our results, therefore, stand in mild (although certainly not irreconcilable) tension with theories of attorney effectiveness that emphasize in-depth knowledge of a court or an institution and long-term personal relationships with the players involved in a sociojudicial setting, to the exclusion of traditional litigator skills such as knowledge of law, investigation of facts, and hard work.[143]

7. *Other Explanations.* — There are other possible explanations for our results. Perhaps the district court judge's legal aid background made him unusually receptive to the GBLS staff attorney's arguments. Perhaps evictors' attorneys, unaccustomed to facing occupants' attorneys in summary eviction proceedings, had grown sloppy over the ten-plus years since a legal assistance organization had operated in the district court's summary eviction calendar. In that case, perhaps the effects we observed might have decreased in magnitude had the study extended over time as the evictors' bar tightened its collective ship. An attempt to list all possible explanations would be futile. In Appen-

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tices in the latter category may warrant further scrutiny, on the grounds that unintended side effects and inefficiencies may be present.

[142] *Cf.* Iyengar, *supra* note 140, at 26 (noting that given strong statistical evidence that public defenders produce better outcomes for criminal defendants than do private attorneys compensated on an hourly basis under the Criminal Justice Act, "it is unclear why the federal government does not simply hire more public defenders").

[143] *See, e.g.*, Peter F. Nardulli, *"Insider" Justice: Defense Attorneys and the Handling of Felony Cases*, 77 J. CRIM. L. & CRIMINOLOGY 379, 382 (1986) (describing, in terms unflattering to lawyers, an extreme version of such a theory in which a criminal defense attorney's personal ties to the local court community are "stock-in-trade since . . . they are sorely lacking in professional skills and knowledge").

dix I, we describe the fact pattern underlying the District Court Study in detail to allow cross-study comparisons.

E. Limits of the Analysis

There are several possible limits to the present District Court Study. To begin, any of the characteristics of the District Court Study that we label "explanations" for the results could also be recast as limits of the study, in the sense that the results might not generalize to situations in which one or more of these explanatory factors are absent. We discuss two additional limits, both of which are also present in the only other randomized studies of the effects of legal representation in housing.[144] As we explain, our study advances the field in this area by pushing some of these limits, but the limits still exist.

1. Pieces of Litigation as Study Units. — A first limit to the District Court Study is that we focused on specific pieces of litigation, not the entirety of an evictor/occupant relationship. In an unknown number of cases, probably a small number, the particular piece of litigation in our study did not resolve the parties' relationship, and further litigation probably ensued.[145]

By way of example, in at least one treated-group case of which we are aware, the following chain of events occurred: The landlord served a facially defective notice to quit. After a GBLS staff attorney moved to dismiss, the landlord agreed voluntarily to dismiss the case. For our purposes, this agreement meant that the occupant retained possession at the end of the piece of litigation that entered our study, so we coded this case accordingly, and that was the end of the matter as far as this case's contribution to the District Court Study. We happen to know in this case that the landlord did as one would expect, meaning that the landlord served a corrected notice to quit on the occupant, then filed another lawsuit. Thus, litigation between the parties continued, but the subsequent litigation was not part of our dataset.

Some might argue that this chain of events made our coding of this transaction misleadingly pro-occupant. We do not agree. The process of filing a first lawsuit, recognizing that the defective notice to quit made the result virtually preordained, having the first lawsuit dismissed, serving a second notice to quit, and initiating a second lawsuit took several months. During that time, the occupant remained in possession of the unit and, on the theory that bad conditions in the unit were sufficient to constitute a breach of the implied warranty of habitability, withheld rent. The longer time period of rent withholding in-

[144] *See* Seron et al., *supra* note 9, at 430–31. These issues are also present in our previous work. *See* Greiner & Pattanayak, *supra* note 8, at 2184–95.

[145] *See* Seron et al., *supra* note 9, at 430 (articulating the same limit).

creased the occupant's bargaining leverage. The allegations of bad conditions in the occupant's answer in the first lawsuit (an official document filed with the court, no less) negated any contention that the landlord might have raised regarding lack of notice of the bad conditions or insufficient time to remedy them. And in the time spent defeating the first case, the tenant could have been looking for a new unit, should she have chosen to do so. In short, the defeat of the first case placed the parties in a position different from what they would have been in had the first case gone forward on the merits.[146]

2. *Only Some Socioeconomic Consequences.* — Previous randomized studies on the effects of offers of representation have measured only legal outcomes, such as whether a court entered a *judgment* of possession,[147] or whether an administrative law judge *ruled* in favor of an applicant for unemployment benefits.[148]

Legal outcomes are critically important. They represent the outputs of the adjudicatory process, which is supposed to be a fair one whose results depend on the facts and the law. The coercive power of the state will be available to enforce legal rulings. However, legal consequences are not the only ones that matter. In the present study, we coded two outcomes — actual possession and EvictorMonthsRentLost — that were not purely legal, and they were the outcomes we considered most important because they represented two important on-the-ground facts in occupants' lives. The measurement of these two variables constitutes a substantial step forward in the study of the consequences of offers of representation.

Why do we highlight this advantage of our study in a discussion of our study's limits? Because there are other socioeconomic outcomes we could not measure, in part because the Task Force that assembled this District Court Study (and its Housing Court Study counterpart) dedicated none of the $385,000 it raised to the evaluation. To repeat a clarification we made earlier, our EvictorMonthsRentLost measurement can be thought of as measuring the financial consequences of the litigation under the assumption that a judgment ordering an occupant to pay $1000 is the same as an actual cash payment from the occupant of $1000, even though it is likely that some (perhaps most) occupants were either judgment-proof or so transient that they posed debt-collection challenges. With a modest amount of funding, we might have been able to keep track of occupants (and evictors) over time and

[146] From examining case records, we believe that in the District Court Study dataset, subsequent litigation between the same two parties over the same unit likely was rare.

[147] Seron et al., *supra* note 9, at 426.

[148] Greiner & Pattanayak, *supra* note 8, at 2153.

to measure, for example, how much occupants actually paid[149] (as opposed to how much they were legally obligated to pay), or whether they were still in possession of the relevant housing unit X months after entering the study, for X = 6, 12, 18, and so forth. We might have been able to measure whether an offer of GBLS representation in summary eviction proceedings had any short- or medium-term effects on employment, health, income, spending habits, or other indicators.[150]

This is one way in which research on the effects of offers and actual use of representation must become more sophisticated. The present District Court Study, and the Housing Court Study we simultaneously conducted, are the first to measure socioeconomic as opposed to purely legal outcomes. Yet a limited number of telephone contacts aside, the bulk of the outcome information in these studies concerned legal outcomes. And legal outcomes tell only part of an overall story.

F. Overgeneralization and Undergeneralization

It would be a mistake to overgeneralize the results of this District Court Study. For example, one might be tempted to view the District Court Study results as supplying conclusive evidence confirming the need for a civil *Gideon* right in a variety of adjudicatory settings in which basic human needs are at stake, or for a civil *Gideon* right in summary eviction proceedings, or for a civil *Gideon* right for a certain select group of summary eviction occupants (although identifying and describing this select group of occupants may not be simple). Although we are sympathetic to a civil *Gideon* right in certain settings as a matter of aspiration, how much support one should find for a civil right to counsel in the District Court Study depends on how much weight one assigns to each of the possible explanations for our results that we articulated above, as well as to others on which we did not focus. For example, if one assigns weight to our sixth possible explanation, the service delivery model, then the success of a civil *Gideon* right in protecting litigants who would otherwise self-represent depends on whether the state's eventual delivery system consists of professional staff attorney specialists (analogous to public defenders[151]),

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[149] As Greiner et al., *supra* note 23 (manuscript at 24–26), make clear, simply calling occupants to inquire about actual financial consequences would not have been effective. Given sufficient funding and lead time, we would have explored other means of collecting relevant information, such as face-to-face meetings that would have allowed more probing questioning as well as waiver forms allowing us to obtain information from (particularly institutional) landlords.

[150] For an outline of a range of outcomes that might be studied, as well as a thorough but expensive method of outcome collection, see generally NPC RESEARCH, CIVIL RIGHT TO COUNSEL SOCIAL SCIENCE STUDY DESIGN REPORT: FINAL REPORT (2009), *available at* http://www.npcresearch.com/Files/Civil_Right_to_Counsel_Design_Report_0409.pdf.

[151] At least upon initial review, the most credible of the several studies comparing the effectiveness of public defenders to, for example, Criminal Justice Act attorneys are two randomized stud-

relies in large part on (perhaps mandatory) pro bono representation by nonspecialists or specialists in some other field, or issues vouchers that would allow the litigant to access the private market.[152] If one assigns weight to the fourth possible explanation, the adjudicatory setting in which the study took place, then an alternative answer may be transforming the district court's handling of summary eviction cases, although considerable resources might be needed to make such a transformation successful.

It would also be a mistake to undergeneralize the results of this District Court Study. If one assigns weight to the first of our hypothesized explanations — which is that the outreach, intake, and screening system played a role in allowing GBLS staff attorneys to produce strong results for their clients — then legal assistance entities in a variety of settings may be able to increase their effectiveness by investing greater resources in outreach, intake, and screening. This possibility suggests that research, particularly rigorous and quantitative research, should focus on this initial phase of provider operations.

The discussion above makes clear that we need to expand the research agenda.

IV. FUTURE RESEARCH: EXPANDING THE RESEARCH AGENDA

Calls to increase the evidential basis for access-to-justice-promoting measures, whether based in courts or in delivery of legal services or in something else, have been increasing for some time. With the three randomized studies we have completed to date,[153] the three other randomized studies we currently have underway,[154] and the three that were completed before we began our research program,[155] it is now clear that the community of persons interested in access to justice can actually answer this call. Doing so will require innovative thinking, will require the legal services community, the judiciary, and the agen-

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ies: Anderson & Heaton, *supra* note 140; and Iyengar, *supra* note 140. Nonrandomized studies in this area may be of suspect credibility for many of the reasons discussed in Greiner & Pattanayak, *supra* note 8, at 2189.

152 *Cf.* Seron et al., *supra* note 9, at 430 ("The Pro Bono Project also involved volunteer lawyers, mostly from corporate law firms, not lawyers with experience in Housing Court. In interviews with [a professional provider's] staff attorneys who managed the Pro Bono Project, they reported that it took a notable amount of their time to work with volunteer lawyers to prepare cases.").

153 We refer here to Greiner & Pattanayak, *supra* note 8; the Housing Court Study reported in Greiner et al., *supra* note 23; and the present District Court Study.

154 We have underway randomized studies of legal services programs in disability and divorce proceedings, as well as a randomized evaluation of a mediation program justified in part on access-to-justice grounds.

155 We refer here to the two studies in STAPLETON & TEITELBAUM, *supra* note 10, as well as in Seron et al., *supra* note 9.

cies that administer critical assistance programs to be willing to take risks, and will require academia to commit to the production and recognition of practical research. We hope to continue contributing to these collective efforts in the future. An overall research program will also require ideas about research designs and methods that will produce useful results. We discuss such ideas in this Part, continuing a project we began in prior work.[156]

A. A Broader Range of Outcomes

This is a simple point, one to which we have referred above: those interested in understanding the effect of access-to-justice-enhancing interventions, whether court-based or legal services–based, must expand the range of outcomes researchers study. We provide four examples of how such an expansion might occur.

First, as suggested above, we should recognize that clients seek legal assistance not just to alter adjudicatory outputs but also to improve socioeconomic outcomes. For instance, a litigant in divorce proceedings may desire a court order that spousal support essential for needed medicines continue, but a more important question may be whether the litigant is actually able to obtain those medicines, or able to access the health care system more generally. One of the randomized studies we currently have underway will attempt to measure the effect of legal interventions on the shape of divorce decrees. With proper design and funding, we can also measure whether legal interventions achieve client goals in terms of access to the health care system and other public systems. In particular, we can learn from researchers in the medical community by making greater use of administrative and other records systems;[157] a recent randomized study on the effect of offers of public health insurance demonstrated the power of measuring outcomes via examination of administrative records.[158]

Second, we should recognize that legal problems may be only a portion of, or perhaps merely symptoms of, the range of challenges

[156] We refer here to Greiner & Pattanayak, *supra* note 8.

[157] *See generally* Jane Yakowitz, *Tragedy of the Data Commons*, 25 HARV. J.L. & TECH. 1 (2011).

[158] Amy Finkelstein et al., *The Oregon Health Insurance Experiment: Evidence from the First Year* (Nat'l Bureau of Econ. Research, Working Paper No. 17190, 2011), *available at* http://www.nber.org/papers/w17190.

By way of example, we are actively pursuing a randomized study of the effect of offers of financial counseling and of legal representation for defendants in consumer debt collection cases. Should we be successful in fielding this study, we intend to assess the effect of these interventions on changes in study subjects' credit scores (such as those compiled by Experian, Equifax, TransUnion, and/or Innovis). Credit scores are an important indicator of access to the credit markets and, more broadly, economic health.

those in need face, and that the effect of legal interventions on these challenges is measurable.[159] For instance, summary eviction proceedings based on nonpayment of rent or failure to maintain proper conditions in the unit may be due to mental or physical health challenges a tenant faces. We have demonstrated in this Article that we can measure the effect of different levels of legal assistance by whether the tenant retains possession of the unit, as well as the financial consequences of the legal proceeding. Yet we can also measure how often different levels of legal assistance result in proper referrals to agencies and programs designed to assist with such challenges. Better yet, we can measure whether different levels of legal assistance result in the resolution of these challenges. Both measurements would require nimble field operations that include identification of and cooperation with agencies that provide certain services and that track case outcomes, but both seem feasible. Meanwhile, other and broader sources of information on socioeconomic well-being exist — sources of information that, to our knowledge, have not been exploited in access-to-justice studies. For example, for any intervention designed in part to improve financial health and stability, credit reports and scores would seem to provide a source of outcome information in the form of a set of measurements that credit card companies, landlords, and employers frequently use to assess whether to grant a person access to a socioeconomic good or opportunity. We are currently in the process of constructing a study that relies on credit scores and reports (pursuant to appropriate consent) in constructing outcome variables.

Third, we should recognize the underlying purposes of some of the social programs that form the setting for legal interventions. For instance, unemployment benefits are designed in part to allow the worker to spend her time searching for a new, permanent job instead of engaging in temporary work to make ends meet.[160] We have demonstrated previously that we can measure whether different levels of legal assistance increase the probability that a claimant will obtain benefits.[161] We can also measure, using state records (pursuant to appropriate waivers by study subjects, and with the cooperation of state workforce agencies), whether different levels of legal assistance increase the probability that the claimant will find new employment within a specified period of time.[162]

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[159] Some portions of the medical community appear to have come to the mirror-image conclusion: "Not every illness has a biological remedy." NAT'L CENTER FOR MED.-LEGAL PARTNERSHIP, http://www.medical-legalpartnership.org (last visited Dec. 1, 2012).

[160] *See* Cal. Dep't of Human Res. Dev. v. Java, 402 U.S. 121, 131–32 (1971).

[161] *See* Greiner & Pattanayak, *supra* note 8, at 2121–32.

[162] *Id.* at 2122.

Fourth, we should recognize the importance of litigant perceptions of fairness, both of the process and of the outcomes of adjudicatory systems. Our research thus far has demonstrated that we can measure the effect of different levels of legal intervention on adjudicatory outputs and on some socioeconomic outcomes — that is, objective indicators of effectiveness. We can also use interviews and surveys to measure whether different levels of legal assistance alter litigant perceptions of procedural fairness,[163] litigant satisfaction with the substantive outcome, and litigant respect for (or contempt of) the law.[164]

B. A Broader Range of Interventions: Using Represented Cases as a Yardstick

In *Turner v. Rogers*,[165] the Supreme Court continued a line of reasoning finding that the due process imperative (or lack thereof) to provide counsel in a civil adjudication depends in part on the trappings and characteristics of the adjudication, and that these trappings are often within the control of either the adjudicator or the designer of the adjudicatory system.[166] Seizing upon this aspect of *Turner*, networker and judicial educator Richard Zorza argues that the case encourages, and may constitutionally compel, the use of at least some of a list of

[163] For examples of the use of interviews and surveys to measure perceptions of litigant fairness, see E. ALLAN LIND & TOM R. TYLER, THE SOCIAL PSYCHOLOGY OF PROCEDURAL JUSTICE (1988); and TOM R. TYLER, WHY PEOPLE OBEY THE LAW (2006).

[164] One of us is implementing surveys in a randomized control trial to test the effect of a court-based intervention designed in part to improve litigant satisfaction with the adjudicatory process in federal court. The surveys are administered to inmates as part of a randomized evaluation of the Inmate Early Mediation Program in the United States District Court for the District of Nevada. Details are available from the first author of this paper.

[165] 131 S. Ct. 2507 (2011).

[166] *See* Vitek v. Jones, 445 U.S. 480, 497–500 (1980) (Powell, J., concurring in part) (relying on the nature of civil commitment proceedings to conclude that, although the danger of commitment required that the to-be-committed person receive some form of assistance (at state expense if necessary), due process did not require that she receive a lawyer); Gagnon v. Scarpelli, 411 U.S. 778, 786–91 (1973) (finding a right to counsel in parole revocation proceedings in only very few cases, with most individuals having no right to counsel in part because of "the informal nature of the proceedings and the absence of technical rules of procedure or evidence," *id.* at 786–87). *But see* Lassiter v. Dep't of Soc. Servs., 452 U.S. 18, 31–32 (1981) (eschewing any examination into the characteristics of a proceeding to terminate parental rights in favor of a focus on whether counsel could have changed the outcome of the particular case at issue given its underlying facts).

The empirical basis for the *Turner* Court's holding that appropriate court systems can obviate a need for counsel is open to question, particularly given how casually the Court treated the question of whether the procedures the South Carolina trial court used in the case violated due process norms. *See Turner*, 131 S. Ct. at 2519–20. After taking eighteen paragraphs to conclude that a father who had failed to make court-ordered child support payments did not have a right to counsel in a civil contempt proceeding that resulted in his one-year incarceration, the Court dedicated one paragraph to holding that the father's incarceration nevertheless violated due process. *See id.* at 2515–20.

"best practices" for adjudicators handling self-represented litigants.[167] Such practices were developed over the past ten to fifteen years by persons and institutions likely to have knowledge of the issues posed.[168] Zorza's list of best practices rests in part on the idea that unless self-represented litigants are told (among other things) the purpose of the proceeding, the issues to be decided, the governing substantive law, and the type of evidence considered relevant, they are unlikely to respond well to, for example, questions posed by an adjudicator and designed to elicit information needed for a legally correct decision.[169]

We applaud these and other efforts to address the increasing problems that self-represented litigants cause for courts, and we intend no suggestion that efforts on this score should cease. Nevertheless, our question is the following: is there any credible, quantitative evidence that these best practices or other adjudicator-centered techniques work, in the sense of preventing the unknowing or ill-considered forfeiture of legal rights, or in the sense of producing the right litigation outcomes?[170] Self-representing litigants find themselves in an unfamiliar, intimidating, and pressure-filled setting in which decisions of great personal importance will be made quickly. Does any evidence support the assertion that orally explaining complicated legal concepts a single time to such a litigant will suffice to induce her to understand and internalize the communicated information? Although the analogy is a stretch, the experience in a legal setting that shares some of these characteristics may be instructive: "In what likely would have been a major surprise to the *Miranda* court, modern studies demonstrate that roughly eighty percent of suspects waive their Miranda rights and talk to the police."[171] Perhaps most important for this Article, how would further analysis assess whether court-centered techniques are working? What would evidence that techniques are working look like?

If one wants to measure the effect of implementing judicial best practices, it might seem intuitive to randomize their use across cases

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[167] *See* Zorza, *supra* note 131, at 17.

[168] *See id.*

[169] *Id.* at 17–20. Similarly, during the hearing, best practices suggest that judges explain the evidentiary rulings they make as well as the requirements of any court order. *Id.* at 19–20.

[170] *See* Richard Zorza, *Turner v. Rogers*: The Implications for Access to Justice Strategies 5 n.13 (Nov. 2011) (unpublished manuscript), *available at* http://www.colorado.edu/law/centers/byronwhite/docs/RichardZorzaAccessJusticeStrategies.pdf ("[*Turner*] has come in for criticism . . . [for] the lack of any record demonstrating whether the procedures suggested by the Solicitor General [and endorsed by the *Turner* majority] in fact are adequate to provide meaningful access, and the opinion's failure to specify how a court should determine whether a particular set of procedures is adequate to provide meaningful access . . . .").

[171] Mark A. Godsey, *Reformulating the* Miranda *Warnings in Light of Contemporary Law and Understandings*, 90 MINN. L. REV. 781, 792 (2006) (footnotes omitted).

and to compare case outcomes between treated and control groups.[172] For example, in the randomly selected treated group of cases, the adjudicator provides handouts or flyers describing the nature of the upcoming proceeding as well as forms designed to elicit information prior to the hearing. At the outset of the hearing, the adjudicator explains the purpose of the proceeding, the applicable substantive law, the nature of the potentially relevant evidence, and the procedures to be followed. The adjudicator then explains the basis of her decision in open court. And still, in this randomly selected treated group, the adjudicator follows other suggested best practices and techniques. In the remainder of cases (the randomly selected control group), the adjudicator implements only some — or none — of these procedures. Comparison of adjudicatory outcomes between treated and control groups will show the effect of the techniques under examination vis-à-vis a baseline of not using them. Surveys of litigants could provide information about whether either group's members, for example, had greater confidence in the results of the litigation, felt they had the chance to tell their stories, were more likely to comply with any judicially imposed obligations, or viewed the judicial system with greater confidence.

We believe such direct, randomized comparisons should be pursued, as they represent a powerful way to assess whether judicial best practices change case outcomes, litigant perceptions, and other outcomes of import. Nevertheless, this design constitutes an incomplete way to assess whether court-based reforms succeed in providing access to justice, at least to the extent that access to justice includes a focus on adjudicatory outputs. In particular, studies of the type described in the previous paragraph have at least two potential shortcomings, one conceptual, the other at the level of implementation. First, conceptually, would such studies tell researchers and policymakers what they really want to know? A randomized comparison of best practices with current practices would measure whether best practices change case outcomes, but are these changed case outcomes the right outcomes? Perhaps the results produced by adjudicatory systems using these best practices fail to achieve something close to the "right" results in a high enough number of cases to permit the conclusion that the systems comply with minimum due process standards, despite the fact that the "concept of due process is flexible."[173] To put this idea more bluntly: perhaps all judicial best practices do is alter case outcomes from being horrendously unfair toward self-represented litigants to being merely moderately unfair toward self-represented litigants, with the "moder-

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[172] *See generally* Michael Abramowicz, Ian Ayres & Yair Listokin, *Randomizing Law*, 159 U. PA. L. REV. 929 (2011).

[173] Morrissey v. Brewer, 408 U.S. 471, 481 (1972).

ately unfair" results nevertheless failing to reach a minimal due process floor. The randomized control trial design tells researchers and policymakers nothing about where some set of outcomes fits on an absolute scale such as "sufficient to meet minimum due process standards" versus "insufficient to meet due process standards."[174] Thus, researchers and policymakers would not know whether the outcomes of the cases assigned to the treated group (the one in which judges are proactive, explain concepts, and implement other "best practices") are fundamentally fair.

Second, fielding studies of this type would pose operational challenges. If randomization is to be done at the level of the individual case (the most reliable methodology to assure that results reflect the effect of different adjudicator protocols), then adjudicators must switch their courtroom styles and routines from one case to the next. Such changes might not be easy for adjudicators to make, particularly with respect to the conduct of hearings. In other words, it may not be possible for adjudicators to do their job of applying law to facts and resolving disputes while at the same time nimbly switching from, for example, proactive questioning to passive acceptance of party-offered evidence. Thus, studies of this design type should be attempted, but they could present operational challenges, and they would require supplementation even if implemented successfully.

A complement to the approach described above would extend the concept utilized in this Article to access-to-justice-promoting measures other than limited legal assistance. In other words, an additional way to test the effectiveness of procedural reforms, such as the best practices discussed above, would be to follow the methodology of this Article: implement the reforms in a particular adjudicatory setting and then conduct a randomized trial comparing outcomes for cases receiving offers of full representation with outcomes for cases receiving no such offers. Were this framework implemented in a court or administrative proceeding in which a high percentage of the opposing litigants (such as the evictors in the District Court Study) enjoy representation, then the results of the treated group (where both parties are represented) could, under certain assumptions, constitute an approximation of legally correct outcomes. If outcomes for the control and treated groups look similar, then we have evidence (although not conclusive evidence[175])

[174] To be clear, we are focusing here on adjudicatory outputs and legal outcomes, which are what we think many people refer to when they speak of the "results" of a legal proceeding. However, both access to justice and due process are broader concepts and include, for example, the right to be heard.

[175] *See* Greiner et al., *supra* note 23 (manuscript at 33–45) (refusing to accept that all was well in a housing court despite the similarity in outcomes between a treated group, which received of-

that the adjudicator best practices or other access-to-justice-promoting measures are working in the sense defined above. In other words, from an access-to-justice point of view, a finding that an offer of representation has little effect on case outcomes might be viewed as good news.[176]

What is needed to make this concept work? Again, the District Court Study provides some guidance. In our treated group, 97% of occupants (and 86% of evictors) were represented. Meanwhile, in the control group, only 11% of the occupants (but 96% of evictors) found counsel. Although we cannot be definitive, we suspect that these figures stem from the following two facts: (i) no legal aid provider had operated regularly in the district court's summary eviction calendar for over a decade, making offers of full representation both highly valued and difficult to obtain, and (ii) a substantial percentage of evictors hired lawyers. These are two characteristics that can be observed in advance when deciding where a new study should proceed.

The message here is that one practical way (concededly, a way that rests on certain assumptions) to assess the effectiveness of access-to-justice-promoting measures is to implement those measures and then to conduct a study in which one randomizes offers of full representation. The District Court Study demonstrates the potential of this concept.

Further, if one accepts this concept and believes that the District Court Study succeeded in implementing it, then the results we present here suggest that court- and law-based interventions to promote access to justice may have serious limits.[177] As discussed above[178] and in greater detail in Appendix III, the Quincy District Court in which the present study took place, as well as other actors in the system, had already implemented several access-to-justice-promoting measures, including: (i) simplified rules of civil procedure, (ii) ready availability of simplified legal forms in checkbox format, (iii) mediation for at least some cases, and (iv) reduced formality in the courtroom and in the conduct of evidentiary hearings. Accepting the arguable proposition

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fers of full representation, and a control group, which received referrals to a provider's lawyer-for-the-day program).

[176] *See* Steve Eppler-Epstein, *What Can We Learn if We Assume Greiner and Pattanayak Are Right?*, CONCURRING OPINIONS (Mar. 28, 2011, 12:35 PM), http://www.concurringopinions.com/archives/2011/03/what-can-we-learn-if-we-assume-greiner-and-pattanayak-are-right.html ("Improving an adjudicative system can increase the number of people for whom we [in the legal aid community] have little impact — and that's a good outcome!").

[177] For an example of an article advocating court- and law-based measures — to the exclusion of appointing counsel — to deal with access-to-justice issues, see Benjamin H. Barton & Stephanos Bibas, *Triaging Appointed-Counsel Funding and Pro Se Access to Justice*, 160 U. PA. L. REV. 967 (2012).

[178] *See supra* pp. 916–17.

that the results of proceedings in which both sides are represented constitute a rough proxy for the legally correct outcomes of summary eviction cases, the outcomes of the treated group in the District Court Study (a group in which 86% of evictors and 97% of occupants were represented) show approximately what the legally correct outcomes of a subset of summary eviction cases in Quincy District Court should look like. These outcomes stand in sharp contrast to those of the control group in the District Court Study. The conclusion follows that the control group outcomes were not legally correct, or not as close to legally correct as those of the treated group. And this was true despite all the access-to-justice-promoting measures the Quincy District Court undertook, and despite the substantial unbundled assistance most of the control group received. It would appear that, like unbundled legal assistance programs, court-based access-to-justice-promoting measures have limits.

CONCLUSION

Particularly when viewed together with the results of the four randomized studies that preceded it,[179] as well as with the Housing Court Study that proceeded simultaneously,[180] the District Court Study demonstrates the power of the randomized experiment to distinguish situations in which a legal intervention impacts legal and socioeconomic outcomes from those in which it does not. We can make access to, and administration of, justice more evidence-based. There are benefits to doing so.

[179] STAPLETON & TEITELBAUM, *supra* note 10 (two separate studies); Greiner & Pattanayak, *supra* note 8; Seron et al., *supra* note 9.
[180] Greiner et al., *supra* note 23.

APPENDIX I: FACTUAL BACKGROUND, LEGAL SETTING, STUDY DESIGN, AND FIELD OPERATION

We provide here a detailed description of the factual and legal setting for the District Court Study. For two reasons, good scholarly practice requires much more detail on these issues than we provided in Part II. First, the ongoing effort to understand where, when, how, and at what level to provide legal assistance given limited resources may eventually reach the point at which there are enough rigorous studies to make cross-study comparisons worthwhile. Such comparisons cannot proceed without detailed descriptions of the outreach and intake mechanisms used, the exact nature of assistance offered, the service provider and the attorneys who provided the representation, the court that was the site of the study, and other potentially relevant characteristics of the overall setting.[181] Second, a recurring claim among many with whom we have discussed the prospects of rigorous quantitative study of legal services is that one cannot randomize in the legal aid setting, particularly in housing and in family law. Some of the objections are ethical in nature; we hope to address these in a future paper, although we suggest that oversubscription makes randomization permissible. Yet often the objections are operational and concern whether a randomized design has enough flexibility to address the day-to-day challenges of the legal setting, or whether legal outcomes (particularly in housing and family law) are sufficiently measurable to make quantitative analysis relevant and useful.[182] We hope that a detailed description of our implementation will counter these objections and will help to stimulate a large-scale movement toward randomized evaluation in

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[181] We acknowledge that cross-study comparisons of the kind we anticipate involve a large amount of guesswork, but suggest that this is the kind of guesswork upon which broad policy decisions are necessarily based.

On the need for this kind of research more generally, see JEFFREY SELBIN, JOSH ROSENTHAL & JEANNE CHARN, CTR. FOR AM. PROGRESS, ACCESS TO EVIDENCE: HOW AN EVIDENCE-BASED DELIVERY SYSTEM CAN IMPROVE LEGAL AID FOR LOW- AND MODERATE-INCOME AMERICANS (2011), *available at* http://www.americanprogress.org/wp-content/uploads/issues/2011/06/pdf/evidence.pdf; Laura K. Abel, *Evidence-Based Access to Justice*, 13 U. PA. J.L. & SOC. CHANGE 295 (2010); and Gregg G. Van Ryzin & Marianne Engelman Lado, *Evaluating Systems for Delivering Legal Services to the Poor: Conceptual and Methodological Considerations*, 67 FORDHAM L. REV. 2553 (1999). For the need for this sort of research into the functioning of court systems more generally, see Mark Spottswood, *Evidence-Based Litigation Reform*, 51 U. LOUISVILLE L. REV. 25 (2012).

[182] For example, one former legal aid attorney has suggested that randomized trials may require providers to abandon merits screens for cases or, relatedly, to represent frivolous cases. Margaret Monsell, *What Difference Representation: Case Selection and Professional Responsibility*, CONCURRING OPINIONS (Mar. 28, 2011, 5:11 PM), http://www.concurringopinions.com/archives/2011/03/what-difference-representation-case-selection-and-professional-responsibility.html. This Article demonstrates that these statements are incorrect.

legal assistance specifically and in programs addressing due process values more generally.

A. Massachusetts Summary Eviction Law: Substance and Procedure

Summary eviction processes exist in almost every state and are statutory creations designed to adjudicate quickly who has the right to possess a housing unit.[183] Although summary eviction involves a wide variety of legal circumstances, in our dataset three scenarios occurred. In 15% of our cases, a purchaser in a foreclosure sale sought to evict either former homeowners who remained in the unit (about half of the 15%) or tenants who had been renting from the former owners (the other half of the 15%).[184] In approximately 17% of our cases, a landlord sought to evict a tenant who had allegedly committed some kind of misconduct, such as being involved in some way in the commission of a crime or creating a disturbance in the unit.[185] In the remaining 68% of our cases, a landlord sought to evict a tenant for allegedly failing to pay rent.[186] Although the procedural and substantive law differed somewhat for each type of proceeding, some legal aspects were common to all cases. We provide an overview of these common aspects in this section.

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[183] Velazquez v. Thompson, 451 F.2d 202, 204 (2d Cir. 1971).

[184] Note that particularly in the case of a tenant, the applicable law changed with the August 7, 2010, effective date of a Massachusetts statute governing such evictions. MASS GEN. LAWS ch. 186A (2010). (Portions of this statute were drafted by students and staff at the Harvard Legal Aid Bureau. *See Foreclosure Legislation Drafted by Bureau Students Signed into Law*, HARV. LEGAL AID BUREAU (Aug. 9, 2010), http://www.harvardlegalaid.org/news/47-foreclosure/86-foreclosure-legislation-drafted-by-bureau-students-signed-into-law.) By August 7, 2010, 78% of the cases in the District Court Study had gone to judgment, and 96% of the relevant complaints had been filed.

[185] There were also a few cases of no-fault eviction, but not a sufficient number to make up a separate category.

[186] In the District Court Study, with the exception of three hiccup cases, potential clients were not study eligible unless litigation had been filed. In two of the three cases, the occupants were randomized a few days before litigation was filed, but litigation was in fact filed within a few days, so there was little effect on the study. In the third hiccup case, randomization occurred after the evictor had served litigation documents on the occupant (thus, both the occupant and GBLS thought a case had commenced) but before these documents had been filed with the court. On the courthouse steps (literally), a GBLS intern negotiated a settlement of the case, so no case was ever filed. In the other 126 of 129 cases, randomization occurred after litigation was filed.

Greiner et al., *supra* note 23 (manuscript at 41–43), make clear that in the second of the randomized studies that we designed involving Massachusetts summary eviction proceedings, the Housing Court Study, different eligibility criteria were used. Specifically, a potential client could contact the relevant legal aid provider before being sued, when she received a document called a "notice to quit" from a would-be evictor. Thus, 126 of 129 (98%) cases in the District Court Study started out as "complaint" cases, in the language of the Housing Court Study, and two of the remaining three became complaint cases very quickly. The Housing Court Study, by contrast, involved a mixture of notice to quit and complaint cases.

1. District Courts. — The study we report here took place in one of Massachusetts's sixty-two district courts. The district courts are courts of fairly general jurisdiction, addressing small- to medium-sized civil matters, as well as criminal matters up to medium-level felonies.[187] Geographically, the sixty-two district courts cover the entire state. District court civil jurisdiction extends to summary eviction claims as well as to 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. Summary eviction cases, regardless of the court in which they took place, are subject to the Massachusetts Uniform Summary Process Rules, a set of rules designed in part under the recognition that "time is of the essence in eviction cases."[188] These rules governed the procedures used in summary eviction lawsuits in the District Court Study, as the following subsections describe.

2. Before the Lawsuit. — Before filing a summary eviction action, an evictor has to lay a legal foundation by serving the housing unit occupant with a "notice to quit," a one- or two-page demand meeting certain legal requirements, among them (usually) a specification of a date by which the occupant is required to vacate the unit.[189] The time period to be specified depends on the asserted grounds for the eviction. By far the two most common time periods were fourteen days following the receipt of the notice to quit, corresponding to an eviction for nonpayment of rent,[190] and thirty days, corresponding to evictions both for tenant misconduct and for the expiration of a lease (or a termination of a month-to-month tenancy).[191] Massachusetts law requires that the notice to quit provide additional information, the exact nature of which again depends on the legal setting. For example, a fourteen-day notice to quit for nonpayment of rent has to recite that under certain circumstances the tenant has a right to cure the nonpayment by paying the alleged arrearage within a certain time period,

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[187] *District Court Department*, MASS. CT. SYS., http://www.mass.gov/courts/courtsandjudges/courts/districtcourt (last visited Dec. 1, 2012).

[188] MASS. UNIF. SUMM. PROCESS R. 1 cmt.

[189] *See, e.g.*, MASS. GEN. LAWS ch. 186, § 12 (2010). Sample notice-to-quit forms can be found online at *Notice to Quit for Non-Payment of Rent*, PLYMOUTH COUNTY SHERIFF'S DEP'T, http://www.pcsdma.org/Forms/14%20Day%20Notice%20to%20Quit.pdf (last visited Dec. 1, 2012) (fourteen days); and *Notice to Quit*, BOS. APARTMENTS, http://www.bostonapartments.com/rentips-noticetoquit.htm (last visited Dec. 1, 2012) (thirty days). It is not entirely clear that the forms include all the information needed to support an eviction proceeding.

[190] MASS. GEN. LAWS ch. 186, §§ 11, 11A, 12.

[191] *Id.* § 12 (addressing the case of expiration of a lease, or the case of an eviction for fault after the termination of the leasing document); *see also* Shannon v. Jacobson, 160 N.E. 245, 246–47 (Mass. 1928) (stating that the statute does not provide for termination of a lease simply via a notice to quit and that a landlord must terminate a lease by reentry or a stated violation of an operative lease term before commencing a summary process action).

and that if she did so, no eviction action would lie.[192] A thirty-day notice to quit based on misconduct has to identify the incidents or actions that formed the basis of the alleged lease violation.[193] And of course the occupant must be served with the notice to quit.[194] These procedural requirements provide possible defenses to summary eviction lawsuits, and one might or might not believe that occupants with attorneys found it easier to assert them.

3. Complaint to Judgment. — If the occupant remains in the unit beyond the (legally correct) time period specified in the notice to quit,[195] the evictor files a summary eviction complaint. The court-provided complaint form is a single page and requires only the most basic information: essentially the names of the evictors and the occupants, the address of the unit, and a one-line description of the alleged grounds for the eviction (including a brief accounting of any alleged rent arrearage).[196] Once the evictor files proof of service of the complaint and pays the filing fee, the clerk "enters" the complaint and sets the trial for the first available court day no sooner than ten days later. In practice, in the District Court Study, this procedure meant that the overwhelming majority of evictors served occupants with the summons and complaint, then filed on a Monday, so that trial would be set for the Thursday of the following week.

Massachusetts law provides various defenses, counterclaims, and other strategic options to tenants facing eviction actions. For example, Massachusetts statutory law partially codifies a nonwaivable warranty of habitability and allows the occupant under certain circumstances to use bad conditions in the unit to justify nonpayment of rent.[197] Massachusetts statutory law also imposes duties on landlords to abate lead in units rented to families with children under the age of six.[198] Final-

[192] MASS. GEN. LAWS ch. 186, §§ 11, 11A, 12.

[193] *See, e.g.*, Strycharski v. Spillane, 69 N.E.2d 589, 591 (Mass. 1946).

[194] MASS. GEN. LAWS ch. 186, § 12 ("given to the other party"); *see also* Hodgkins v. Price, 137 Mass. 13, 16–17 (1884); May v. Rice, 108 Mass. 150, 152 (1871).

[195] A lawsuit filed too early — that is, before the grace period provided in the notice to quit for the occupant to vacate voluntarily — could be dismissed, which provided another procedural defense in some cases. *See, e.g.*, De Nuccio v. Caponigro, 157 N.E. 159, 160 (Mass. 1927); Decker v. McManus, 101 Mass. 63, 64 (1869).

[196] *Summary Process (Eviction) Summons and Complaint*, COMMONWEALTH OF MASS. (Sept. 1, 2005), http://www.mass.gov/courts/courtsandjudges/courts/districtcourt/summary_process_complaint_rev.pdf.

[197] In most cases, the evictor or her agents must know of the bad conditions, MASS. GEN. LAWS ch. 239, § 8A (2010), making it important for the tenant to allege them in an answer and be ready to prove that she told the landlord about the conditions. *See also* Bos. Hous. Auth. v. Hemingway, 293 N.E.2d 831, 839–40 (Mass. 1973) (holding that the diminution in value caused by conditions could justify withholding of rent).

[198] MASS. GEN. LAWS ch. 111, § 197(a) (2010); *see also* Bencosme v. Kokoras, 507 N.E.2d 748, 750 (Mass. 1987); Mass. Rental Hous. Ass'n v. Lead Poisoning Control Dir., 729 N.E.2d 673, 677 (Mass. App. Ct. 2000).

ly, a landlord who fails to return a security deposit plus interest[199] (minus properly documented sums due for damage to the unit) within thirty days of termination of a tenancy is liable for three times the amount of the deposit and interest.[200] A landlord who fails to put a security deposit in a separate bank account that the landlord's creditors cannot reach also faces treble damages.[201] As illustrated by the security deposit treble damages provisions, some defenses to a summary eviction action for possession also form the basis for counterclaims. Putting aside Massachusetts law, federal regulations applicable to public housing and Section 8 units impose additional requirements on landlords, some procedural (such as providing for pre-eviction administrative meetings[202]) and some substantive.[203] Not all theories are available in all kinds of actions. Nevertheless, these and other protections can provide defenses, counterclaims, and concomitantly, bargaining leverage to occupants facing summary eviction actions, if properly used.

In short, and in possible contrast to other areas of law, Massachusetts summary eviction suits were frequently not, or need not have been, disputes that turned on a single, relatively straightforward issue, such as the circumstances surrounding a separation from employment.[204]

During the District Court Study, an excellent summary process eviction answer form[205] was available online[206] and was used by lawyers and pro se litigants alike. The form allowed an occupant (or her lawyer) to check boxes to assert a variety of federal and state law defenses and counterclaims. The form cited applicable constitutional provisions, statutes, and summary process rules, but it also provided

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[199] By filing a summary eviction action, an evictor alleges that any tenancy had been terminated, and under Massachusetts law, the filing of an answer alleging violation of the security deposit laws constitutes a tenant's demand for repayment. Lopes v. Williams, 2010 Mass. App. Div. 227, 228–29 (Dist. Ct. 2010).

[200] Castenholz v. Caira, 490 N.E.2d 494, 497 (Mass. App. Ct. 1986); *see also* MASS. GEN. LAWS ch. 186, § 15B (2010).

[201] MASS. GEN. LAWS ch. 186, § 15B(7).

[202] *See, e.g.*, Thorpe v. Hous. Auth., 393 U.S. 268, 283–84 (1969); Caulder v. Durham Hous. Auth., 433 F.2d 998, 1003–04 (4th Cir. 1970). *But see* Swann v. Gastonia Hous. Auth., 675 F.2d 1342, 1345 (4th Cir. 1982) (stating that such a hearing is not strictly required by statute).

[203] *See* 24 C.F.R. § 982.310 (2010) (providing, inter alia, protections against no-fault evictions and evictions for failure of the public housing authority to pay its share of the rent).

[204] *See* Greiner & Pattanayak, *supra* note 8, at 2124.

[205] *Cf.* Turner v. Rogers, 131 S. Ct. 2507, 2520 (2011) (highlighting the lack of an easy-to-use form that would elicit critical information in holding that a civil contemnor's incarceration violated the Due Process Clause).

[206] *Representing Yourself in an Eviction Case: The Answer*, *supra* note 66.

factual options that occupants could select with check marks.[207] The answer form was thus a powerful tool serving multiple purposes, including prompting the occupant to recall the factual grounds for possible defenses and counterclaims, as well as easing the assertion of those defenses and counterclaims in a legally cognizable manner. The form also had a check box for a jury trial demand.[208]

Massachusetts law requires the occupant to file an answer within a week of the entry of the complaint, and by default the trial is scheduled for ten days after entry (meaning three days after the answer due date).[209] Under Massachusetts Uniform Summary Process Rule 7, however, if an occupant files and serves discovery on the evictor within one week of the entry of the complaint, the trial date is automatically postponed for two weeks.[210] Thus, propounding discovery is another powerful tool for occupants and serves at least three purposes: (i) to obtain information about the lawsuit in support of possible defenses and counterclaims; (ii) to establish bargaining leverage by increasing costs on an evictor, such as by laying the foundation for a motion to compel if the evictor did not respond to the discovery fully; and (iii) to delay the proceeding, which is usually in the occupant's interest because absent emergency relief the occupant retains possession while the action is pending (and is almost always free to move out at any time if she so desired). During the District Court Study, check-box forms for interrogatories and requests for production of documents,

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[207] *Id.* For example, under the heading "Bad Conditions in My Home and Other Claims: Mass. Gen. Laws c. 239, § 8A; c. 93A; and/or Implied Warranty of Habitability," *id.* at 7, the following statement appeared:

> □ I have a defense and counterclaim because of past or present problems in or around my home that the landlord knew or should have known about, including but not limited to the following:
> - □ cockroaches, other insects, mice or rats
> - □ water entry or leakage
> - □ lead paint
> - □ defective locks or security problems
> - □ defective or leaky windows
> - □ defective ceilings, walls, or floors
> - □ problems with heat and/or hot water
> - □ other: ____

An immediately subsequent portion in the answer allowed the occupant to check off the ways in which she informed the evictor of these conditions. *Id.* Again, putting aside the ease with which relevant legal provisions and doctrines became available to the occupant, one should not underestimate the importance of forms of this nature in prompting occupants to remember relevant facts.

[208] *Id.* at 4.

[209] On answer timing, see MASS. UNIF. SUMM. PROCESS R. 3. Regarding the trial date, see *id.* 2(c).

[210] The rule required "proper service and filing" of the discovery requests, *id.* 7(e), in contrast to practices applicable in, for example, most federal district courts, where discovery materials are not ordinarily filed with the court.

similar in format and ease of use to the answer form described above, were also available, and they were also used by both lawyers and pro se litigants alike.[211]

Jury trials are available in all Massachusetts courts handling summary eviction actions.[212] The choice to demand a jury might be informed by the usual concerns applicable to any piece of litigation, such as whether a lay group of peers is likely to provide a more or less favorable ruling than would a judge. In summary eviction, an additional reason for an occupant to consider a demand is that jury trials are more difficult to schedule than are bench trials and thus can provide a source of delay. For this reason, it is often in an occupant's interest to demand a jury trial, which could be done by checking a box if the occupant had access to the answer form described above.

Once the initial pleadings were done, litigation would ensue and could include the standard pestilential mix of motions (to compel responses to discovery, for a preliminary injunction, to dismiss, for summary judgment, and so forth), status conferences, partial and full settlements, and other accoutrements of court fights. As we detail below, in the District Court Study cases, evidentiary hearings of any kind were rare,[213] and almost all cases reached judgment via settlement, default, or dismissal.

4. Judgment and Post-Judgment. — Eventually, judgment for the evictor or the occupant should have issued, although occasionally no judgment technically did issue.[214] If the judgment involved some kind

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[211] *See Representing Yourself in an Eviction Case: Discovery*, *supra* note 67.

[212] MASS. UNIF. SUMM. PROCESS R. 8 cmt.

[213] *See supra* p. 933.

[214] In some cases, no formal judgment would actually issue, in the sense that there was no separate docket entry reflecting a judgment for the evictor and indeed, no separate document in the record called a "judgment." We might have coded such situations as judgments for the evictor, except that in some cases, settlement terms (for example, judgment to enter for the evictor or no execution to issue pending compliance with a repayment program) were supplemented by an agreement that if the occupant completed repayment, the case would be dismissed. Clearly, in such cases, the parties contemplated further court action: either an evictor's motion to issue an execution if the occupant missed payments, or a dismissal if the occupant completed repayment. Yet the record showed no further court action at all (we checked records in some cases more than twelve months after the date upon which the case was to be dismissed). Under such circumstances, we did not code a case as having a judgment for either party unless we saw a docket entry entitled "judgment" and a separate writing with that title. With respect to actual possession, in such cases, if the time period scheduled for the repayment passed with no evictor request for an execution, we followed the advice of several housing attorneys by assuming that the occupant retained possession, the theory being that had the arrearage not been paid, the evictor would have taken court action. Finally, regarding whether a writ of execution for possession issued in the evictor's favor, because constables' notices were not filed with the court, and because the legal requirement that writs of execution be returned satisfied or unsatisfied was uniformly ignored, we had no way of knowing what percentage of writs of execution were actually used to evict an occupant forcibly. Nevertheless, the issuance of a writ of execution for possession was never a good result for the occupant.

of relief for either party, then absent a court order to the contrary, the judgment alone was insufficient to empower the prevailing party to obtain that relief coercively. Rather, the court had to issue a writ of execution, ordinarily not available until ten days after the judgment.[215] Writs issued for possession (if the judgment was for the evictor) or for damages (in favor of either party). The writ of execution for possession was again itself insufficient to allow an evictor to enter into a housing unit to remove the occupant's belongings and change the locks. Rather, an evictor had to request that a constable issue a forty-eight-hour notice to the occupant, and after the forty-eight hours, entry and removal could proceed.[216] The district court had the power to stay a writ,[217] which effectively meant that the evictor could not request a constable's notice during the stay.[218]

The judgment, writ, and constable's notice procedure were important in summary eviction actions because parties could bargain around these various stages. Differing arrangements carried differing levels of peril for occupants, in the forms of differing risks of erroneous evictions and differing default time periods in which occupants might have had to move out.[219]

B. Court Personnel and Practices

1. The Judge and His Call Practice. — For almost all District Court Study cases and rulings, one particular judge sat on the bench.[220] The judge was appointed to the bench in 1989 and to the relevant district

[215] MASS. GEN. LAWS ch. 239, § 5(a) (2010).

[216] *Id.* § 3.

[217] *Id.* § 10.

[218] All writs of execution were supposed to be returned to the court and marked as either satisfied or unsatisfied. *See* MASS. GEN. LAWS ch. 235, § 17 (2010). In practice, this procedure was almost never observed.

[219] For example, suppose an evictor sued for nonpayment of rent and the parties settled with an agreement contemplating that the occupant would repay some or all of the arrears over a specified period, and at the end of that period, the tenancy would be reinstated. There were various procedural postures through which such a settlement might have been effectuated. First, the parties might have agreed that no judgment would issue and required the evictor, if she believed that the occupant breached the agreement (such as by not making the requisite payments), to move the court for an order requesting entry of judgment, followed by an execution. Second, the parties might have agreed that judgment would enter for the evictor, but no writ of execution would issue unless the evictor by motion alleged and proved a breach of the agreement. Third, the parties might have agreed that both judgment and a writ of execution would issue in the evictor's favor, but that the writ would be held by evictor's counsel, and that counsel would proceed with a constable's notice only if there were a breach of the agreement. Note that in this last arrangement, the court need not have been consulted before the evictor proceeded; at best, the forty-eight-hour constable's notice allowed the occupant to attempt to file an emergency motion for a stay.

[220] In terms of caseload, district court personnel told us that the court handled about 1280 summary eviction cases in fiscal year 2010.

court in 2000.[221] Before his appointment, he had a lengthy career as a practicing lawyer that included three years running GBLS's elder law unit. The judge also served in a state office dedicated to the delivery of human services, as well as in the office of the Massachusetts Attorney General. One would not expect a judge of this background to railroad pro se litigants or summary eviction occupants. Indeed, our observation of the judge's handling of his summary process calendar confirmed what one might infer from this background: this was not a judge who gave self-represented litigants (evictors or occupants) short shrift. Instead, as we discuss in greater detail below, the judge strenuously sought terms upon which the parties could settle, but in doing so he conducted the courtroom in a manner respectful of all parties and with due regard to the real-life consequences of his rulings.

We describe here the judge's practice of making successive "calls," by which he effectively winnowed the number of cases that required his personal attention, as well as his habits with respect to cases not winnowed. Readers familiar with the paper reporting the results of our other randomized experiment in summary eviction proceedings, the Housing Court Study,[222] may note similarities with the description provided above. Indeed, with the potentially important exceptions of the hallway negotiation and mediation practices, the procedures and judicial habits in the district court and the housing court we studied were surprisingly similar.

Most weeks, the district court handled its summary eviction calendar on Thursday mornings, ordinarily beginning at 10:00 a.m. The "First Call" proceeded as follows: A case was called. If the evictor or movant was not present, the case was dismissed or the motion was denied. If the evictor or movant was present but the occupant was not, a default judgment was entered or, after a brief colloquy, the motion was ordinarily granted. If both parties were present, then before attempting to ascertain the nature of the dispute or case, the judge ordinarily inquired whether the parties had engaged in settlement discussions that day. If they had not, he sent them into the hallway adjacent to the courtroom for a negotiation. If they reported that they had attempted to settle but could not do so, the judge would either refer the parties to a mediator or hold the case to the end of the call to give it his personal attention, as described below. From our observations, it appeared that the judge attempted to refer as many cases as possible to mediation,[223] and that these referrals were often made before he as-

[221] *Honorable Mark S. Coven*, MASS. CT. SYS., http://www.mass.gov/courts/courtsandjudges/judgesandjudicialofficers/covenm.html (last visited Dec. 1, 2012).

[222] Greiner et al., *supra* note 23.

[223] Often, there were not enough mediators to handle all of the cases that the judge desired to send to mediation.

certained the nature of the dispute or case. After every case had been called once, the summary eviction process paused for a short time, during which time the judge might retire to chambers, or perhaps hear a criminal matter.

"Second Call" followed. For cases in which the parties had negotiated in the hallway but had been unable to reach an agreement, the judge attempted to refer them to mediation, but a shortage of mediators made mediation the exception as opposed to the rule. For mediated cases in which no settlement had been reached, the judge gave the cases his personal attention. The judge would again pause the summary eviction calendar before "Third Call" began. Any cases still remaining unsettled received the judge's attention, as described below. Through this process, the judge typically winnowed a call list of thirty to sixty cases down to a handful, perhaps three to five, requiring his personal attention.

At any point, if the parties reported a settlement, the judge quickly read the written terms. Sometimes, the judge spoke briefly with the settling parties, reviewing essential terms of the proposed settlement. On rare occasions, the judge "requested" alterations in the terms of the settlement agreement. This "request" was prefaced with language such as, "Here's what I want you to do," followed by the nature of the request. Although occasionally a party expressed reticence to make the requested change, it appeared to us that the parties always acted as the judge "requested." At no point in such proceedings would the record reflect that the judge had made a ruling or issued an order.

For cases requiring the judge's personal attention, meaning those that did not settle in hallway negotiations or mediation, the judge allowed each party three to five minutes to summarize its view of the case. Ordinarily without swearing the parties in,[224] the judge asked factual questions designed to elicit the information needed to understand the case. For example, the evictor (or her attorney) typically included in her summary of the case the essential reasons supporting the eviction action (nonpayment of rent, misconduct, foreclosure, and so forth). The judge's questions normally elicited additional needed details: exactly which month's rent had not been paid, the kind of housing unit at issue (institutional landlord versus owner-occupied structure, Section 8 or public housing or private market, and so forth), and the status of a potential sale of the housing unit in a foreclosure set-

[224] The choice appeared to be a matter of judicial style. On one morning we observed, a different judge handled the district court's summary eviction docket. On the basis of this one day's worth of observation, it appeared to us that this other judge swore in parties (thus effectively conducting a quasi-trial) as soon as they began making factual statements in their summaries of the case. But it did not appear that this judge swore in attorneys (who presumably would not speak from personal knowledge in any event) during colloquies.

ting. The judge frequently did the same to the occupant, asking for information regarding the source and amount of the occupant's income. These factual representations were accepted unless contradicted by the other party.

Having gathered some information regarding the underlying facts and the nature of the issue to be decided, the judge typically attempted to persuade the parties to settle. For example, if the occupant reported a figure that seemed to the judge to be insufficient to sustain a tenancy going forward, the judge might say something like, "That's not enough to keep the unit going forward. You can't pay the rent with that. When can you move out?" In a case in which the term of a tenancy had expired, the judge might say to the occupant something on the order of, "The landlords don't want you as tenants any more. There's nothing I can do about that. When can you move out?" To evictors pressing for immediate move-out dates, the judge might be as blunt as, "I'm not going to order them out that quickly." The judge often allowed occupants who had initially defaulted to remove the default on motion. It appeared to us that the judge took allegations of problematic conditions, such as lead in a unit in which a young child lived, quite seriously. Sometimes the judge threatened to deny evictors' motions, and then would suggest that evictors settle.

The colloquy process usually produced what the record would reflect as a party agreement. Although one might question exactly how voluntary the "agreement" process was, we did not in our personal observations see results that struck us as facially unreasonable, nor did we detect pro-evictor or pro-occupant favoritism.

We personally observed a few truly contested rulings and one trial. The fifteen-minute trial commenced after the parties described radically different conditions, and different reasons for those conditions, in the unit during a colloquy with the judge. The judge swore in both parties; allowed the evictor's counsel to conduct a direct examination of her client; very briefly questioned the evictor himself; allowed the pro se occupant to cross (there were no questions); questioned the occupant, who largely testified via narration; allowed evictor's counsel to cross (there were about five minutes of questions); asked the parties if either had exhibits, additional witnesses, or anything else to say (there was nothing of this sort); and ruled. The trial was respectfully conducted, although quite rapid and something of a surprise.[225]

2. *Mediation.* — In the district court that we examined, an alternative dispute resolution entity that offers both professional and volun-

[225] The case was scheduled for trial that day, but most summary eviction cases went through multiple trial dates, with trial almost never occurring. In this case, the judge, in the midst of the standard colloquy attempting to induce settlement, gave little warning before instructing the parties to raise their right hands.

teer dispute resolution services ran the mediation program.[226] Mediators in the district court were volunteers. When possible, they worked in pairs consisting of one woman and one man. The style was facilitative, party centered, and party driven. Ordinarily, the mediation began with both parties present, and the mediators invited each to provide a short statement of its view of the case. The mediators then caucused with each party, attempting to get each to avoid summarizing past events and to focus on how to resolve the dispute going forward. But these procedures were subject to change if the parties thought that a different method of proceeding would facilitate resolution. The mediators attempted, wherever possible, to avoid suggesting potential solutions or settlement terms, on the theory that suggestions from mediators would be unduly directive. Mediators had limited, if any, training in housing law, although through experience they were familiar with the typical ways in which district court summary eviction parties settled cases.

Mediation in the District Court Study was different from that in the Housing Court Study. In extreme contrast to the "mediations" conducted in the Housing Court Study by housing specialists (employees of the housing court), the volunteer mediators in the district court did not investigate the underlying facts, predict how the judge might rule, enforce settlement terms or court orders, evaluate the merits of the case themselves, or make more than a minimum number of suggestions of settlement terms to the parties.

One final note: at least before the Task Force's efforts resulted in the entry of GBLS staff attorneys into the district court's summary eviction practice, and for at least some time afterward (and continuing to an undetermined extent today), the court-provided settlement form, which was used by mediators and in hallway settlement negotiations, provided only for a judgment to enter for the evictor. GBLS drafted, and provided to the district court, a settlement form in which judgment could enter for either party, but was unable to assure its universal use and availability.[227]

C. GBLS and Its Attorneys

The service provider that participated in the District Court Study, GBLS, is a large legal aid entity with multiple specialized units addressing employment, housing, family, immigration, and elder law issues, among others. GBLS has a strong reputation in the local com-

[226] Our description of the process stems from observation of mediation sessions (including some observations by a research assistant), an interview of two mediators conducted on August 4, 2011, descriptions of the process from GBLS staff attorneys, and internet research. The mediators we interviewed asked not to be identified.

[227] Email from Stefanie Balandis, *supra* note 46.

munity in which it operates as well as in the national community of legal aid entities. It traces its roots back to legal assistance activities beginning over a century ago. GBLS had previously offered representation to tenants facing summary eviction in the district court that was the site of our study, but it had mostly ceased to do so over a decade before our study began.[228] Moreover, there was essentially no other entity in the region that regularly offered any kind of legal assistance to occupants on the district court's summary eviction calendar.

The Task Force generated funding sufficient for one full-time GBLS staff attorney plus support, which included an allocation for GBLS's outreach, intake, and screening programs, for about seventeen months. For thirteen of the seventeen months, GBLS chose to dedicate half of two of its staff attorneys' time to the District Court Study, and these two lawyers provided the representation for the overwhelming majority of treated-group cases. Both attorneys were specialists in low-income housing matters, with fifteen and thirty years of experience, respectively. Their practices covered a broad range of housing issues, including disability-related problems as well as efforts by tenant organizations to preserve existing units as affordable housing. From our limited observation, both attorneys appeared to possess a great deal of dedication, skill, and zeal. As part of their representation of treated-group occupants, both regularly advocated for their clients with third-party entities that provided various forms of nonlegal support, such as small grants to assist with rent arrearages or services to former homeowners enabling them to buy back homes that were previously "under water."[229] In four of the seventeen months, the primary GBLS staff attorney was a new practitioner a year or two out of law school; as one might expect, this attorney received substantial supervision from more senior GBLS lawyers.

D. The Field Operation: Design and Implementation

1. *Outreach, Intake, and Determination of Study Eligibility.* — GBLS's outreach, intake, and screening system is important to this Article for two reasons. First, it was the primary way that occupants received the limited legal assistance that about 70% of the occupants in our dataset enjoyed. Second, the individualized, proactive, specific, and timely nature of this system provides a potential explanation for the results we observed in the District Court Study reported here, as

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[228] Occupants who came to GBLS's regular housing clinics at its downtown Boston offices might be evaluated for possible representation, but there was no outreach to occupants in the district court's geographic jurisdiction, nor was there an established program of representation there.

[229] The District Court Study attorneys' advocacy on behalf of clients with nonlegal sources of assistance should be contrasted with the practices of the attorneys in the Housing Court Study. *See* Greiner et al., *supra* note 23 (manuscript at 12–13).

well as for the contrast between the results in the District Court Study and the Housing Court Study.[230]

Recall that in the district court most summary eviction cases were filed on Mondays (with trial set for the Thursday of the following week). On Tuesday afternoons, GBLS employees (sometimes the staff attorneys themselves) went to the district court, examined the summary eviction complaints entered the day before, used the information in the complaint to make their best guesses regarding which occupants would be eligible for assistance, and mailed the chosen occupants a form letter inviting them to one of two instructional clinics: one held on Fridays in the offices of a community-assistance organization located near the district court, the other held on Mondays in GBLS's offices (about ten miles from the district court, in a downtown location accessible by public transportation). The letter stated that the clinic would last two to three hours, would provide assistance in filling out answer and discovery forms, would review the occupant's rights, and would describe the district court's procedures. The letter also suggested that some occupants would be eligible for entry into a pool from which some would be randomly selected to receive an offer of full representation from a GBLS staff attorney. As a result, the provider's outreach system was (i) individualized, because letters were sent to the particular potential client; (ii) proactive, meaning GBLS made the first communication (as opposed to waiting for the potential client to call); (iii) specific to a particular matter, as opposed to offering general legal assistance; and (iv) timely, in the sense that the outreach letter arrived a few days after the occupant would have received a summons to appear in the district court's summary eviction calendar. The individualized, proactive, specific, and timely outreach process is similar to that used by providers in two previous randomized studies showing substantial improvements in outcomes due to an offer of representation,[231] although a separate study that applied this kind of intake system showed no improvements.[232]

At the clinics, GBLS staff attorneys, in addition to providing the limited assistance identified in the previous paragraph, also interviewed occupants they viewed as possibly study eligible. To be eligible for an offer of representation, an occupant had to meet GBLS's income screen[233] and pass a conflicts check. In addition, and as noted above,[234] the terms of the Task Force's grant further limited eligibility

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[230] *See id.*

[231] *See* STAPLETON & TEITELBAUM, *supra* note 10, at 72 (Zenith study); Seron et al., *supra* note 9, at 419, 423–25.

[232] STAPLETON & TEITELBAUM, *supra* note 10, at 67 (Gotham study).

[233] GBLS assists persons earning up to 125% of federal poverty income guidelines.

[234] *See supra* note 56 and accompanying text.

to three classes of cases: disability (related to the reasons for the eviction), criminal misconduct (again, related to the eviction), and substantial injustice.[235] As matters turned out, GBLS classified 90% of the cases as belonging to the "substantial injustice" category.[236] While time did not always allow for an exhaustive interview, the attorneys also attempted to determine whether the occupant had viable defenses to the summary eviction action[237] and whether the occupant's income was sufficient to meet the financial obligations of the unit (or could be made sufficient through appropriate action, such as restructuring of the mortgage). In addition, the attorneys examined the complaint to see whether the evictor appeared to be represented by counsel.[238]

About 70% of the 129 cases determined to be study eligible came from the above-described clinics. GBLS attorneys recruited the remaining 30% when present in the district court on Thursday for the summary eviction calendar, as follows: When a GBLS attorney was present at the beginning of the First Call, the judge usually asked the attorney to stand up and suggested that occupants with questions could approach him or her if time allowed. In some cases, the judge specifically suggested that the occupant obtain a quick consultation with the providing attorney before proceeding.[239] Unsurprisingly, the

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[235] As discussed in the main text, *supra* p. 918, GBLS attorneys in the District Court Study, in contrast to their Housing Court Study counterparts, *see* Greiner et al., *supra* note 23 (manuscript at 14), submitted cases for randomization only if they thought a traditional attorney-client relationship would alter outcomes.

[236] The fact that 90% of the cases were classified as "substantial injustice" spurred one of the many disagreements among those involved in the District and Housing Court Studies regarding how to interpret the results. Here, the disagreement was over how much of a role the Task Force's three categories had in constraining the set of cases GBLS submitted to us for randomization, and how much of a role these case categories had in explaining the large treatment effects we observe here. Our view is that these categories played only a limited role with respect to either aspect, but we have little hard evidence to support (or refute) our intuition here. We mention these facts in part to alert readers to substantial disagreements among those involved in these studies regarding how the data should be interpreted.

[237] *See supra* pp. 963–64.

[238] For the small number of cases that seemed to be on the borderline, the attorneys waited to examine the evictor's discovery responses before deciding whether the case was study eligible. In a few of these instances, the evictor failed to respond to discovery requests, in which case GBLS staff attorneys assisted the occupant in filling out a standardized motion to compel.

[239] The above description makes clear that GBLS staff attorneys provided quick advice on several occasions to occupants, including possibly some in our control group. This quick advice was nothing close to the magnitude of the assistance provided in the lawyer-for-the-day program in the Housing Court Study, *see* Greiner et al., *supra* note 23 (manuscript at 12). Nevertheless, unless one believes that this advice could have made occupants worse off (by, for example, raising occupants' expectations and inducing them to refuse settlement offers that they should have accepted), the fact that some occupants received even more legal assistance than that described above makes our results all the more startling. On the possibility that legal advice might raise litigant expectations in a possibly counterproductive way, see JOHN M. GREACEN, SELF REPRESENTED LITIGANTS AND COURT AND LEGAL SERVICES RESPONSES TO THEIR NEEDS 20–21 (2002), *available at* http://www.courts.ca.gov/xbcr/partners/SRLwhatweknow.pdf.

fact of the staff attorneys' presence also spread by word of mouth. Staff attorneys gathered as much information as possible via a rapid interview, if possible requested continuances of the proceedings,[240] and submitted study-eligible cases to us for randomization.

Upon concluding that a case was study eligible, the staff attorney read a short script describing the nature of the District Court Study and the opportunity for an offer of representation it provided. Upon obtaining consent,[241] GBLS filled out and submitted to us a one-page information sheet for randomization.

2. Randomization and Outcome Collection. — The randomized design used in this study was driven by two facts. First, because GBLS had not offered any form of systematic legal aid in the district court for over a decade (no one had), it had little information about how many study-eligible cases it would see from week to week. Second, regardless of case flow, it would not have done to use a study design that would have instructed GBLS to offer representation to five cases in one week and to no cases in the next three. Moreover, because the amount of work each case would require was difficult to predict in advance, and because the two attorneys' other responsibilities might have varied from week to week, the design had to be flexible enough to adjust to attorney workload.

We could conceive of only one randomized design that fit these circumstances: a succession of batches in which the attorneys told us as they transmitted each batch the number of cases their current caseload allowed to be randomized to treatment (an offer of full representation). Operationally, the attorneys sent the scanned, single-page information sheets described previously to us in a password-protected email at periodic intervals (averaging once every two to three weeks) along with short notes stating how many cases[242] their current workloads would allow them to represent fully. We randomized each batch accordingly. There were sixty-seven batches: thirty-seven of a single case, thirteen of two cases, eight of three cases, four of four cases, four of five cases, and one of six cases. Of the resulting 129 cases, 76 were randomized to treatment (an offer of a traditional attorney-client relationship from a GBLS staff attorney), and 53 to control (no such offer).

This design preserved the primary advantage of randomization in that it prevented any kind of "selection effect" from arising. As two of

[240] In a small number of cases, the attorneys determined that it would be unwise to request a stay without being able to represent to the judge that they would definitely be appearing in the case as attorney of record. In these cases, the attorneys randomized the cases on site.

[241] GBLS reported that very few eligible occupants refused to participate.

[242] For batches containing a single case, attorneys specified the probability that the case would be assigned to the treated group.

us have discussed elsewhere,[243] with nonrandomized designs in studies attempting to measure representation effects, it is possible (and we think likely) that, for example, attorneys select only the strongest cases for representation, or that only motivated and articulate potential clients seek representation, or that judges assign representation when they know they are likely to impose consequences upon an occupant. Any of these mechanisms, as well as others that are possible, result in a represented group that is fundamentally different from an unrepresented group. For this reason, comparing the outcomes of the represented group to the unrepresented group would provide little if any credible information on the effect of representation; one does not know whether to attribute any differences or similarities in outcomes to the representation or to the other differences between the two groups. Randomized assignment of cases to treated and control groups destroys these so-called "selection effects."

However, the design we used had certain disadvantages, the most important of which was that we could not block cases according to case type (meaning we could not separate nonpayment of rent cases from misconduct cases from foreclosure cases). This fact posed some challenges in the analysis, which we identify and discuss in detail below.

Regarding outcomes, we scanned district court case files for the randomized cases.[244] In a small handful of control group cases (around five), the record was unclear regarding the outcome of the case, so we attempted to call the occupant to obtain needed information. Surprisingly, we were successful in all but one case. In treated-group cases in which the record was unclear, we requested needed information from the GBLS staff attorney on the case.

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[243] Greiner & Pattanayak, *supra* note 8, at 2188–95.

[244] As discussed *supra* note 186, there was one case for which there was no case file.

APPENDIX II: FIVE STATISTICAL CHALLENGES

We discuss here five statistical challenges that arose in our analysis. Readers may wish to skip or skim this appendix if they are familiar with intention-to-treat effects and complier average causal effects, instrumental variables, and statistical generalizations thereof;[245] with the concept of outcomes defined for only certain types of study subjects; with principal stratification and censoring due to "death;"[246] with the issues posed by statistical outliers; and with multiple-testing penalties.

In discussing these challenges, we do not wish to overstate the difficulties they pose. To the contrary, as we will explain, all five are reasonably well understood in the statistical literature. Rather, we address these issues in some detail because each in various forms and with various levels of sophistication has been cited to us to support the argument that randomized studies are not possible or useful in the housing/eviction arena, or in the analysis of the effectiveness of representational interventions more generally. Our goal is to dispel such arguments by explaining these issues fully and addressing them adequately in our analysis.

A. *The "Challenge" of Crossover or Noncompliance*

As noted above, there have been four previous randomized evaluations of civil legal assistance in the United States. In all four, researchers randomized an *offer* of full representation from a particular service provider (or service provider consortium), it being potentially unethical and unquestionably impractical to randomize actual use of full representation. And in all four studies, researchers faced what statisticians call "crossover" or "noncompliance" issues, meaning that not all potential clients in a broad sense "complied" with the treatment to which they were randomized. Some potential clients randomized to an offer of full representation ended up pro se, and some potential clients randomized to no such offer found representation elsewhere.

Under such circumstances, there were two causal effects potentially of interest: the causal effect of receiving an offer of full representation

[245] *See generally* Joshua D. Angrist, Guido W. Imbens & Donald B. Rubin, *Identification of Causal Effects Using Instrumental Variables*, 91 J. AM. STAT. ASS'N 444 (1996); Guido W. Imbens & Donald B. Rubin, *Bayesian Inference for Causal Effects in Randomized Experiments with Noncompliance*, 25 ANNALS STAT. 305 (1997).

[246] *See generally* Constantine E. Frangakis & Donald B. Rubin, *Principal Stratification in Causal Inference*, 58 BIOMETRICS 21 (2002); Sander Greenland & James M. Robins, *Identifiability, Exchangeability, and Epidemiological Confounding*, 15 INT'L J. EPIDEMIOLOGY 413 (1986); James Robins, *A New Approach to Causal Inference in Mortality Studies with a Sustained Exposure Period — Application to Control of the Healthy Worker Survivor Effect*, 7 MATHEMATICAL MODELING 1393 (1986).

from the particular service provider versus not receiving such an offer — that is, the effect of what GBLS actually did; and the causal effect of the actual use of full representation (from any source) versus actually proceeding pro se, which turned in part on events beyond GBLS's control. The first measured the effectiveness of GBLS's full-representation program, incorporating the facts that not all potential clients offered full representation would take advantage and that there were sources of full representation other than GBLS's program. Credible inferences with respect to this first effect were more readily available because the offer of full representation (in contrast to the use thereof) was randomized. The second effect identified above, the effect of actual use of representation from any source versus no assistance, might be of interest to a designer of an adjudicatory system who desires to create a decisionmaking apparatus that does not depend on whether a litigant uses a lawyer. Credible inferences with respect to this second effect were statistically challenging because the processes by which potential clients actually used or eschewed full representation were not random. Something analogous to selection effects, difficult to adjust for in the area of legal representation, were very likely present.[247]

In the District Court Study we report in this Article, noncompliance was low — lower, in fact, than in any previously reported randomized study of the effect of counsel in U.S. civil litigation.[248] Only 3% of the treated group declined to take advantage of the provider's offer of representation, while only 11% of the control group found representation elsewhere. With figures this low, we address the problem of noncompliance by ignoring it. In theory, a statistical framework developed elsewhere[249] could address this "problem," but with only 129

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[247] Greiner & Pattanayak, *supra* note 8, at 2166–70 (providing an explanation designed to be accessible to those not steeped in program-evaluation techniques).

[248] In Stapleton and Teitelbaum's "Zenith" Study, 17.6% of the youths in delinquency proceedings randomized to an offer of representation by study lawyers ended up unrepresented, while 38.7% of those randomized to no such offer ended up represented. STAPLETON & TEITELBAUM, *supra* note 10, at 52. In these authors' "Gotham" Study, the corresponding figures were 17.3% (treated group unrepresented) and 11.4% (control group ending up represented, with 2% being represented by project lawyers due to appointments that ran counter to a previous agreement with the court). *Id.* In the Manhattan Housing Court Study, the relevant figures were 44.0% and 3.7% (rounded up to 4% as reported by the authors), with the high rate of the treated group unrepresented due to an initial system (later abandoned) that prevented experimental-group clients from receiving representation if a screening attorney deemed their cases simple enough not to require full representation. Seron et al., *supra* note 9, at 423–25. Finally, the Massachusetts Unemployment Study reported relevant figures that were probably less than those experienced in the "Zenith" Study and the Manhattan Housing Court Study, but more than those experienced in the "Gotham" Study. Greiner & Pattanayak, *supra* note 8, at 2128 n.27.

[249] *See* Greiner & Pattanayak, *supra* note 8, at 2166–70 (providing an accessible explanation of this framework); *id.* at 2131–32 & n.34 (drawing parallels to the field of biostatistics).

observations and a small amount of crossover between treated and control groups, these techniques would not work. This fact is actually good news. What it means is that we can analyze the effect of a GBLS offer of representation, which was randomized, and because a GBLS offer of representation corresponded so closely to actual use of representation (and a lack of an offer of GBLS representation corresponded closely with the lack of use of any form of representation whatsoever), we conclude that the results for a GBLS offer of representation closely approximate the results for actual use of representation from any source.

There is a lesson here. Some observers of these studies have purported to have no interest in the causal effect of an offer of representation, despite its importance in assessing the effectiveness of a legal services provider's operation (as opposed to a single aspect of it), and in understanding how a provider's operation actually operates (as opposed to how the provider wished it would operate). For such individuals, measuring the causal effect of actual use of representation from any source has been a challenge, given that (in contrast to offers) actual use generally cannot ethically be randomized. The answer to this challenge is straightforward: run pilot studies in "virgin but desperate" territory, meaning in geographic locations and in legal arenas in which representation is both (i) highly desired by the clientele who would make use of it, and (ii) essentially unavailable from any source other than the pilot study. Under such circumstances, there will be low crossover between treated and control groups, and the causal effect of the offer will closely approximate the causal effect of actual use.

B. The Challenge of Outcomes Defined for Only Certain Types of Cases

In legal terms, cases in our dataset generally fit into one of three categories: post-foreclosure (in which the occupant might have been either a tenant or a holdover former homeowner); eviction for cause (meaning a landlord sought to evict a tenant who had violated a lease term by, for example, keeping a pet or committing a crime); and eviction for nonpayment of rent.[250] Some outcomes, such as who ended up in possession of the unit at the end of the case, made sense and were well defined for all types of cases. Yet consider the challenges of measuring the amount of a money judgment in one or the other party's favor, or even better, the amount of money that was legally obligated to flow from one party to another (either in the form of an actual order to pay or in the form of waived rent), regardless of whether these obli-

[250] There were also a small handful of cases in which the evictor provided the occupant with a thirty-day "no-fault" notice to quit, such as might be issued if a landlord alleged that the lease term had expired or a month-to-month tenancy were to be terminated.

gations were encapsulated in a judgment. One could measure this variable for all case types, but do comparisons across case types make any sense? A monetary judgment in the first case type, post-foreclosure, frequently represented a payment promised by the evictor to the occupant in a "cash for keys" arrangement, meaning that the new owner promised an incentive payment conditioned on the occupant's vacating by a certain date. In the second case type, eviction for cause, money was unlikely to be a principal issue, so in many such cases (for example, those without substantial monetary counterclaims by the occupant), the monetary judgment amount would be zero. In the third type of case, nonpayment of rent, in contrast, a judgment often represented the amount of an arrears owed by the tenant to the landlord. Thus, money outcomes will typically make sense only for nonpayment-of-rent cases. Other outcomes we measure, such as the amount of any writ of execution for payment of money, or whether repairs to the unit were ordered, share this quality of being relevant only for cases of a certain type.

The best way to handle this issue of case categories with different outcomes of interest is to block cases at the time of randomization, meaning that cases of different types are randomized in different pots or bins. That way, the proportion of treated and control units does not differ across case type, and the desired analysis can proceed separately by case type. The cases can always be combined when analyzing outcomes, such as possession, that are relevant for all case types. Yet as noted above, the randomization scheme we were forced to use prevented us from blocking by case stage or case type. We thus were required to address this issue at the analysis phase. We did so by limiting our analysis dataset for each outcome to cases for which that outcome was well defined, and by limiting our analysis techniques to permutation testing and to statistical modeling, eschewing more traditional measures that depend on comparisons of means, standard deviations, and distribution-driven comparisons (such as *t*-tests).

C. The Challenge of Contingent Outcomes

Did an offer of GBLS representation increase the amount of time provided to occupants to vacate their units (measured from the date of the entry of the complaint)? For ease of reference, we call this amount of time a "vacate period." Length of vacate period is potentially an important outcome; from conversations with staff attorneys in both the District Court Study and the Housing Court Study, we learned that attorneys attempted to provide clients with an option to stay in the unit, but if that proved impossible, they attempted to provide as much time as possible for an orderly moveout. This system of dual goals highlights the nature of the problem here: the outcome, the amount of time provided to occupants to vacate their units, only makes sense for occupants who either had to or did move out. If the lawyers succeeded in their first goal of providing clients the option to stay in the unit, then for clients who chose this option, the outcome of length of time to vacate is undefined.

With all this in mind, suppose we uncritically compared average vacate period length as between treated and control groups. Would the comparison make any sense? Only if the offer of representation had no effect on the probability that an occupant did not retain possession. If, as appeared to be true in the District Court Study, the offer of representation did have an effect on whether the occupant retained possession, then a simple treated-to-control-group comparison gives misleading results. The offer of representation might have resulted in a class of cases, possibly those that would have received the longest vacate periods, that became cases in which the occupant did not vacate at all. Thus, in comparing treated to control groups in the vacate period outcome, we would no longer be comparing two groups identical up to statistical variation in all ways except for the offer of representation.

A thought experiment can clarify: Suppose eviction cases varied in the strength of the defenses available. In one case, the occupant had only weak justifications for not paying rent; in a second case, a rat infestation existed and the heating system was balky; in a third case, the landlord had failed to remedy lead paint in a unit with young children and had shut off the heat and water in an effort to force the tenant out. If each of the occupants in these three cases received no offer of representation, they all would have had to move, with the cases' resulting in vacate periods of two months, six months, and ten months, respectively. But if the occupants in these cases did receive an offer of representation, only the first two occupants would have had to move, and they would have obtained vacate periods of three months and seven months. The third case would have had no vacate period because the lawyer would have used the landlord's conduct in a counterclaim to obtain a settlement providing for a sizeable rent waiver, remediation of the lead paint, and the right for the occupant to stay in the unit. Thus, in the third case, there was no vacate period. Now suppose we compared the average vacate time periods in the three cases, control versus treated, and dropped the third case in the treated calculation, because there was no vacate period.[251] Under control, we have $(2+6+10)/3 = 6$ months on average, while under treated we have $(3+7)/2 = 5$ months on average. It appears that on this outcome, the

[251] This thought experiment requires us to pretend that we can observe both the treated and the control outcomes in each case, which is obviously impossible. *See* Paul W. Holland, *Statistics and Causal Inference*, 81 J. AM. STAT. ASS'N 945, 947 (1986) (defining the fundamental problem of causal inference as the inability to observe more than one potential outcome for each unit).

offer of representation was harmful to the occupants. But we know this result is wrong. Removing the third case from the treated-group calculation caused us to compare a control group with weak, medium, and strong cases in it to a treated group that had only weak and medium cases in it. The treated and control groups were no longer identical except for the offer of representation, and we got the wrong answer.

What do we do? In theory, this problem is solvable.[252] In practice, we are again confronted by a dataset with only 129 observations, and there is little that can be done. The best we can do under such circumstances is to be alert to the problem of contingent outcomes, and to evaluate whether a comparison of treated and control groups makes sense for certain variables.

D. The Problem of Outliers

Suppose a veterinarian is interested in comparing the weights of animals captured by two different teams of adventurers. The question is whether one team tends to catch heavier animals than the other. The first team puts its animals in Zoo 1, the second team puts its animals in Zoo 2, and the animals in these two zoos are the "samples" the veterinarian will weigh. All the veterinarian has available to weigh the animals is a bathroom scale. The animals in Zoo 1 are a mouse, three basset hounds, and a cheetah. Zoo 2 has a skunk, three full-grown sheep, and a docile elephant. The veterinarian gives tranquilizers to the mouse, the basset hounds, the cheetah, the skunk, and the sheep (to get them to stay still so that they can be placed on the bathroom scale) and, to avoid wasting time while the tranquilizers work, begins taking weights with the docile elephant. The veterinarian persuades the docile elephant to stand on the bathroom scale, whereupon the elephant crushes the scale into tiny bits. The veterinarian is thus unable to make any useful comparison of average weights for Zoo 1 and Zoo 2.

The elephant was an outlier. Its weight was so different from those of its compatriots that it would have been difficult to use any single scale (bathroom or otherwise) to measure all the animals. We know what happened when the veterinarian attempted to use a bathroom scale for the elephant. But a scale strong enough to weigh the ele-

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[252] For an example of how to proceed, see Zhang & Rubin, *supra* note 109. The key is to realize that the only cases that should be used for a treated-versus-control comparison of vacate periods are those in which the occupants would not have retained possession of the unit regardless of whether they received an offer of representation. For a full explanation of the same structural problem in another legal context, see D. James Greiner, *Causal Inference in Civil Rights Litigation*, 122 HARV. L. REV. 533, 583–87 (2008).

phant would have had trouble even registering the existence of the mouse.

It turns out that regardless of whether the veterinarian had used two different scales and thus gotten accurate weights for all the animals, the usual tests for whether the average weights in Zoo 1 and Zoo 2 were different in a statistically significant sense would not have been of much use, even if it seems as though the animals in Zoo 2 are probably heavier than those in Zoo 1. That is because the traditional tests of the statistical significance of differences in averages depend on three things:

(1) how close the animal weights within Zoo 1 are to each other;
(2) how close the animal weights within Zoo 2 are to each other; and
(3) how different the animal weights of Zoo 1 are from those of Zoo 2.

In the above hypothetical, the animal weights in Zoo 1 seem somewhat similar, and the animal weights in Zoo 2 seem different from those in Zoo 1, but because of the elephant, the weights in Zoo 2 are very different from each other. To a traditional statistical test of whether average animal weights between the two zoos are different, too much may depend on whether the elephant happened to be in Zoo 1 or Zoo 2. The elephant squashes the statistical test just as effectively as it squashed the bathroom scale.

For the particular hypothetical just articulated, it may not seem all that wrong to say that we cannot tell whether Zoo 2's animals really are heavier than Zoo 1's on average. Maybe a lot does depend on that one docile elephant, and there are only three animals in each group. But suppose we change the facts a little. Suppose Zoo 1 has a mouse, thirty basset hounds, and a cheetah, while Zoo 2 has a skunk, thirty full-grown sheep, and a docile elephant. Now it seems as though the animals in Zoo 2 show a pattern of being heavier than those in Zoo 1. Yet we might still run into the same trouble if we use a bathroom scale to weigh all the animals or if we ran traditional statistical tests to see whether the average animal weights differed between the two zoos. Even though we greatly increased the number of animals, and even though a strong pattern that the animals in Zoo 2 seem heavier than those in Zoo 1 appears to be emerging, the presence of the outlying docile elephant renders the animal weights within Zoo 2 so different from each other that the statistical tests may fail to declare the difference in *average* weights between the two zoos statistically significant.

What do we do when it seems as though outliers are preventing the statistical tests from assessing what common sense is telling us? The field of statistics has developed several courses of action. All must be used with caution, but if enough of them say the same thing, then that thing is probably worth believing. Two courses of action are fairly intuitive, and we use both in this Article. The first is simply to get rid of the outlier (the elephant) and measure everything else. Here, even without the elephant, it seems as though the remaining animals in Zoo 2 probably have an average weight that is higher than the average animal weight in Zoo 1. The second is to focus on the median instead of the average. Medians are more resistant to outliers than are averages; here, the median weight for Zoo 2 would not change if we transformed the elephant into a lion or a tiger or a brontosaurus. And statistical tests might well confirm what we think we already know in this hypothetical, which is that the median-weight animal in Zoo 2 (the middle-weight sheep) is heavier than the median-weight animal in Zoo 1 (the middle-weight basset hound), and that this difference is unlikely to be due to pure chance (that is, it is statistically significant).

We use the principles discussed in this section at various points in this Article, but particularly when reporting the results of the analysis of financial consequences of an offer of representation.

E. Multiple-Testing Penalties

A word regarding "multiple-testing penalties." Multiple-testing penalties respond to the following statistical phenomenon: if a researcher compares treated to control outcomes for statistical significance with respect to enough variables, mere random variation will cause some variables to show statistically significant differences even though there is no true difference between treated and control groups. Thus, if the researcher is inclined to set a particular p-value (say, 0.05) as the dividing line between statistical significance and statistical insignificance, and she compared treated to control groups on multiple outcomes, she would need to adjust (meaning lower) that p-value dividing line to guard against false positives (meaning p-values lower than 0.05) that stem solely from an accident of randomization. The danger here is particularly acute if the researcher engages in what is often referred to as "snooping" or "data mining," which is the practice of examining the data after they have been collected for statistically significant differences in outcomes that were not prespecified in the hope of finding statistical significance somewhere.

We did not apply a formal multiple-testing penalty to our results in the District Court Study for several reasons. First, doing so requires that one specify a presumptive hard dividing line between statistical significance and statistical insignificance; again, portions of the quantitative community have what we view as an unfortunate fetish for the value of 0.05. In fact, the 0.05 cutoff is arbitrary. Second, we care about two outcomes above all others: possession of the unit at the end of litigation and months of rent lost by the evictor. Although we regrettably did not follow best practices of publicly prespecifying these outcomes, the fact is that we did plan from the beginning of the District Court Study to examine them along with one other: the length of the case from complaint to judgment. As it turned out, for all three variables, the two-sided *p*-value of the difference in means for the treated and control groups was 0.01. Thus, if we were to set the significance level at 0.05 (again, we would prefer to allow the reader to set her own significance level), we would be happy to apply the "highly conservative Bonferroni correction."[253] Under the Bonferroni correction, we would declare significant comparisons with *p*-values of 0.05/3 = 0.0167 (3 being the number of comparisons) or less and conclude that all three results were statistically significant.

For the remainder of the results reported here, we concede that although we did identify them all before examining the data, we did not identify them all before data collection began. However, for the most part, these results fall into three categories: (i) tests on variables where the *p*-value is so small that any reasonable correction would not change the conclusion of statistical significance, as with the possession outcomes other than actual possession (see Table 3, *supra*), (ii) tests showing that results are not statistically significant, such as in the court-burden measurements other than case length, so applying a multiple-testing penalty would be unproductive, or (iii) tests we conducted in order to provide additional and potentially interesting, but less important, information so the exercise of adjusting a significance level is not worth the trouble.

[253] Valtteri Kaasinen et al., *Personality Traits and Brain Dopaminergic Function in Parkinson's Disease*, 98 PROC. NAT'L ACAD. SCI. 13,272, 13,275 (2001), *available at* http://www.ncbi.nlm.nih.gov/pmc/articles/PMC60860/pdf/pq013272.pdf. Note that this correction is attributed to the Italian probabilist Bonferroni and is so well known in the field of statistics that it is referred to without citation.

APPENDIX III: DETAILS OF THE EVICTORMONTHSRENTLOST CALCULATION

As discussed in the main text, we calculated a quantity called EvictorMonthsRentLost, short for Evictor Months of Rent Lost, for each case involving allegations of nonpayment of rent or a nontrivial monetary counterclaim. The basic idea here is that we would like to know the number of months of rent saved by the occupant in the summary eviction process, where the "savings" could come from either waived rent or damages paid from the evictor to the occupant. That figure is not directly estimable from court records, so we estimate instead the number of months of rent the evictor lost, which is more (although not perfectly) accessible. We consider that the number of months of rent lost by the evictor is a strong, although imperfect, proxy for the number of months of rent the occupant gained. A key advantage of this calculation is that it captures the fact that the same amount of money probably meant different things to tenants in different financial circumstances. That is, a $500 rent waiver probably meant one thing to a working tenant paying $1000 per month in rent for a market-rate apartment, and quite another to a tenant on Section 8 and SSI paying $50 per month in rent.

We calculated EvictorMonthsRentLost as follows: We accepted the amount of any arrears alleged in the complaint as true and added to this figure the monthly rent (using at this point the evictor's alleged rent in case the amount was in dispute) that accrued from complaint to judgment. We called that amount 0. In other words, if the evictor were to obtain a court judgment for, or if the record disclosed evidence of actual payment of, the full arrears alleged in the complaint plus the rent accruing during litigation, the case was coded as a 0. From the case records, we then calculated the actual amount that the evictor either received as payment or was awarded as a judgment, relative to this 0. Finally, we divided by one plus the monthly rent amount (this time using the occupant's version of what the rent was in case the amount was in dispute). Why "one plus" the monthly rent amount? For two or three occupants on Section 8, the monthly rent amount was 0, and we could not divide by 0.

Discussions with housing attorneys as well as our own telephone conversations with notice-to-quit case occupants in the Housing Court Study led us to assume that once a nonpayment-of-rent case was filed, the occupant ordinarily stopped paying further rent unless the record indicated otherwise. In the handful of cases in which the record was unclear, and when information was unavailable from either GBLS staff attorneys or the tenants themselves (via the telephone), we made this assumption in both treated and control group cases. One other detail on the calculation: unless the record affirmatively demonstrated otherwise, we assumed that the evictor kept any amount previously paid as last month's rent but that she returned any security deposit. We suspect that an offer of representation made it more likely that the occupant would actually receive a security deposit back, in which case our results again tend to understate the treatment effect.

An example may clarify: Suppose an evictor alleged that the monthly rent was $900/month, while the occupant contended that the rent was $600/month. The evictor's complaint alleged that the rent was three months in arrears, so the complaint sought $2700 plus a judgment of eviction. The case took three months to litigate, and the parties settled for a waiver of all past-due rent through a vacate date of one month later, so four months of unpaid rent accrued until completion of the case. The occupant had previously paid the evictor $900 as a last month's rent. The EvictorMonthsRentLost calculation would be as follows: -$2700 in the complaint minus 4 × $900 (evictor's version of monthly rent) accrued during the case plus $900 in last month's rent for a subtotal of -$5400, divided by $601/month (one plus occupant's version of monthly rent) for a value of -8.985 months of rent.

As this example illustrates, negative numbers represent losses to the evictor, so negative numbers were generally good for the occupant. Positive numbers could result if, for example, the evictor obtained a judgment or agreement requiring the occupant to pay court costs or attorneys' fees.

There is one further complication here worth mentioning. To calculate EvictorMonthsRentLost, we tried to include all nonhomeowner cases in which the evictor sued for nonpayment of rent or in which the occupant could have asserted monetary counterclaims. In some cases, it took an exercise of judgment to decide whether a monetary counterclaim had been asserted. For a completely objective measurement, we examined only cases in which the evictor issued a fourteen-day notice to quit; a fourteen-day notice to quit was appropriate only in cases in which the basis for the summary eviction action was nonpayment of rent. The figures for that measurement: -6.4 treated group, -1.7 control group, $p = 0.02$. In short, a statistically significant and substantively important increase in occupant savings of approximately 4.5 months of rent was still present.

Note that limiting the set of cases to those with fourteen-day notices to quit leaves out at least two important categories of cases: (i) those in which the evictor sued alleging "misconduct," but the "misconduct" involved was a chronic failure to pay rent; and (ii) those in which an evictor who had bought the unit in a foreclosure sale sued to evict a tenant who had signed a lease with a former homeowner, and in which the new homeowner or evictor sought "rent" (technically called "use and occupancy") from the occupant. This type of case could involve serious counterclaims, because depending on how the original lease was written, the new homeowner might have become responsible for upkeep of the unit and, for example, utility payments as of the moment of the foreclosure sale purchase. And it might turn out that the new homeowner was a bank or a trustee representing holders of securitized assets, entities unaccustomed to administering housing units or keeping up with utility payments.

We conclude with three reasons to believe that the EvictorMonthsRentLost measurements we report in the main text understate the actual difference a GBLS offer of representation caused.

First, regarding outliers: The treated group, but not the control group, has three pro-occupant outliers, meaning results that were so "good" from the occupant's point of view that they play the role of the docile elephant in the discussion in Appendix II. In particular, three treated-group cases resulted in cash and waived rent in the amounts of \$5000, \$12,000, and \$21,000 for tenants on Section 8 who were paying (or who should have been paying) no rent. In the \$21,000 case, the rent was actually \$36/month. In the two other cases, the rent was \$0/month. Since two of these tenants were paying no rent, \$5000 and \$12,000 represented an infinite amount of rent, and infinity was a difficult number with which to work. To avoid that problem, when calculating EvictorMonthsRentLost, we added \$1/month to the rent value used in the denominator. That gave us values of EvictorMonthsRentLost of -5000, -12,000, and -570. These values were much, much larger (meaning more negative and thus more pro-occupant) than the next closest value (around -150, also a treated-group case). Including these outliers in our calculations, our estimate for the average EvictorMonthsRentLost in the treated group went from -9.4 to -280.6. But for the reasons discussed in Appendix II, this difference in treated average (-280.6) to control average (-1.9) is not statistically significant (p = 0.40), even though the difference between -9.4 and -1.9 *is* statistically significant. We know what is going on here (again, see Appendix II), but the fact that we understand this result does not mean it makes any sense. We address this problem by removing the three "best" cases from the treated group, while keeping all the "best" cases in the control group; that gives us the figures reported in the main text. If we shift focus from a comparison of averages to a comparison of medians, the median in the treated-group EvictorMonthsRentLost is -3.0 versus a control group median of 0.0, p = 0.01. This difference of three months, as measured by medians, is less than the difference in averages discussed in the main text, but the difference in medians is still sizable and statistically significant.

The second reason why the figures quoted in the main text may be a substantial understatement is that as of the time of this writing, there was one case that had not yet reached resolution. This case was in the treated group, and in it, the GBLS staff attorneys anticipated achieving a highly favorable monetary result, on the order of -25 or less (meaning 25 or more months of rent in the occupant's favor). So the figures quoted in the text are likely to become more extreme. Note that the single most pro-occupant outcome achieved in the control group was -22. Meanwhile, if for the moment we accept GBLS staff attorneys' guess on these figures, then the ten most pro-occupant results all come from the treated group.

The third reason why the figures in the text are conservative focuses on a couple of control group cases for which the case records were not clear enough to permit discernment of the financial consequences of the litigation. In these control group cases, we presume highly pro-occupant results. By way of example, one case involved a Section 8 tenant whose portion of the rent at the time of the complaint was approximately $800 per month. The landlord filed a summary eviction action alleging an arrears of $5690. In a subsequent motion, the tenant acknowledged that he was completely "broke" and was caring for a disabled child, but offered to pay $800 total to be able to stay in the unit indefinitely. Judgment was entered for the evictor for $5690. From post-judgment filings, we drew an inference that the tenant was in fact allowed to stay in the unit (we feel confident about this inference, although it was not a certainty). From that inference, we assumed that, going forward from the date of the complaint, the relevant housing authority adjusted the portion of the tenant's rent downward to $0 (or something small enough that the tenant could pay it). That left us the question of what to do with the $5690 in arrears when calculating EvictorMonthsRentLost. Recalling that this was a control group case, and desiring to make our inferences conservative, we presumed that the landlord simply forfeited these arrears and calculated the EvictorMonthsRentLost as [-5690 (the lost arrears) + 800 (the judgment)]/[800 (the rent amount) + 1] = -6.1, meaning that we presumed that the landlord lost half-a-year's rent payments.

We think that this assumption is unlikely; the much more likely result is that the relevant housing authority adjusted the tenant's portion of the rent retroactively all the way back through the amount of the arrears. If we are right, then the landlord was paid in full (by the housing authority, except for the $800 the tenant actually did pay); otherwise, we suspect, the landlord would have requested a judgment for the full amount of the arrears (for all the landlord knew, the tenant might win the lottery the next day and be able to pay a judgment). We had no firm evidence of our suspicion, however, and could not reach the tenant by telephone. To keep our inferences conservative, we proceeded as just explained.