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THE GREINER STUDIES



Randomized Investigation of Legal Aid Outcomes

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When does legal aid to the poor make a big difference in people's lives? When does representation matter less?

Harvard Law School Prof. Jim Greiner is a statistician and lawyer. A few years ago, with Cassandra Wolos Pattanayak and later Jonathan Hennessy, Greiner started five randomized studies designed to measure the impact of legal assistance on low-income people's cases (or, more accurately, the impact of "offers" of legal assistance). Results from three of those studies are now available. These results suggest that sometimes offers of legal assistance make a big difference to low-income clients' cases, and sometimes such offers make little difference.

If that last sentence gets your dander up, I hope to help you think through why, despite this reaction, you should pay attention to the Greiner studies. However, if you accept that sometimes our work as poverty lawyers is more effective than at other times, I may help you think here about how legal aid programs can implement lessons from the Greiner studies.

New Data from Greiner

Outcome measurement is hardly new to legal services programs, which apply most of these measurement systems internally. The Legal Services Corporation requires specific reporting measures for its grantees nationwide. Many legal aid programs have collected customer satisfaction data for years.¹ In the 1990s United Way agencies nationally began requiring their grantees to use "logic models," which describe the use of inputs to conduct specific activities or programs and measure success in delivering specific short-term and longer-term outcomes.² More recently some states have experimented with "results-based accountability," which seeks to use community-linked goals to set and achieve program goals.³

¹Russell Engler, *Connecting Self-Representation to Civil Gideon: What Existing Data Reveal About When Counsel Is Most Needed*, 37 *FORDHAM URBAN LAW JOURNAL* 37, 88 (2010) ("Whether it is a positive or negative result that a program increases customer satisfaction without impacting case outcomes is part of the conversation on values and goals.").

²UNITED WAY OF AMERICA, *MEASURING PROGRAM OUTCOMES: A PRACTICAL APPROACH* (1996).

³MARK FRIEDMAN, *TRYING HARD IS NOT GOOD ENOUGH: HOW TO PRODUCE MEASURABLE IMPROVEMENTS FOR CUSTOMERS AND COMMUNITIES* (2006).

Prof. Russell Engler, in arguing that consideration of a civil right to counsel must be made “in context”—with an awareness of when counsel is most effective—has summarized many externally conducted outcome measurement studies.⁴ Greiner has also offered a list of prior studies.⁵

Greiner Studies Randomized, So Less Likely to Be Wrong

Most outcome data we collect have several problems. First, for much of them we have no point of comparison. We know that we serve countless clients, many of whom experience good results. But because we lack a comparison group—one whose members did not have the benefit of our work—we cannot know for sure whether those good results were a result of our work or would have happened anyway. And we certainly do not know whether those results are the best we can do because we usually do not compare the results to results from other client service options.

Take, for example, program data collection that is designed to describe the impact of representation on clients seeking social security disability benefits. You are likely to find that attorneys in your program succeed in obtaining social security disability benefits for many of their clients. Your data collection model may predict that when your staff represent these clients, they will have a success rate of 60 percent; and your staff may exceed that prediction by obtaining benefits for 65 percent—an apparently excellent result. However, you cannot know whether the clients might have done as well with brief advice as to how to represent themselves. You do not even really know whether the clients your staff members are representing have a 65 percent likelihood of success with no

representation at all. You may suspect this is not the case, but how can you know for sure? You might be able to screen clients intentionally for those most likely to succeed with or without representation. This screening could also happen unintentionally; the staff may be making reasonable assessments of which are “good cases.” But the difference could be vast between “good cases” and those in which clients really need your help. Disability claimants with “good cases” might get benefits even without your assistance. To maximize your impact for clients, you might want to look not for the “good cases” but for those where the applicant will lose without your help, and win with your help. If your screening rules are biased toward winnable cases, your usual “outcomes” study will not help you understand whether your staff is taking the right cases or really making a difference.

If this example seems ridiculous (“it’s obvious that these lawyers are making a difference, there’s no way the clients would win on their own!”), reserve your judgment until we discuss the first Greiner study a few paragraphs below.

Second, while prior “controlled” studies of legal help do attempt to compare two scenarios, they are susceptible to error because non-random-study designs allow the results to be biased by the rules used to set up the study. For example, Greiner discusses the numerous studies purporting to find that a juvenile who has a lawyer in a delinquency proceeding is more likely to be detained or to suffer other restrictions. These studies looked at court files, identified the juveniles who were represented by counsel and those who were not, and compared their results. But these studies suffer from what statisticians refer to as “selection effects”: the studies ignore that the de-

⁴Engler, *supra* note 1, at 44–66; see also Russell Engler, *Toward a Context-Based Civil Right to Counsel Through “Access to Justice” Initiatives*, 40 CLEARINGHOUSE REVIEW 196 (July–Aug. 2006).

⁵D. James Greiner & Cassandra Wolos Pattanayak, *Randomized Evaluation in Legal Assistance: What Difference Does Representation (Offer and Actual Use) Make?* 49–55, SOCIAL SCIENCE RESEARCH NETWORK (Sept. 7, 2011), <http://bit.ly/HOo54g>. This long list of studies, which use a variety of methodologies, includes a study of housing cases conducted in Manhattan in the early 1990s. One of a few prior randomized studies of legal work, it found very significant benefits of representation (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 AND SOCIETY REVIEW 419 (2001). Unfortunately, as Greiner notes, the study’s methodology was altered midstream (D. James Greiner et al., *The Limits of Unbundled Legal Assistance: A Randomized Study in a Massachusetts District Court and Prospects for the Future* 4 n.9, SOCIAL SCIENCE RESEARCH NETWORK (March 29, 2012), <http://bit.ly/HtHJ7T>).

cision whether to appoint counsel is not random. When judges think incarceration is likely, they are more likely to assign counsel, and so higher incarceration rates for represented juveniles would be expected. Rather than assessing the effectiveness of lawyers, these studies confirm that because judges appoint counsel for youth who are about to be incarcerated, the youth with counsel are more likely to be incarcerated.⁶

While the data that legal services programs have heretofore collected do demonstrate that legal aid is creating valuable results, the data are not comparative or accurate enough to make the best resource allocation decisions. The legal aid network has a small fraction of the staff and resources we need to represent all in need. How can we get better information about which advocacy strategies are most effective and in which cases do we make more (or less) of a difference?

Randomization

Greiner and others argue that the most valuable way to answer resource allocation and strategy questions is to conduct randomized controlled studies. The basic idea is this: in practice areas in which we turn away clients we should, for some period, test our case-acceptance assumptions and beliefs. We do this by establishing the pool of likely clients and, instead of applying finer and finer distinctions among these clients, allow a researcher using a randomizing technique to tell us which clients we will represent.⁷ We collect outcome data for the two sets of clients and turn the data over to the researcher for analysis. This is Greiner's approach in three studies available thus far.

Some suggest that such randomized studies are not ethical. However, studies of human subjects are ethical if conduct-

ed appropriately, and Harvard's Committee on the Use of Human Subjects approved all of Greiner's studies. Legal services everywhere in the United States are, as the statisticians would say, "oversubscribed," that is, far more eligible people want legal aid than can be helped with current resources. As long as we are turning so many people away, why not randomize the process and learn from the results?

Others might object on the basis that our triage system reflects well-founded, incontrovertible knowledge. The legal aid network uses scarce resources to help people in great need; if our current decisions about whom to help and how to help them are fully and well informed, abandoning those good decisions in favor of randomized assignment of counsel would be immoral. Numerous writers have referred to this as the "parachute problem"; in the words of one, "[w]e shouldn't allow randomized tests of parachutes, because we already have strong evidence that they are effective."⁸

However, if you are willing to consider that our triage decisions are based on beliefs that could be wrong, the most moral course of action is likely randomized assignments that produce information to help us design a better triage system. If the beliefs that drive triage are wrong, the triage system is arbitrary: "[R]andomness seems arbitrary, but arbitrariness is often more troubling when it is non-random."⁹ And the Greiner studies suggest that our beliefs are wrong more often than we might think.

Greiner has released results from three studies: the Harvard Legal Aid Bureau's unemployment practice; an expanded housing law practice by Greater Boston Legal Services lawyers in Quincy, Massachusetts; and another expanded hous-

⁶Greiner and Pattanayak, *supra* note 5, at 62–63. Another problem with retrospective studies is what has been called the "Texas Sharpshooter Fallacy"—an unskilled gunslinger who shoots random holes in the side of a barn, then draws a bull's-eye around the place where shots randomly clustered (see Atul Gawande, *The Cancer Cluster Myth*, *NEW YORKER*, Feb. 8, 1999, at 34).

⁷Clients not represented in these studies receive other assistance to proceed *pro se*, but not representation.

⁸Michael Abramowicz et al., *Randomizing Law*, 159 *UNIVERSITY OF PENNSYLVANIA LAW REVIEW* 929, 973 (2011).

⁹*Id.* at 969. Another recognized expert in developing randomized studies to improve human services is Esther Duflo, professor of economics at Massachusetts Institute of Technology and coauthor of *POOR ECONOMICS: A RADICAL RETHINKING OF THE WAY TO FIGHT GLOBAL POVERTY*; her work is described in Ian Parker, *The Poverty Lab*, *NEW YORKER*, May 17, 2010, at 78.

ing law practice by Neighborhood Legal Services lawyers in Lynn, Massachusetts.

Harvard Legal Aid Bureau Unemployment Compensation Study

At the Harvard Legal Aid Bureau, law students, trained and supervised by attorneys, serve clients in a number of areas of legal need. Greiner studied the unemployment compensation cases. Screeners identified callers who were eligible for services: they were low-income, had been denied unemployment compensation on initial application, and sought representation for their “first-level appeal.” Virtually all callers agreed to participate in the study.¹⁰ Callers were divided randomly into two groups; one group was offered representation by second- and third-year law students and the other group was told that representation was not available and sent information about other resources.¹¹

The results of the study are startling. People offered representation had no greater success in getting unemployment compensation than people who were not offered representation. Furthermore, those in the study who were *not* represented and yet won their appeals received their benefits more quickly on average.¹²

Since its release, Greiner’s paper on the Harvard Legal Aid Bureau study has been the subject of much discussion. One criticism has been that the study randomized “offers” rather than “receiving” of legal services. As is always true in studies of this type, including the two housing studies summarized below, some of the people offered legal services did not follow through; some people denied legal services from the Harvard Legal Aid Bureau received them elsewhere. In fact, some of the Harvard students were so concerned about certain individuals be-

ing turned away that they gave the prospective clients’ contact information to Greater Boston Legal Services lawyers, who in turn contacted the individuals to offer representation. Because this study was not designed to compare those represented by Harvard Legal Aid Bureau to those who went unrepresented, some argue that the study does not yield useful information about the effectiveness of Harvard Legal Aid Bureau’s representation in unemployment cases.¹³

Greiner gives two responses. First, the studies measure the impact of what legal services programs control: the offer of assistance. We cannot control whether people accept our offers or whether they find representation elsewhere.¹⁴ Thus Greiner explains that he is measuring what a program actually does (offer assistance that may or may not be accepted), not what the program hopes to do (assist all the people offered assistance). Second, Greiner argues that although the data do not allow definitive answers, over 90 percent of the group offered assistance from the Harvard Legal Aid Bureau were represented at their hearings, versus about 40 percent of the group not offered assistance. Yet the difference in success rates between the two groups was within the margin of error and therefore not statistically significant—that is, the difference could be random rather than meaningful.¹⁵ Thus, although twice as many people in the Harvard Legal Aid Bureau group were represented as in the group of people that the bureau did not offer to help, that huge difference did not measurably affect the rate of success. If Harvard Legal Aid Bureau representation had a large impact on the rate of success, that result would have been apparent even though those not offered representation were a mixture of represented and unrepresented.

¹⁰Greiner & Pattanayak, *supra* note 5, at 21 n.94.

¹¹*Id.* at 21.

¹²*Id.* at 26–40.

¹³Bob Sable, What Difference Representation—A Response, CONCURRING OPINIONS (March 28, 2011), <http://bit.ly/GTSwal>.

¹⁴Greiner & Pattanayak, *supra* note 5, at 8–12.

¹⁵*Id.* 47.

The key point: there is good reason to doubt something we may tend to assume—that almost every time we offer to represent a client, we change that client’s outcome for the better. Greiner’s Harvard Legal Aid Bureau study does what all useful research does: it makes us think and ask more questions. If we are to reach a deeper understanding, these questions must be the subject of further research. Perhaps the study demonstrates that legal aid advocates in the Boston area have succeeded, after decades of effort, in pushing the unemployment compensation system to be truly accessible to the unrepresented.¹⁶ Perhaps unemployment hearing officers impose a higher burden of proof on claimants who are represented; there is evidence that sometimes litigants “lawyer up” when there is a lawyer on the other side.¹⁷ Or perhaps those who find and call the Harvard Legal Aid Bureau are among the most capable of unemployment claimants, and the people who would benefit most from representation are less capable of finding the bureau or less likely to call it.¹⁸

Without further research, we cannot know whether these hypotheses explain the study’s results. But that lack of certainty is a crucial bit of knowledge. That we cannot yet agree on why Harvard Legal Aid Bureau offers of representation had no measurable impact on clients confirms that we cannot be confident in our triage designs. Without research-based evidence, we do not know which triage or other service decisions are best. To argue that the beliefs that underlie our triage decisions are unshakably true is no longer reasonable, nor is it reasonable any longer to argue that to test our triage decisions against randomized study is unethical.

Greater Boston Legal Services Housing Study

The results of Greiner’s study of eviction representation offered by attorneys from Greater Boston Legal Services in a district court in Quincy, Massachusetts, are equally startling.¹⁹ In this study all eligible applicants for assistance were offered instruction on the legal process for evictions, along with help in filling out an answer-and-discovery request. The applicants were then randomly divided between those who would be offered representation and those who would not be offered further assistance.

Greiner found that the tenants who were offered representation were far more likely to be able to remain in their homes—66 percent compared to 38 percent.²⁰ The tenants offered representation also ended up much better off financially—on average, tenants offered representation received 9.4 months worth of cash back or rent waivers, compared to 1.9 months’ worth of cash or rent waiver for those not offered representation.²¹ Some measures suggested a reduced need for judicial resources; half as many judicial rulings were required in proceedings when tenants were offered representation.²²

Greiner notes that 70 percent of the tenants in the study had their first contact with legal aid as a result of the “legal aid provider’s proactive, individualized, timely, and selective outreach and intake system,” which “provides a potential explanation for the results we observed in the District Court Study reported here, as well as for the contrast between results in the District Court Study and the

¹⁶*Id.* 48.

¹⁷Orley Ashenfelter & David Bloom, *Lawyers as Agents of the Devil in a Prisoner’s Dilemma Game* (National Bureau of Economic Research, Working Paper No. 4447, 1993), <http://bit.ly/GTNFB>.

¹⁸Greiner & Pattanayak, *supra* note 5, at 49.

¹⁹D. James Greiner et al., *The Limits of Unbundled Legal Assistance: A Randomized Study in a Massachusetts District Court and Prospects for the Future*, SOCIAL SCIENCE RESEARCH NETWORK (March 29, 2012), <http://bit.ly/HtHJ7T> [hereinafter Greiner (Quincy)].

²⁰*Id.* at 34

²¹*Id.* at 37. Unpaid rent that was forgiven to settle the case and payment from the landlord to the tenant to vacate the premises and to settle the case were the primary sources of cash back or rent waivers.

²²*Id.* at 41. Greiner notes that, unlike some other results, this finding is right on the border of statistical significance.

[Neighborhood Legal Services] Housing Court Study [summarized below].”²³ In the Quincy study, legal aid’s offer of assistance to a tenant made all the difference in the world.

Neighborhood Legal Services Housing Study

The third set of results available thus far comes from Greiner’s study comparing eviction representation and “lawyer of the day” assistance offered by Neighborhood Legal Services in a specialized Housing Court in Lynn, Massachusetts.²⁴ Offered by Neighborhood Legal Services are a standard eviction defense and a “lawyer of the day” program, in which *pro se* eviction defendants can get in-court advice and representation in negotiation and mediation efforts but not for motions or hearings. The “lawyer of the day” program uses both *pro bono* private lawyers and the same legal aid lawyers who represent other clients. The Lynn study measures the difference in results for tenants who were offered legal aid representation and those who were offered *pro se* assistance and then were referred to the “lawyer of the day” program.

Greiner found no measurable difference in the outcomes experienced by these two groups of tenants. Of those offered representation, 33 percent were able to remain in their homes, compared to 34 percent of those referred to the lawyer of the day.²⁵ Tenants offered representation received an average of 1.8 months’ worth of cash back or rent waivers, compared to 1.6 months’ worth of cash back or rent waiver for those referred to the lawyer of the day—a difference that was not statistically significant.²⁶

One possible meaning of the Lynn study is that “lawyer of the day” programs are as effective as full legal representa-

tion. However, Greiner cautions against jumping to that conclusion.²⁷

Comparing the Quincy and Lynn Results

Tenants offered representation in Quincy fared far better than those not offered representation. Tenants referred to the “lawyer of the day” program in Lynn did just as well as those offered representation. On the surface, one might conclude that since these studies suggest that lawyer of the day is just as good as representation in Lynn (but presumably less resource-intensive), and representation is far better than going without a lawyer in Quincy, legal aid programs should focus scarce eviction-assistance resources on “lawyer of the day” programs. Or one might conclude that lawyers provide less advantage in a specialized housing court than in a court of general jurisdiction.

But Greiner thinks something else may be going on. He undertakes further analysis that, he concedes, is risky for a statistician because the Quincy and Lynn studies were conducted separately with no control or randomization between them. As a result the concern emerges as speculation rather than a proven result: is it possible that *both* Lynn tenants who are offered representation and Lynn tenants referred to lawyer of the day are doing *badly* in court? Greiner is struck by the similarity in the rates of retaining possession among three groups of tenants:

- Lynn tenants offered representation: 33 percent;
- Lynn tenants referred to lawyer of the day: 34 percent; and
- Quincy tenants given *pro se* resources but no representation: 38 percent.

By contrast, recall that 66 percent of Quincy tenants offered representation

²³*Id.* at 8, 23.

²⁴D. James Greiner et al., *How Effective Are Limited Legal Assistance Programs? A Randomized Experiment in a Massachusetts Housing Court*, SOCIAL SCIENCE RESEARCH NETWORK (March 15, 2012), <http://bit.ly/H6Ug15> [hereinafter Greiner (Lynn)].

²⁵*Id.* at 36.

²⁶*Id.* at 38.

²⁷*Id.* at 49 *et seq.*

retained possession of their homes. What might be the explanation?

One possibility is that the two studies are not really comparable, and we should take the numbers at face value. In this view, legal aid can do nothing to keep more than about a third of tenants in Lynn in their homes, and our lesson is that lawyer of the day is not only cheaper but as effective in helping people as offering representation. In Quincy, where there is a demonstrated opportunity to keep two-thirds of tenants in their homes, a “lawyer of the day” approach may do just as well; that question has not yet been studied.

If the two studies are not comparable, the reason could be that the Lynn court environment—some combination of judges’ predispositions and local legal culture—is much less receptive to tenants. That the results for *pro se* tenants in the Quincy study are similar to the results for *all* tenants in the Lynn study is certainly notable. In Lynn perhaps only tenants who will prevail under any circumstance will keep possession of their homes, and no form of legal help will improve those odds. If true, this would suggest that legal aid programs devote resources to educational or advocacy efforts aimed at changing the legal culture in Lynn in order to help it better differentiate cases of tenants with valid but less obvious claims.

The Quincy lawyers’ “proactive, individualized, timely, and selective outreach and intake system” may also explain their success.²⁸ In considering the results of the Harvard Legal Aid Bureau study, one might think that *some* set of clients must have been unable to handle these cases on their own, no matter how welcoming the administrative system. The Quincy lawyers may have found those clients through their outreach and intake system.

Another possibility Greiner suggests is that a difference in the lawyering strategies used in representation cases may explain the difference in results.²⁹ In Quincy the legal aid lawyers pushed discovery very aggressively and requested jury trials.³⁰ This was not the case in Lynn, where the lawyers filed very few motions in any cases, even those in which they provided full representation. Requests for jury trials on behalf of tenants who were represented increased only slightly.³¹ Perhaps particularly aggressive motion practice with frequent jury requests makes a difference for tenants, while relying on negotiations does not. Or perhaps aggressive lawyering makes a difference in Quincy but not in Lynn.

Greiner notes numerous other differences that might matter between the studies. A few more Quincy tenants lived in subsidized housing. A few more Lynn tenants needed an interpreter.³² While none of these factors leaps out as a likely reason for the starkly different results, one or more of the factors could play a role. And maybe some other factor that Greiner did not identify is driving the difference.

Or maybe ... or maybe ... or maybe

Limits of Randomized Studies

Randomized studies can bring out unbiased information about areas of legal aid work that involve large numbers of individual cases and show concrete, measurable results. Studies can, for example, impart a wealth of information about how to maximize effectiveness for clients facing eviction, seeking child support, or seeking disability or other public benefits.

Future randomized studies could offer information about whether legal representation or other assistance has a long-term impact on other measures of client

²⁸Greiner (Quincy), *supra* note 19, at 8.

²⁹Greiner (Lynn), *supra* note 24, at 50–56.

³⁰*Id.* at 49–51.

³¹*Id.* at 50–51.

³²*Id.* at 51–52.

well-being. Perhaps representing people seeking Supplemental Security Income benefits can be shown to reduce homelessness or emergency room visits as a result of better income and health insurance. Perhaps representing immigrant victims of domestic violence seeking legal immigration status can be demonstrated to increase the likelihood that clients live in stable homes and pay taxes. Studies that look at these effects are, however, likely to require more resources for research.

Similarly some results that we now describe as “intangible” may be demonstrable through randomized study. One reason we provide free legal aid to low-income people is to increase access to justice. A randomized study could measure whether people offered significant *pro se* supports and those offered representation are less likely to fail to appear in court than people who are not offered any assistance or support.

However, some aspects of legal aid work—important and time-consuming though they may be—cannot be assessed through randomized studies because they are unique rather than numerous. While policy advocacy and class litigation can bring vital help to thousands of low-income clients at a time, comparing more (and less) successful policy or class-litigation approaches through randomized study likely would be difficult. We should be careful not to devalue important work simply because we cannot evaluate it through randomized study (a point with which Greiner agrees).³³

Conclusions

Randomized study of legal aid is just beginning. To answer more of our questions, more studies are needed and will no doubt be undertaken, requiring more researchers and more legal aid programs committed to inquiry. In the long run, this effort will benefit our clients. One commentator compares research into the efficacy of legal aid interventions with research into the efficacy of medical treatments—an ef-

fort that is now central to medical practice but at one time was new:

[H]ealthcare outcomes research is resource intensive and time consuming. Lack of easily accessible electronic databases, particularly long-term outcome tracking databases, can result in needing manual data collection which further adds to the resources needed to conduct such research. Decades of empirical and health services research was needed to pave the way to the current successes in using outcomes research to evaluate the quality of healthcare. This will be a challenge for evaluating the quality of legal services.³⁴

Although data from randomized studies of legal aid are just beginning to come in, an immediate and tangible result can already help to energize legal aid programs. The questions surrounding interpretation of the studies prompt us to ask ourselves (or to revisit) essential questions:

- Which areas of law make the most compelling claims on our limited resources?
- In which forums is our help most needed or effective?
- How much does *pro se* assistance help clients?
- Which representation strategies or approaches have the best impact for our clients?
- Which clients need us the most, and are we effectively reaching out to and prioritizing those clients?

Many of us in the legal aid community believe strongly that we know the answers to many of these questions. Yet many of us disagree about what are the “right” answers. We must put our “knowledge” (or our beliefs) to the (randomized) test in order to maximize the benefit of legal services resources to the clients of the future.

³³See Greiner & Pattanayak, *supra* note 5, at 69–71.

³⁴Ramesh C. Sachdeva, *Measuring Quality of Legal Services—Implementing Outcomes Research in Law*, 11 MICHIGAN STATE UNIVERSITY JOURNAL OF MEDICINE AND LAW 1, 17 (Winter 2007).



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