Lawyers With More Experience Obtain Better Outcomes

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Abstract

Work experience acquired through on-the-job-training has been shown to lead to greater success in many occupations, but evidence of a causal connection between experience and success is sparse for appellate lawyers. Do experienced attorneys obtain better outcomes for their clients? Adopting a strategy for causal inference that could be applied to almost any peak court, we assess how similarly-situated novice and experienced attorneys fare against a comparable—and high quality—opponent: the federal government. We find that, on average, the outcomes obtained by experienced attorneys are significantly better than the outcomes they would have obtained had they been novices. This result shores up the importance of attending to attorneys in models of judicial behavior.

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With only limited fanfare, litigation in the U.S. Supreme Court has undergone a fundamental—and abrupt—transformation. In the not-so-distant past, novice lawyers were the norm. No longer. Today nearly two-thirds of the private-sector attorneys appearing before the justices had argued at least one prior case, compared with under a quarter in the 1980s (Online Appendix 2.2). These experienced lawyers, many of whom work in Washington, D.C. firms, are so in demand that they earn an hourly wage 15 times higher than the average U.S. lawyer (Weiss, 2015).

The emergence of a highly professionalized private-sector bar in the United States and elsewhere (Chang, Chen and Lin, 2019; Hanretty, 2019) raises a number of important questions, not the least of which is whether the outcomes obtained by experienced attorneys are significantly better than the outcomes they would have obtained had they been novices. "Of course" would seem the obvious answer, and one reinforced by long-standing theory on human capital (*e.g.,* Becker, 1962, 1964; Mincer, 1962). Tracing to Adam Smith's *The Wealth of Nations* (1776), human capital theory holds that through work experience, individuals acquire on-the-job-training, sometimes highly specialized training, which leads them to be more productive, more successful, and, in turn, to earn higher wages (*e.g.,* Brown, 1989; Maranto and Rodgers, 1984). In other words, the "experience-productivity profile slope[s] upwards" because many jobs, including appellate lawyering, require learning (Medoff and Abraham, 1980, 703; see also Abrams and Yoon, 2007). For this reason, litigants who retain experienced Supreme Court lawyers should be at an advantage relative to litigants who hire first-timers,[1] just as are auction houses that hire art connoisseurs (Ashenfelter and Graddy, 2003; Bonus and Ronte, 1997), students taught by experienced teachers (Chetty, 2011), and companies that invest heavily in "EX" (employee experience) (Morgan, 2017).

Evidence of the importance of experience in appellate lawyering is not hard to find (*e.g.,* Haire, Lindquist and Hartley, 1999; Szmer, Songer and Bowie, 2016; McAtee and McGuire, 2007; McGuire,

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[1]Justice Robert H. Jackson (1951, 802), a seasoned Supreme Court litigator, put it this way: "Experience before the Supreme Court is valuable, as is experience in any art. One who is at ease in its presence, familiar with its practice ... holds some advantage over the stranger to such matters." Many scholars agree (Feldman, 2016; Lazarus, 2008; Wahlbeck, 1997), though they would add that the experienced attorney—just as the experienced lobbyist—is better able to convey information the justices need to reach the results they most desire (Johnson, Wahlbeck and Spriggs, 2006; Lazarus, 2008; McGuire, 1993).

1995, 1998).[2] But that evidence is indirect because the studies (all observational) show only an association between experience and case outcomes (or judicial votes); they were not designed for causal inference (Abrams and Yoon, 2007, 1153). Excluded are crucial steps such as assessing balance between pre-treatment covariates (*e.g.,* whether the attorney represented the petitioner or respondent) and the treatment assignment variable (whether the attorney was a novice or not) and then "limiting inferences to a carefully selected matched subset" of the data (Iacus, King and Porro, 2019, 46; see also Cochran and Rubin, 1973; Ho et al., 2007). Equally vexing for causal inference is the inclusion in the studies of post-treatment covariates—variables occurring after the attorneys briefed or argued the case, such as whether the Court invalidated a law or issued a decision with multiple legal provisions or whether the *New York Times* reported the decision on its front-page *the day after* the Court issued it. Even incorporating the legal area of the dispute, if identified after resolution, may be suspect owing to issue fluidity (McGuire and Palmer, 1996). As a general matter, only pre-treatment covariates should be included in any causal analysis because treatment can affect post-treatment covariates (Rosenbaum, 1984; Wooldridge, 2005).

To attend to these and other complications arising from the use of observational data to estimate the causal effect of attorney experience, we deployed a two-stage strategy that could be applied to almost any apex court in the world. We first assembled a dataset uniquely suited to the task at hand—here, a dataset of government cases, which allows for comparisons between novice and experienced attorneys without requiring a host of assumptions about which side has the "net advantage" (see note 3). Second, we adapted the theory and tools of causal research outlined in Iacus, King and Porro (2019) and elsewhere (Iacus, King and Porro, 2012, 2011; Ho et al., 2007) to make a valid causal inference over whether the outcomes obtained by experienced attorneys are significantly better than the outcomes they would have obtained had they been novices.

Applying the theory and tools to the data yields results consistent with human capital theory: on average, attorneys with experience, relative to first-timers, are significantly more likely to win their cases and attract the votes of justices. These findings suggest that models of U.S. Supreme Court decisions that exclude attorney experience are underspecified. The same might hold for the federal circuits and other apex courts in the U.S. states or elsewhere.

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[2]Then again, some studies find no effect of experience. Compare, *e.g.,* McAtee and McGuire (2007) and Johnson, Wahlbeck and Spriggs (2006).

1 Using Government Cases to Estimate the Causal Effect of Attorney Experience

To estimate the effect of attorney experience on case outcomes and votes, we developed a dataset of all orally argued U.S. Supreme Court decisions, issued between the 1980 and 2017 terms, in which the Office of the Solicitor General (OSG) represented the petitioner (appellant) or respondent (appellee) (Online Appendix 1). In other words, we focus exclusively on litigation in which one side's attorney opposed the federal government.

This approach has two advantages. First, it provides leverage on assessing attorney experience because many studies demonstrate the uniformly high quality of the government's lawyers (*e.g.,* Black and Owens, 2012; Lazarus, 2008; Salokar, 1992; Scigliano, 1971). Had we analyzed non-OSG cases, the problem of imbalances in quality, experience, and resources between the *two* sides in any given case would have emerged. Scholars have tried to solve this problem by weighting, categorizing, or otherwise accounting for the "net advantage" of one litigant or attorney over the other (*e.g.,* McAtee and McGuire, 2007; Sheehan, Mishler and Songer, 1992; Szmer, Johnson and Sarver, 2007)—all solutions that require a host of (usually unverified) assumptions.[3] A focus on OSG cases (that is, holding constant the attorneys' opposition) avoids the problem altogether because the comparison is not how novice and more experienced attorneys fare when they face each other; it is rather how both types of attorneys fare against a comparable opponent: the OSG. Of course, we verify the assumption embedded in this design choice; namely, that the OSG's attorneys have been of uniformly high quality (Online Appendix 5.1).

A second advantage of the federal-case approach is that it may easily transport to other high courts. Contemporary studies demonstrate that the central government is a highly successful litigant in peak courts in Canada (Alarie and Green, 2017), Israel (Eisenberg, Fisher and Rosen-Zvi, 2011), Italy (Pellegrina and Garoupa, 2013), Norway (Grendstad, Shaffer and Waltenburg,

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[3]Examples include (1) within-category homogeneity in quality and expertise (*e.g.,* all individuals or all states or all businesses are equivalent); (2) correct ordering of litigant types (*e.g.,* small businesses have better representation than unions); and (3) interval-level scaling and subtraction (*e.g.,* the difference between big business versus local government is one-third the difference of individuals versus small business).

2015), Taiwan (Chen, Huang and Lin, 2015), and the UK (Hanretty, 2019), among others. Further, because governments are typically involved in a large fraction of litigation, a sufficient number of cases is rarely a problem. For example, the central government is a party in over 50% of the cases in the Israeli Supreme Court (Weinshall, Epstein and Worms, 2018) and in about a third of cases in the Supreme Court of India (Haynie et al., 2007). The numbers in our dataset are comparably large: between the 1980 and 2017 terms, the OSG represented one of the sides in almost 30% of all orally argued cases (9,671 votes cast by 21 justices in 1,080 cases) (Online Appendix 1).

For each case in our dataset, the treatment is whether or not the attorney opposing the OSG had argued at least one prior case in the Supreme Court regardless of whether the case is in our dataset or not; that is, we count *any* prior argument.[4] (We assess robustness using various cutoffs, such as five or more prior cases. Online Appendix 5.2.) In 33% of the cases, the non-OSG party was represented by an experienced attorney. The outcome variable is whether or not (1) the Court held in favor of the private-sector attorney (35% of the cases) or (2) the justice voted in favor of the private-sector attorney (39% of the votes) (Online Appendix 2.1).

2 Estimating the Causal Effect of Attorney Experience

2.1 Strategy and Main Results

The question we ask is whether the outcomes obtained by experienced attorneys are significantly better than the outcomes they would have obtained had they been novices—in other words, the quantity of interest is the average treatment effect on the treated (ATT) for the cases in the sample. A simple bivariate regression of case outcome (in favor of the private-sector attorney or the government) on whether or not the attorney had argued at least once before shows that experience has a positive and significant effect on winning. Likewise, justices are significantly more likely to vote in favor of the side represented by attorneys with experience (Online Appendix 4 and Figure 2 below).

But these regressions prove little. Because cases are not randomly assigned to treatment,

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[4]To assess the effect of attorney experience, the literature focuses on the attorney arguing the case rather than the counsel of record (e.g., McAtee and McGuire, 2007; Johnson, Wahlbeck and Spriggs, 2006). We followed suit, though this was not an especially consequential choice: the arguing attorney and lead counsel are the same in about 90% of the cases (Online Appendix 2.2).

experienced and novice attorneys are likely to differ in ways related to outcomes. And, in fact, on almost all pre-treatment covariates suggested in the literature, significant differences exist between the two attorney types; for example, experienced attorneys represented the petitioner in 54% of the cases versus 44% for the first-time litigators (Online Appendix 3.1). More to the point, the relationship between the covariates and treatment assignment is sufficiently strong that any statistical method applied to the data is likely to produce highly model-dependent estimates of the ATT, among other problems (see, *e.g.,* Boyd, Epstein and Martin, 2010; Cochran and Rubin, 1973; Ho et al., 2007).

Dealing with imbalances in an effort to make a credible causal inference requires two steps. First, we adopt the axiom of statistical inference formally introduced in Iacus, King and Porro (2019, 48): that the data in our sample were generated by a stratified random sampling framework (rather than via simple random sampling). Under this axiom, the strata and n for our sample and each (hypothetical) repeated sample are fixed, with the data for each stratum drawn using simple random sampling.

Second, following from the Iacus, et al. axiom we non-parametrically process the dataset using a stratification-based matching approach. Although any class of "monotonic imbalance bounding" methods would be suitable (Iacus, King and Porro, 2011, 2019), we employ coarsened exact matching, which, for this study, created mostly exact matches for the pre-treatment covariates on treatment assignment (Iacus 2009, 2011; see also Online Appendix 3.2).[5] Figure 1 reports the results of this exercise in the form of $\mathcal{L}_1$ (balance) statistics for each pre-treatment covariate suggested in previous studies, plus justice-fixed effects for vote outcomes (see Section 2.2 and Online Appendix 5 for additional covariates used to assess the robustness of the main results).

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[5]Because all covariates in the primary models are categorical, they are exact matched; several covariates in the robustness tests are coarsened (Online Appendix 5.4).
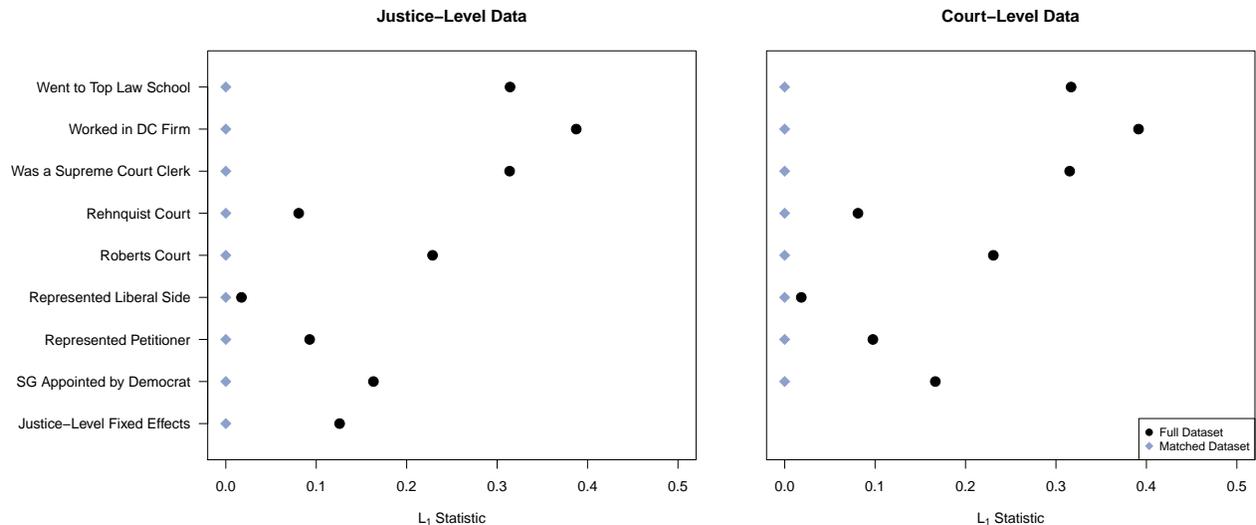
Figure 1: Comparison of balance in the full and matched datasets. The points are the $\mathcal{L}_1$ balance statistic for each covariate; these values have a theoretical range of 0-1 with higher values indicating more imbalance. The relatively large values associated with the circles (the full dataset) highlight the large imbalance in the unmatched dataset. The number of observations and details about the covariates are in Online Appendixes 2.3 and 3.2.

With the balanced sample in hand, the causal identification strategy is to use logistic regression to adjust for the set of pre-treatment variables shown in Figure 1, such that we can credibly assume that the potential outcomes are conditionally independent of treatment status given the covariates (see Boyd, Epstein and Martin, 2010; Ho et al., 2007; Iacus, King and Porro, 2019).[6] Figure 2

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[6]We also assume common support because coarsened exact matching automatically excludes observations outside the common support region (Iacus, King and Porro, 2011) and that both components of SUTVA hold. First, there is no reason to expect interference across cases. Because many attorneys argue multiple cases each term, the pool of experienced attorneys is large enough that assignment of one case to an experienced attorney does not affect the probability that an experienced attorney would be more or less likely to handle a different case. Second, although some experienced attorneys argued more cases than others, the findings remain robust to different specifications of the treatment (Online Appendix 5.2), suggesting that variation in treatment does not appear to be violated.

shows the results: the ATT estimates for attorney experience for outcomes at the justice-vote and Court-case levels (see Online Appendix 4 for the coefficients).
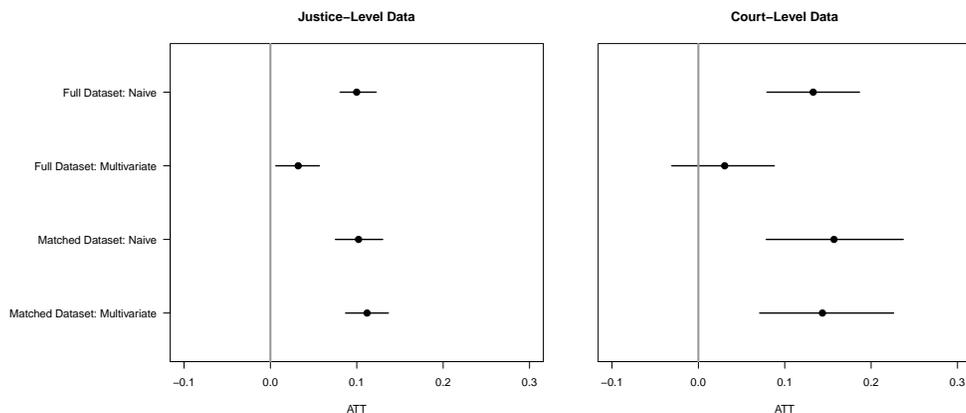

Figure 2: ATT estimates for attorney experience. The lines represent 95% confidence intervals for the average treatment effect for the treated. Naive models are binary regressions that include only the treatment (attorney experience); multivariate models include the treatment plus the pre-treatment covariates listed in Figure 1.

Overall, the panels indicate that the outcomes obtained by experienced attorneys are significantly better than the outcomes they would have obtained had they been first-time lawyers. The results for the justices' votes are relatively consistent for the matched and unmatched data: an experienced attorney, relative to a novice, increases the likelihood of capturing a vote by about 11 percentage points. Note, though, that in the full dataset no significant difference emerges between experienced and novice attorneys at the Court level. Only by matching were we able to unearth the fairly large experience-based advantage of a nearly 14 percentage-point increase in the likelihood of success.

2.2 Robustness Checks

We ran many checks on the treatment and pre-treatment covariates, including applying cut-offs to attorney experience (Online Appendix 5.2), adding covariates to capture the ideological alignment of the justice and the attorney (with and without justice-fixed effects) (Online Appendix 5.4), and segregating out various issue areas (Online Appendix 5.5). We also considered whether one or more justices were driving the experience effect (Online Appendix 5.3). None of these checks did damage

to the results presented in Figure 2.

3   Discussion

Not only does the finding of a significant causal effect of attorney experience validate predictions following from human capital theory; it also carries implications for the study of judicial behavior. Chiefly, although the statistical models in many recent studies of U.S. Supreme Court decisions almost always include a covariate for the participation of the federal government (*e.g.,* Epstein, Landes and Posner, 2013; Hall, 2018; Hume, 2017), the same studies do not account for private-sector lawyers. More generally, a survey of articles published in five disciplinary journals, in the five-year window between 2014-18,[7] shows that not one Court even contemplated the possibility that attorneys affect case outcomes, judicial votes, or both. Figure 2, along with the well-entrenched literature on human capital, suggests that this is a gap of some consequence in the United States and perhaps elsewhere too. Considering the transportability of the theory, methods, and data developed here, it should be straightforward for scholars to fill.

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[7]*American Journal of Political Science, American Political Science Review, Journal of Law and Courts, Journal of Politics,* and *Political Research Quarterly.* For the *Journal of Law and Courts* we went back to the first issue (published in 2013).

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