Revolving Doors and Political Selection

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Abstract

Revolving door laws restrict public servants from representing private interests before the government after they leave office. While these laws mitigate potential conflicts of interest by imposing constraints on post-public service employment, they also may affect the pool of candidates for public positions, by lowering the overall benefits of holding office. We study the consequences of revolving-door laws for legislator selection across U.S. states, exploiting the staggered roll-out of laws across states over time. We find that fewer new candidates enter politics in treated states and that incumbent legislators are less likely to leave office, leading to an increase in uncontested elections. The results on entry in particular are driven primarily by independent candidates. We provide a simple model of politician career incentives to interpret the results.

JEL Codes: D72, D73, K16

I Introduction

Public servants worldwide face legal restrictions, to varying degrees, on their ability to leave the public sector and represent private interests before the government in which they served. So-called "revolving door laws" often involve constraints on how a former official may interact with government, (e.g., whether they can interact with the department or ministry in which they served), and a cooling off period of months or years. Most often, the revolving door is presented as a problem, in that it can compromise the integrity of lawmakers and regulators, who trade favorable treatment of companies and industries for a promise (implicit or otherwise) of comfortable and remunerative post-office employment. The concern is that former officials exploit the connections they made in government to serve private rather than public interests. As a result, there have been many calls for regulations to limit revolving door behaviors, and many states have passed such laws over the past half-century, most commonly via the imposition of a "cooling off" period after a public official leaves office.

Such revolving door restrictions have been presented as a partial solution to the revolving door problem. If a politician is required to wait for up to two years before working as a lobbyist, they stock of connections may have depreciated, and they may have decided to seek alternative employment. Yet as with most policies, revolving door restrictions involve trade-offs, not merely benefits, relative to the status quo. Whereas much of the public discourse as well as academic research on revolving door laws has focused on post-office behaviors, these rules may also affect selection into and out of government. Restrictions on post-office employment may reduce the pool of candidates for public positions, by lowering the overall benefits of holding office. In doing so, revolving door restrictions may also change the composition of candidates, both by reducing the qualifications of those who do choose to seek office and also by selecting for those with more public-minded preferences. Finally, revolving door laws may encourage officials to stay in office longer since they postpone the benefits of departure, which has ambiguous consequences depending on whether one believes turnover is positive or negative overall.

In this paper, we study the effects of revolving door laws and examine their consequences for the entry and exit from state-level politics in the

United States. The relative autonomy enjoyed by states in setting conflict-of-interest regulations makes this an apt testing ground for the effects of revolving door rules. Eleven states have no restrictions on post-office employment at all, and the 39 states with such rules in place passed them at very different times. Furthermore, the timing of passage and propensity of states to pass revolving door restrictions is, perhaps surprisingly, uncorrelated with state geography or income (we discuss motivations for their passage in Section IV). For example, Massachusetts passed the earliest revolving door statutes, in 1962, while Delaware, Vermont and Oklahoma were the most recent. States without any revolving door laws include northern blue states like Minnesota and Illinois, as well as southern red states like Texas.

We exploit the staggered adoption of laws across states over time to identify the causal effect of revolving door restrictions by comparing the characteristics and behavior of legislators running for or serving in office in states that have already adopted laws (Treated) to those of legislators in states that adopt laws later on or never adopt them (Control), for office-holders during 1968–2022. We employ both standard two-way fixed effects models as well as the approach of Sun and Abraham (2021) to ensure that our analysis is robust to treatment effect heterogeneity.

Our findings indicate large effects of revolving door laws both on political selection and competition. First, there is a significant increase in the probability that incumbents seek reelection: after the passage of a revolving door law, there is a 3 percentage point increase in the probability of an incumbent rerunning (relative to a baseline of 78 percent). Interestingly, we find the opposite effect of revolving door laws on the entry of new candidates, who are *less* likely to stand for election. On average 0.1 fewer new candidates enter relative to a mean entry rate of 0.87, indicating that revolving door laws make it ex-ante less attractive to hold state office. This reduction may account at least in part for the relatively large increase in incumbent win rate, which increases by 4 percentage points relative to a base rate of 74 percent. And the decline in entry also contributes directly to the increased likelihood that a candidate stands unopposed: this increases by 7.5 percentage points from an already-high 32 percent; for incumbents, there is an increase of 7 percentage points in the probability of standing unopposed (baseline of 29 percent). Overall, our findings suggest that the

collective effect of revolving door laws is to reduce the competitiveness of political races through both the entry and exit margins.

Our final set of analyses explore the heterogeneous consequences of revolving door laws based on candidate characteristics. We consider, in particular, heterogeneity by gender, political affiliation, tenure, and human capital as captured by whether a politician holds an advanced degree. The one systematic finding is that, while revolving door restrictions reduce exit by lower human capital legislators, there is no such shift for those with advanced degrees. While this finding is best seen as preliminary, it suggests that revolving door laws may, consistent with standard "opportunity cost" models of political selection, dissuade politicians with better outside opportunities from holding office.

Most directly, our work contributes to the body of research that studies the causes and consequences of revolving door employment and regulations. Several studies, in particular, explore how limits on post-office activities affect legislators' and regulators' behavior while in office. For example, Strickland (2020) shows that fewer legislators are hired by interest groups as lobbyists when states implement longer cooling-off periods, suggesting a decay in the value of former government insiders over time. For the most part, researchers find that this value comes from personal connections (McCrain, 2018; Blanes I Vidal et al., 2012; Bertrand et al., 2014).[1] One can in turn see the potential benefits of restricting the revolving door in, e.g., Egerod (2023), who presents evidence that U.S. companies that employ former members of Congress receive more favorable regulatory and tax treatment, while Tabakovic and Wollmann (2018) show that patent examiners are more lenient toward future private sector employers in adjudicating their patent applications.[2]

More generally, we speak to the vast literature on political selection, broadly defined. A key insight from this body of work is that the quality and quantity of candidates depends critically on both the relative costs of running for office as well as on future career prospects (Bo and Finan,

[1]Emery and Faccio (2020), by contrast, argues for the opposite.

[2]Some evidence is more neutral or even positive – Shepherd and You (2020) in particular finds that staffers who later become lobbyists increase their congressman's productivity (and contacts with lobbying firms) while still employed in government; they argue that this productivity effect serves as a signal to future employers. Law and Long (2012), in an early study of revolving door rules and utility regulation, finds limited effects on electricity prices.

2018). Intuitively, revolving door laws may affect both who chooses to run for office as well as the set of officeholders that choose to stay in office – we aim to formalize these intuitions in a very simple model we present in the next section. Our paper explores a new determinant of political selection by exploring how revolving door laws impact selection, a consequence that has largely gone unstudied until now.

Finally, we note several papers that are particularly relevant for our work, in that they consider the link between post-office employment opportunities and political selection. Weschle (2021) looks at how campaign finance laws affect the decision to leave office, because less restrictive fundraising rules make staying in office more attractive relative to revolving door employment opportunities. In this analysis, revolving door restrictions are a confound, whereas we focus on them as the object of direct interest. Egerod (2022) shares our interest in linking post-office employment to the decision to hold office, though with a very distinct perspective – linking the successes of senators-turned-lobbyists to the choice of current senators to leave public office for private sector employment. Finally, the concurrent work of Gamalerio and Trombetta (2023) also considers the link between revolving door regulations and political selection, albeit in an entirely different context of local officials in Italy. Their identification comes from a city population threshold above which there is a cooling off period after leaving office for the transition into the management of State Owned Enterprises; their main finding is that revolving door restrictions *increase* the education of elected politicians.

The rest of the paper is organized as follows. The next section presents an illustrative model that generates a set of intuitive empirical predictions about how revolving door laws may impact the decision to run for office, and the decision to stay in office. In Section III we present the difference-in-differences model we will use in our empirical analysis. Section IV describes the datasets we employ, Section V provides our empirical results, and Section VI concludes.

II Illustrative Model

The reader focused on empirical results may wish to skip to the next section. In the material that follows, we provide a framework to illustrate

the following intuitive implications of revolving door rules:

1. All else equal, a potential candidate will be less likely to run for office because, for at least a subset of candidates, the value of holding office declines.

2. Candidates selecting out of running for office will be those with qualifications that would be valued more in the private market since these individuals give up more by holding office, which is now less valuable. (Candidates with social motivations for entering office will be relatively unaffected, since they have stronger non-pecuniary objectives from holding office.)

3. Incumbents will be more likely to stand for reelection. This is true both for those in office at the time revolving door laws are imposed, as well as those elected after the law's passage. For the former, revolving door laws reduce the benefit of an outside option they might have anticipated when they initially stood for election, while for the latter, only money-motivated types (who leave office relatively quickly to become lobbyists) are dissuaded from holding office, so those that remain are, on average, more likely to want to remain in politics.

We now turn to our theory, in which we consider a three-period model. For illustrative purposes, we include a number of simplifications to the structure of elections. Our approach roughly follows that of Dal Bó et al. (2017), in which we consider prospective politicians who differ in their social motivations and abilities, with an exogenous probability of election p (that is, we abstract from the election process itself).

A candidate c considers the choice of whether to run for office, seek private sector (non-lobbying) employment, or act as a lobbyist at three points in time:

1. *Run for Election* – We consider a prospective candidate who is weighing the decision to run for office or remain in the private sector. We assume an exogenous probability of election, which is independent of ability. If elected, they receive wage w and social service utility s in this period; if c is not elected (or chooses not to run) then her outside option is private earnings $o(q)$, where q reflects ability. If c is not

elected initially, we presume they stay in the private sector in later periods.

2. *Stand for reelection or lobby* – For elected candidates, we assume that the incumbency advantage is such that if they choose to rerun, they will be elected with certainty (i.e., there is a perfect incumbency advantage), earning $w + s$. If they choose not to rerun they have the option of working as a lobbyist, earning L, or working in the private sector for $o(q)$.

3. *Stand for reelection or lobby* – If a candidate chooses to stay in office at $t = 2$, they once again may run again at $t = 3$, or exit to earn lobbying or private sector pay.

We consider two regimes: with versus without revolving door regulations. We will assume that the cooling off period imposed by such rules is such that, for the first period after leaving office, c cannot work as a lobbyist, and hence will earn their private sector outside option $o(q)$.

We start with the case in which there are no constraints on post-office lobbying. Taking a politician who chooses to run for office at $t = 1, 2$, their choice at $t = 3$ is straightforward: they will run for office if and only if $w + s > L$ and $w + s > o(q)$. For reasons that will be apparent shortly, we ignore the latter condition, as any individual for whom this condition fails and $w + s < L$ will not stand for office even in period 1. Now, turning to period 2, the same condition will straightforwardly hold: an official elected at $t = 1$ for whom $w + s < L$ will obtain greater utility as a lobbyist in both periods, and will exit politics at $t = 2$.

Turning to the decision to run initially, the value from running is $(1 - p)3o(q) + p(w + s + V)$, which is weighed against $3o(q)$ – simply earning the outside option in each of three periods. V is the 'continuation value' that differs depending on whether the politician prefers public office or lobbying. If the per-period continuation value is less than $o(q)$, then the candidate will not choose to run, which is why we ignore this case above.

Overall, for the case when there are no constraints on post-office lobbying, there are two 'types' with two straightforward conditions for choosing to run:

- The 'service-minded' type, who will run iff $w + s > o(q)$, i.e., the per-period benefit of holding office exceeds the outside option.

- The money-focused type, who will run iff $\frac{w+s}{3} + \frac{2L}{3} > o(q)$, i.e., the total payoff from holding office for the first period and then lobbying for two periods exceeds the outside option.

If we consider candidate attributes (q, s) that will lead c to stand for office, it is readily seen graphically in Figure 3, panel (a). For simplicity of presentation, we set $o(q) = q$, so that the decision to run for office is the lightly-shaded area below and to the right of $s = q - w$ for the service-minded type, which includes the area to the right of $s = L - w$. For those to the left of $s = L - w$ (the money-motivated type), those who run for office will choose to lobby, and are in the darkly-shaded area below and to the right of $\frac{w+s}{3} + \frac{2L}{3} = q$. We can further distinguish, among those who choose to lobby, between those who are diverted to public office because of the opportunities that come with lobbying – they lie between $s = q - w$, the condition that defines a willingness to stand for office in the absence of lobbying – and $\frac{w+s}{3} + \frac{2L}{3} = q$ (which defines a willingness to stand for office and then lobby).

Our main interest is in how the quantity and type of candidates change with the passage of a revolving door law. We begin by looking at the case of a prospective politician, i.e., an individual deciding whether to run for office. For service-minded individuals, there is no change since they never lobby in the unconstrained case. For money-focused politicians, the cooling-off period implies a period of private earnings before acting as a lobbyist, so that under revolving door rules the decision to run for office is governed by $w + s + o(q) + L > 3o(q)$, which reduces to $\frac{w+s}{2} + \frac{L}{2} > o(q)$. Superimposing this constraint on the previous conditions for running for office in panel (b) of Figure 3, we see that this dissuades a subset of money-motivated (lobbying) politicians from seeking office; service-minded types are unaffected. This leads to lower entry into politics, and also a higher likelihood of standing for reelection among those who do. Intuitively, this is because the revolving door law leads to a substitution of one period of lobbying earnings to the private sector option. This leads to a loss of the highest-ability types, since there is now just one period of lobbying to offset the loss of private sector earnings.

To explore the consequences for incumbents at the time the cooling-off period is implemented, we consider the case of a politician that has already chosen to run, and is deciding whether to remain in office at $t = 2$. The

socially-motivated type is once again unaffected. For money-motivated incumbents – those that satisfy the condition $\frac{w+s}{3} + \frac{2L}{3} > o(q)$ – there was a sufficient combination of lobbying and office-holding benefits to motivate them run for office initially. The new condition for leaving office to lobby (rather than staying in office) is now $o(q) + L > 2(w + s)$. Intuitively, for a politician that has sufficiently low s or particularly high q, this condition will hold, and they will maintain their plan of leaving politics after one term in office. Consider instead an official with very low outside opportunities and relatively high s. Such an official would be unwilling to absorb the cost of getting only o(q) during a cooling-off period. Given that a subset of those that had planned to exit as lobbyists are induced to remain in office (while there is no effect on those who had never planned to lobby), the overall effect is again to increase the likelihood that incumbents stand for reelection. We illustrate again in Figure 3 in panel (c) – we subdivide the "would-lobby" group into those below and to the right of $q + L = 2(w + s)$, who now choose simply to say in office, versus those above this line, who still choose to lobby.

We conclude by reiterating that our objective in this paper is not to test the model itself, but rather to demonstrate how a simple, standard model of electoral politics with lobbying restrictions can yield intuitive predictions. Our approach illuminates potential impacts of such restrictions on political outcomes.

III Methodology

We exploit the staggered adoption of laws across states over time to identify the causal effect of revolving door laws on electoral competition and political selection. We compare the characteristics and behavior of legislators running or serving in office in states that adopt laws (Treated) to those of legislators in states that adopt laws later on or never adopt them (Control).

Our main estimating equation is the following two-way fixed-effects difference-in-difference specification:

$$y_{i,s,t} = \beta Treated_{s,t} + \alpha_s + \delta_{g,t} + \epsilon_{i,s,t} \tag{1}$$

Y_{ist} are the outcomes of interest, measured either at the legislator-election-year level or at the district-election-year level. $Treated_{s,t}$ is a dummy that takes the value of 1 if a revolving-door law in in place in State s in election-year t. We further include state fixed effects α_s, as well as flexible calendar-year fixed effects interacted with an indicator for belonging to the never-treated group of states, $\delta_{g,t}$. This allows us to flexibly control for the fact that never-adopters might be on different trends relative to states that adopt laws at some point. Standard errors are clustered at the state level, allowing for serial correlation over time. To investigate pre-trends, as well as the dynamic evolution of the treatment effect, we also estimate a non-parametric event-study specification:

$$Y_{ist} = \alpha_s + \delta_t + \sum_{k=-9}^{k=8} \beta_k * D^k + \epsilon_{ist} \tag{2}$$

Relative to more standard difference-in-differences designs, our framework is further complicated by the fact that elections do not take place every calendar year. Thus, we have to carefully define our relative time indicators, which do not simply reflect the difference between the calendar year and the year of adoption of the law. Instead, we define relative time indicators, D^k, in terms of the number of elections that took place before/after the adoption of the law.

Our main coefficients of interest, β and the set of β_k's, recover the average treatment effect on the treated (ATT) of the passage of revolving door laws under the assumptions of parallel trends, no anticipation, and homogeneous paths of treatment effects across cohorts.[3] Our dynamic specification allows us to inspect the plausibility of the first two assumptions. For the third assumption, we proceed in two ways. First, we estimate the weights attached to each of the 2x2 comparisons across states and periods, as recommended by de Chaisemartin and D'Haultfœuille (2020). We find that all the ATTs receive non-negative weights, thus suggesting that our TWFE estimates are not biased by the presence of negative weighting. Furthermore, in the Appendix, we provide results using an alternative estimator

[3]A recent literature has highlighted the limitations of estimating TWFE models with staggered adoption designs in the presence of treatment effect heterogeneity (de Chaisemartin and D'Haultfœuille, 2020; Borusyak et al., 2021; Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021).

that is robust to treatment effect heterogeneity, the Interaction-Weighted estimator proposed by Sun and Abraham (2021).

IV Data

As a point of departure in our empirics, we construct a dataset tracking the first date that a revolving door statute appeared in state law. We combine data from the National Conference of State Legislatures (NCSL)[4], WestLaw and HeinOnline.

State statutes vary in the particular restrictions, both in terms of which officials are covered by it and the length of the 'cooling-off' period. Some proscribe lobbying for as brief a period as six months, and some as long as two years (though Florida passed a 6-year ban in 2022, it is after the end of our sample period). For reasons of statistical power, we focus here on the date of passage as the relevant treatment date, and do not differentiate amongst variants on these rules, as long as they cover legislators.

Figure 1 shows the year of passage for the 39 states that, as of the present, have passed revolving door legislation for state legislators. As the figure shows, many of these laws were passed in the early 1990s and early 2010s. We show the geography of revolving door laws in Figure A1, which suggests little obvious geographic or socioeconomic predictors of the presence of such a law or its passage date.

To better understand the context surrounding these laws, we begin by qualitatively investigating the determinants of revolving door laws adoption. Using historical newspapers, we find that many of these laws were passed in response to state-specific scandals. These scandals were often related to corruption or bribery among government officials, and sometimes were specifically related to lobbying activity immediately post-exit. Beyond scandals with specific officials, legislation passage has been prompted by receiving a failing grade or a low ranking amongst states regarding ethics and government accountability by third-party watchdog groups. For earlier adopting states, the wake of the Watergate scandal lead to the passage of ethics legislation that closed the revolving door. Many of the early adopters of the revolving door ban were from Democratic states. Prior to 1995 only

[4]See `https://www.ncsl.org/ethics/revolving-door-prohibitions` for the NCSL list, last accessed February 14, 2024.

1 of the 17 states that passed revolving door legislation was a Republican legislature, while 3 of 17 had split legislatures.[5] Next, we proceed to empirically examine the state-level correlates of revolving door adoption and passage date. We include as predictors (all fixed at 1990-2000 values) the log of population and GDP per capita from the BEA, percent minority and urban population shares from the US Census and Democratic vote share (from the MIT Election Lab). We also include the standard deviation of Democratic vote share during 1976-1992 and a measure of corruption, which is the number of state and local officials convicted of corruption during 1976-2002, from Glaeser and Saks (2006), scaled by public employment. Lastly, we include an indicator for the presence of active corporate finance bans in the 1990s, which comes from La Raja and Schaffner (2014). The results shown in Table A2 suggest that none of these variables significantly effect the passage of the law. The results in Table A3 suggest that only the presence of corporate contribution bans are correlated with the timing of adoption, where there is a weak negative effect.

We combine the data on prohibition laws with several candidate-term and district-term-level outcomes, which we obtain from the following sources. State legislator data come from the Harvard State Legislative Election Returns, which contain state legislative general election returns from 1968 through 2016 for all 50 states. We augment these data with election returns from 2018-2022 obtained from Carl Klarner.[6] We provide summary statistics for the candidates and legislators in Table 1. We note that the rate of uncontested elections is quite high – 31% percent. Anticipating our results, this statistic may make one particularly concerned about the potential consequences of revolving door restrictions in further limiting entry and/or competition.

While these data are informative about politicians' careers in State legislatures, and their choices of running for and leaving office, they lack critical information about legislators' careers before and after office. To fill this gap, we collected new data on the biographical information of the

[5]Kansas was the only Republican legislature to adopt a revolving door law pre-1995, in 1972. The three split legislatures that passed laws pre-1995 were Washington, Kentucky, Ohio and Alaska.

[6]Klarner did the original data collection for the Harvard SLER database. The more recent data may be obtained from `https://www.klarnerpolitics.org/`, last accessed February 14, 2024.

universe of candidates and legislators. Each candidate's biography was collected via Google search, and processed using ChatGPT to extract and organize the candidate's work history and educational experience. This process is generally equivalent to hand-collecting biographical information, and we have manually validated that the use of the large language model does not appear to bias our results. However, we are limited by candidates for whom no biographical information appears in a Google search. We consider this data collection process as one of the key contributions of this project, and we hope it will represent a valuable data source for the broad research community. We currently use this data to measure politician human capital, as indicated by whether a politician has an advanced degree. This variable was obtained for approximately 79 percent of the politician-cycle observations in our sample.

We also include additional state-level time-varying controls. Most importantly, we include annual legislative salary data from the Book of the States, to control for the relative attractiveness of the outside option. This provides data on nominal legislator salaries from 1998-2020, which we convert to real 2010 dollars. To fill in the gaps for earlier years and for states with missing annual salary data, we supplement with data from Bowen and Greene (2014).

V Results

In this section, we examine how the passage of revolving door laws impacts political entry and exit, using the framework of Section III.[7]

We begin with the decision to enter politics. As our model makes clear (and in line with popular discourse as well as straightforward intuition),

[7]Note that we attempted to examine entry into lobbying around the passage of these laws, but found it to be problematic, both conceptually and empirically. Most importantly, by definition, we only observe post-office lobbying after a legislator retires from politics. But as emphasized in Section II, the exit decision is itself impacted by revolving door laws. There may also be limits to our ability to observe lobbying activity, since former officials may work as advocates for private interests – still counter to revolving door restrictions – without formally registering as a lobbyist. Furthermore, it is challenging to match legislators to lobbying records, since the matching must be done by name; the resulting fuzzy match adds noise to any estimated relationship. When we attempted to do so via `FollowtheMoney.org`, we matched only 4% percent of legislators to lobbying records. Finally, the lobbying data are only available starting in 2006, which gives us very few pre-passage observations.

revolving door restrictions lower the benefits of holding office. This is true almost by definition, as the laws constrain prospective politicians' objective function. In these analyses, the unit of observation is a district-by-election cycle, and the outcome is the number of candidates that appear for the first time. We present our main results via event plot in Figure 4, along with the regression analog in Table 2, column 1. In line with our prediction, we see a clear and discernible drop in entry that is roughly timed to the passage of the law. The point estimates imply a sizeable effect: the results in 2 suggest that revolving door laws cause a drop of 0.1 in new candidates, relative to a sample mean of 0.87.

Before turning to the effect on exit, we note that a decline in entry is observed even in the election cycle before the law's passage. That is not surprising, given the gap in timing between the writing of a bill in committee, its passage into law, and its implementation. To take just a pair of examples among many, Nevada's 2015 law was already reported as making its way through committee in early 2013 by the *Las Vegas Sun* ("Which bills made the cut in the Nevada Legislature? Here are the biggest," April 24, 2013), and Delaware's law, which went into effect on January 1, 2017, was signed by the governor on September 4, 2014.

We next provide comparable analyses for the margin of incumbent exit, in which the analysis is at the district-cycle level, and the outcome is whether the incumbent representing the district chooses to stand for reelection. Recall in our model we may expect both short- and long-term reductions in exit. In the short-run, incumbents that held office at the time the law is passed now have lower "continuation value" from leaving office to lobby, so some subset of would-be lobbyists choose instead to continue as legislators. In the longer run, since would-be lobbyists are dissuaded from entering politics, we expect that more (public-motivated) candidates will enter politics with the intention of staying in office.[8]

Our results on incumbents' decisions to remain in office appear in Figure 7 and Table 2, column 2. We observe a clear increase in the probability that an incumbent chooses to stand for reelection. The baseline probability

[8]We also note – outside the model – that legislators who choose to pass a revolving door law may be less inclined toward lobbying. Under this interpretation of the short-run effect, a reduction in exit could be the result of selection as well. We are in the process of collecting roll call data that may allow us to compare the exit decisions of supporters versus opponents of lobbying restrictions.

is quite high – the mean rerun rate is 78 percent – and the passage of a revolving door law is associated with a further 3.2 percentage point increase, as shown in Panel A of the figure. Given the decline in entry, it is unsurprising that the impact on reelection is even higher – a 4.1 percentage point increase relative to an (unconditional) mean incumbent reelection rate of 74 percent (Table 2, column 3, and Figure 8). This is consistent with the idea that stricter revolving doors laws reduce the returns to leaving office, and thus may make incumbents more likely to remain in office rather than pursue the outside option.

Finally, in Figure 5 and Table 2, column 4, we show the results for the total number of exits, including both incumbent legislators and challengers. We again find a significant drop in exiting, indicating a decline in political churn, potentially because of the lack of entry.

Taking stock of our results thus far, which indicate both higher incumbency and lower entry, we now look at the likelihood that a candidate stands for election unopposed, which is a potential consequence of these two separate selection effects. In Figure 6, we show the effects of revolving door laws on the probability of having an unopposed (single-candidate) election. We find that post-reform, the probability that the election is unopposed is 8.7% higher (Table 2, column 5), compared to the pre-reform period. (We observe a similar pattern for unopposed incumbents; see Figure 6, and also Table 2, column 6.) This is a very large impact when compared to the sample mean of just under 31% unopposed races.

To summarize our results thus far, we show a decline in both entry and exit as a result of revolving door restrictions, which together appear to reduce political competitiveness. We conclude be examining, using the sparse covariates currently available to us, how the exit effects differ by politician characteristics.[9] In Table 3, we look at how various exit margins are impacted by gender, political affiliation (with Democrats as the omitted category), and tenure. The one consistent pattern across all exit measures is that an advanced degree increases the relative likelihood of departure following the introduction of revolving door restrictions. The coefficient in the interaction of $AdvDegree$ and $Treated$, as compared to the direct ef-

[9]It is not straightforward to examine the entry margin, since entry itself is the outcome that may be impacted by revolving door laws.

fect of $Treated$, suggests that the exit decisions of advanced-degree-holders are entirely unaffected by revolving door restrictions, while those without advanced degrees are considerably more likely to remain in office. This final result provides tentative support for the "opportunity cost" view of political selection, in line with our simple model's prediction that revolving door restrictions may (relatively) dissuade legislators with higher outside options from holding office.

VI Conclusion

In this paper we study the political consequences of revolving door laws that impose a cooling off period on state-level public officials before they engage in lobbying. To organize our results, we provide a simple model which formalizes the intuition that revolving door restrictions will reduce entry into politics (since it lowers the benefit of office) and also lead to selection of politicians that are more likely to stay in office (by reducing the value of leaving office).

We examine these core predictions in a difference-in-difference framework that exploits the staggered introduction of revolving door laws across U.S. states, as well as the presence of states that have never adopted revolving door laws at all.

In line with our intuitive predictions, there is a significant increase in the probability that incumbents seek reelection, and also a reduction in entry. Together, these effects lead to an increase in incumbent win rate. Overall, our findings suggest that the collective effect of revolving door laws is to reduce the competitiveness of political races through both the entry and exit margins.

Given the importance of robust political competition for the functioning of democracy, we see our findings as suggesting that there are important costs from revolving door laws to weigh against any benefits via reduced or delayed lobbying.

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Tables and Figures

Table 1: Summary Statistics

	PANEL A: District-Level					
	Mean	Median	S.D.	Min	Max	N
NNew Entrants	0.89	1.00	0.87	0.00	21.00	76,513
Incumbent Runs	0.78	1.00	0.41	0.00	1.00	76,513
Incumbent Wins	0.74	1.00	0.44	0.00	1.00	76,513
Runs Unopposed	0.31	0.00	0.46	0.00	1.00	76,513
Incumbent Unopposed	0.28	0.00	0.45	0.00	1.00	76,513
NExiters	0.80	1.00	0.79	0.00	19.00	76,513
N Candidates	1.90	2.00	1.23	1.00	48.00	76,513
	PANEL B: Candidate-Level					
	Mean	Median	S.D.	Min	Max	N
Female	0.21	0.00	0.41	0.00	1.00	134,754
White	0.90	1.00	0.31	0.00	1.00	84,522
Democrat	0.52	1.00	0.50	0.00	1.00	125,854
Republican	0.44	0.00	0.50	0.00	1.00	137,971
Independent	0.09	0.00	0.28	0.00	1.00	137,971
Adv. Degree	0.22	0.00	0.42	0.00	1.00	109,037
Total Tenure	13.25	12.00	9.23	2.00	74.00	84,903
Vote Share	0.54	0.52	0.28	0.00	1.00	134,717
Wins	0.57	1.00	0.50	0.00	1.00	137,971
Voluntary Exit	0.41	0.00	0.49	0.00	1.00	137,971
Involuntary Exit	0.27	0.00	0.45	0.00	1.00	137,971
Lobbying within 1 Year After Leaving Office	0.02	0.00	0.13	0.00	1.00	78,318
Lobbying within 2 Years After Leaving Office	0.02	0.00	0.15	0.00	1.00	78,318
Lobbying Ever After Leaving Office	0.05	0.00	0.21	0.00	1.00	78,318
Lobbying Before Running for Office	0.08	0.00	0.27	0.00	1.00	137,971

Notes: The table shows summary statistics for our main sample. Panel A shows summary statistics aggregated at the district-by-election level, Panel B, shows summary statistics aggregated at the candidate-level. Information on election-level outcomes (vote-share, number of entrants/exiters) and certain candidate-level information (political affiliation, tenure) come from the Klarner SLERs Database. Data on gender is constructed using candidate first names. Lobbying behavior post-exit is determined by matching to the Follow the Money database, as described in Section IV. Advanced degree information is collected through biographical candidate data collection described in Section IV.

Table 2: Revolving Doors and Political Selection

	(1) NNew Entrants	(2) Incumbent Runs	(3) Incumbent Wins	(4) NExiters	(5) Unopposed	(6) Incumbent Unopposed
Treated	-0.123**	0.0307*	0.0388*	-0.127***	0.0851**	0.0744*
	[0.0516]	[0.0180]	[0.0193]	[0.0377]	[0.0376]	[0.0387]
DepVarMean (Pre-T)	0.912	0.781	0.735	0.812	0.308	0.273
Observations	76512	76512	76512	76512	76512	76512
R-sq	0.0918	0.0276	0.0291	0.0839	0.154	0.132

Notes: The table above shows the results of our main specification in Section III. Regressions are run at the district-election level. NNew entrants (1) is the sum of new entrants in a given district-election period, and NExiters (2) is the sum of exiters in a given district-election period. Incumbent runs (2) is a binary indicator equal to 1 if the incumbent runs in the election, and incumbent wins (3) is a binary variable equal to 1 if the incumbent wins. Unopposed (5) is a binary indicator equal to one if an election is unopposed. Incumbent Unopposed (6) is a binary indicator for whether an incumbent is running unopposed.

Table 3: Revolving Doors and Incumbent's Exits, heterogeneity by Candidates' characteristics

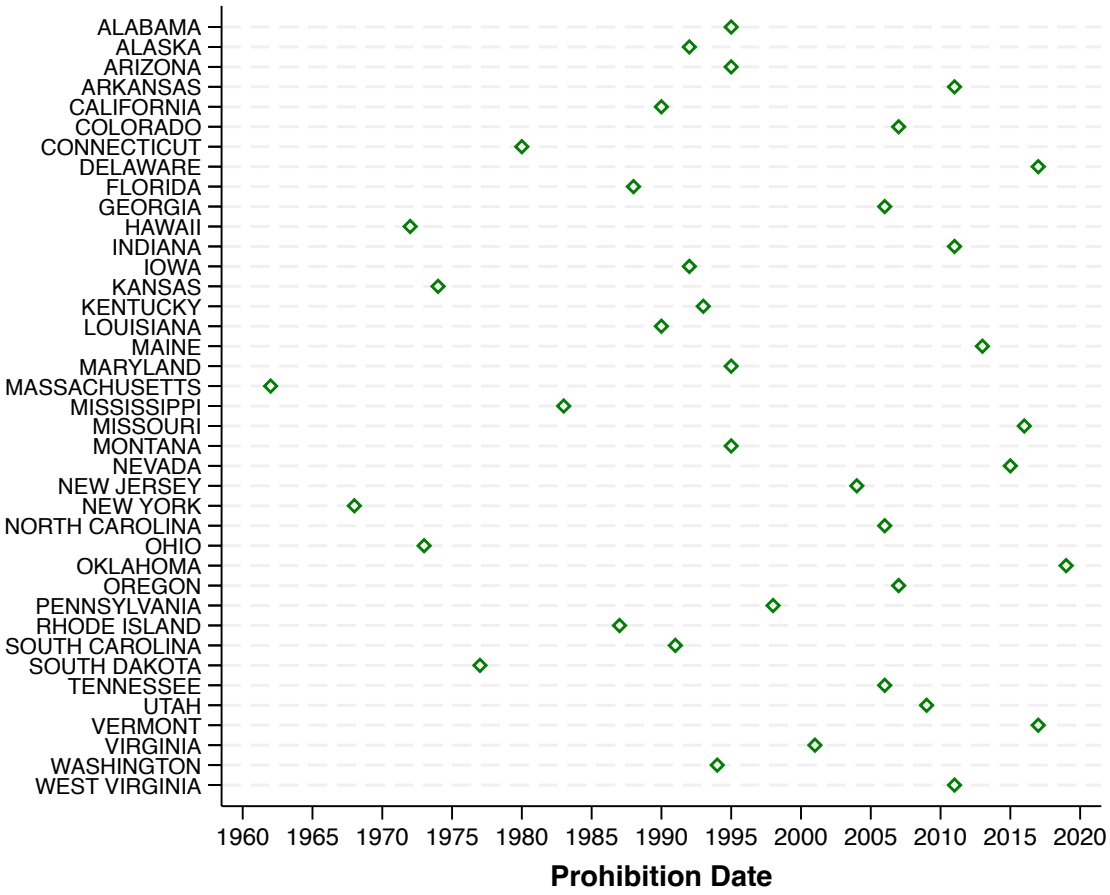
	(1) depart	(2) depart	(3) depart	(4) depart
Treated	-0.0460**	-0.0542***	-0.0151	-0.00893
	[0.0205]	[0.0147]	[0.0309]	[0.0135]
Treated × Male	-0.00684			
	[0.0199]			
Treated × Adv. Degree		0.0341**		
		[0.0153]		
Treated × Republican			-0.0376	
			[0.0498]	
Treated × Independent			-0.0231	
			[0.0256]	
Treated × Over 4yrs Tenure				-0.0935*
				[0.0458]
DepVarMean				
Observations	134748	109021	138032	138032
R-sq	0.0187	0.0303	0.0714	0.0664

	(1) Incumbent Runs	(2) Incumbent Runs	(3) Incumbent Runs	(4) Incumbent Runs
Treated	0.0520**	0.0740***	0.0243	-0.00336
	[0.0254]	[0.0194]	[0.0409]	[0.0119]
Treated × Male	0.0107			
	[0.0287]			
Treated × Adv. Degree		-0.0518**		
		[0.0197]		
Treated × Republican			0.0348	
			[0.0648]	
Treated × Independent			-0.0185	
			[0.0428]	
Treated × Over 4yrs Tenure				0.0366
				[0.0224]
DepVarMean				
Observations	134748	109021	138032	138032
R-sq	0.0315	0.0694	0.106	0.368

	(1) Incumbent Wins	(2) Incumbent Wins	(3) Incumbent Wins	(4) Incumbent Wins
Treated	0.0546**	0.0760***	0.0243	0.000951
	[0.0265]	[0.0203]	[0.0418]	[0.0124]
Treated × Male	0.0115			
	[0.0290]			
Treated × Adv. Degree		-0.0432**		
		[0.0183]		
Treated × Republican			0.0427	
			[0.0652]	
Treated × Independent			-0.0222	
			[0.0435]	
Treated × Over 4yrs Tenure				0.0351
				[0.0239]
DepVarMean				
Observations	134748	109021	138032	138032
R-sq	0.0339	0.0711	0.106	0.351

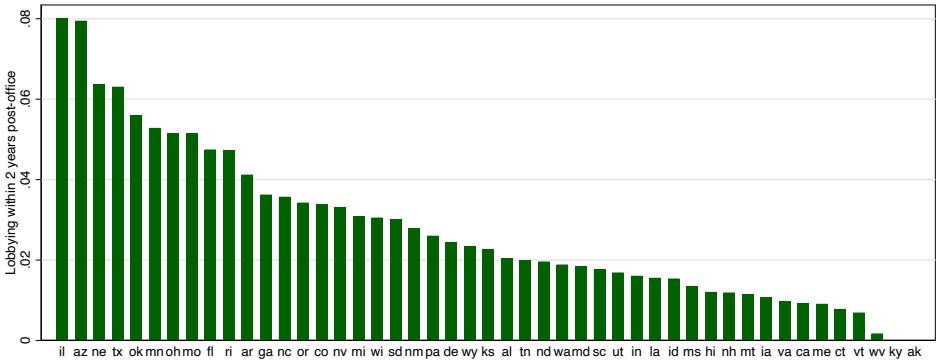
Notes: The table above shows heterogeneity of treatment across candidate gender, education, party and tenure. The upper table shows the heterogeneous effects on candidate exits, the middle table and lower table show the effects on the probability of the incumbent running and winning (respectively). Information on party and tenure come from the Klarner SLERs database. Data on gender is constructed using candidate first names. Advanced degree information is collected through biographical candidate data collection described in Section IV.

Figure 1: Revolving Door Prohibition Date by State



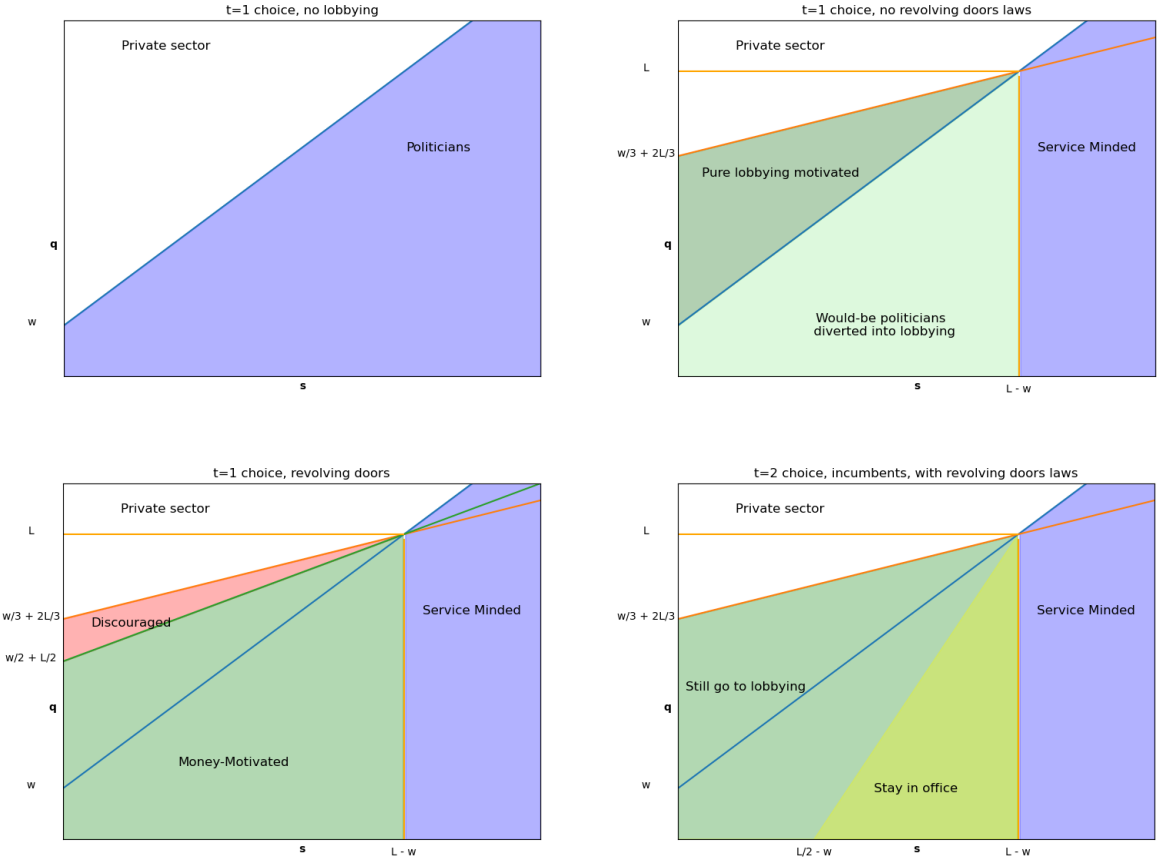
Notes: Table presents the passage date of revolving door prohibition laws by state. Information on revolving door legal codes and legislative histories come from National Conference of State Legislatures (NCSL), Westlaw and HeinOnline databases. *Note*: For the 11 states without prohibitions, (1) 6 states have no prohibitions: Idaho, Michigan, Nebraska, New Hampshire, North Dakota, and Wyoming, (2) 4 states have prohibitions that apply to executive officeholders only: Arkansas, New Mexico, Texas and Wisconsin and (3) 1 state only dictates cooling-off periods for involvement in state procurement contracts: Illinois.

Figure 2: Legislators Becoming Lobbyists within 2 years of leaving office



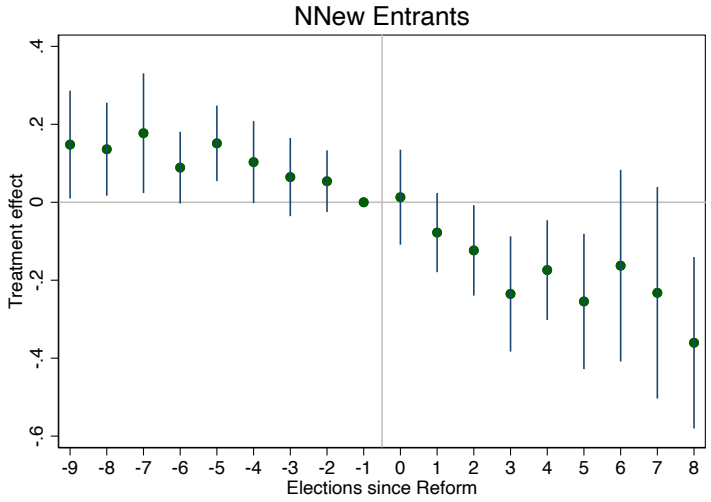
Notes: These figures show the percentage of legislators who are matched to registered lobbyist data and register as lobbyists within 2 years of level office, ordered by magnitude. Lobbyist indicators come from the fuzzy merging of legislator and lobbyist names, provided by Follow The Money, as described in Section IV.

Figure 3: Model Illustration



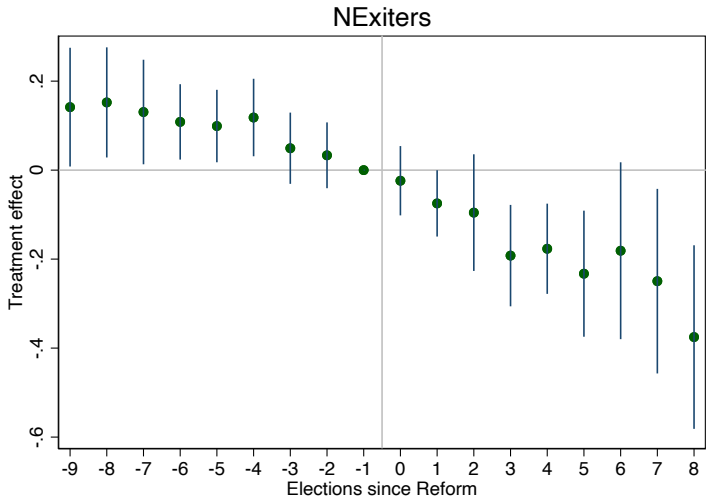
Notes: The figures above provide intuitions for the key implications of our conceptual framework as described in Section II, under the assumption that $o(q) = q$. Figures 1-3 illustrate the implications of revolving door laws in the single-period case (t=1). Figure 1 shows the division of private and public sector labor without lobbying. Figure 2 shows the division of labor among private and public sector workers, now introducing lobbying laws (where lobbyists are indicated in green). Figure 3 shows the impact of revolving door laws, particularly the group that is "diverted" to public office in the presence of such laws (indicated in red). Figure 4 illustrates the two period case ($t-2$), showing the impacts of revolving-door laws on incumbent politicians, specifically their choice to remain in office (light green) or exit into lobbying (dark green).

Figure 4: Revolving Door Laws Deter Entry into Politics



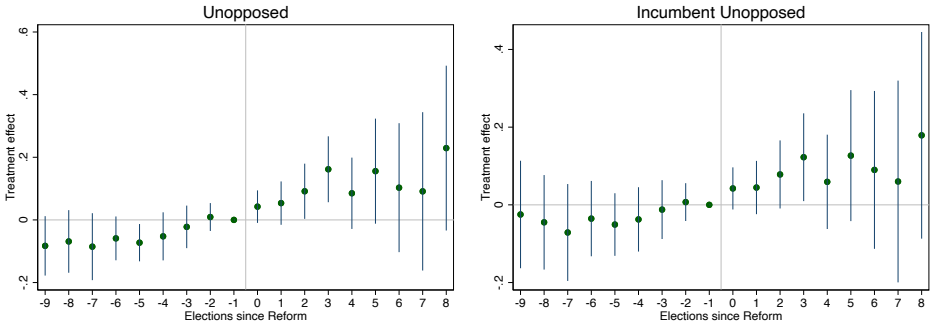
Notes: The figure shows the dynamic difference-in-differences estimates of the causal effect of revolving door laws on the total number of candidates that run for the first time. Relative time here reflects the number of elections before and after the passage of the law (calculated using term length). New entrants are defined as those who appear for the first time in the election data. Regression estimates include state and year fixed effects. Standard errors are clustered at the state-level.

Figure 5: Revolving Door Laws Deter Exit out of Politics



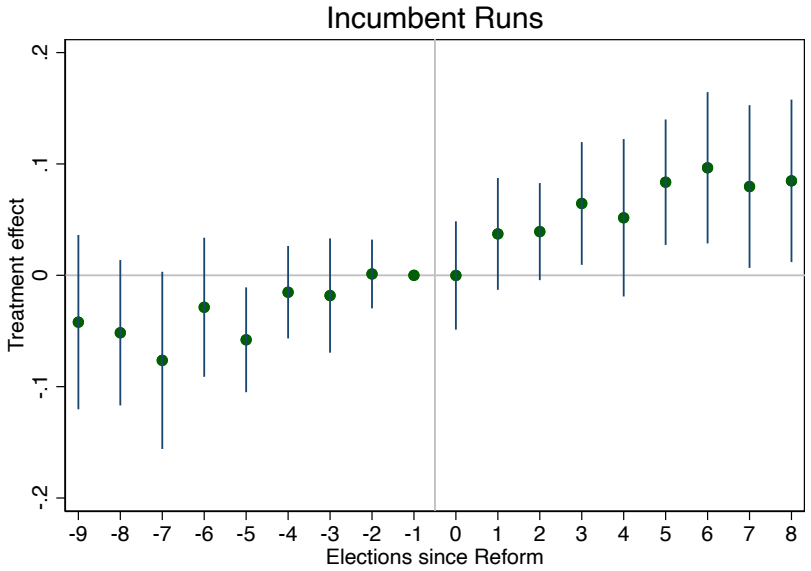
Notes: The figure shows the dynamic difference-in-differences estimates of the causal effect of revolving door laws on the total number of legislators that leave politics. Relative time here reflects the number of elections before and after the passage of the law (calculated using term length). Exiters are defined as those that leave the election data and do not reappear in subsequent years. Regression estimates include state and year fixed effects. Standard errors are clustered at the state-level.

Figure 6: Revolving Door Laws Increase Uncontested Elections



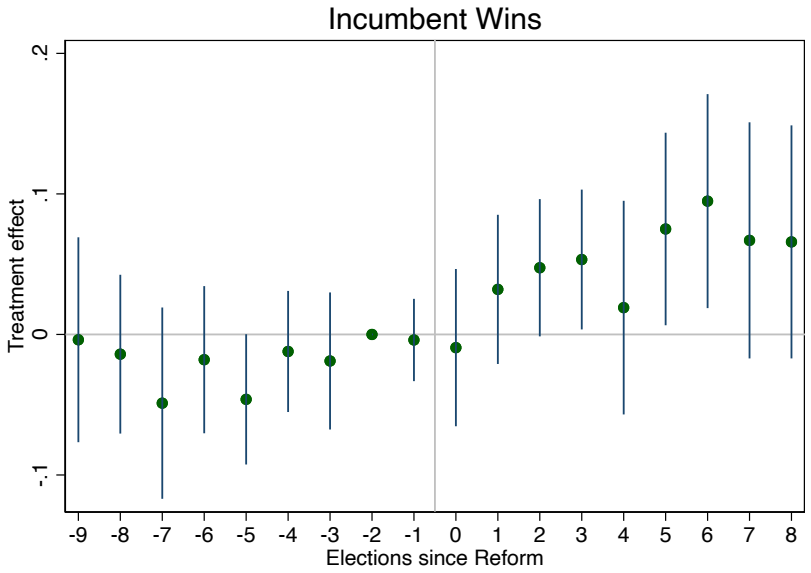
Notes: The figure shows the dynamic difference-in-differences estimates of the causal effect of revolving door laws on the prevalence of uncontested elections. Relative time here reflects the number of elections before and after the passage of the law (calculated using term length). The left panel shows the effects of revolving door laws on unopposed elections - unopposed elections are defined as an election within a given year/district where the total number of candidates is equal to one. The panel of the right shows the effect of revolving door laws on the likelihood of an incumbent standing unopposed in an election. Regression estimates include state and year fixed effects. Standard errors are clustered at the state-level.

Figure 7: Revolving Door Laws Increase the Probability of Re-Running



Notes: The figure shows the dynamic difference-in-differences estimates of the causal effect of revolving door laws on the log number of total candidates running in a given election. Relative time here reflects the number of elections before and after the passage of the law (calculated using term length). Regression estimates include state and year fixed effects. Standard errors are clustered at the state-level.

Figure 8: Revolving Door Laws Increase the Probability of Incumbent Winning Re-election



Notes: The figure shows the dynamic difference-in-differences estimates of the causal effect of revolving door laws on the log number of total candidates running in a given election. Relative time here reflects the number of elections before and after the passage of the law (calculated using term length). Regression estimates include state and year fixed effects. Standard errors are clustered at the state-level.

Appendix

Table A1: Summary Statistics, by Treatment Status

	PANEL A: District-Level			
	Ever Adopters	Never Adopters	Diff.	Std Diff.
	(1)	(2)	(2)-(1)	(3)/S.D(1)
NNew Entrants	0.88	0.91	0.03	0.035
	(0.85)	(0.91)	[0.000]	
Incumbent Runs	0.78	0.78	-0.00	0.000
	(0.41)	(0.42)	[0.196]	
Incumbent Wins	0.74	0.74	-0.00	0.000
	(0.44)	(0.44)	[0.142]	
Runs Unopposed	0.32	0.31	-0.01	-0.021
	(0.47)	(0.46)	[0.002]	
Incumbent Unopposed	0.28	0.27	-0.02	-0.044
	(0.45)	(0.44)	[0.000]	
NExiters	0.79	0.82	0.03	0.039
	(0.77)	(0.83)	[0.000]	
N Candidates	1.89	1.93	0.04	0.034
	(1.19)	(1.30)	[0.000]	
	PANEL B: Candidate-Level			
Female	0.20	0.23	0.02	0.050
	(0.40)	(0.42)	[0.000]	
White	0.91	0.87	-0.03	-0.103
	(0.29)	(0.33)	[0.000]	
Democrat	0.52	0.52	-0.00	0.000
	(0.50)	(0.50)	[0.814]	
Republican	0.45	0.43	-0.01	-0.020
	(0.50)	(0.50)	[0.000]	
Independent	0.08	0.11	0.03	0.111
	(0.27)	(0.31)	[0.000]	
Adv. Degree	0.22	0.22	0.00	0.000
	(0.42)	(0.42)	[0.891]	
Total Tenure	13.55	12.70	-0.85	-0.091
	(9.33)	(8.99)	[0.000]	
Vote Share	0.55	0.53	-0.02	-0.071
	(0.28)	(0.28)	[0.000]	
Wins	0.57	0.56	-0.00	0.000
	(0.50)	(0.50)	[0.137]	
Voluntary Exit	0.41	0.41	-0.00	0.000
	(0.49)	(0.49)	[0.959]	
Involuntary Exit	0.27	0.27	-0.00	0.000
	(0.45)	(0.44)	[0.320]	
Lobbying within 1 Year After Leaving Office	0.01	0.02	0.01	0.083
	(0.12)	(0.15)	[0.000]	
Lobbying within 2 Years After Leaving Office	0.02	0.03	0.01	0.071
	(0.14)	(0.17)	[0.000]	
Lobbying Ever After Leaving Office	0.04	0.06	0.02	0.100
	(0.20)	(0.24)	[0.000]	
Lobbying Before Running for Office	0.08	0.09	0.02	0.077
	(0.26)	(0.29)	[0.000]	

Notes: The table shows summary statistics, separately for states who ever vs. never adopted revolving door laws. Columns (1) and (2) show the variable means for the ever adopter states vs. the never adopter states. Column (3) shows the difference between (2) and (1), while column (4) gives the difference scaled by the standard deviation of (1). Panel A shows summary statistics aggregated at the district-level, Panel B, shows summary statistics aggregated at the candidate-level.

Table A2: Predictors of Laws' Adoption

	Ever Passed RD Law
Log(Pop 1996)	0.03
	[0.074]
Log(GDP Per Capita 1996)	-0.28
	[0.743]
% Urban 1990	0.00
	[0.007]
% Minority 1990	-0.00
	[0.007]
Dem Vote Share 1996	1.71
	[1.306]
SD Dem Vote Share 1976-1992	2.42
	[3.924]
Corruption Rate	0.54
	[0.625]
Corp Contrib Ban 1990s	-0.16
	[0.138]
NP Circulation 1996	-0.04
	[0.584]
N	49

Standard errors in brackets

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: The tables above shows the state-level correlates of Revolving Door adoption. The dependent variable is a binary indicator for whether a given state ever passed a Revolving Door law. State-level covariate data come from the sources described in Section IV. The regression is run using state and year fixed effects, with standard errors clustered at the state level.

Table A3: Predictors of Timing of Adoption

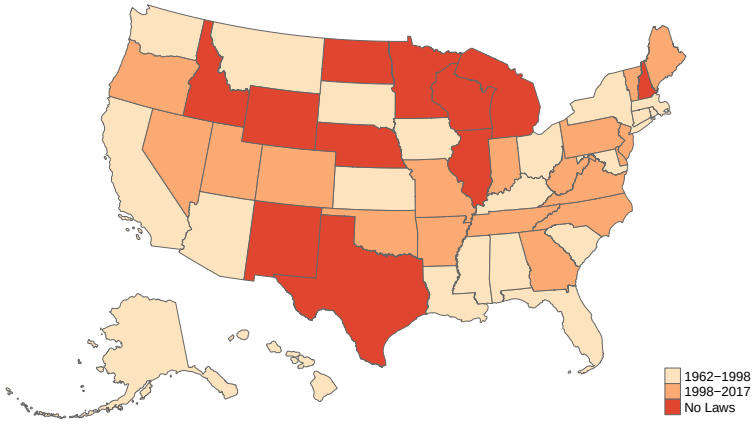
	RD Prohibition Date
Log(Pop 1996)	-0.75
	[2.809]
Log(GDP Per Capita 1996)	-16.88
	[27.332]
% Urban 1990	-0.16
	[0.230]
% Minority 1990	-0.23
	[0.249]
Dem Vote Share 1996	-31.41
	[49.375]
SD Dem Vote Share 1976-1992	50.27
	[133.860]
Corruption Rate	-34.10
	[23.633]
Corp Contrib Ban 1990s	-9.26*
	[5.378]
NP Circulation 1996	-0.98
	[19.377]

Standard errors in brackets

* $p < .1$, ** $p < .05$, *** $p < .01$

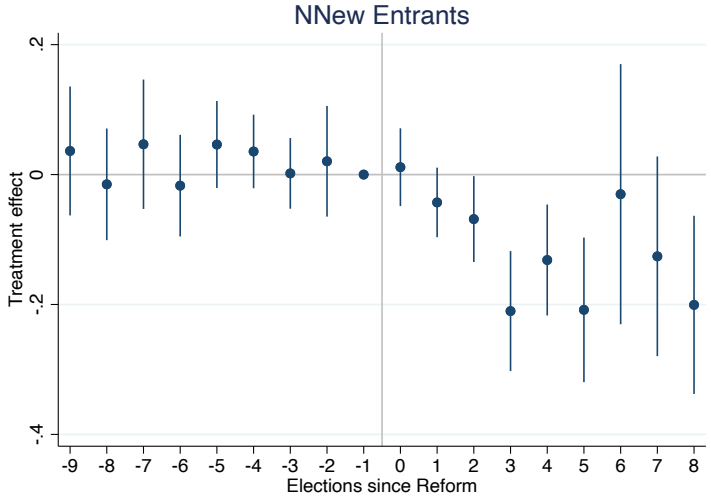
Notes: The tables above shows the state-level correlates of Revolving Door adoption. The dependent variable is the passage date of a state's Revolving Door Laws. State-level covariate data come from the sources described in Section IV The regression is run using state and year fixed effects, with standard errors clustered at the state level.

Figure A1: Passage of Revolving Door Laws by State



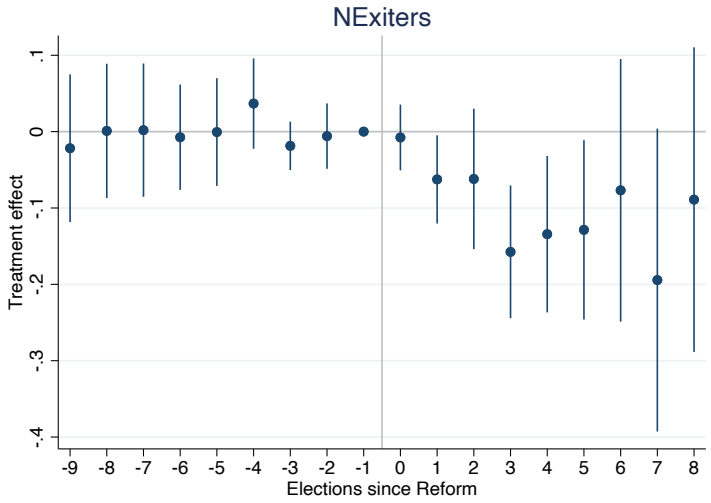
Notes: The map above shows the presence and timing passage of Revolving Door Laws by State, as it refers to the treatment of state legislators. States shaded in light orange adopted their first law affecting state legislators pre-1998. States shaded in dark orange adopted their first law affected state legislators post-1998. States shaded in red have never adopted a Revolving Door law against state legislators, which consists of (1) states who have never passed any type of Revolving Door Law and (2) states who have passed a revolving door law, but one that does not apply to state legislators.

Figure A2: Revolving Door Laws Deter Entry into Politics (Sun & Abraham, 2021)



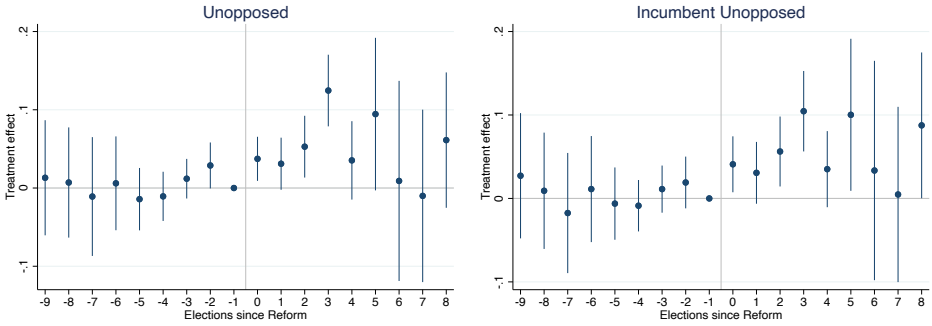
Notes: The figure shows the effect of revolving door laws on the total number of candidates that run for the first time, using the interaction weighted estimator as in Callaway and Sant'Anna (2021). New entrants are defined as those who appear for the first time in the election data. Relative time here reflects the number of elections before and after the passage of the law (calculated using term length). Regression estimates include state and year fixed effects. Standard errors are clustered at the state-level.

Figure A3: Revolving Door Laws Deter Exit out of Politics (Sun & Abraham, 2021)



Notes: The figure shows the estimated effect of revolving door laws on the total number of legislators that leave politics, using the interaction weighted estimator as in Sun and Abraham (2021). Exiters are defined as those that leave the election data and do not reappear in subsequent years. Relative time here reflects the number of elections before and after the passage of the law (calculated using term length). Regression estimates include state and year fixed effects. Standard errors are clustered at the state-level.

Figure A4: Revolving Door Laws Increase Uncontested Elections (Sun & Abraham, 2021)



Notes: The figure shows the dynamic difference-in-differences estimates of the causal effect of revolving door laws on uncontested elections, using the interaction weighted estimator as in Callaway and Sant'Anna (2021). The left panel shows the effects of revolving door laws on unopposed elections - unopposed elections are defined as an election within a given year/district where the total number of candidates is equal to one. The panel of the right shows the effect of revolving door laws on the likelihood of an incumbent standing unopposed in an election. Relative time here reflects the number of elections before and after the passage of the law (calculated using term length). Regression estimates include state and year fixed effects. Standard errors are clustered at the state-level.