

Revolving Doors, Former Clerks, and the U.S. Supreme Court

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Abstract

Reformers decry the “revolving door” in Congress and the executive branch. Government officials often lobby when they leave office, and are highly influential when so doing. We examine whether revolving door lobbying is a problem at the U.S. Supreme Court. We do so by analyzing former High Court law clerks. The data show that, unlike their counterparts in other branches, former Supreme Court clerks do not possess proprietary information that they can use to their advantage. They are no more likely to win their cases at the merits stage than similar attorneys who never clerked. Nor are they any more successful at the agenda stage than similar attorneys who never clerked. These results inform our understanding of lawyer influence on the Supreme Court and offer insight into reforms policymakers might consider to reduce revolving door influence in the political branches.

Reformers decry the “revolving door” in Congress and the executive branch. They argue that when government employees and policymakers leave public service and lobby to influence their former colleagues, something is wrong with “the system,” a system in which insider information, personal relationships, and lobbying loopholes influence policy outcomes. One study found that 56% of the revenue from lobbying firms between 1998 and 2008 could be attributed to people who previously worked in the federal government (Vidal, Draca and Fons-Rosen 2012). Another study found that of the 50 top lobbyists in Washington, 34 previously worked in the federal government (Eisler 2007). The concern, of course, is that people who walk through the revolving door exercise undue influence over public policy. While recent studies have shown that revolving door lobbying influences legislative and executive branch policies (Cain and Drutman 2014; Vidal, Draca and Fons-Rosen 2012; Baumgartner et al. 2009), it is unclear whether those who walk through the Supreme Court’s revolving door similarly influence justices. That is, do former Supreme Court clerks enjoy unique influence over High Court decisions?

Many law firms seem to believe that former clerks can influence case outcomes at the High Court. Former clerk signing bonuses tell the tale. When the justices’ law clerks departed at the end of the 2014 term, many of them collected signing bonuses as large as \$330,000 from the private firms with which they signed on to work. These bonuses, combined with hefty salaries, allowed former clerks to earn roughly a half million dollars in just their first year away from the Court (Kendall 2012). And it was not just the 2014 clerks who walked away with fists full of money. Over the last two decades, clerks have received substantial bonuses to work for private law firms. Clearly, the firms that pay them believe they earn their money back through legal influence and greater success before the Court. But do they? Are former clerks uniquely influential?

To analyze the revolving door at the High Court, we examine whether former Supreme Court law clerks are more likely to win their cases before the Court than otherwise similar attorneys who never clerked for the Court as well as whether they are more likely to get their

cases placed on the Court’s docket at the agenda stage. We focus specifically on whether their insider information gives them an upper hand over other attorneys—that is, whether their personal connections with the justices and with the process offer them greater influence. After employing matching methods and parametric models, we find that former Supreme Court clerks are unable to capitalize on their personal connections and inside information to win cases. They are no more likely to succeed than other highly skilled lawyers.

These results make at least three contributions. First, they speak to the broad question of policymaking influence. While revolving door lobbying influences other policymakers, the equivalent does not occur on the Supreme Court. This is an important finding that offers ideas for how policymakers might limit influence elsewhere. Second, the results tell us something about lawyer influence at the Supreme Court. Previous studies have found that some attorneys are more likely to win their cases before the Court, but they have largely been silent as to why. Our results take a first step towards answering why some attorneys are more likely to win than others. Finally, these results have practical implications to the extent that they might save litigants money. There appears to be nothing uniquely advantageous about hiring former clerks: Supreme Court law clerk status is simply a cue that an attorney is highly capable. Other attorneys, however, are just as capable—and they may be cheaper.

In what follows, we begin by discussing the literature on revolving door lobbying. We then examine what we know about lawyer influence before the Court, and then specifically about the influence of former clerks as lawyers before the Court. We then analyze our data and measures, and discuss our matching approach. We present our results and offer some comments about what they mean more broadly.

Revolving Doors and Influence Peddling

Journalists and reformers believe revolving door lobbying influences public policy. The so-called revolving door is “the process by which [governmental workers] seamlessly leave government service to represent private clients before the very same congressional [or

other] offices for which they previously worked” (Cain 2014, 2; Gormley 1979).¹ For example, the Los Angeles Times recently published a story about how “Ex-Congressmen become stealth lobbyists” (Hiltzik 2015). The New York Times lamented the “corruption inherent in the open revolving door between Congress and K Street” (Edsall 2011). Reformers have dedicated programs to undo the dynamic.² There is something wrong, they argue, about a system in which the regulators later can lobby for the regulated.

Congress purportedly has recognized the revolving door problem and taken some steps to address it. In 1995, Congress passed the Lobbying Disclosure Act to make more transparent who was lobbying by imposing lobbyist registration requirements. In 2007, Congress passed the Honest Leadership and Open Government Act to deal with the problem once again (this time in the wake of the Jack Abramoff scandal). This Act more clearly targeted the revolving door. It set time limits on how long members had to wait to lobby after they left office (called a “cooling off” period). Cabinet secretaries and senior staff received their own cooling off periods. Former senate and House staffers also had to wait one year before lobbying various committees or members. As many have pointed out, though, former public officials nevertheless “routinely capitalize on an array of loopholes” to lobby (Lipton and Protesse 2014). A recent paper by staff at the Federal Reserve Bank of New York (Lucca and Trebbi 2014) shows that some government workers are more likely to leave regulatory jobs for the private sector after they have become experts on complex regulations; that is, they take regulatory jobs, learn complex regulations and then leave for the private

¹The revolving door metaphor does not focus only on the exit side of things; it also focuses on the entrance side by, for example, examining regulators who come to government from the regulated industry (Cohen 1986). Another aspect of the metaphor focuses on how future career prospects paralyze regulators—that government officials, who know they can leave office and earn large paychecks lobbying for regulated entities, will not create or impose certain policies against those regulated entities while in office for fear of not being hired.

²See, e.g., <http://www.cleanupwashington.org/lobbying/page.cfm?pageid=40>.

sector where they might be able to leverage that knowledge.

Empirical scholarship supports the claim that revolving door lobbying influences public policy. For example, Baumgartner et al. (2009) find that using former well placed officials as lobbyists can generate influence. Groups that hire a “covered official” (a federal government employee in the last 20 years) are more likely to observe lobbying success than groups without one. Lazarus and McKay (2012) find that universities are more likely to receive earmarked monies when they employ lobbyists who walked through the revolving door. Vidal, Draca and Fons-Rosen (2012) find that lobbyists who previously worked for sitting members of Congress earn larger salaries than lobbyists who did not. What is more, they discover that “lobbyists connected to US senators suffer an average 24 percent drop in generated revenue when their previous employer leaves the Senate” (Vidal, Draca and Fons-Rosen 2012, 3732). They also find that ex-staffers become less likely to lobby when their senators leave office. Bertrand, Bombardini and Trebbi (2011) find that lobbyists who previously worked for still-sitting members of Congress tend to shift their area of focus when the former member shifts his or hers. If a member switches committees, the lobbyist changes his or her focus.

Just why former government workers make for effective lobbyists is not hard to understand. They tend to accrue substantive expertise that they can later deliver to potentially information-starved policymakers. They also tend to understand the policymaking process well. They know the relevant veto pivots as well as which people can get things done. And, they can deliver political or other contextual information to policymakers, such as information about constituent demand or how industries will be able (or willing) to comply with new policies.

Most important for our immediate purposes—and a large part of the revolving door story—former officials make for effective lobbyists because they can rely on *personal access* to and *inside information* about policymakers when lobbying (Drutman 2014). For example, former congressional staffers have personal relationships with their former employers. This relationship grants them greater access to members as well as an enhanced degree of credi-

bility. It also allows them to use what they know personally about the members to persuade them. As one study put it:

[T]hey are able to reach their former colleagues in ways that strangers are not able to. Former staffers also have insider knowledge of the quirks and idiosyncracies of the processes and personalities of decision making within their former institutions. They know what makes different key people tick—what they are likely to do in response to what, and how best to approach them. . . having these relationships also makes former staffers privy to intelligence that others may lack (Drutman 2014, 89).

Having worked for their former employers, they learned inside information about them which they can later use to their advantage. They likely also gained access to relevant networks within which their employers still operate. They can use all of these bits of “relational capital” (Vidal, Draca and Fons-Rosen 2012, 3732) upon leaving to influence their former employers.

Former Supreme Court Law Clerks As Revolving Door Lobbyists

Does this same kind of revolving door influence exist at the High Court? It would appear, at least, that top law firms believe so. These firms lust after former clerks, in part, because of their expected influence at the High Court.³ One partner at a large firm stated: “We have found it’s a terrific investment for us. . . They are incredibly smart, and they’re up on big issues in the law. . . They have this unique view of how judges think, of how the justices interact” (Kendall 2012). Former Acting Solicitor General Walter Dellinger, who has hired numerous former clerks at his law firm, states: “there’s a very strong overlap. . . [between former clerks and] extraordinary talent. . . one of the least appreciated things in the practice of law, is lawyering that rates even above truly excellent lawyering” (Lithwick 2007). And, he adds, “if you’re working on billion-dollar cases, the client is willing to pay more for truly excellent work” (Lithwick 2007). Another high-powered lawyer agrees. Tom Goldstein (of

³In addition, firms believe hiring former Supreme Court law clerks makes them look prestigious to clients and potential clients (Ward, Dwyer and Gill 2014; Kendall 2012).

SCOTUSblog fame) argues: “The combination of the clerks’ skill, that they could become leading lawyers in the firm, and the reputational benefit to having them, justifiably creates this incredible demand” (Lithwick 2007). Another firm states: “requiring our new lawyers to have completed a federal clerkship assures that they have unique insight into how judges work...” (Lat 2016).

Surprisingly, only a handful of empirical studies examine whether former clerks are, in fact, more likely to win their cases than attorneys who never clerked for the Court. As McGuire (2000) states: “there is little systematic knowledge about the impact of former clerks when they return to the Court as advocates” (123). To be sure, some data suggests former clerks *appear* before the Court more than other attorneys (McGuire 2000; O’Connor and Hermann 1995; McGuire 1993), but whether this means they are more influential is unclear. In their examination of attorney influence at oral argument, Johnson, Wahlbeck and Spriggs (2006) examine whether former Supreme Court clerks received higher grades in Justice Blackmun’s personal records. They did. Yet the subsequent analysis showed that though better grades led to a heightened chance of victory, former clerks as attorneys for appellants actually made justices *less* likely to reverse on the merits.

McGuire (2000) offers the most direct study on the success of former law clerks before the Supreme Court. His theory is that former clerks can reduce a justice’s information uncertainty regarding the policy consequences of a vote, and thereby secure victory. Looking at all attorneys who appeared (either on the briefs or at oral argument) during the Court’s 1982 term, he finds that former clerks won more cases than non-clerks—but with a caveat: only clerks that remained in Washington to practice law exercised such influence. Former clerks who practiced outside Washington enjoyed no heightened success. While helpful, these findings have their limitations. First, they are time-bound. Former clerks have in recent years been much more active before the Court than they were in the 1982 term. Beginning in 1986, firms began to offer signing bonuses to hire clerks (Ward, Dwyer and Gill 2014). Since then, the competition to hire former clerks has looked more like college football

signing day than law firm hiring. Second, the study examines the number of former clerks representing the petitioner minus the number of former clerks representing the respondent (i.e., the net number for the petitioner). This assumes a marginal effect of former clerks that may or may not exist.

Simply put, it remains unclear whether former clerks can rely on any sort of inside information or personal connections to influence Supreme Court cases. Are they like those who walk through the revolving door in Congress and the executive, or is there a separate—and less influential—door for former Supreme Court law clerks? The empirical question demands resolution. In what follows, we seek to provide an answer.

Data, Matching, and Measures

To examine whether former law clerks can use their inside information and personal connections to influence Supreme Court justices, we collected data on all orally-argued cases decided between the Court’s 1979 and 2014 terms ($N = 3336$).⁴ To determine whether being a former law clerk to the High Court causes an attorney to be more likely to win, we focus on a “but for” question—would the attorney have won but for having clerked? As others have argued (Black and Owens 2012; Boyd, Epstein and Martin 2010; Epstein et al. 2005), the perfect method to employ when answering this question would be to conduct an experiment. But since we cannot use Supreme Court justices and live cases in an experimental setting, must rely on observational data. To approximate the experimental approach, though, we rely on matching.

The theory of matching is intuitive. The analyst takes the data she has collected on the topic of interest and then matches observations such that the values of covariates in the treatment group and control group are as close as possible to each other. Observations that do not match across groups—that is, observations that are imbalanced—are discarded.

⁴We examine fully, orally argued, signed opinions, orally argued per curiam opinions, and judgments of the Court.

In other words, the goal is to retain data such that the treatment group is identical to the control group (i.e., the data are balanced), with the only difference between the two being the presence of the treatment. In that sense, matching can be thought of as something like a post-hoc experimental design.

We employ coarsened exact matching (CEM) (Blackwell et al. 2010; Iacus, King and Porro 2009). The CEM software allows us to define the level of imbalance in the data that is tolerable. We can evaluate how many matched units remain, given that level of balance. In other words, the CEM approach allows the analyst to define acceptable balance not just in some overall sense, but for each individual pre-treatment variable of interest. This is important because for continuous variables, exact matching is often unnecessary. For example, in what follows, we will examine whether attorney success is a function of (former clerk status and) attorney experience. Previous scholarship suggests that *levels* of attorney experience matter more than one unit increments. The marginal increase in one previous case is likely to matter more at the low end of the experience spectrum than at the high end. Stated otherwise, the marginal increase of one previously argued case matters much more to the novice than to the veteran. Under the CEM approach, we can specify the level of coarsening that is acceptable.⁵ We can match on clusters or groupings of covariates to achieve balance while still maintaining enough observations to execute the study. Thus, we can match attorneys who argued 11 previous cases with those who argued 15 previous cases, but can refuse to match attorneys who argued 1 previous cases with those having argued 0 previous cases. Once the data are more balanced, we can fit a standard parametric model to the data. Such approximately balanced data lead to stronger statistical estimates and less model dependence.⁶ “The only inferences necessary [after approximately balancing the

⁵Note that automated algorithms exist for coarsening variables exist, as well. With the exception of our oral argument experience variables we utilize pre-defined coarsening rules, which are analogous to “binning” process used in constructing a histogram.

⁶If the data are exactly matched, parametric models are not needed to make inferences.

data] are those relatively close to the data, leading to less model dependence and reduced statistical bias than without matching” (Iacus, King and Porro 2012, 1).

We matched our treatment (former Court clerk) and control groups (not former Court clerk) on a host of characteristics that likely influence whether an attorney wins or loses in Court but that are non-randomly distributed between former law clerks and non-former clerks. As we discuss more fully below, we measure each attorney’s oral argument experience, the party’s net resource advantage, the number of supporting and opposing amicus briefs, petitioner status, the party’s ideological distance from the Court median, whether there was dissent in the lower court that heard the case, whether the Court stated it granted review in the case to clear up conflict among the circuits, and whether the United States Solicitor General’s office supported or opposed the attorney’s position. Stated simply, our goal here is to compare observations such that the attorneys in the treatment and control groups are functionally the same with the exception that the attorney in the treatment group was a former High Court clerk while the attorney in the control group was not.

Attorney Experience. We matched on the prior oral argument experience of each attorney. We do so to account for the possibility that attorney experience leads to success. If experience before the Court leads to success, and former law clerks have more experience, one might wrongly infer that former clerks—because they are former clerks—are more likely to win. We downloaded from LexisNexis the oral argument transcripts from each case in our sample. We then identified each attorney who appeared. Across the 3336 cases we examined, there were 7993 attorney appearances distributed over approximately 4345 unique attorneys. Since our unit of analysis is each attorney in each case, we started with 7993 potential

The analyst can simply perform a difference of means test to infer causation. Where, as here, one deals with approximately balanced data, parametric models are appropriate to make such inferences. Using traditional parametric methods (e.g., logistic regression) on a matched data set can account for any remaining imbalance without being overly dependent upon the functional form imposed on the data (Ho et al. 2007a).

observations and from there, matched the data. We calculated the number of total prior cases each attorney in our sample orally argued before the Supreme Court prior to the case at issue.⁷

Our measure, then, is dynamic, looking backward from each case to the totality of the lawyer's previous oral arguments before the Court. We coarsen our oral argument matches into one of the following five "categories:" 0 previous cases, 1 previous case, 2-10 previous cases, 11-20 previous cases, 21-50 previous cases, and 51 or more previous cases. That is, we match a former clerk with 0 previously argued cases only with a non-former clerk with 0 previously argued cases, but might match an attorney with 3 previous cases with an attorney who argued 8 previous cases. This approach is supported by existing literature⁸ and is (thankfully) robust to alternative coarsening specifications.⁹

We also employed a second approach for measuring attorney experience. Whereas the previous approach examined each attorney's previous argumentation experience (and then

⁷We examined each attorney's oral argument experience rather than his or her presence on a brief or status as counsel of record for two reasons. First, oral argument experience has already been established as a reasonable measure of attorney experience (Johnson, Wahlbeck and Spriggs 2006). Second, as McGuire (1998) points out, the United States Reports do not consistently record which attorneys were on the briefs in a case. They do, however, consistently record the identity of the attorney who orally argued the case.

⁸McGuire (1998), for example, utilizes an ordinal measure for determining whether the petitioner, respondent, or neither party has *any* amount of additional experience. Johnson, Wahlbeck and Spriggs (2006) use (one plus) the natural logarithm of an attorney's amount of oral argument experience.

⁹Alternative coarsening schemes we tested include, for example, (1) separating out attorneys with 51-100 and 101 or more appearances and (2) separating out attorneys in the 2-10 range by splitting out those with 2-5 appearances from those with 6-10 appearances. Across dozens of alternative approaches considered we obtain substantively similar results.

examines whether that attorney won), our second approach brings in the experience of the amicus curiae who orally argued before the Court as well. This second approach recognizes that amici typically support a side in a case and, when they are allowed by the Court to provide oral argument, can make a stronger case for the supported party. Thus, as a second examination, we measure the cumulative amount of experience possessed by attorneys on each side. We use the same coarsening levels described above.

Net Resource Advantage. We also match litigants on their general resource advantages. If it is the case that only the most lucrative clients can hire former clerks, and lucrative clients generally are more likely to win, we must account for each party's resources. If better resourced parties win more often, and they are more likely to hire former clerks, we might wrongly attribute success to former law clerk status when, instead, the resource advantage matters most. We follow others (Black and Owens 2012; Collins 2007, 2004; Songer, Sheehan and Haire 1999; Sheehan, Mishler and Songer 1992; Songer and Sheehan 1992) and order litigants along a sliding resource scale. Like Collins (2004, 2008), we assign each petitioner and respondent to one of ten categories, which we present in ascending order of resources: poor individuals, minorities, non-minority individuals, unions or interest groups, small businesses, businesses, corporations, local governments, state governments, and the U.S. government. The weakest category – poor individuals – is coded as 1 whereas the strongest category – the U.S. government – is coded as 10. We then subtract the ranking for the opposing side from the ranking for the side being supported by the specific attorney we examine. This approach identifies the relative differential between the two sides. Positive (negative) scores reflect cases in which the attorney under examination (opposing side) was advantaged.¹⁰ Thus, if

¹⁰An alternative approach would involve matching on the rank of the attorney's side and the opposing side. We opted to match on the differential for two reasons. First, we believe the difference in resources is more important than the actual identity of the parties. Second, when we try to match on the identity of the parties, we are unable to retrieve enough matches to make meaningful inferences. The more precise we make the match, the more data we lose.

the OSG squared off against a state government, the resource differential would be 1. If a state government squared off against a local government, the resource differential would be 1. We would then match on that resource differential of 1. (We would not match either of those two instances with a case where the resource differential was anything other than 1.)

Amicus Briefs. A number of studies show that amicus curiae briefs influence the choices justices make (Collins 2004, 2008). As such, we believed it necessary to control for the presence of amicus briefs. Using data provided by Collins, which we updated, we created a variable that measured the number of amicus briefs supporting each attorney’s side, which we then used in the CEM algorithm.

Petitioner Status. We also matched cases based on whether the attorney – including those that appeared as amicus curiae – represented (or supported) the petitioner. We did so to control for the Court’s well-known trend to reverse cases it reviews (Perry 1991).

Ideological Congruence with the Court. Finally, a large portion of judicial decision making turns on the ideological preferences of justices (Segal and Spaeth 2002). As such, we determined, for each attorney in each case in our sample, his or her ideological distance from the Court median, following the same procedure as Johnson, Wahlbeck and Spriggs (2006). We determined, first, the ideological direction of the lower court decision as reported in the Supreme Court Database.¹¹ If the lower court decision was liberal (conservative) we coded the petitioner as making a conservative (liberal) argument. If the petitioner’s argument was conservative, we coded our variable as the Court median’s ideal point, as estimated by Martin and Quinn (2002). If the argument was liberal, we coded our variable by multiplying the Court median’s Martin-Quinn score by -1.

Case Quality. We also sought to control for the quality of the case. We suspect that the underlying quality of the case could lead to victory. It is also possible that certain types of lawyers are better at spotting higher (or lower) quality cases. If so, we must recognize that quality might drive case outcomes and justice votes. Accordingly, we match on two

¹¹The Supreme Court Database can be found at <http://scdb.wustl.edu/>.

measures of case quality. First, we examine whether a judge in the lower court dissented. Most circuit court cases do not observe dissents because dissent is costly (Epstein, Landes and Posner 2013). It requires time and effort of the dissenting judge; it also makes life more difficult for the majority opinion writer. Indeed, forcing busy colleagues to respond to a dissent can impose social costs on the dissenter. We suspect that judges are more likely to dissent in cases where the losing party presents a high quality argument. Thus, we code whether (=1) or not (=0) there was a lower court dissent in the case.¹² Second, we examine whether the lower courts conflicted over the proper interpretation of the law. When the lower courts conflict, it signals that there are multiple reasonable outcomes in a case. That is, each party might have a strong argument. These cases should be distinguished from those without conflict—and thus, the outcome seems clearer. Thus, we code for whether (=1) or not (=0) there was conflict among the lower courts.¹³

Solicitor General Support. Last, and certainly not least, we account for the position of the Solicitor General’s office. As numerous scholars have shown, the Solicitor General wins regularly before the Court (Black and Owens 2011). While there are numerous theories as to why this may be the case, it is clear that the OSG wins often. As such, we opted

¹²To be sure, judges do not dissent simply for legal reasons. Yet we know they often dissent to alert their superiors that their colleagues have deviated from the law (Cross and Tiller 1998).

¹³We obtain data for both these measure by looking to the Supreme Court Database. The Database, of course, codes whether the Court referenced the presence of a dissent below or the presence of conflict. In our context, this “selection effect” is likely beneficial because it highlights those cases that seem to be of higher quality. That is, when the Court mentions a lower court dissent, it often does so to highlight the strong legal arguments on both sides of the case. In many instances, the Court goes so far as to support the lower court dissent. See, e.g., *CSX Transport v. Georgia State Board of Equalization* (2007); *Planned Parenthood v. Danforth* (1976).

to determine whether the OSG weighed in on the side of one party. After all, if the OSG weighs in on the side of former law clerks more than others, and the OSG causes the win, we might wrongly infer an effect from the former clerk. Thus, we match on whether the OSG supports the attorney's position.

Before we turn to our methods and results, we pause briefly to discuss factors on which we do *not* match. First, although we match on whether the OSG supports, opposes, or is uninvolved in a case, we do not match on whether an individual attorney is specifically affiliated with the OSG. We do not match on it because we want to avoid post-treatment bias (PTB). PTB occurs when one includes a variable that is plausibly a consequence of the treatment and also related to the outcome of interest. For example, suppose one were examining the effect of a new drug on a patient's overall health versus a standard treatment. PTB would occur if, in addition to including an independent variable for treatment vs. control group, one also included an independent variable for the severity of drug-related side effects experienced by the patient. Although side effects undoubtedly impact the patient's overall health, they are a consequence of treatment and, as such, should not be included in the statistical analysis. Returning to lawyers and the Court, the OSG hires only the best and brightest attorneys to work in its office. It recruits top-rated individuals with strong educational credentials and work experience, which would, of course, include clerking for a Supreme Court justice. Seen through the lens of PTB, a consequence of being a clerk is that you are more likely subsequently to be employed by the OSG. To avoid contaminating the impact of being a clerk, we do not match on this factor (though we do exploit it below to gain leverage on whether clerk influence actually exists).

Second, we do not match attorneys on whether they are based in Washington, D.C. The logic here mirrors that of the OSG discussion above. Former clerks are coveted because of their purportedly unique experience peeking behind the secretive curtain of the Court. For the past few decades, those experiences have been primarily valued by firms in D.C. who regularly appear before the Court. Being a clerk makes you more likely to be hired by a

D.C. firm, which, again, makes it inappropriate to match on this factor.

Methods and Results

Once we matched our data on the pre-treatment covariates described above, we fit a logistic regression model to examine the impact of being a former clerk on winning. Our unit of analysis is the attorney. Our dependent variable is whether each attorney won (1 = yes; 0 = no) his or her case. Our main covariate of interest is a binary indicator for whether the attorney was or was not in the treatment group (i.e., was or was not a former Supreme Court law clerk). We also include as controls each of the pre-treatment variables we identified above. We do so upon the advice of (Ho et al. 2007b), who suggest it will neutralize any imbalance that remains in the data after matching.

Do former clerks wield unique influence at the Court? At first blush, the data seem to suggest that clerks wield a unique influence, but upon further investigation, the story is more nuanced.

Figure 1 is a density plot that illustrates the marginal treatment effect of being a former High Court law clerk on an attorney's probability of success before the Court. To generate the values shown in the figure, we followed a Clarify-like (King, Tomz and Wittenberg 2000) procedure and simulated 5000 values from our logistic regression model. The values portrayed in the figure show the impact of being a former law clerk if one has a 0.50 baseline probability of winning a case. The thick solid vertical line denotes the median simulation interval; the dashed vertical lines denote the 95% simulation intervals. As the thick solid line illustrates, we observe a small, but statistically significant impact of being a former clerk. An attorney with a 50% chance of winning would see that likelihood increase to around 55% if she was a former law clerk. The lower and upper bounds on this estimate are roughly 51% and 59%, respectively. This initial finding seems to be a small but significant influence from former clerks. Indeed, although a 5% greater likelihood of winning is not overwhelming, we suspect litigants would seek out (and pay for) any possible advantage available to them, given the policy and legal consequences of Supreme Court cases.

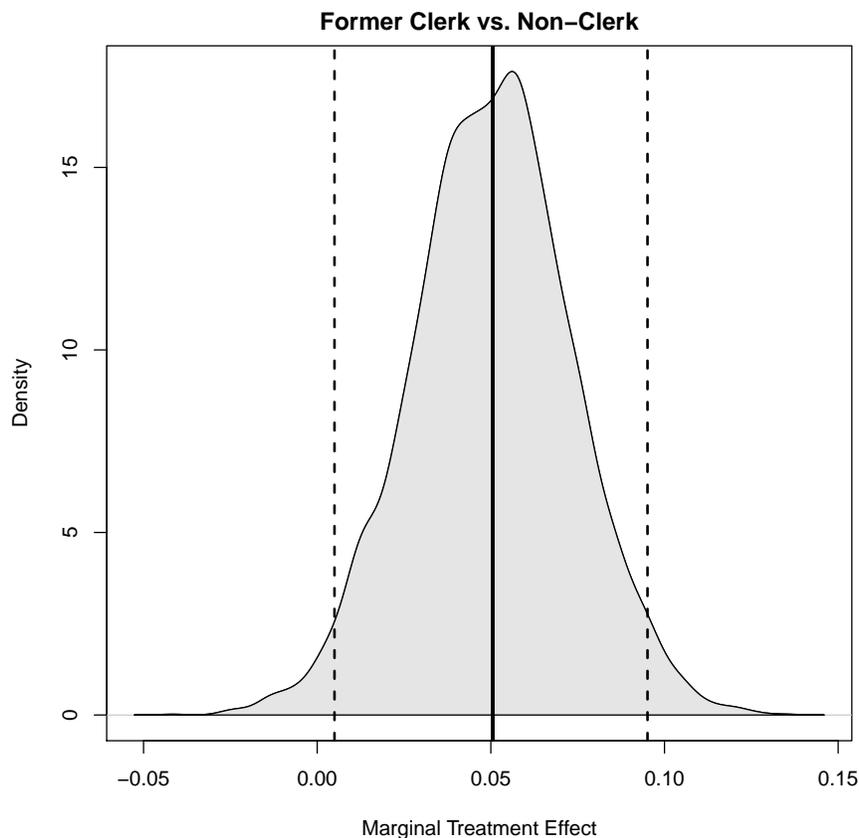


Figure 1: Marginal treatment effect of former clerk status on probability of winning a case.

Yet the story cannot end there. Although matching allows us to reduce imbalance and facilitate better comparisons, the power of matching is conditional on the analysts' ability to identify and measure confounding factors that might differ between the treatment and control groups. In this vein, one type of variable is noticeably absent from the previous analysis: *attorney quality*. Though we have gone to great lengths to equalize case-level attributes among our observations, it is still likely that some attorneys are simply more skilled or capable than others. Attorney skill is a confounder because, of course, skilled individuals are more likely to be selected for a coveted Supreme Court clerkship and are also more likely to win cases when they argue before the Court. Through this lens, it is possible the result above reveals nothing other than the fact that attorneys who are more highly skilled (i.e., those who are picked to clerk on the Court) win more often than their

less-skilled counterparts.

Elite Attorneys and Former Law Clerk Status. To examine whether lawyer skill explains the results above—and not some kind of proprietary information or personal contacts derived from being a clerk—we compare the effect of former clerk status within two elite groups of attorneys: those who work in the Office of the Solicitor General (OSG); and those who are based in Washington, D.C.

To begin with, we isolate appearances by OSG attorneys. Then, we examine which of them previously worked as clerks for the Supreme Court and which of them did not. We focus on OSG attorneys because the OSG is a highly selective employer and likely to employ only the best and most-skilled lawyers. Some OSG attorneys previously clerked on the Court; others did not.¹⁴ If OSG attorneys who once clerked for the Court win more than OSG attorneys who never clerked, we can believe they have some unique insight they leverage into success. If there is no difference between the two, then we can safely assume the results we found above are driven simply by lawyering skills.

By a similar logic, we also isolate appearances by Washington, D.C.-based attorneys and separate them into former clerks and non-former clerks. Again, the logic is that attorneys who work inside the Beltway and argue before the Supreme Court are, on average, likely to be of higher quality than non-D.C. attorneys. If D.C. lawyers who once clerked for the Court win more than D.C. lawyers who never clerked for the Court, we can infer there is something uniquely influential about being a former clerk. If there is no difference between the two, we have further evidence that lawyering skills explain influence at the Court and

¹⁴While this difference could still be attributable to skill or ability (i.e., former clerks are uniformly smarter/better than non-former clerks), we think this is probably unlikely. Even if there still is an ability gap between the two, the magnitude of the gap will be substantially smaller than between a former clerk and any other attorney drawn from the population of non-former clerks, which is to say there will be enhanced balance between the treatment group (former clerks) and the control (non-former clerks).

not something that is unique to former clerks.

Our methodological approach is, with one exception, identical to the one we employ above. The one difference is that for the OSG-only comparison, we no longer match on whether the OSG supported, opposed, or was not present in the case. This is no longer necessary since all of the observations involve the OSG. In lieu of this variable, however, we do match on the ideological distance between the president and the Supreme Court median. We do so to capture any general ideological feature involved in the OSG's representation. The matching covariates for the D.C.-only analysis are identical to those used earlier.

Figure 2 below shows results from these two additional analyses. The left panel illustrates, for OSG attorneys, the effect of being a former clerk compared to not having that experience. The thick vertical line, horizontal ticks, and thin vertical line denote 80%, 90%, and 95% simulation intervals, respectively (all two-tailed). The point estimate for the effect of being a former clerk is actually negative, though, as the panel shows, the effect does not even approach conventional levels of statistical significance ($p = 0.57$). The right panel shows, for Washington D.C.-based attorneys, the effect of being a former clerk. It tells an identical story: once you create a more level playing field in terms of attorney quality (or at least plausible manifestations of it), *a former Supreme Court law clerk is no more (or less) successful than someone who never clerked on the Court.*¹⁵

¹⁵As with any null finding, one must always be concerned about whether there truly is no effect or if it merely is an artifact of not having enough data to recover an effect that actually exists. The matched sample sizes for the two analyses in Figure 2 are 615 (OSG-only) and 264 (D.C.-only). We obtain identical results with the unmatched regressions, which have 1936 and 1926 observations for the OSG-only and D.C.-only samples, respectively. Given these values, we are reasonably confident that no large positive former clerkship effect exists.

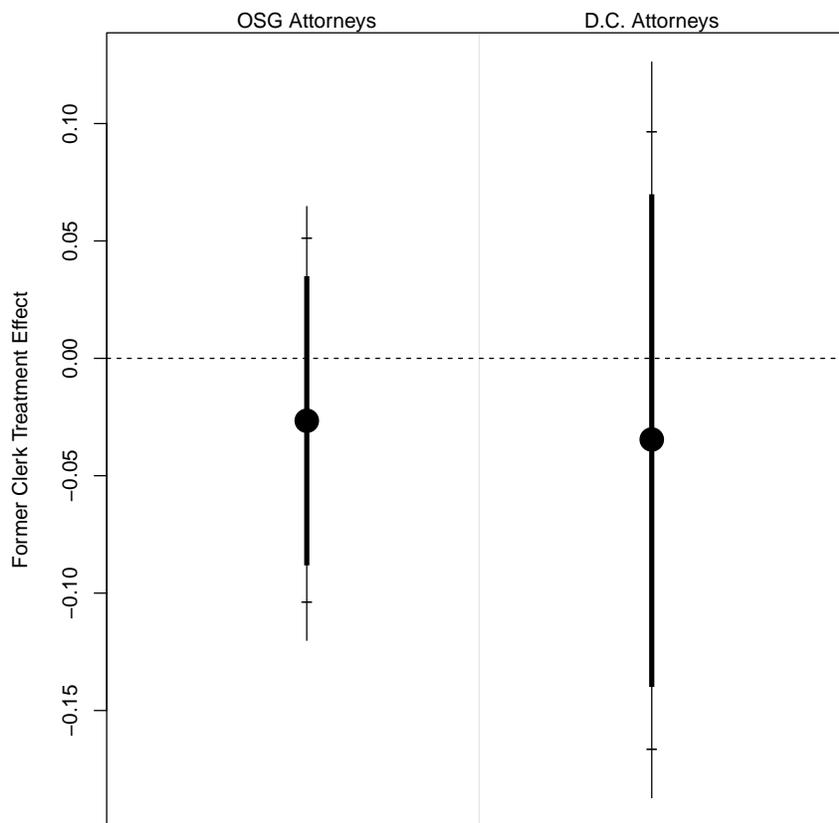


Figure 2:

Personal Connections and Former Law Clerk Status. Thus far, it appears that former law clerks are no more likely to win their cases than similar attorneys who never clerked for the High Court. But perhaps that is asking too much. Perhaps, instead, the former clerk's advantages come in the form of their personal connections to the justices. Perhaps they are able to capture their own justice's vote, or maybe they are able to use their experiences to capture the votes of the justices who served on the Court when they clerked. With these questions in mind, we now shift our focus to looking for differential levels of success within the subset of individuals who were former clerks. We look at two specific comparisons. First, we examine whether a former clerk is more successful at capturing the vote of a justice for whom she worked versus the votes of justices for whom she did not work. That is, is a Breyer clerk more likely to capture Breyer's vote than the votes of the other justices? Second, we examine whether a former clerk was more successful at capturing the vote of a justice who

was on the Court when the clerk served (as opposed to justices who were not on the Court then). For example, was an attorney who clerked for Justice Ginsburg in the 1993 term more likely in 2014 to capture the votes of Justices Ginsburg, Scalia, Kennedy and Thomas than the votes of Chief Justice Roberts and Justices Breyer, Alito, Sotomayor, and Kagan? The theoretical intuition for these comparisons comes from the possibility that former clerks might provide justices with unique and specialized information. If this is what leads them to win, we would expect to observe former clerks doing better among the justices they came to know at the Court. We continue to match on all of the previously-identified covariates, though our ideological congruence measure is now calculated for each individual justice as opposed to the Court median.

Figure 3 affirms the finding of no former clerk influence. The left panel asks whether a former clerk arguing before the Court is any more likely to win the vote of the justice for whom she clerked as opposed to the vote of any other justice on the Court. The answer to this question is a clear “no.” Clerks have no halo effect when dealing with their former bosses. What is more, the right panel illustrates that former clerks receive no halo effect from the justices who served on the Court during the term when they clerked (i.e., justices we call “Term Justices”). Clerks are no more likely to receive the votes from Term Justices than from non-Term Justices. Indeed, if anything, there is a small amount of evidence that this clerk is actually less likely to win the votes of justices with whom she is presumably more familiar.¹⁶

¹⁶Bearing in mind footnote 15, the matched sample sizes for these analyses are as follows: 3092 (left panel) and 7206 (right panel).

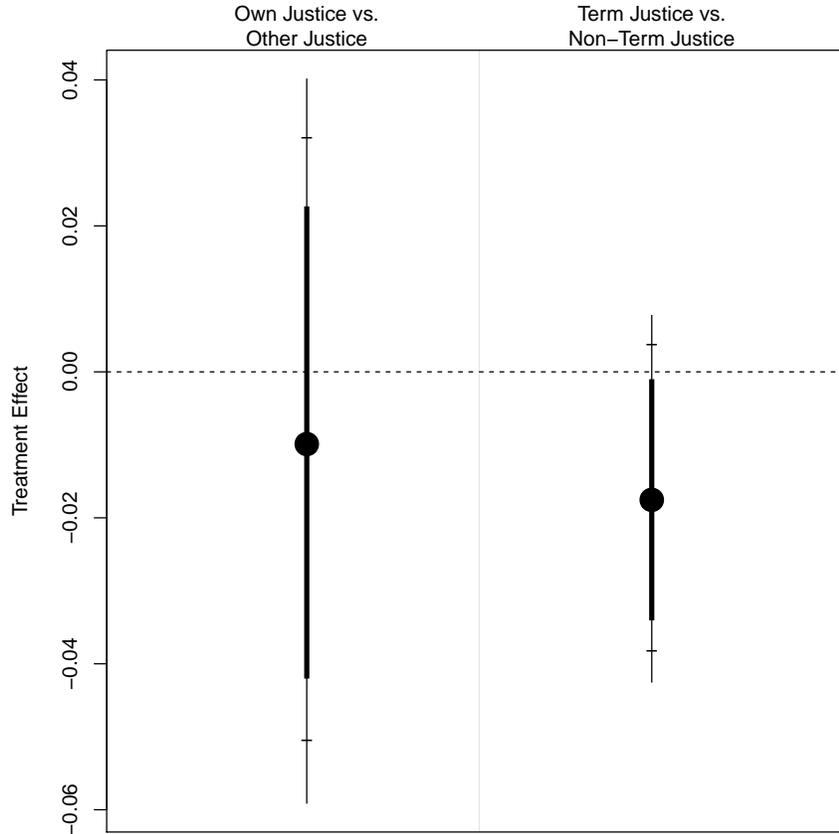


Figure 3:

Simply put, former Supreme Court law clerks do not appear to be any more influential than other skilled lawyers of the Supreme Court Bar. They are successful, yes. And they win their cases more than most attorneys. But they do not win more than other highly skilled attorneys.¹⁷

Former Clerks and Agenda Success

Of course, it could be that the primary benefit a former clerk provides is knowledge about the agenda setting process. After all, most of the work clerks do is reviewing cert petitions and making cert recommendations to the justices. So, looking for clerk influence at the merits stage might neglect a potentially more likely area of influence: the agenda stage.

¹⁷We also examined attorney sex, caseload, and complexity (as measured by total amici briefs) and none of these factors impact our findings.

If it is the case the former clerk influence is more likely to manifest itself at the agenda stage, we must examine that before we can confidently believe former clerks wield no unique influence.

Accordingly, we examined how former clerk status influenced the Court's agenda setting decisions. Using the Blackmun Papers (Epstein, Segal and Spaeth 2007), we collected attorney data for all petitions that made the Court's discuss list from the 1986-1993 terms. We measured all the the standard agenda setting variables (Black and Owens 2009), including alleged conflict, weak conflict, strong conflict, whether the US Supported or opposed review whether the lower court reversed the court below it, whether there was dissent in the lower court, whether the lower court struck a law as unconstitutional, whether the lower court was an en banc review, whether the lower court decision was unpublished, the number of amici briefs filed at the agenda stage, whether the lower court's decision was mentioned in U.S. Law Week, and whether the voting justice was closer to the expected outcome on the merits than to the status quo (Black and Owens 2009). We also collected data on the number of former clerks on each side of the case as well as the amount of oral argument experience enjoyed by each side. Our unit of analysis is the case side.

Just as we did with the merits models above, we employ matching. The variables on which we match in this analysis include: whether the attorney's side represented the petitioner, the total oral argument experience for the side, whether a former law clerk represented the opposing party, and case quality. To measure case quality, we employed an item response theory model that included all the canonical agenda setting variables in the literature, including: strong conflict, weak conflict, alleged conflict, the number of amici, whether there was a dissent in the court below, and whether the United States supported review.

Our dependent variable is whether each justice voted to grant review to the petition. It equals 1 when the justice voted to grant review, to note probable jurisdiction, and to "Join-3." It equals 0 when the justice voted to deny review, or voted to dismiss an appeal.

We examine two separate treatment effects. Our first treatment is whether the lead attorney on the side was a former clerk. Our second treatment is whether any of the attorneys involved on the side were former clerks. (The control, then, reflects attorneys who never were clerks.)

Figure XXX shows the results of these two models—and it shows clearly that former clerks provide no additional benefit beyond what other skilled lawyers offer. Justices are no more likely to grant review to a petition where the lead attorney (counsel of record) is a former clerk; nor are they any more likely to grant review to a petition filed by a team of attorneys composed of at least one former clerk. Put plainly, we retrieve the same null effects in the agenda model as we did in the merits model.

[Add Figure Here Later]

Conclusion

Revolving door lobbying is seen by many as a tremendous problem in government. Former members of congress and their staffers regularly retire from the legislature and go on to lobby their former colleagues. Former regulators go on to lobby for the very interests they once regulated. Highly visible anecdotal evidence suggests revolving door influence exists. Soon after he lost reelection, former Senate Majority Leader Tom Daschle joined a lobbying firm on K Street. Of course, he claimed he was a “strategic adviser” and not a lobbyist—and therefore not precluded by law from offering his services. On the other side of the aisle, former Speaker Newt Gingrich later lobbied on behalf of Freddie Mac (though he claimed he was paid simply to offer “historical advice”). These former government official tend to be highly successful in their lobbying efforts—and that leaves a bad taste in many people’s mouths.

Yet our data show that this track record of success does not exist at the Supreme Court. Former law clerks are no more likely to win their cases than other highly skilled lawyers. When we pit them against other high quality lawyers, former clerks are no more likely to win. What is more, they are not any more likely to capture their former justice’s

votes than other justices' votes. And they are no more likely to get their cases heard than lawyers seeking Supreme Court review who never clerked for the High Court.

So, why bother hiring former clerks if they are no more influential than other skilled attorneys? In his commentary on law clerk signing bonuses, Lat (2007) remarked: "it is difficult to understand why firms fight for the right to shower 26 year olds with cash." Actually, it is not so difficult to understand. *Former clerk status is a signal clients can use to determine whether an attorney is highly skilled.* No more. No less. The value to a firm from hiring former law clerks is simply that it makes the signal of high quality easier for clients to observe. A potential client may not know how skilled a lawyer or firm is, but that client can easily see whether the lawyer is a former Supreme Court clerk. In short, former clerks are not the only highly skilled and influential Supreme Court litigators, but they are among the most readily identifiable ones.

What this means, more broadly, is that highly skilled lawyers can influence outcomes at the High Court. Good lawyering matters. It could be, of course, that these highly skilled lawyers are better able to frame cases in terms of justices' preferences (Segal and Spaeth 2002), but we think the more likely explanation is that they are better crafters of persuasive legal arguments. That is, they are not simply policy parrots for justices, but songbirds that can motivate them. So, while former clerks are no more influential than other highly skilled lawyers, highly skilled lawyers are effective at influencing justices.

The next issue to address is what these findings tell us about possible reforms one could make to the other branches to reduce the influence of revolving door lobbying. (Note that we neither support nor oppose such reforms, but merely discuss their possibilities.) First, there are strict provisions in the judicial system against *ex parte* communications with judges. As a consequence, former clerks are unable to convey information outside the bounds of the case record. This limits the utility of whatever personal information about the justices they may have. Is this limitation plausible for the other branches? Likely not. There are no well defined "records" akin to a judicial record to which one must confine one's self when lobbying

other government actors. But one potential reform would be to ensure all communications between lobbyists and politicians also include a disinterested third party to observe and report on lobbying efforts. Similarly, one could require all lobbying communications be in written form and released publicly. As former Supreme Court Justice Louis Brandeis once stated: “Sunlight is said to be the best of disinfectants” (Brandeis 1913).

Second, and more obviously, justices have tenure protection and do not suffer consequences for ruling against former clerks. This dynamic surely separates justices, on the one hand, from members of Congress and regulators, on the other. The independence of the federal judiciary, in general, protects justices from concerns about how others will react to their rulings. Clearly, we do not want members of Congress to hold lifetime tenure. But one could generate the same degree of independence as justices by prohibiting members of Congress or regulators from ever lobbying in the future (or by imposing significant restrictions on when they can lobby). If regulators are not concerned about a future career with the regulated, for example, they may not have trepidation when it comes to regulating them. So lifetime bans on lobbying could generate the independence necessary to govern dispassionately.

The story for the Court seems positive. The same kind of revolving door dynamic that plagues Congress and the executive does not seem to infect the Supreme Court. Former clerks are unable to exert unique influence over the justices beyond that exerted by other highly skilled lawyers.

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