Revolving Door Laws and Political Selection*

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Abstract

Revolving door laws restrict public officials from representing private interests before government after leaving office. While these laws mitigate potential conflicts of interest, they also may affect the pool of candidates for public positions by lowering the financial benefits of holding office. We study the consequences of revolving door laws for political selection in U.S. state legislatures, 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 decline in entry is particularly strong for independent and more moderate candidates, which may increase polarization. We provide a model of politician career incentives to interpret the results.

JEL Codes: D72, D73, K16

*We thank Victor Verma for outstanding support in the data collection process, Benjamin Egerod, Alexander Fouirnaies, Emeric Henry, and Luigi Zingales for helpful discussion and comments, as well as seminar participants at the Stigler Affiliate Fellows Conference, the CEPR Symposium, Boston University, CEMFI, Sciences Po, Columbia University, Northwestern University, Northeastern University, University of British Columbia. All remaining errors are our own.

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. Within the U.S., many states have passed so-called "revolving door laws" over the past half-century, most commonly through the imposition of a cooling-off period after a public official leaves office.

Revolving door laws have been presented as a partial solution to the revolving door problem, but these restrictions involve trade-offs. On the benefit side, if a politician is required to wait a year or two before working as a lobbyist, it may depreciate the value of their connections, thus reducing the ex-official's ability to exploit ties to former colleagues in government. Whereas much of the public discourse has focused on this aspect of revolving door restrictions, these rules may also affect selection into and out of government, since 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 stronger non-pecuniary preferences for holding office – in particular stronger partisan ideologies. Finally, revolving door laws may encourage officials to stay in office longer since these rules postpone the benefits of departure.

In this paper, we study the consequences of revolving door laws for entry and exit from state-level politics in the United States. The relative autonomy states enjoy in setting conflict-of-interest regulations makes this an apt testing ground for the effects of revolving door rules more generally. As of March 2025, thirty-nine states had passed revolving door restrictions at different times, while eleven states had no restrictions on post-office employment. Furthermore, the timing of passage and propensity of states to pass revolving door restrictions are, perhaps surprisingly, uncorrelated with state geography or income; we discuss motivations for their passage in Section III. 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 Arkansas and

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 adopted laws (treated) to those of legislators in states that adopt laws later on or never adopt them (control), for officeholders during 1968–2022. We employ both standard two-way fixed effects models and the approach of Sun and Abraham (2021) to ensure that our analysis is robust to treatment effect heterogeneity in staggered difference-in-difference designs.

Our analysis is guided by a simple model of political competition, in which incumbents have already paid the cost of entering politics, while the loss of lobbying opportunities is a cost to be considered by political aspirants. This "opportunity cost" framework yields two primary predictions. First, lobbying restrictions reduce entry, since the value of entering politics decreases as a result. Second, incumbent exit decreases, for two reasons: among existing incumbents, the loss of immediate lobbying opportunity makes exit less attractive, and the reduction in lobbying opportunities means that those who *do* enter are less motivated by financial concerns.

We find support for both predictions. For the entry margin, we find that, on average, 0.12 fewer new candidates enter relative to a mean entry rate of 0.91, indicating that revolving door laws make it ex-ante less attractive to hold state office. By contrast, there is a significant increase in the probability that incumbents seek reelection: After the passage of a revolving door law, the probability of an incumbent standing for reelection goes up by 3.1 percentage points on a baseline of 78%. The decline in entry may also account at least in part for the relatively large increase in incumbent win rate, which increases by 3.9 percentage points relative to a base rate of 74 percent. The decline in entry also contributes directly to the increased likelihood that a candidate stands unopposed, which increases by 8.5 percentage points from an already-high 31 percent. For incumbents, there is an increase of 7.4 percentage points in the probability of standing unopposed on a baseline of 27 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 modeling framework makes some further, more nuanced predictions about the types of politicians that are discouraged from running for office. First, higher-ability individuals – who were previously on the margin of remaining in the private sector – are more likely to opt out of holding office as a result of the reduced financial benefits of politics; in contrast, candidates with more extreme ideologies are relatively unaffected because their motives for holding office were non-financial to begin with. By the same reasoning, among officeholders, revolving door laws encourage the exit of more moderate politicians. To examine our predictions on candidate quality, we develop a new dataset on state legislators' educational credentials using a combination of web scraping and text processing via generative AI. While our results on quality are mixed, we find clear support for the predictions on ideology. Most notably, entry of independents declines more following the passage of revolving door laws relative to Democrats and Republicans; there is also a steeper decline in entry by moderates from all parties (based on the measure of Bonica, 2014) relative to the decline observed for more extreme candidates. The decline in ideologically moderate candidates suggests a possible link from revolving door restrictions to concerns expressed by Hall (2019) on the increased polarization of the candidate pool in U.S. state politics.

Our model also makes several predictions on the cross-sectional correlates of politicians who become lobbyists: they should be ideologically less extreme and higher ability; both of these stem from lower non-pecuniary motivations for staying in office. In line with this prediction, we find that candidates who eventually register as lobbyists tend to be more moderate, a pattern that is observed for Independents, Republicans, and Democrats alike.[1] Politicians-turned-lobbyists also are more likely to have a higher-level degree, and to have graduated from a higher-ranked college.

While our analysis focuses on political selection—examining who enters and exits politics—we acknowledge that this represents just one channel through which revolving door laws ultimately influence policy. Policy outcomes are equilibrium phenomena shaped by numerous factors, including institutional constraints, voter preferences, and interest group activities,

[1]This finding aligns with Egerod and Tran (2023), who document that former Members of Congress with strong partisan voting records are less likely to gain corporate board seats.

in addition to legislator characteristics. By rigorously documenting how revolving door laws alter the candidate pool and incumbent behavior, our analysis provides a critical foundation for understanding the policy implications of these regulations, though a comprehensive assessment of final policy outcomes lies beyond the scope of this study. The ideological and quality-related selection effects we identify are plausibly first-order considerations in understanding how revolving door restrictions might eventually translate into policy changes.

Most directly, our work contributes to the body of research that studies the causes and consequences of revolving door employment and regulations. While we know of no prior work that examines the impact of revolving door rules and election-related outcomes, several studies 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).[2] One can in turn see the potential benefits of restricting the revolving door in several recent studies: Egerod (2023) presents evidence that U.S. companies that employ former members of Congress receive more favorable regulatory and tax treatment; Emery and Faccio (2025) find that firms that employ former regulators are more likely to receive procurement contracts; Tabakovic and Wollmann (2018) shows that patent examiners are more lenient toward future private sector employers in adjudicating their patent applications; and Kalmenovitz et al. (2022) show, based on a salary bunching design, that federal government employees value revolving door opportunities, and are more lenient toward businesses as a result.[3]

More generally, we contribute 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 the relative costs

[2]Emery and Faccio (2025), by contrast, argues for the opposite.

[3]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.

of running for office as well as on future career prospects (Besley, 2005; Diermeier et al., 2005; Dal Bó and Finan, 2018; Gulzar, 2021). Our paper explores a new determinant of political selection by exploring how revolving door laws impact who decides to enter and exit politics, a consequence that has largely gone unstudied until now.[4]

Our finding that revolving door laws screen out ideologically moderate candidates from entering state legislative races contributes to the literature on state legislative polarization. Existing research identifies various drivers of increasing polarization, including the candidate pool (Hall, 2019; Handan-Nader et al., 2022; Phillips et al., 2024) and nationalization of state elections (Rogers, 2016; Hopkins, 2018). Our results suggest that revolving door regulations, by impacting the attractiveness of public office, can alter the composition of the candidate pool, in particular reducing the number of moderate candidates willing to run. The resultant increase in extremism of the candidate pool may contribute to the observed growth of polarization in state legislatures; our work suggests a novel mechanism through which public policy can indirectly and unintentionally influence the ideological composition of elected officials.

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 confounder, whereas we focus on them as the object of direct interest. Egerod (2022) shares our interest in relating 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 the entirely different context of local officials in Italy. Their main finding is that revolving door restrictions *increase* the education of elected politicians. While we do

[4]By studying the effect of revolving door laws on the labor supply of politicians, we also speak to a large literature in labor economics studying the role of dynamic career returns and outside options in determining occupational choice (Roy, 1951; Keane and Wolpin, 1997; Sullivan, 2010; Altonji et al., 2016; Taber and Vejlin, 2020).

not observe any such pattern (and our theoretical framework predicts the opposite), several institutional differences may explain these differences. First, the Italian context focuses specifically on transitions to state-owned enterprises and bureaucracy, whereas our U.S. setting primarily involves transitions to private-sector lobbying. This distinction is crucial because, in the Italian context, politically connected positions may serve as critical "parachutes" for low-human-capital politicians who have fewer attractive private-sector alternatives. By contrast, in U.S. state legislatures, lobbying opportunities may be particularly valuable to and targeted toward higher-ability politicians who can leverage policy expertise and connections to command premium compensation. Second, the difference in political level (municipal vs. state) and system (parliamentary vs. presidential) likely creates different selection mechanisms and career incentives. These contrasting findings highlight that the impact of revolving door regulations is highly context-dependent, suggesting careful consideration of institutional environments when designing such restrictions.

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. Section III describes the datasets we employ. In Section IV we present the difference-in-differences model we will use in our empirical analysis. Section V provides our empirical results, and Section VI concludes.

II Illustrative Model

In this section, we provide a framework to illustrate the implications of revolving door rules on political selection. Our model's implications are that the effects of revolving door laws are:

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. The candidates who select out of running for office will be those with qualifications that would be valued more in the private market (who give up relatively more as the value of holding office declines), while

those with stronger ideological motivations (who were less motivated by pecuniary concerns in the first place) will be relatively unaffected.

3. Incumbents will be more likely to stand for reelection, both for those in office at the time revolving door laws are imposed, as well as those elected after the law's passage. In both cases, the value of exiting politics declines, regardless of their initial motivations for entering politics.

4. The increase in incumbents running for reelection is driven by lower ability politicians (who have weak outside earning opportunities) and more moderate politicians (since more extreme politicians stand for reelection regardless of lobbying opportunities).

We consider a three-period model that provides a simplified representation of the electoral process. Our approach roughly follows that of Dal Bó et al. (2017), in which we consider prospective politicians who differ in their non-pecuniary motivations and abilities. While non-financial concerns may incorporate a range of benefits from holding office, we focus on political ideology, given its close connection to the theoretical framework developed in Hall (2019) and the availability of relevant data based on donations and roll call votes. In short, as in a standard citizen-candidate model, politicians with more extreme ideologies gain greater utility from running for office, since the policies that would be enacted in their absence are further from their own desired policies.

Also as in Dal Bó et al. (2017), we assume an exogenous probability of election p – that is, we abstract from the election process itself. We return to consider how relaxing this assumption may affect our model's prediction later in this section.

A candidate c evaluates 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* – Prospective candidate c weighs the decision to run for office or remain in the private sector. As noted above, we assume an exogenous probability of election, which is independent of ability. If elected, c receives wage w and non-pecuniary utility i in this period; if c is not elected (or chooses not to run) then their outside option is

private earnings q, where q reflects ability. If c is not elected initially, we assume 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), generating utility $w+i$. If they choose not to rerun they have the option of working as a lobbyist, earning L, or working in the private sector earning 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 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 $w + i > L$ and $w + i > q$. For reasons that will be apparent shortly, we ignore the latter condition, as any individual for whom this condition fails and $w + i < L$ 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 $w + i < L$ will obtain greater utility as a lobbyist in both periods, and will exit politics at $t = 2$.

For the decision to run initially, the value from running is $(1 - p)3q + p(w + i + V)$, which is weighed against $3q$ – 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 q, then the candidate will not choose to run, which is why we ignore this case above.

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

- The 'ideological' type, who will run if and only if $w + i > q$, i.e., the per-period benefit of holding office exceeds the outside option.

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

We illustrate the candidate attributes $\{q, i\}$ that will lead c to stand for office in Figure 1. In panel (a), we show the relevant combinations in the absence of any revolving door opportunities, which is simply those with sufficiently high ideological concerns to offset the loss of private earnings – the darker purple-shaded region. In panel (b), we add the opportunity to earn post-office lobbying income. This has two effects – first, more candidates run for office, since this loosens the constraint for "money-focused" types, leading to the entry of candidates in the darker-shaded green region. It also shifts the behavior of candidates who would have run for office and stayed in politics, to exiting politics after $t = 1$ to lobby – the less ideologically-motivated candidates in the light-shaded green region.

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 "ideological" individuals, there is no change since they never lobby in the unconstrained case. For money-focused politicians, the cooling-off rule 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 $w + i + q + L > 3q$, which reduces to $\frac{w+i}{2} + \frac{L}{2} > q$. Superimposing this constraint on the previous conditions for running for office in panel (c) of Figure 1, we see that this dissuades a subset of money-motivated (lobbying) politicians from seeking office. Ideological types are unaffected, hence there is an overall reduction in the number of candidates. By the same reasoning, our model implies a relative increase in more ideologically extreme candidates, since only less ideological ones opt out as a result of the law. Further, because the subset of (money-motivated) candidates that opt out of running for office are those with the best outside options (the triangular wedge at the top of the shaded region), we also expect a relative decline in the prevalence of higher-ability candidates.

Turning to the decision to stand for reelection, we also predict a lower exit rate, i.e., a higher likelihood of standing for reelection among those who do run. Intuitively, this increase is because the revolving door law

leads to a substitution of one period of lobbying earnings to the private sector option so that, conditional on having decided to run for office, it is less attractive to leave. We can see the specific $\{q, i\}$ that are affected by this change in panel (d) – a subset of politicians who would have gone into lobbying now remain in office for additional terms, because the financial payoff of lobbying has declined. This lighter-shaded region is to the right of those who still go into lobbying despite revolving door laws, and to the left of those who would have stayed in politics even in the absence of revolving door restrictions. Thus, revolving door laws result in an increase in the proportion of incumbents who rerun that are relatively moderate, and also an increase in the proportion of exiting politicians that are moderates. Note that we should observe these effects whether or not politicians are elected before or after the passage of revolving door restriction. There is no clear prediction on ability in this case. While, for a given level of ideology, we predict a higher exit rate of higher-quality politicians, the overall impact on quality cannot straightforwardly be signed.

There are several natural questions that arise in mapping the model to features of electoral politics. Most obviously, since revolving door laws lead to reduced entry, we need to consider how the model's predictions might change if we were to endogenize the probability of election, p.

In the model as specified above, there is no impact on the decision to run for office initially, since a higher win probability decreases the chances of simply earning the outside option – when compared against the decision not to run, p thus drops out of the expression. If we add a cost of running – another element we have omitted to simplify the exposition above – then p will not cancel out so neatly. Increasing p will then straightforwardly raise the value of running for office, relative to the cost of mounting a campaign. In equilibrium, this should reduce the impact of revolving door laws on exit (holding the cost of a campaign costly) but not eliminate this effect entirely. For incumbents, the lower probability of entry naturally reinforces the higher probability of staying in office from revolving door laws – the cost of running a campaign is further justified (or even reduced in cost) if competition declines.

While there are further directions for augmenting the model, such as allowing for differential lobbying earnings as a function of seniority, or incorporating a market for lobbyists, our model aims to highlight the poten-

tial impacts that revolving door restrictions may have on political economy outcomes that we can capture in the data, and to do so in the context of a simple, standard model of electoral politics.

III Background, Data, and Descriptive Statistics

As a starting point in our empirical analysis, 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), WestLaw and HeinOnline.[5]

State statutes vary in their particular restrictions, both in the officials covered by them and in 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 on the date of passage as the relevant treatment date, and do not differentiate among variants in these rules, as long as they cover legislators.

Figure 2 shows the year of passage for the 39 states that, as of 2022, had passed revolving door legislation for state legislators. As the figure shows, many of these laws were passed in the early 1990s through the early 2010s. We show the geography of revolving door laws in Appendix Figure A1, which suggests little obvious geographic or socioeconomic predictors of the presence of such a law or its passage date; shortly, we will examine in more detail whether the passage of such a law or its date of passage is correlated with state attributes.

Our identification strategy, described in detail in the next section, exploits differences in timing across states in the passage of revolving door laws. Since, as described below, we may obtain data on electoral outcomes only for 1968 onward, we use 18 states passing revolving door laws 1998 and later as our 'treated' legislatures, since this allows for sufficiently many pre-passage electoral cycles – even for states with 4-year cycles – to estimate pre-trends; we also include the '11 never-treated' states as controls.

[5]See `https://www.ncsl.org/ethics/revolving-door-prohibitions` for the NCSL list, last accessed February 21, 2025.

The decisions of whether to pass revolving door legislation and when to do so are not random. At least as reported in the media, legislators had diverse motivations for passing laws that would constrain their own post-office activities. These often are attempts at cleaning up legislatures' public images due to poor performance on integrity "report cards" (e.g., Maine and Nevada) and ethics scandals, both state-specific (e.g., North Carolina and New Jersey) and national (both Kansas and Hawaii passed their laws in response to Watergate). Perhaps because the laws were often a reaction to public pressure, in each case legislation was passed with very strong support. We were able to collect roll call results for 16 of the 18 states that passed revolving door restrictions since 1998; in all cases, at least 75% of votes were cast in favor, and in 8 of 14 states at least 90% were in favor (in Indiana and West Virginia support was unanimous).

Additionally, in a number of cases, revolving door restrictions were passed as part of a broader suite of ethics reforms. Other changes include more stringent financial disclosure rules, campaign finance restrictions, and rules limiting the acceptance of gifts. We estimate about a third of laws passed in our sample are part of broader ethics legislation, with the remainder being amendments/additions to existing legislation.

Both of these features of reform complicate the interpretation of our results: political competition may directly react to ethics scandals, and other features of reform may also impact political entry and exit. However, revolving door restrictions make some distinct predictions from other rules that were generally part of ethics reforms as well as the taint of scandal that might have prompted reform. Both scandal and most ethics rules make it less desirable to hold office, which should increase exit and decrease entry. By contrast, revolving door restrictions make immediate departure from politics less attractive, and thus as we saw in our model, may encourage incumbents to remain in office. (Further, to the extent that the effects we document do in fact capture the consequences of ethics rules and restrictions more broadly, that remains of interest even if it cannot be linked to any specific reform; we reiterate, however, that many reforms were more focused on the revolving door specifically.)

To address the broader concern that the decision to pass revolving door laws is correlated with other features of a state's economic or political circumstances, we empirically examine whether there are significant state-

level predictors 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 Bureau of Economic Analysis, 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. Additionally, we include an indicator for the presence of active corporate political contribution bans in the 1990s, which comes from La Raja and Schaffner (2014). Lastly, we include 1996 newspaper circulation data from Djourelova et al. (2024) as a measure of local newspaper presence, which is likely to affect the ability of voters to learn about revolving door lobbyists. The results shown in Appendix Tables A1 and A2 suggest that none of these variables is significantly correlated with the passage of revolving door laws.

We combine our data on prohibition laws with a range of candidate-term and district-term-level outcomes, which we obtain from the following sources. Data on state legislative election results come from the Harvard State Legislative Election Returns (SLERs) database, which contains state legislative general election returns for 1968-2016 for all 50 states. We augment these data with election returns from 2018-2022 obtained from Carl Klarner.[6]

We use a range of sources to capture legislators' attributes beyond the decision to stand for and retire from office. Gender is imputed using a historical name-based approach, which provides a mapping from legislator name to gender.[7] Information on candidate race and ethnicity is merged in from a companion dataset, also compiled by Carl Klarner. For ideology, we focus primarily on the Campaign Finance (CF) scores developed by Bonica (2014). These are constructed based on the ideologies of a politician's donors, which in turn are calculated based on the full portfolio (at all levels of government) of politicians supported by a donor. The major advantage of CF scores relative to the roll-call-based measure of Shor and McCarty

[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 21, 2025.

[7]Gender is constructed from names using the following R-package: gender.

(2011) is that it is possible to measure ideology even for candidates who never hold office.

In addition to these basic demographic characteristics, we also collected new data on the biographical information of the universe of candidates (i.e., not limited to elected legislators) who participated in state legislative elections during 1968-2022. We consider this data collection process to be a contribution of this project, and hope it will represent a valuable data source for the broader research community. The process to construct this dataset was as follows. Each candidate's biography was collected via Google searches using information on name, state and year that an individual first ran for office. The resulting search data was then processed using ChatGPT to extract and organize the candidate's work history and educational experience. We ran this process on 130,132 unique candidates. The resulting output identified 73,877 candidates (56.8%) with education information available in the extracted text (either undergraduate degree/institution or highest degree/institution), and 46,586 candidates (35.8%) with identifiable work history information. On a small sample of candidates, we manually assessed the prevalence of false positives and false negatives in these machine-generated biographies, focused on candidate age, which allowed us to assess whether the incorrect person was, in all likelihood, found via Google search. Of the 100 candidates we chose at random, we were able to identify an age through manual inspection for 45; for all of these candidates, our process correctly identified the age for all of them. For the remaining 55 for whom no age was available, our process correctly identified 39 as having no relevant information, but assigned an incorrect age to 16. These findings suggests a relatively high degree of accuracy (84%) and also that false positives may be a bigger concern than false negatives. We additionally looked at the correlates of missing data of various types – as expected, by far the dominant predictor of missing data is the year when a candidate left office, since records are sparser as we go back in time (see Appendix Table A3).

We use the resulting dataset to generate two education-based measures of politicians' human capital. Specifically, to generate a variable that indicates whether a politician has an advanced degree, we take the information on education and work history from ChatGPT output and identify keywords related to different types of advanced degrees. For instance, we

identify those with a law degree by looking for the keywords such as "JD", "J.D" or "Juris Doctorate" in the education field, as well as looking for keywords like "lawyer," "attorney," or "judge" in the work history field. We use a similar process to identify other types of advanced degrees (MBA, Doctorate, etc.) using the education field. This variable was obtained for approximately 79% of the politician-cycle observations in our sample. From the education field, we have also imputed measures of prestige based on undergraduate college selectivity. To do so we took the 3231 schools that were listed as undergraduate institutions, and then asked ChatGPT to provide the fraction of students who were accepted in 2023 as well as the SAT score inter-quartile range. We define 'high quality" undergrad institutions as those with either (1) an below median acceptance rate or (2) an above median 25th percentile SAT score.

Additionally, we include information on running for federal elections as an alternative measure of candidate quality. To measure this, we use data from Phillips et al. (2024) which contains information on primary/general elections for federal office, and includes an indicator for whether a federal candidate was ever a state legislator. We perform a matching procedure based on either candidate ID (when available) or name, to identify which candidates in the SLERs Database ever ran for federal office. Using information on the timing of federal elections and the timing of legislator entry/exit in the SLERs database, we also construct indicators for whether legislators ran for federal office before or after holding state office.

Data on lobbying activity come from `FollowTheMoney.org` (now a part of OpenSecrets), which provides a list of registered lobbyists for each year and state for 2006-2022.[8] To successfully match legislator names to lobbyist names, we use fuzzy string matching techniques in R to link the databases based on name similarity. Specifically, we begin by assigning a gender to each legislator and lobbyist based on their first name, using name-based identification programs in R. We then match legislator-lobbyist pairs within gender based on first name similarly, conditioning on having identical last names. Doing this within gender avoids matching names that are high in similarity but correspond to different genders (i.e., Eric vs. Erica). With this match in hand, we then link legislators to the registered lobbyist data

[8]Some states have lobbyist information pre-2006, but all states have lobbyist information post-2006.

for the state they represented, which is where they are most likely to lobby.[9] Finally, we allow for out-of-state lobbying. To avoid false positives (especially for common names), we allow for matches to the lists of registered lobbyists in states that border the one where an individual served as legislator. We then manually check each legislator-lobbyist match (for both in- and out-of-state matches), to identify and re-code false negatives and remove false positives.[10] After the fuzzy match, we identified 5,960 unique legislators registered as lobbyists, which constitutes approximately 5.6% of all legislators in our data.

Data from the National Survey of State Lobbyists (SSL) provide a representative benchmark to compare with our own lobbying data.[11] The SSL reports that approximately 5% of lobbyists surveyed are former state legislators, which closely matches the percentage of legislators we identify as lobbyists in the SLERs dataset. The SSL also provides information on frequency of lobbying activity by industry/topic area, which allow us to inspect the distribution of lobbying activity across sectors in Appendix Table A4. Former legislators lobby in similar industries to non-legislators, with some modest differences: former legislators lobby more on education and less on tax issues.[12]

We also include in our main analysis additional state-level time-varying controls. Most importantly, we include annual legislative salary data from the Book of the States to control for the financial attractiveness of holding office. This source provides data on nominal legislator salaries from 1998-2020, which we convert to real 2010 dollars. To fill in the gaps for earlier

[9]State does not vary within legislator, i.e., no politician in our data has been elected to multiple state legislatures.

[10]False negatives can arise from names with low string similarity, but can be identified as common nicknames or misspellings, such as "Richard" vs. "Ricahrd." False positives include legislators who lobby in the same year for which they are in office (and thus are unlikely to be the same person in the lobbying register), or names with high string similarities that are obvious mismatches created by the (imperfect) fuzzy match, such as Ryan and Bryan.

[11]The survey was conducted by researchers from 12 American universities, with support from several nonpartisan organizations including the National Institute for Civil Discourse (University of Arizona), the Thomas S. Foley Institute for Public Policy and Public Service, the William Ruckleshaus Center, and Washington State University's Division of Governmental Studies and Services. Further details on the data can be found in Schreckhise et al. (2024).

[12]While data on lobbying industries are available at the client-level in the FollowTheMoney database, lobbyist-level information is sparse and inconsistently available across states and years, which is why we need to rely on the SSL data for these figures.

years and for states with missing annual salary data, we supplement with data from Bowen and Greene (2014).

We provide summary statistics for candidates and legislators in state legislative elections in Table 1. Note that the rate of uncontested elections is quite high – 31%. 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.

Additionally, we provide summary statistics comparing state candidates and elected legislators to members of the U.S. Congress in Appendix Table A5. We find that state candidates/legislators are similar to national legislators in terms of age, at an average of about 42-45 years old. They are also comparable in gender composition, with the U.S. Congress having slightly more male legislators on average compared to state legislatures. We also see that, while state and national legislatures are similar in terms of race, there are large differences by ethnicity. 6% of elected congress members are of Hispanic origin, whereas this number is only 3% for state legislators and 0.26% for state candidates. The composition of Democrats versus Republicans is similar across state and national legislators. However, there are significantly more independent candidates and legislators at the state-level, 2% and 3% respectively, compared to only 0.2% of elected members of the U.S. Congress. Similarly, there are notable differences in ideology by party (as measured by CF scores and NP-ideology scores for elected state/national legislators based on donation data and roll-call voting). We see that Democratic state legislators tend to have more liberal voting behaviors compared to the U.S. Congress, whereas Republican state legislators tend to have more conservative voting behavior. For lobbying activity, we see that state candidates are less likely to be appear as lobbyists than state legislators, at 4% and 11% respectively, and both are far less likely to register as lobbyists relative to the 29% of U.S. Congress members that appear as lobbyists, as reported in Palmer and Schneer (2019).

We conclude this section by comparing the attributes of politicians who go on to register as lobbyists versus those that do not. While not our primary focus, we view these descriptive results as a further contribution of our paper.

In Figure 3, we examine whether there are differences in ideology between lobbyists and non-lobbyists, *within* political groupings (Democrats,

Republicans, and independents), based on CF scores. As noted above, focusing on this measure allows us to look at how ideology may differ for elected legislators and also for the broader pool of candidates. In panel (a) we show the CF score distribution for all candidates, irrespective of party affiliation. Non-lobbyists clearly exhibit more extreme ideologies on both the left and right. Panels (b) and (c) show the distributions for Democrats and Republicans separately. In both cases, non-lobbyists have more extreme ideologies, emphasizing that the pattern in panel (a) is not simply a function of fewer independents becoming lobbyists. Finally, in panel (d) we show the distributions for independents; for this subgroup as well, non-lobbyists have more extreme ideologies. In the context of our model, the greater moderation of lobbyists in the cross-section is a natural result of a greater financial rather than ideological focus. To the extent that future lobbyists are most apt to be deterred by revolving door laws, this provides suggestive evidence in support of the prediction that such laws will differentially select out moderate political candidates.

Turning to ability, in Appendix Table A6 we compare lobbyists versus non-lobbyists along our three dimensions of quality – having an advanced degree, attending a high-quality undergraduate institution, and running for federal office – and do so separately for Democrats, Republicans, and independents. We consistently observe higher quality measures for lobbyists as compared to non-lobbyists. While this is not a strong prediction of our model, given its dependence on the initial distribution of attributes in the population, this difference highlights another potential consequence of dissuading would-be lobbyists from entering office.

IV 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:

$$Y_{ist} = \beta Treated_{st} + \alpha_s + \delta_{gt} + \epsilon_{ist} \quad (1)$$

Y_{ist} are the outcomes of interest, measured at the district-election-year level. $Treated_{st}$ 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 and calendar-year fixed effects interacted with an indicator for belonging to the never-treated group of states, δ_{gt}. This allows us to flexibly control for the fact that never-adopters may 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.[13] To investigate pre-trends, as well as the dynamic evolution of the treatment effect, we also estimate a non-parametric event-study specification:

$$Y_{ist} = \alpha_s + \delta_{gt} + \sum_{k=-9}^{k=8} \beta_k * D^k + \epsilon_{ist} \quad (2)$$

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.[14] 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

[13] Results are virtually unchanged if we use district rather than state fixed effects.

[14] 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).

our TWFE estimates are not biased by the presence of negative weighting. Second, in the Appendix, we provide results using an alternative estimator that is robust to treatment effect heterogeneity, the interaction-weighted estimator proposed by Sun and Abraham (2021).

V Results

In this section, we examine how the passage of revolving door laws impacts political entry and exit, using the framework of Section IV. We begin with the decision to enter politics. As our model makes clear (and in line with popular discourse as well as straightforward intuition), revolving door restrictions lower the benefits of holding office. This is true almost by definition, as the laws constrain prospective politicians' objective function. In our first 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.

Figure 4 presents our main results, 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 Table 2 suggest that revolving door laws cause a drop of 0.12 in new candidates, relative to a sample mean of 0.91.[15]

We next provide comparable analyses for the margin of incumbent exit, so 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 ideologically-motivated

[15]A slight decline in entry appears already in the election cycle before the law's passage. While not statistically significant, such an early decline would not be surprising, given the gap in timing between the writing of a bill in committee, its passage into law, and its implementation. For example, 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.

candidates will enter politics with the intention of staying in office.[16]

Figure 5 and Table 2, column 2 present our results on incumbents' decisions to remain in office. We observe a clear increase in the probability that an incumbent chooses to stand for reelection. The baseline probability is quite high – the mean rerun rate is 78% – and the passage of a revolving door law is associated with a further 3.1 percentage point increase. Given the decline in entry, it is unsurprising that the impact on reelection is even higher – a 3.9 percentage point increase relative to an unconditional mean incumbent reelection rate of 74% (Table 2, column 3, and Figure 6). 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 of lobbying.

Finally, Figure 7 and column 4 of Table 2 show the results for the total number of exits, including both incumbent legislators and challengers. We again find a significant drop in exiting, by about 0.13, indicating a decline in political churn, potentially because of the lack of entry.

Given that we have found 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. Figure 8 shows 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 8.5 percentage points higher (Table 2, column 5), compared to the pre-reform period. We observe a similar pattern for unopposed incumbents in Figure 8, and Table 2, column 6. This is a very large impact when compared to the sample mean of just under 31% unopposed races.

We then investigate heterogeneity in the effects of revolving-door laws based on two institutional characteristics of state legislatures that concur to shape political selection. Appendix Tables A8 and A7 present analyses based on legislative salary levels and whether the legislature is full-time or part-time, respectively. The results reveal a consistent pattern: revolving door laws have substantially stronger effects in both high-salary and

[16]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. Given that revolving door laws appear to be most commonly passed as a result of external pressures, this is less likely to be a dominant explanation.

full-time legislatures compared to their low-salary and part-time counterparts. These more pronounced effects likely reflect two complementary mechanisms. First, professional and high-salary legislatures tend to attract candidates with greater professional qualifications and higher-value outside options, for whom post-legislative career restrictions represent a more significant constraint. Second, legislators in these contexts are more dependent on their political career path, making the devaluation of post-legislative lobbying opportunities particularly consequential for their career decisions. Overall, these findings suggest that revolving door restrictions have more profound effects on political competition precisely in those legislative settings designed to attract more professionalized political service.

V.1 Robustness

V.1.1 Alternative Estimators

Our main analyses rely on two-way fixed effects (TWFE) dynamic difference-in-differences estimators. The use of a dynamic event-study specification alleviates the most obvious concerns related to the presence of treatment effect heterogeneity over time that would lead to biases in the standard static TWFE estimator (Goodman-Bacon, 2021). One might still be concerned about potential treatment effect heterogeneity across units. We therefore adopt the interaction-weighted estimator developed by Sun and Abraham (2021), which is specifically designed to account for such heterogeneity in staggered difference-in-differences settings. Appendix Figures A2 – A4 display the results using this alternative estimation method. The coefficients maintain similar patterns of significance and magnitude as in our primary specifications, which bolsters confidence in our main findings. The consistency across methodological approaches suggests that our results capture genuine policy effects rather than artifacts of a particular estimation technique.

We further validate our findings by implementing the estimator proposed by de Chaisemartin and D'Haultfoeuille (2022). We do not simply perform the de Chaisemartin and D'Haultfoeuille estimator, but also conduct the calculation of negative weights importance to assess the potential bias in our TWFE estimates. This diagnostic reveals that none of the possible 2x2 comparisons receive negative weights in our setting, suggesting

that the concerns about TWFE bias are limited in our application.

V.1.2 Alternative Control Group

When estimating Sun and Abraham (2021), we proceed by using the last treated cohort as our control group, an approach appropriate for settings without never-treated units. To test the sensitivity of our results to this choice, we construct an alternative "rolling control group" that incorporates all not-yet-treated units available in each time period. The results from this alternative approach, presented in Appendix Figures A5 to A7, demonstrate remarkable stability when compared to our primary estimates. Both the magnitude and statistical significance of the treatment effects remain consistent across specifications, indicating that our findings are robust to different control group constructions. Again, this consistency strengthens our confidence that the observed effects represent causal impacts rather than artifacts of specific methodological choices.

V.1.3 Accounting for Other Policy Changes

A potential concern is that our identification strategy may be confounded by other policy changes occurring during the same period that could independently affect the career choices of politicians. We focus on three key policy changes that might influence our estimates: (1) changes in politicians' wages, (2) reforms to campaign finance laws, and (3) the presence and modification of legislative term limits.

Compensation for elected officials varied considerably across states during our sample period, with many we test the robustness of our results by controlling for changes in politicians' salaries. Appendix Table A9 reproduces the analysis in Table 2 but now including controls for changes in real wages of state legislators. Our estimates remain statistically significant and similar in magnitude across all outcomes, suggesting that wage changes do not drive our results.

States also enacted various campaign finance reforms during our sample period that could affect politicians' fundraising abilities and, consequently, their career decisions (Weschle, 2021). We combine information from the National Conference of State Legislatures as well as Weschle (2021) and Fouirnaies and Fowler (2022) to trace changes over time across state legis-

latures in campaign finance regulation and contribution limits. Appendix Table A10 reproduce the analysis in Table 2 but now including controls for changes in campaign contribution restrictions. As before, our estimates remain statistically significant and similar in magnitude across all outcomes, suggesting that campaign finance reforms are not driving our findings.

Third, the introduction or modification of term limits in some states during our sample period may have also influenced politicians' career horizons and decision-making. Using information on term limit legislation from Fouirnaies and Hall (2022) and the National Conference of State Legislatures, we create an indicator for states that enacted or modified term limits during our period of analysis. Results in Appendix Table A11 show that accounting for term limit changes does not meaningfully alter our main results.

Finally, we conduct a more comprehensive test by simultaneously controlling for all three policy changes. The results, presented in Appendix Table A12, remain consistent with our baseline findings. This provides further evidence that our estimated effects are not driven by these concurrent policy changes.

V.2 Heterogeneity by Quality and Ideology

While our main analysis focuses on the quantity of politicians entering and exiting politics, in this final section, we explore whether these impacts vary by politician quality and ideology. Our model makes more straightforward predictions for some of these patterns than others. In particular, we expect entry of more extreme and lower quality candidates, because of the reduced financial benefits of holding office. Among officeholders, we predict that both exiters *and* incumbents who remain in office will be more moderate relative to the earlier period; it is difficult to sign any change in average quality.

As in the previous section, our analysis is at the district-election-cycle level, using the specification in Equation 1. The outcome in each case is a count of the number of legislators of a given type. Throughout, we present the results for all politicians as a benchmark (paralleling our prior results on overall quantity) followed by comparable analyses for subsets of the data based on quality and ideology. Finally, since we will be looking at

the effect of revolving door laws on subsets of politicians, by definition the base rate will be smaller for these later columns, so we include the mean of the dependent variable as well to serve as a benchmark in interpreting effect sizes.

We begin by looking at differences in quality, as captured by our three proxies: a graduate degree; higher-quality undergraduate institution; and whether a candidate eventually seeks federal office. We present these results in Tables 3 and 4 for entry of new candidates and exit versus rerun decisions respectively.[17] Focusing first on the entry margin, while we observe a decline in entry for higher-quality candidates, it is roughly of the same scale as the decline in entry overall (e.g., from column 1, the decline in entry relative to the base rate of 0.90 is 18.2%, as compared to the decline in entry of candidates with a higher-level degree of 18.4% relative to the baseline of 0.14). Turning to the results in Table 4, it is difficult to make any statement about the relative impact on either exit or the decision of incumbents to rerun – the point estimates in columns 2–4 are too imprecisely measured to even sign the impact.

Overall, we find no evidence in favor of a differential impact on higher-quality politicians from the passage of revolving door laws. It is worth reiterating that we had no clear prediction for the exit margin, so the noisy estimates are perhaps unsurprising.

Turning to ideology, we again consider the differential effects for entry (Table 5) and exit/rerunning (Table 6). Focusing first on entry, we begin by examining whether Democrats, Republicans, or independents are most responsive to the passage of revolving door laws. The coefficients for both political parties are very similar in magnitude and, when scaled by their respective base rates, marginally lower than the overall impact in column 1 (e.g., the point estimate of -0.054 in column 2 is 14.4% when scaled by the base rate, as compared to the overall impact of 18.2% in column 1). Given these marginally smaller effect sizes, it is then unsurprising that we observe a larger relative impact on independents. In column 4, the coefficient on $Treated$ is larger in magnitude, -0.0664, and much larger when scaled by the relatively low base rate of independent entry ($0.0664/0.135 = 49\%$). In the final column of Table 5 we provide results based on the CF ideology

[17]Event plots corresponding to the analyses presented in this section may be found in Appendix Figures A9-A11.

score which, recall, is calculated based on a combination of a candidate's set of donors and the voting records of politicians generally supported by those donors. We show the results for "moderate" candidate entry, where a moderate candidate is one whose ideological distance between themselves and the median of the legislature is below the average among all candidates for a given state legislature-year election. The estimated effect size is again far larger than the overall treatment effect when scaled by the relatively low base rate of 0.177 ($0.695/0.177 = 39.3\%$).

For exit and rerun decisions, recall that we expect to see a relative increase in *moderates* for both exiters and incumbents that rerun.[18] Focusing first on the exit decision, we find a disproportionate effect on relatively moderate candidates. As compared to the overall treatment effect on exit of 19.4% (0.155/0.80), for independents and moderates the point estimates imply treatment effects of 49.7% and 36.8% respectively. The results on incumbents rerunning are more mixed: while the treatment effect for moderates is relatively large (-12.2% versus -5.6% for rerunning overall, when scaled by the base rate), we do not observe any significant effect for independents, though this could be because of the very low proportion of districts in which there is an incumbent independent standing for reelection in general (less than 0.014).

To summarize, we see clearer evidence of a shift in entrant and exit ideology as a result of revolving door laws: we observe a disproportionate decline in independents and moderates who run for office and a decline in exit of moderates. The decline in moderates in the candidate pool in particular is a concern flagged by Hall (2019), and suggests a potential role for revolving door restrictions in the increased polarization of state-level politics.[19]

[18]This may be seen in Figure 1, where the change in who reruns is captured by the light green region, which is between the previous set of incumbents who chose to rerun in purple, and the ones that exited previously and continue to exit in dark green.

[19]We note, however, that it is more difficult to evaluate the long-term impact of revolving door restrictions on the ideological composition of legislators, since among standing legislators, moderates are less likely to exit after the passage of such rules. However, the entry effect will lead to a more extreme set of incumbents over time. Assessing the long-run equilibrium effects is beyond the scope of our paper.

VI Conclusion

In this paper we study the political consequences of revolving door laws that impose a cooling-off period on state-level legislators 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 the 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. Also in line with our modeling framework, we see a relatively large reduction in entry of relatively more moderate politicians, since they are relatively more motivated by financial concerns.

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. A further step in this agenda is to understand how revolving door laws ultimately impact policy. Political selection is one potentially important channel. While our paper provides a set of insights that we expect will be useful in making such an overall assessment, evaluating these consequences will require an accounting of a wider range of factors, and it is an agenda that we leave for further work.

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Tables and Figures

Table 1: Summary Statistics

Panel (a): District-Election Level						
	Mean	Median	S.D.	Min	Max	N
Number of New Entrants	0.90	1.00	0.87	0.00	21.00	78,239
Incumbent Runs	0.78	1.00	0.41	0.00	1.00	78,239
Incumbent Winner	0.74	1.00	0.44	0.00	1.00	78,239
Runs Unopposed	0.32	0.00	0.47	0.00	1.00	78,239
Incumbent Unopposed	0.29	0.00	0.45	0.00	1.00	78,239
Number of Exiters	0.81	1.00	0.79	0.00	19.00	78,239
Number of Candidates	1.81	2.00	0.92	1.00	37.00	78,239

Panel (b): Candidate Level						
	Mean	Median	S.D.	Min	Max	N
Female	0.21	0.00	0.41	0.00	1.00	137,976
White	0.90	1.00	0.30	0.00	1.00	86,607
Democrat	0.52	1.00	0.50	0.00	1.00	127,884
Republican	0.44	0.00	0.50	0.00	1.00	141,254
Independent	0.09	0.00	0.29	0.00	1.00	141,254
Adv. Degree	0.20	0.00	0.40	0.00	1.00	141,254
Total Tenure	7.81	6.00	9.06	0.00	56.00	141,254
Vote Share	0.55	0.53	0.28	0.00	1.00	141,228
Wins	0.57	1.00	0.50	0.00	1.00	141,254
Voluntary Exit	0.60	1.00	0.49	0.00	1.00	141,254
Involuntary Exit	0.02	0.00	0.14	0.00	1.00	141,254
Lobbying Ever After Leaving Office	0.04	0.00	0.20	0.00	1.00	141,254

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 some 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 our data to the FollowTheMoney database, as described in Section III. Advanced degree information is generated through the online biographical candidate data collection described in Section III.

Table 2: Revolving Door Laws and Political Selection

	(1) Number of Exiters	(2) Number of New Entrants	(3) Single-Candidate Election	(4) Incumbent Runs Unopposed	(5) Incumbent Runs	(6) Incumbent Winner
Treated	-0.155***	-0.164***	0.101***	0.0897**	0.0440**	0.0497***
	[0.0388]	[0.0502]	[0.0339]	[0.0349]	[0.0176]	[0.0169]
DepVarMean (Pre-T)	0.824	0.924	0.310	0.275	0.777	0.731
Observations	78239	78239	78239	78239	78239	78239
R-sq	0.0829	0.0919	0.159	0.136	0.0274	0.0293

Notes: This tableshows the results of our main specification in Section IV. Regressions are run at the district-election level, and outcome variables are constructed from the Klarner SLERs Database. Number of Exiters (1) is the sum of exiters in a given district-election period and Number of New Entrants (2) is the sum of new entrants in a given district-election period. Single-Candidate Election (3) is an indicator equal to one when a district-election has only one candidate running unopposed. Incumbent Runs (5) and Incumbent Runs Unopposed (4) are binary indicators equal to 1 if the incumbent runs for re-election and runs unopposed for re-election, respectively. Incumbent Winner (6) is a binary indicator for whether an incumbent ran and won re-election in a given district.

Table 3: Revolving Doors Laws and Entry for Higher-Quality Candidates

	(1) All	(2) Adv. Degree	(3) High Quality Edu	(4) Future Federal
Treated	-0.164***	-0.0256**	-0.0168**	-0.00722***
	[0.0502]	[0.0118]	[0.00770]	[0.00248]
DepVar Mean	0.900	0.139	0.0783	0.0118
DepVar SD	0.872	0.373	0.282	0.108
Observations	78239	78239	78239	78239
R-sq	0.0919	0.0325	0.0269	0.00664

Notes: The table above shows the effect of revolving door laws on entry of higher-quality candidates. Advanced degree information and undergraduate quality indicators are generated through the online biographical candidate data collection described in Section III. Data on former state legislators running for federal elections comes from Phillips et al. (2024).

Table 4: Revolving Doors Laws and Exit for Higher-Quality Incumbents

Panel A: Number of Exiters

	(1) All	(2) Adv. Degree	(3) High Quality Edu	(4) Future Federal
Treated	-0.155***	-0.0191*	-0.00671	-0.00492**
	[0.0388]	[0.00987]	[0.00788]	[0.00210]
DepVar Mean	0.807	0.127	0.0725	0.00897
DepVar SD	0.794	0.350	0.268	0.0944
Observations	78239	78239	78239	78239
R-sq	0.0829	0.0189	0.0194	0.00540

Panel B: Incumbent Runs

	(1) All	(2) Adv. Degree	(3) High Quality Edu	(4) Future Federal
Treated	0.0440**	0.00615	0.0177	-0.00399
	[0.0176]	[0.0134]	[0.0164]	[0.00585]
DepVar Mean	0.779	0.206	0.155	0.0261
DepVar SD	0.415	0.405	0.362	0.159
Observations	78239	78239	78239	78239
R-sq	0.0274	0.0243	0.0775	0.0112

Panel B: Incumbent Wins

	(1) All	(2) Adv. Degree	(3) High Quality Edu	(4) Future Federal
Treated	0.0497***	0.00897	0.0216	-0.00352
	[0.0169]	[0.0134]	[0.0167]	[0.00563]
DepVar Mean	0.736	0.196	0.148	0.0256
DepVar SD	0.441	0.397	0.355	0.158
Observations	78239	78239	78239	78239
R-sq	0.0293	0.0232	0.0751	0.0111

Notes: This table shows the effect of revolving door laws on exit for higher-quality candidates. The upper table shows results for candidate exits, while the middle and lower tables show the effects on the probability of the incumbent running and winning, respectively. Advanced degree information and undergraduate quality are generated through the online biographical candidate data collection described in III. Data on former state legislators running for federal elections comes from Phillips et al. (2024).

Table 5: Revolving Doors Laws and Entry for More Moderate Candidates

	(1) All	(2) Democrat	(3) Republican	(4) Independent	(5) Moderate
Treated	-0.164***	-0.0540*	-0.0412	-0.0664***	-0.0695***
	[0.0502]	[0.0278]	[0.0319]	[0.0172]	[0.0230]
DepVar Mean	0.900	0.374	0.367	0.135	0.177
DepVar SD					
Observations	78239	78239	78239	78239	78239
R-sq	0.0919	0.0713	0.0767	0.0958	0.0937

Notes: This table shows the effect of revolving door laws on entry for candidates with differing levels of ideology. Information on candidate party affiliation (Democrat/Republican/Independent) comes from the Klarner SLERs Database. Prior lobbyist activity is determined by matching to the FollowTheMoney database, as described in Section III. Moderates are defined as having ideological distance between themselves and the median of the legislature that is below the average among all candidates for a given state legislature-year election, based on the CF score of Bonica (2014).

Table 6: Revolving Doors Laws and Exit for more Moderate Politicians

Panel A: Number of Exiters

	(1) All	(2) Democrat	(3) Republican	(4) Independent	(5) Moderate
Treated	-0.155***	-0.0489*	-0.0426*	-0.0636***	-0.0504***
	[0.0388]	[0.0242]	[0.0249]	[0.0153]	[0.0171]
DepVar Mean	0.807	0.347	0.332	0.128	0.137
DepVar SD					
Observations	78239	78239	78239	78239	78239
R-sq	0.0829	0.0383	0.0546	0.0867	0.123

Panel B: Incumbent Runs

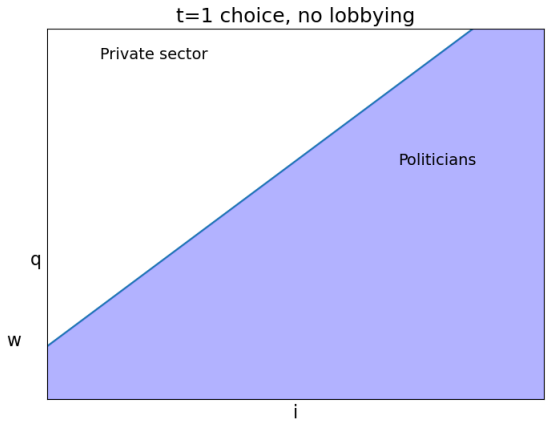
	(1) All	(2) Democrat	(3) Republican	(4) Independent	(5) Moderate
Treated	0.0440**	-0.00475	0.0498	-0.000302	0.0366*
	[0.0176]	[0.0490]	[0.0398]	[0.00135]	[0.0196]
DepVar Mean	0.779	0.411	0.361	0.0137	0.299
DepVar SD					
Observations	78239	78239	78239	78239	78239
R-sq	0.0274	0.0556	0.0491	0.326	0.156

Panel C: Incumbent Wins

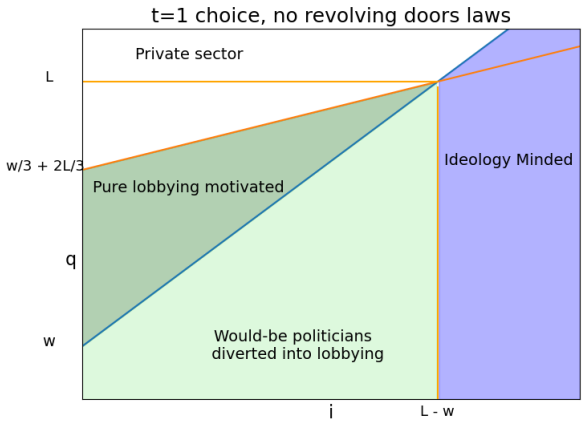
	(1) All	(2) Democrat	(3) Republican	(4) Independent	(5) Moderate
Treated	0.0497***	-0.00388	0.0537	0.000373	0.0394*
	[0.0169]	[0.0482]	[0.0399]	[0.000481]	[0.0203]
DepVar Mean	0.736	0.388	0.341	0.0103	0.290
DepVar SD					
Observations	78239	78239	78239	78239	78239
R-sq	0.0293	0.0553	0.0505	0.324	0.150

Notes: This table shows the effect of revolving door laws on exit for candidates with differing levels of ideology. The upper table shows the the results for candidate exits, while the middle table and lower table show the effects on the probability of the incumbent running and winning, respectively. Information on candidate party affiliation (Democrat/Republican/Independent) comes from the Klarner SLERs Database. Prior lobbyist activity is determined by matching to the FollowTheMoney database, as described in Section III. Moderates are defined as having ideological distance between themselves and the median of the legislature that is below the average among all candidates for a given state legislature-year election, based on the CF score of Bonica (2014).

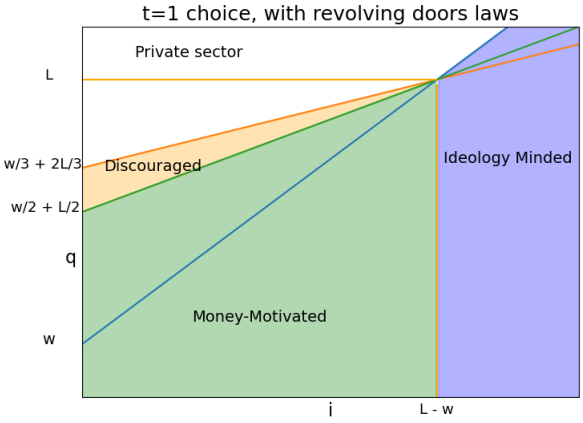
Figure 1: Model Illustration



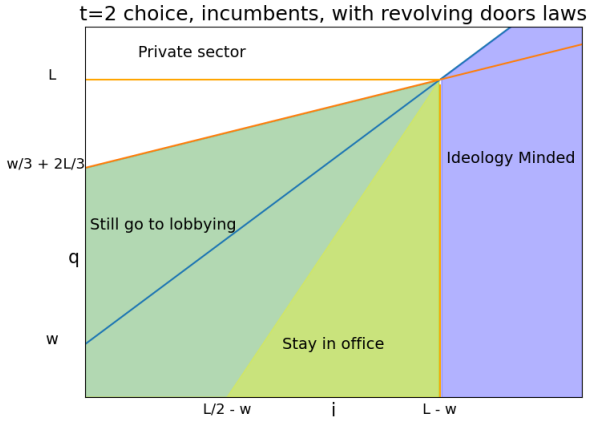
(a) No Lobbying



(b) Lobbying, No Laws



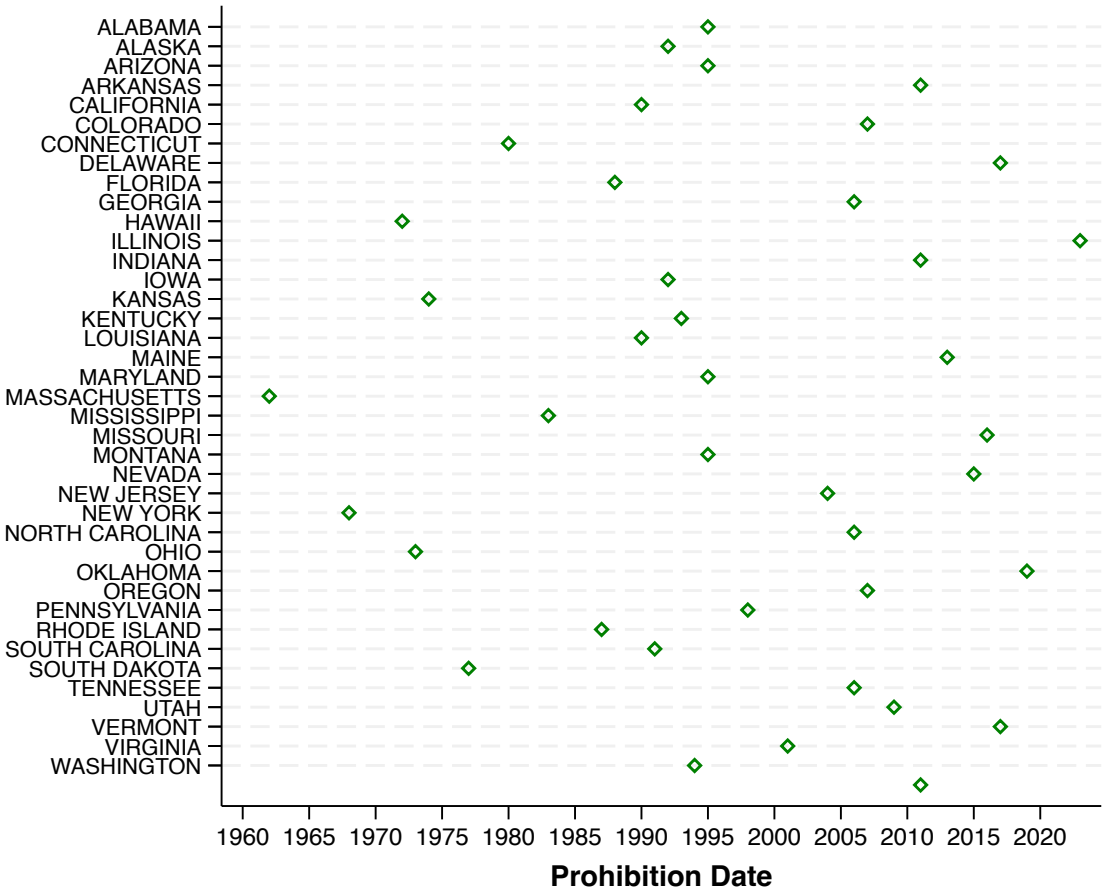
(c) Lobbying, With Laws, $t = 1$



(d) Lobbying, with Laws, $t = 2$

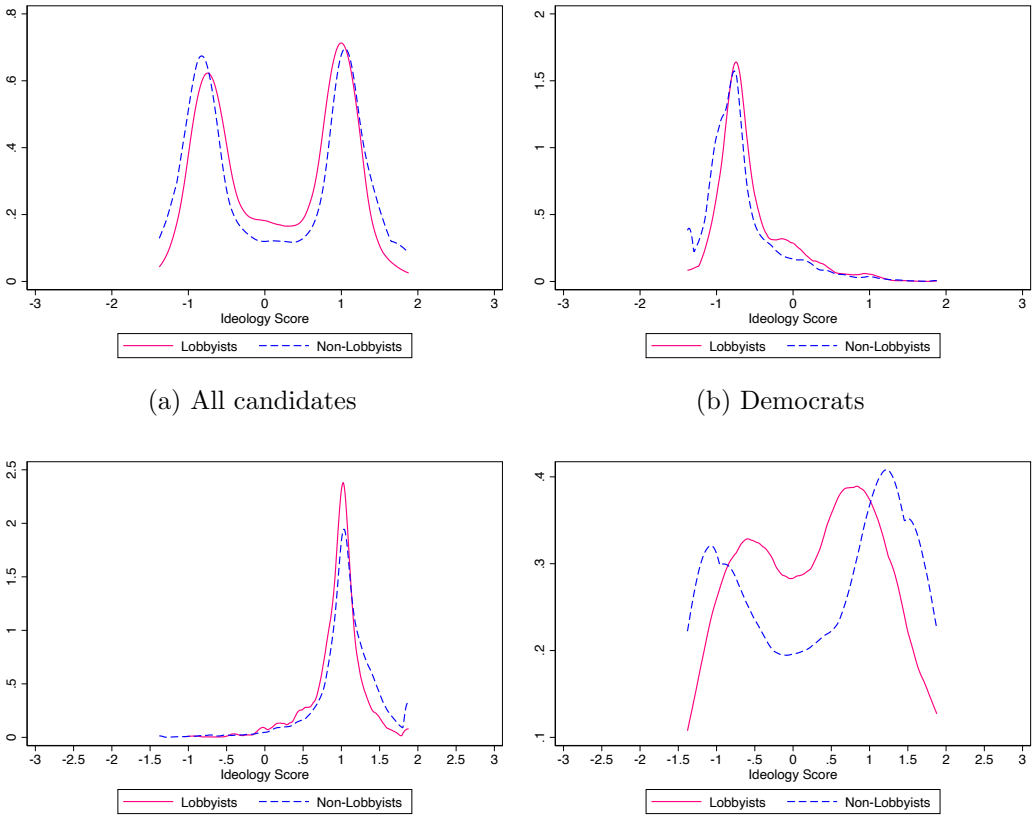
Notes: The figure above provides intuitions for the key implications of our conceptual framework as described in Section II. Panels (a)-(c) illustrate the implications of revolving door laws at $t = 1$. Panel (a) shows the division of private and public sector labor without lobbying. Panel (b) shows the division of labor among private and public sector workers without revolving door laws (where lobbyists are indicated in green). Panel (c) shows the impact of revolving door laws, particularly the group that is "diverted" to public office in the presence of such laws (indicated in light red). Panel (d) illustrates the decision at $t = 2$, 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 2: Revolving Door Prohibition Date by State



Notes: This figure shows 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 10 states without prohibitions, (1) 5 states have no prohibitions: Idaho, 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 prohibits lobbying for the duration of resigned terms: Michigan.

Figure 3: Ideology by Lobbying Status and Political Affiliation



(a) All candidates

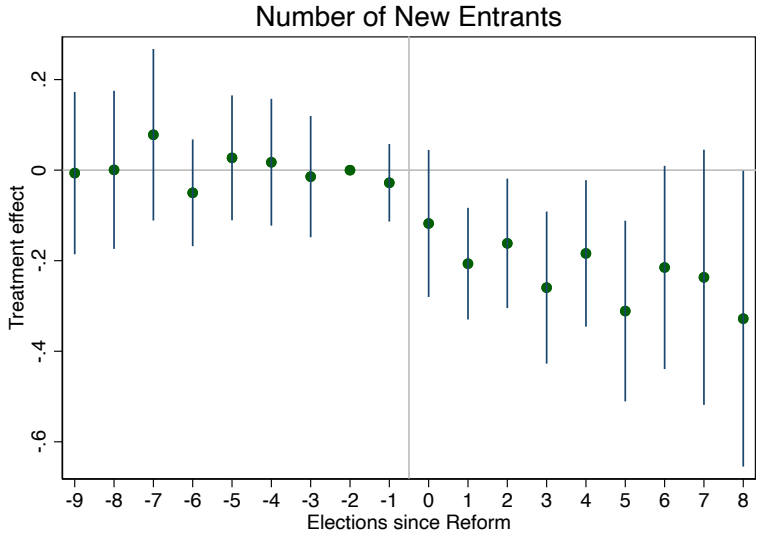
(b) Democrats

(c) Republicans

(d) Independents

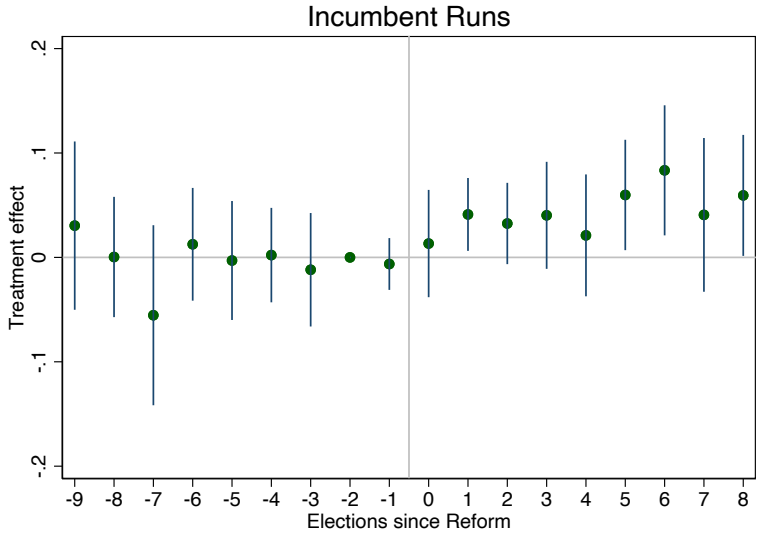
Notes: The figure above shows the distribution of ideology across candidates by lobbying status and party affiliation. Data on candidate-level ideology come from Bonica (2014). Lobbying status is determined from the matching process described in Section III. Party information comes from the Klarner SLERs database. Panel (a) shows the distribution of ideologies for lobbyists vs. non-lobbyists, across all candidates. Panels (b), (c) and (d) show the distribution of ideology for lobbyists vs. non-lobbyists for Democratic, Republican, and independent candidates, respectively.

Figure 4: Revolving Door Laws Deter Entry into Politics



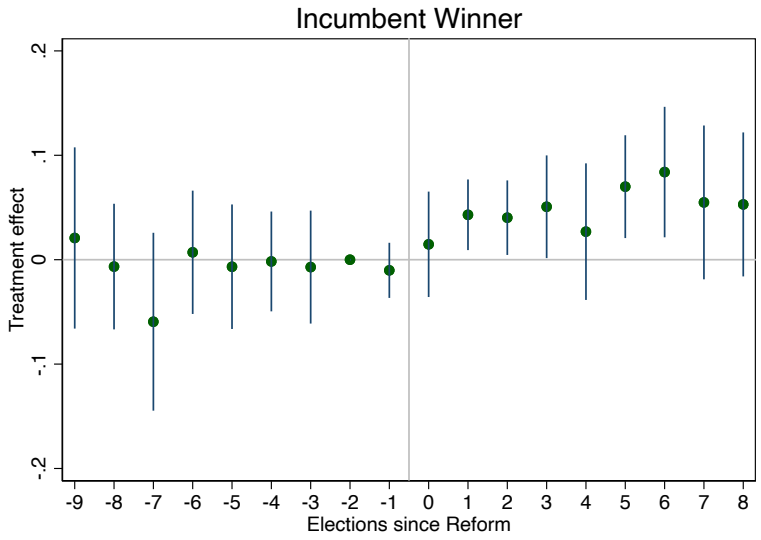
Notes: The figure shows the dynamic difference-in-differences estimates of the causal effect of revolving door laws on the total number of candidates that run for the first time. Relative time reflects the number of elections before and after the passage of the law (calculated using term length). New 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 Increase the Probability of Re-Running



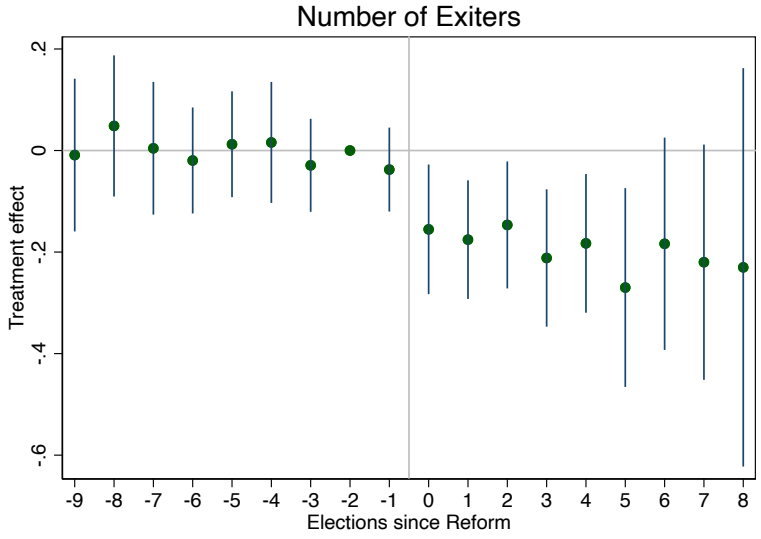
Notes: The figure shows the dynamic difference-in-differences estimates of the causal effect of revolving door laws on the probability of candidates running for re-election. Relative time reflects the number of elections before and after the passage of the law (calculated using term length). Regression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

Figure 6: Revolving Door Laws Increase the Probability of Incumbent Winning Re-election



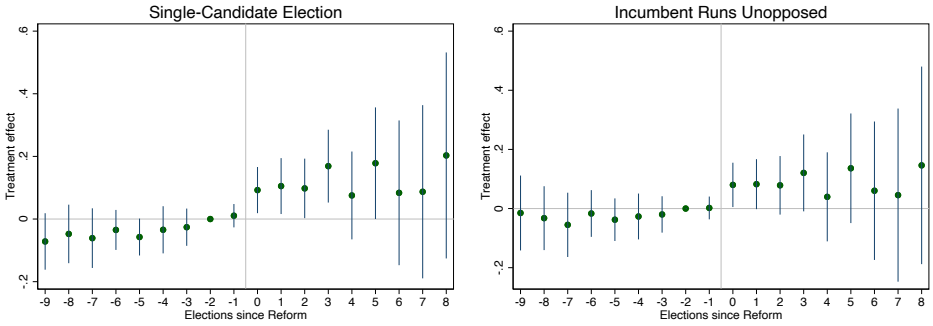
Notes: The figure shows the dynamic difference-in-differences estimates of the causal effect of revolving door laws on the probability of the incumbent candidate winning re-election. Relative time reflects the number of elections before and after the passage of the law (calculated using term length).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

Figure 7: Revolving Door Laws Deter Exit from 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 state politics. Relative time 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 8: 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 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 the probability of unopposed elections (where the 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.

Appendix

Table A1: 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 covariates come from the sources described in Section III. Robust standard errors in parentheses.

Table A2: 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 Law. State-level covariates come from the sources described in Section III. The regression is run using state fixed effects, with standard errors clustered at the state level.

Table A3: Predictors of Missing Biographical Data

	Full Sample	Post-1998 Sample
Ever Win	-0.86***	-0.86***
	[0.048]	[0.064]
Male Indiator	-0.05***	-0.05*
	[0.019]	[0.026]
Democrat Indicator	-0.04***	-0.03**
	[0.013]	[0.015]
Year First Ran	-0.02***	-0.02***
	[0.001]	[0.001]
Observations	107310	66562

Standard errors in brackets

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

Notes: The table above shows the probit regression of various candidate-level characteristics on an indicator for missing biographical data. Information for win, male and year first ran come from the SLERs dataset, and gender is imputed using first names as described in Section III. The left column shows the regression run on the full sample of candidates, and the right column shows the regression run for just the post-1998 sample of candidates. Robust standard errors in parentheses.

Table A4: Lobbying Activity by Industry by Former Legislator Status, Treatment and Party

		State Legislator		Treated State		Party		
	Overall	Yes	No	Yes	No	Dem	Rep	Indep
Agriculture	0.13	0.18	0.13	0.12	0.15	0.09	0.19	0.15
Education	0.34	0.50	0.34	0.36	0.32	0.36	0.38	0.34
Environment	0.38	0.43	0.38	0.41	0.34	0.31	0.45	0.45
Infastructure	0.40	0.46	0.40	0.41	0.39	0.34	0.51	0.44
Labor/Employment	0.30	0.25	0.31	0.34	0.27	0.29	0.39	0.30
Legal/Criminal Justice	0.26	0.28	0.26	0.28	0.23	0.27	0.28	0.23
Military	0.04	0.14	0.04	0.05	0.04	0.04	0.04	0.06
Taxation	0.47	0.61	0.47	0.50	0.44	0.36	0.64	0.52
Single Issue/Ideology	0.26	0.26	0.26	0.30	0.21	0.31	0.24	0.24
Social Welfare	0.56	0.57	0.58	0.60	0.53	0.63	0.53	0.52
Other Issues	0.26	0.12	0.27	0.27	0.25	0.28	0.24	0.31
Obs	1259	72	1132	634	625	359	262	287

Notes: Table shows the average lobbying frequency by industry from the National Survey of State Lobbyists. Frequent lobby indicators for each sector are based on a self-reported scale, equal to 1 for responses of "Sometimes Lobbies" (2) or "Frequently Lobbies" (1), and equal to 0 for responses of "Rarely Lobbies" (3) or "Never Lobbies" (4). Column 1 shows the average by industry across all surveyed lobbyists. Columns 2-3 show the averages separately for former legislators vs. non-former legislators. Columns 4-5 show the averages separately for lobbyists in treated (law passed post-1998) vs. control states. Columns 6-8 show the averages by political party affiliation.

Table A5: Summary Statistics for Candidates/Legislators at the State and National Level

	State Legislators		*National Legislators*	
	Candidates	Elected	Candidates	Elected
Average Age	45.01	42.79	n/a	45.61
Male (%)	76.93	79.05	80.38	83.32
Democrat (%)	50.18	51.96	47.47	49.29
Avg. Ideology (Democrat)	-0.76	-0.52	-0.74	-0.58
Republican (%)	41.18	48.31	43.59	51.47
Avg. Ideology (Republican)	1.10	0.97	1.04	1.02
Independent (%)	18.98	2.5	8.83	0.19
Avg. Ideology (Independent)	0.34	0.29	0.25	-0.1
Black (%)	5.06	8.09	n/a	8.55
Hispanic (%)	1.67	2.93	n/a	6.11
Undergrad Degree (%)	57.66	70.06	n/a	93.30
Advanced Degree (%)	31.80	34.74	n/a	73.21
Lobbyist (%)	4.70	11.47	n/a	29.46
Total (N)	80,643	34,937	n/a	1,019

Notes: For state candidates/legislators, information on gender, race, ethnicity and party come comes from the Klarner SLERs Database (where gender is imputed from first names). State legislator ideology is measured using Schor/McCarty NP scores, which is based on roll-call votes and thus is not available for candidates. Lobbying information comes from a name-based merge to the FTM state lobbyist registers. Information on state candidate/legislator age and education is compiled from a scraping project, which searches for and extracts text from relevant web searches for each candidate. For congress members, gender and ideology come from the DIME Database (Bonica, 2023). Information on age and lobbying activity come from Palmer and Schneer (2018). Information on congress race and ethnicity come from the Pew Research Center article: The Changing Face of Congress. Note that all congressional data is based on congresses between 1992-2014 as per data availability.

Table A6: Quality Indicators for Lobbyists and Non-Lobbyists by Party Affiliation

	Lobbyists			Non-Lobbyists		
	Dem	Rep	Indep	Dem	Rep	Indep
Adv Degree (%)	0.28	0.25	0.24	0.18	0.15	0.12
High Quality Undergrad Edu (%)	0.17	0.16	0.11	0.10	0.09	0.06
Ran for Fed Election (%)	0.04	0.04	0.02	0.01	0.01	0.00
Obs	2985	2615	535	44239	43880	15416

Notes: This table shows the average quality indicators for lobbyists and non-lobbyists by party affiliation. Lobbyists are defined as legislators who are matched to the FollowTheMoney database in the process described in Section III. Advanced degree information and undergraduate quality are collected through biographical candidate data collection described in III. Data on former state legislators running for federal elections is constructed from Phillips et al. (2024).

Table A7: Revolving Doors and Political Selection, Heterogeneity by Salary

	(1) Number of Exiters	(2) Number of New Entrants	(3) Single-Candidate Election	(4) Incumbent Runs Unopposed	(5) Incumbent Runs	(6) Incumbent Winner
Treated	-0.0943*	-0.101*	0.0380	0.0415*	0.0339	0.0340
	[0.0465]	[0.0553]	[0.0257]	[0.0213]	[0.0217]	[0.0223]
Treated ×	-0.102	-0.106	0.106**	0.0829*	0.0174	0.0269
HighSalary	[0.0604]	[0.0684]	[0.0478]	[0.0480]	[0.0159]	[0.0177]
DepVarMean	0.807	0.900	0.325	0.286	0.778	0.735
Observations	78239	78239	78239	78156	78005	78021
R-sq	0.0833	0.0924	0.161	0.136	0.0284	0.0301

Notes: The table above shows the results of our main specification in Section IV, inspecting heterogeneity by whether the state legislative salaries are above the national median. Regressions are run at the district-election level, and outcome variables are constructed from the Klaner SLERs Database. NExiters (1) is the sum of exiters in a given district-election period and NNew Entrants (2) is the sum of new entrants in a given district-election period. Single-Candidate Election (3) is an indicator equal to one when a district-election has only one candidate running unopposed. Incumbent Runs (5) and Incumbent Runs Unopposed (4) are binary indicators equal to 1 if the incumbent runs for re-election and runs unopposed for re-election, respectively. Incumbent Winner (6) is a binary indicator for whether an incumbent ran and won re-election for a given district.

Table A8: Revolving Doors and Political Selection, Heterogeneity by Full vs. Part-Time Status of State Legislatures

	(1) Number of Exiters	(2) Number of New Entrants	(3) Single-Candidate Election	(4) Incumbent Runs Unopposed	(5) Incumbent Runs	(6) Incumbent Winner
Treated	-0.108**	-0.112**	0.0490*	0.0376*	0.0406**	0.0487***
	[0.0445]	[0.0493]	[0.0269]	[0.0188]	[0.0173]	[0.0170]
Treated ×	-0.198***	-0.215***	0.219***	0.221***	0.0152	0.00554
Full-Time	[0.0454]	[0.0550]	[0.0266]	[0.0230]	[0.0155]	[0.0189]
DepVarMean	0.807	0.900	0.325	0.286	0.778	0.735
Observations	78239	78239	78239	78156	78005	78021
R-sq	0.0838	0.0929	0.163	0.139	0.0283	0.0300

Notes: The table above shows the results of our main specification in Section IV, inspecting heterogeneity by whether the state legislature is considered a part-time or a full-time job. Regressions are run at the district-election level, and outcome variables are constructed from the Klaner SLERs Database. NExiters (1) is the sum of exiters in a given district-election period and NNew Entrants (2) is the sum of new entrants in a given district-election period. Single-Candidate Election (3) is an indicator equal to one when a district-election has only one candidate running unopposed. Incumbent Runs (5) and Incumbent Runs Unopposed (4) are binary indicators equal to 1 if the incumbent runs for re-election and runs unopposed for re-election, respectively. Incumbent Winner (6) is a binary indicator for whether an incumbent ran and won re-election for a given district.

Table A9: Revolving Doors and Political Selection, Controlling for Salary Changes

	(1) Number of Exiters	(2) Number of New Entrants	(3) Single-Candidate Election	(4) Incumbent Runs Unopposed	(5) Incumbent Runs	(6) Incumbent Winner
Treated	-0.145***	-0.148***	0.0968***	0.0855**	0.0460***	0.0499***
	[0.0394]	[0.0442]	[0.0333]	[0.0348]	[0.0147]	[0.0140]
DepVarMean	0.795	0.875	0.333	0.295	0.785	0.742
Observations	67819	67819	67819	67819	67819	67819
R-sq	0.0858	0.0935	0.159	0.134	0.0266	0.0285

Notes: The table above shows the results of our main specification in Section IV, controlling now for changes in real legislator wages (scaled to 1998 dollars) as described in Section III. Regressions are run at the district-election level, and outcome variables are constructed from the Klaner SLERs Database. NExiters (1) is the sum of exiters in a given district-election period and NNew Entrants (2) is the sum of new entrants in a given district-election period. Single-Candidate Election (3) is an indicator equal to one when a district-election has only one candidate running unopposed. Incumbent Runs (5) and Incumbent Runs Unopposed (4) are binary indicators equal to 1 if the incumbent runs for re-election and runs unopposed for re-election, respectively. Incumbent Winner (6) is a binary indicator for whether an incumbent ran and won re-election for a given district.

Table A10: Revolving Doors and Political Selection, Controlling for Campaign Finance

	(1) Number of Exiters	(2) Number of New Entrants	(3) Single-Candidate Election	(4) Incumbent Runs Unopposed	(5) Incumbent Runs	(6) Incumbent Winner
Treated	-0.153***	-0.162***	0.103***	0.0910**	0.0429**	0.0493***
	[0.0381]	[0.0494]	[0.0333]	[0.0345]	[0.0175]	[0.0167]
DepVarMean	0.807	0.900	0.325	0.287	0.779	0.736
Observations	78239	78239	78239	78239	78239	78239
R-sq	0.0829	0.0920	0.159	0.136	0.0275	0.0293

Notes: The table above shows the results of our main specification in Section IV, controlling for the presence of corporate finance contribution bans as described in Section III. Regressions are run at the district-election level, and outcome variables are constructed from the Klaner SLERs Database. NExiters (1) is the sum of exiters in a given district-election period and NNew Entrants (2) is the sum of new entrants in a given district-election period. Single-Candidate Election (3) is an indicator equal to one when a district-election has only one candidate running unopposed. Incumbent Runs (5) and Incumbent Runs Unopposed (4) are binary indicators equal to 1 if the incumbent runs for re-election and runs unopposed for re-election, respectively. Incumbent Winner (6) is a binary indicator for whether an incumbent ran and won re-election for a given district.

Table A11: Revolving Doors and Political Selection, Controlling for Term Limits

	(1) Number of Exiters	(2) Number of New Entrants	(3) Single-Candidate Election	(4) Incumbent Runs Unopposed	(5) Incumbent Runs	(6) Incumbent Winner
Treated	-0.118**	-0.119**	0.0847**	0.0747**	0.0255	0.0294*
	[0.0452]	[0.0547]	[0.0330]	[0.0320]	[0.0179]	[0.0172]
DepVarMean	0.807	0.900	0.325	0.287	0.779	0.736
Observations	78239	78239	78239	78239	78239	78239
R-sq	0.0858	0.0956	0.161	0.137	0.0301	0.0322

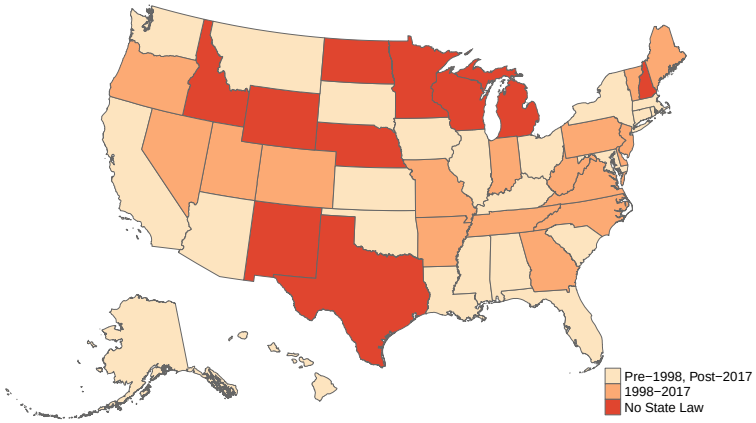
Notes: The table above shows the results of our main specification in Section IV, controlling now for the presence of state legislative term limits as described in Section III. Regressions are run at the district-election level, and outcome variables are constructed from the Klaner SLERs Database. NExiters (1) is the sum of exiters in a given district-election period and NNew Entrants (2) is the sum of new entrants in a given district-election period. Single-Candidate Election (3) is an indicator equal to one when a district-election has only one candidate running unopposed. Incumbent Runs (5) and Incumbent Runs Unopposed (4) are binary indicators equal to 1 if the incumbent runs for re-election and runs unopposed for re-election, respectively. Incumbent Winner (6) is a binary indicator for whether an incumbent ran and won re-election for a given district.

Table A12: Revolving Doors and Political Selection, Combined Controls

	(1) Number of Exiters	(2) Number of New Entrants	(3) Single-Candidate Election	(4) Incumbent Runs Unopposed	(5) Incumbent Runs	(6) Incumbent Winner
Treated	-0.112**	-0.109**	0.0795**	0.0701**	0.0292**	0.0316**
	[0.0447]	[0.0470]	[0.0311]	[0.0309]	[0.0138]	[0.0129]
DepVarMean	0.795	0.875	0.333	0.295	0.785	0.742
Observations	67819	67819	67819	67819	67819	67819
R-sq	0.0886	0.0968	0.161	0.136	0.0292	0.0311

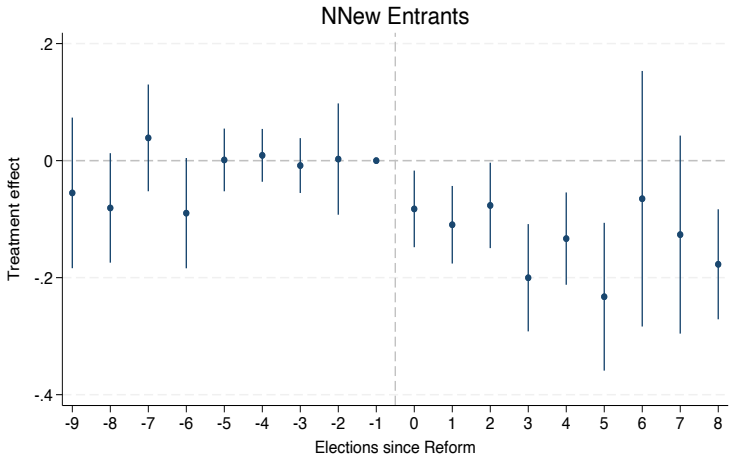
Notes: The table above shows the results of our main specification in Section IV, controlling now for salary, corporate campaign contribution bans, and term limits as described in Section III. Regressions are run at the district-election level, and outcome variables are constructed from the Klaner SLERs Database. NExiters (1) is the sum of exiters in a given district-election period and NNew Entrants (2) is the sum of new entrants in a given district-election period. Single-Candidate Election (3) is an indicator equal to one when a district-election has only one candidate running unopposed. Incumbent Runs (5) and Incumbent Runs Unopposed (4) are binary indicators equal to 1 if the incumbent runs for re-election and runs unopposed for re-election, respectively. Incumbent Winner (6) is a binary indicator for whether an incumbent ran and won re-election for a given district.

Figure A1: Passage of Revolving Door Laws by State



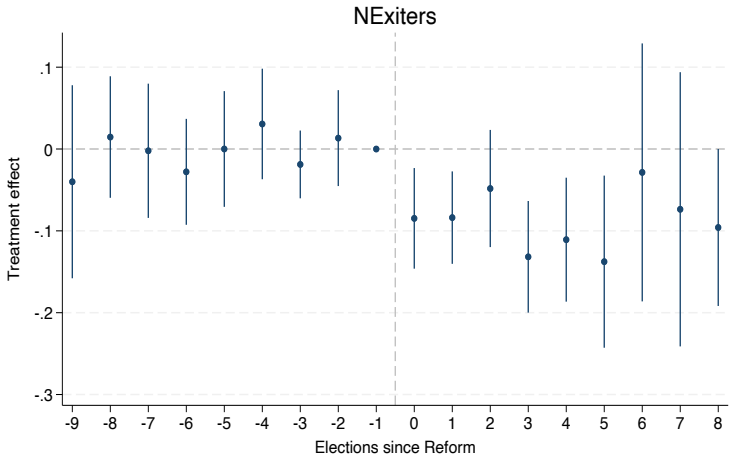
Notes: The map above shows the presence and timing passage of Revolving Door Laws by State, as it refers to the treatment of state legislators. Timing of law adoption is determined by the procedure described in Section III. States shaded in light orange adopted their first law affecting state legislators pre-1998 or post-2017. States shaded in dark orange adopted their first law affected state legislators between 1998-2017 (the sample of analysis). States shaded in red are never-adopting states, which consist 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)



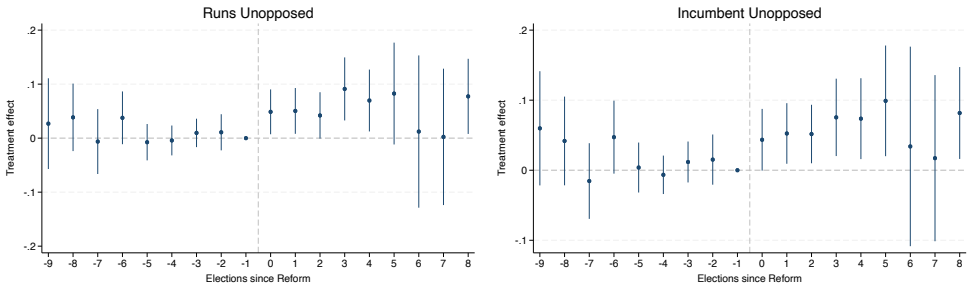
Notes: The figure shows the effect of revolving door laws on the total number of candidates that run for the first time, using the interaction weighted estimator as in Callaway and Sant'Anna (2021). New entrants are defined as those who appear for the first time in the election data. Relative time reflects 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)



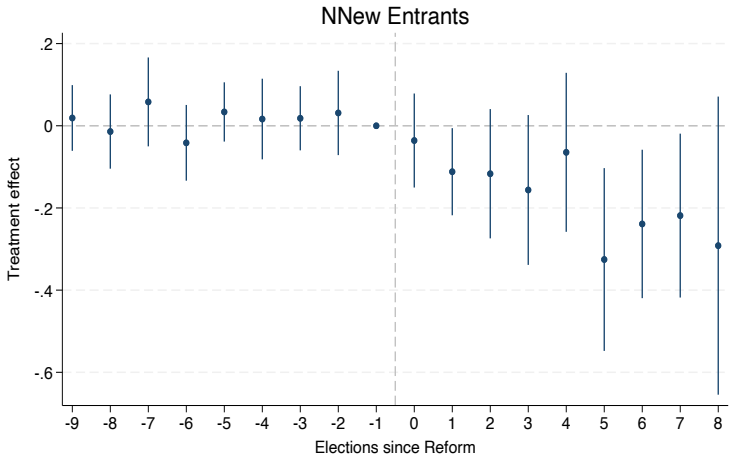
Notes: The figure shows the estimated effect of revolving door laws on the total number of legislators that leave politics, using the interaction weighted estimator as in Sun and Abraham (2021). Exiters are defined as those that leave the election data and do not reappear in subsequent years. Relative time 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)



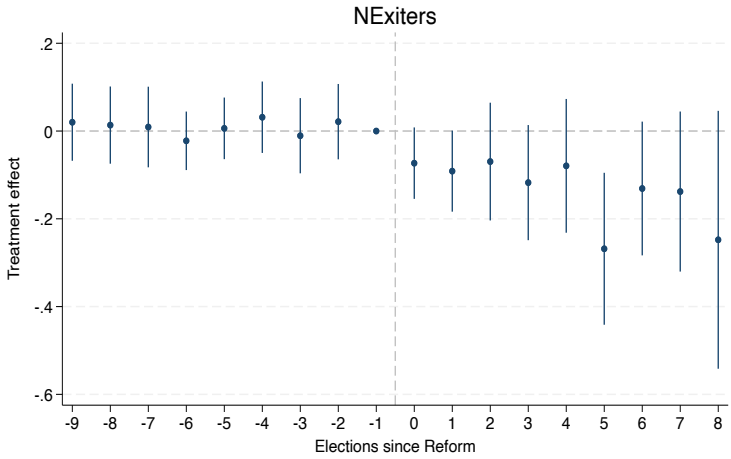
Notes: The figure shows the dynamic difference-in-differences estimates of the causal effect of revolving door laws on uncontested elections, using the interaction weighted estimator as in Callaway and Sant'Anna (2021). The left panel shows the effects of revolving door laws on unopposed elections (where 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 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 A5: Revolving Door Laws Deter Entry into Politics, Stacked Design



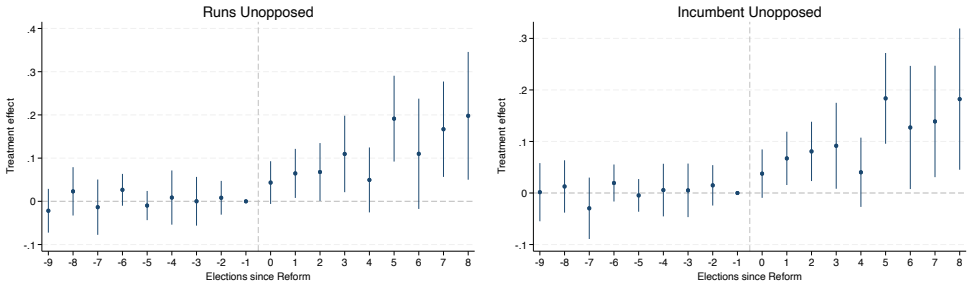
Notes: The figure shows the effect of revolving door laws on the total number of candidates that run for the first time, using the alternative controls approach described in Section V. New entrants are defined as those who appear for the first time in the election data. Relative time reflects the number 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 A6: Revolving Door Laws Deter Exit out of Politics , Stacked Design



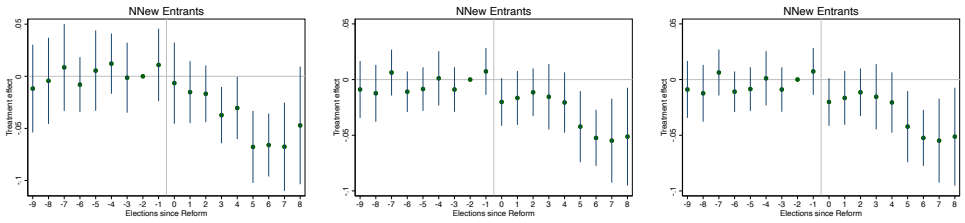
Notes: The figure shows the estimated effect of revolving door laws on the total number of legislators that leave politics, using the alternative controls approach described in Section V. Exiters are defined as those that leave the election data and do not reappear in subsequent years. Relative time 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 A7: Revolving Door Laws Increase Uncontested Elections, Stacked Design



Notes: The figure shows the dynamic difference-in-differences estimates of the causal effect of revolving door laws on uncontested elections, using the alternative controls approach described in Section V. The left panel shows the effects of revolving door laws on unopposed elections (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 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.

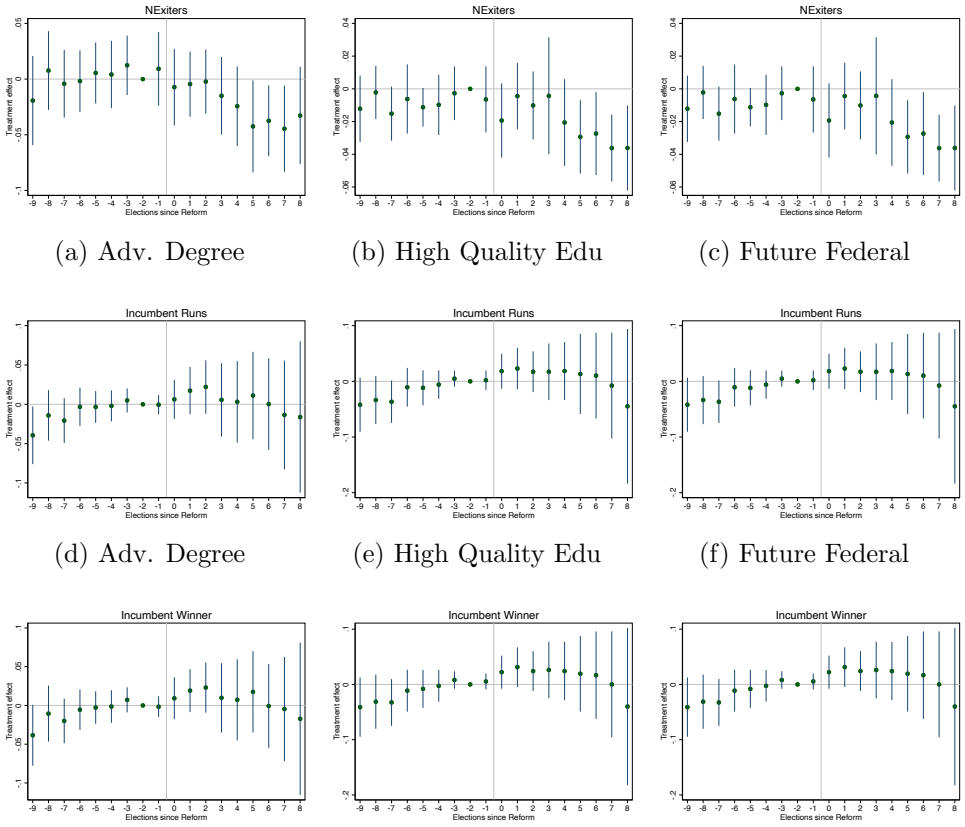
Figure A8: Revolving Door Laws and Entry for Higher-Quality Candidates, Event Studies



(a) Adv. Degree (b) High Quality Edu (c) Future Federal

Notes: The figures show the effect of revolving door laws on the total number of candidates that run for the first time, broken down by measures of quality. Advanced degree information and undergraduate quality are generated through the online biographical candidate data collection described in Section III. Data on former state legislators running for federal elections is constructed from Phillips et al. (2024).

Figure A9: Revolving Door Laws and Exit for Higher-Quality Incumbents, Event Studies



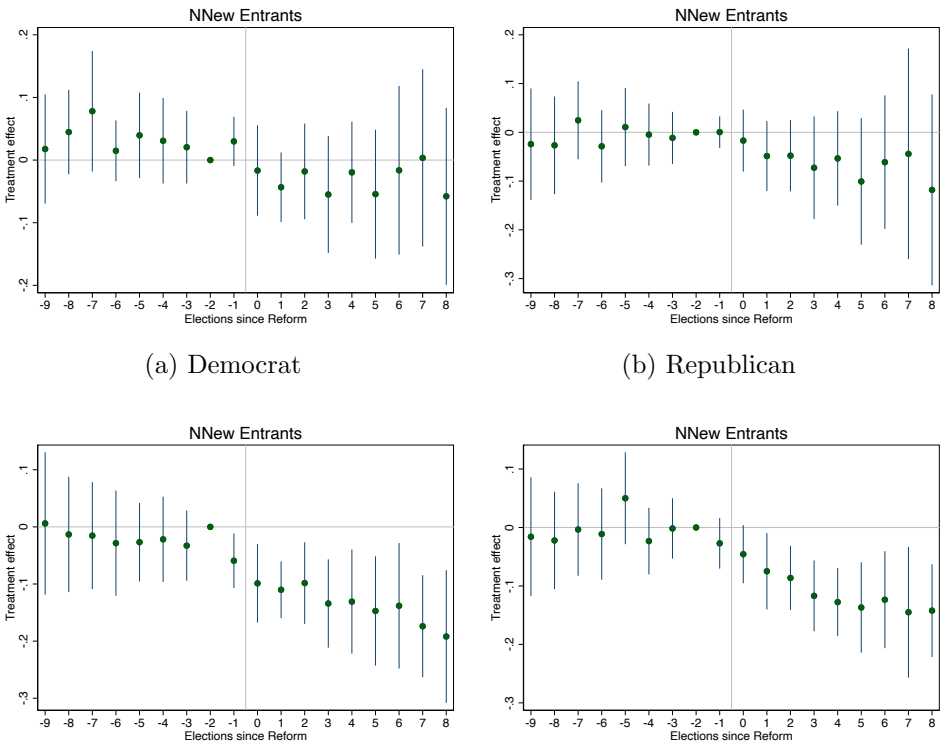
(a) Adv. Degree (b) High Quality Edu (c) Future Federal

(d) Adv. Degree (e) High Quality Edu (f) Future Federal

(g) Adv. Degree (h) High Quality Edu (i) Future Federal

Notes: The figures above show the effect of revolving door laws on the exit and incumbent re-election, broken down by measures of quality. The first row shows the effects on overall exit, the second row shows the effects on the probability of incumbent re-running for election, and the third row shows the effects on incumbent re-election. Advanced degree information and undergraduate quality are generated through the online biographical candidate data collection described in III. Data on former state legislators running for federal elections is constructed from Phillips et al. (2024).

Figure A10: Revolving Door Laws and Entry for More Moderate Candidates, Event Studies



(a) Democrat

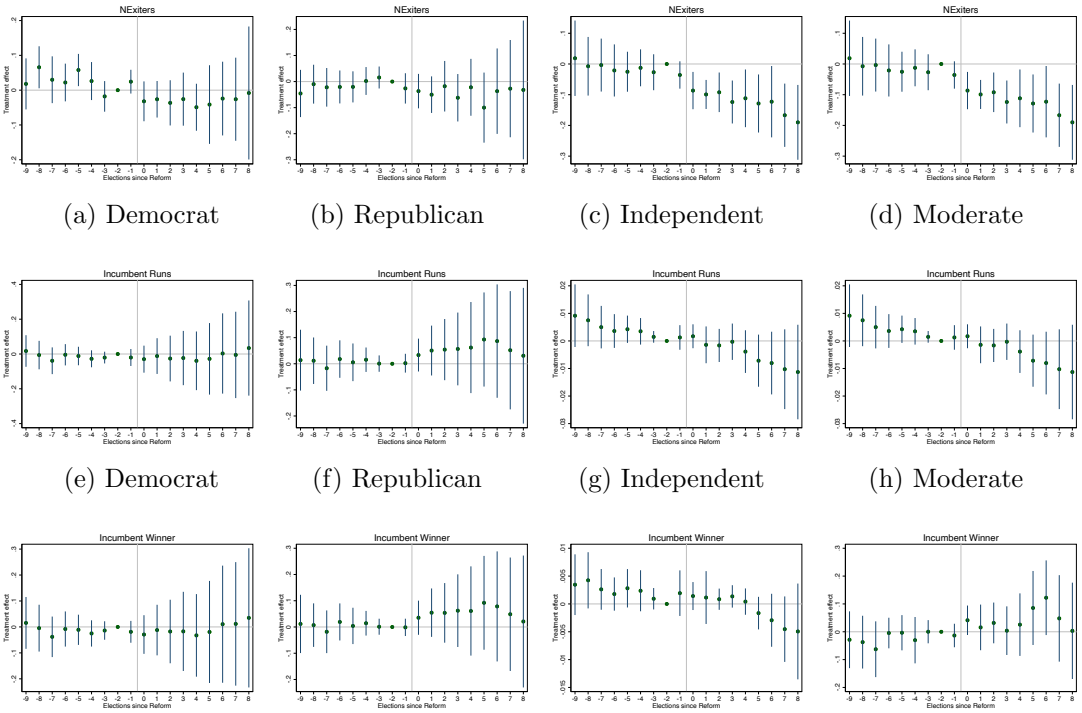
(b) Republican

(c) Independent

(d) Moderate

Notes: These figures show the effect of revolving door laws on the total number of candidates that run for the first time, broken down by measures of ideology. Information on Democrats/Republicans/independents come from the Klarner SLERs database. Moderates are defined as having ideological distance between themselves and the median of the legislature that is below the average among all candidates for a given state legislature-year election, based on the CF score of Bonica (2014).

Figure A11: Revolving Door Laws and Exit for More Moderate Incumbents, Event Studies



(a) Democrat (b) Republican (c) Independent (d) Moderate

(e) Democrat (f) Republican (g) Independent (h) Moderate

(i) Democrat (j) Republican (k) Independent (l) Moderate

Notes: The figures above show the effect of revolving door laws on exit and incumbent re-election, broken down by measures of ideology. The first row shows the effects on overall exit, the second row shows the effects on the probability of an incumbent re-running for election, and the third row shows the effects on incumbent re-election. Information on Democrats/Republicans/independents come from the Klarner SLERs database. Moderates are defined as having ideological distance between themselves and the median of the legislature that is below the average among all candidates for a given state legislature-year election, based on the CF score of Bonica (2014).