different demands made on the lawyer. These are presented as polar opposites. Please circle the number that best represents your position in relation to the two opposites. If the situation in your practice is midway between poles, circle code 3; if your situation is at one or the other extreme, circle 1 or 5; if your position leans somewhat to either pole, circle 2 or 4.

A
The type and content of my practice is such that even an educated layman couldn’t really understand or prepare the documents.  
1  2  3  4  5

B
A para-professional could be trained to handle many of the procedures and documents in my area of law.

The scale was reverse coded so that higher ratings indicate greater complexity. As with the measure of substantive law complexity, the ratings for each field were standardized. The fields investigated by the studies are average or below average in procedural complexity relative to the whole scope of lawyers’ work. The analysis distinguishes between studies in fields of law with procedural complexity scores below average (e.g., family), comprising 8 percent of cases and 17 percent of studies, and about average (e.g., personal income tax), comprising 92 percent of cases and 83 percent of studies.

Complexity ratings are highly stable over the period covered by extant research. The Chicago Lawyers Surveys were administered in 1975 and 1995, permitting comparison of experts’ and practitioners’ ratings of the complexity of substantive law and legal procedure for the fields of law that were included in both surveys (see Heinz et al. 2005:86–88). For both types of legal complexity, the measures used in the meta-analysis are correlated at 1.0 between 1975 and 1995: fields of law that were rated above, below, or about average in 1975 were also rated, respectively, above, below, or about average in 1995 (see also Heinz et al. 2005:328 note 10).

Procedural complexity is also measured by the way adjudication is organized. In the traditional trial courts familiar from television, each party and its representatives must identify legal issues for the court and construct their own legal arguments about them. This adversarial model of adjudication requires parties to understand how to present their cases in legally comprehensible terms and in a formal legal style. People inexperienced in courtroom advocacy are often unfamiliar with these expectations (Conley and O’Barr 1990; Genn 1993; O’Barr and Conley 1988). By contrast, some hearing forums, such as small claims courts and benefits tribunals, were explicitly designed to be simpler to understand and more accessible to people not represented by lawyers (Genn 1993; Ruhanka, Weller, and Martin 1978; Yngvesson and Hennessey 1975). In these more inquisitorial settings, the judge or hearing officer may request specific information from the parties and intervene to assist parties in developing their cases. Rules of evidence are typically relaxed, so information can be presented less formally. The analysis distinguishes between traditional courts and these simplified settings. Eight (or 44 percent) of the studies, contributing 37 percent of total cases, investigate representation in traditional trial courts; seven studies are of tribunals and three are of small claims courts, with these two types of more inquisitorial fora providing 63 percent of the cases in the analysis.

Methods
A central challenge in calculating causal effects from observational data is distinguishing selection from causation (Cook and Campbell 1979; Manski 1995). Statisticians and econometricians have developed a common language for discussing causality in observational data, the counterfactual framework (Winship and Morgan 1999). This framework draws on the treatment and control designs of experimental research and rests on the assumption that all observed units of analysis have potential outcomes in both treatment and control states, “even though they can actually only be observed in one state” (Winship and Morgan 1999:662).
Formally, for each unit we observe a $Y^T$, the outcome under the condition of treatment, or a $Y^C$, the outcome under the condition of control, but we assert that for each unit of analysis there exists the outcome we do not observe: for each observed $Y^T$ (outcome observed for a represented litigant) there is a potential $Y^C$ (outcome achieved if that litigant had not been represented) and vice versa. The effect of treatment on the outcome can then be defined as the difference between $Y^T$ and $Y^C$ for each observation, or

$$\delta = Y^T - Y^C \quad \text{(Winship and Morgan 1999:663).}$$

This difference, $\delta$, or, more typically, an estimate of its true average in a population, is what a researcher interested in causation wants.

The challenge is that observational data do not provide enough information to calculate $\delta$, because each observation provides only one $Y$, the one for the state it actually experienced. Thus, it is possible to think of the problem as one of insufficient information, that is, as a problem of identification. Drawing on Manski's (1989, 1990, 1993, 1995) work on identification problems to estimate the impact of lawyers on case outcomes, I use data to determine the bounds within which outcomes would fall under a counterfactual state of affairs in which lawyer representation were universal, by comparing the range of possible outcomes we could observe if cases that did not receive the “treatment” (lawyer representation) had received it to the outcomes actually observed in the studies.

This strategy estimates the magnitude of lawyers' impact by comparing observed outcomes under the current policy of selection into representation to potential outcomes under a widely embraced “civil Gideon” policy, a state of affairs in which all focal parties involved in the studied types of civil litigation would be represented by attorneys. The Universal Lawyer Representation (ULR) counterfactual is practically significant: it is a potential legal reform surrounded by considerable activity and debate in the United States (e.g., Abel 2006; Engler 2006; Gunn 1995; Scherer 1988). Although the effect estimates are constructed by applying a hypothetical policy change, they are not precise predictions of the impact of that change. This is because the estimates assume that changing the status of any given unit of analysis would not affect the potential outcomes for other units (the “stable unit treatment value assumption” [Winship and Morgan 1999:663]). While plausible, this assumption might not always hold. For example, some studies have found that even a few ordinary litigants start to appear with lawyers in fora where most litigants have previously been unrepresented, judges change their behavior and more accurately apply the law in all cases, thereby changing the potential outcomes for all focal parties, both represented and unrepresented (Fusco, Collins, and Birnbaum 1979). In addition, changing the representation practices of focal parties might also change the behavior of their opponents. For example, if all tenants facing eviction were suddenly provided with lawyers, landlords might change their behavior by moving to universal representation themselves or by more often negotiating solutions informally (Lazerson 1982). The difference between outcomes in a hypothesized counterfactual state of affairs and observed outcomes informs us about the status quo effect of lawyers on case outcomes, but this information alone cannot predict the future.

Effect sizes are expressed as average differences in outcomes between lawyer representation and no representation and between lawyer representation and representation by nonlawyer advocates (NLAs). I use odds ratios because these are a convenient and easily interpretable measure of magnitude (Fleiss 1994). When an odds ratio is 1, there is no average difference between cases with lawyers and those without: the odds of winning with a lawyer are just as good (or bad, as the case may be) as the odds of winning without one. If this ratio is larger than 1.0, lawyer representation is associated with a greater probability of winning than is the case in the comparison group, with the size of the relationship represented in the size of the odds ratio. If the odds ratio is less than 1.0, parties
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Notes
1. One must recognize, of course, that none of these domains is inherently more interesting or newsworthy than others. Their dominance may well be rooted in masculine standards for media coverage (Rodgers and Thorson 2003; Ross 2007, 2010).
2. We also estimated models that included alternative measures, such as the overall proportion of female executives in the state, and we obtained similar results.
3. In addition to this measure, which we present in the analysis, we also estimated analyses with alternative measures, such as the standardized number of paid entertainment employees, and we obtained similar results.
4. This becomes even clearer when noting that the rate of women working in journalism has not increased over the past two decades and was the same in 2011 as in 1999: only 36.9 percent of all reporters, photographers, editors, producers, and supervisors were women (American Society of News Editors 2011).

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