# 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 build a new dataset to 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.

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# 1 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 conflicts of interest arising from post-public-sector employment, 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 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 which, to the extent that turnover is associated with better political and economic outcomes (Marx et al., 2025; Bazzi et al., 2025), is a cost of limiting revolving door behaviors.

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 that states enjoy in setting conflict-of-interest regulations makes this an apt testing ground for the effects of revolving door rules more generally. Given the importance of state politics for U.S. democracy (Squire and Moncrief, 2019) as well as the pipeline from state legislatures to national politics, selection into these bodies is of direct interest in its own right.[1]

As of December 2025, forty states had passed revolving door restrictions at different times, while ten 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.

[1] Approximately 40% of federal lawmakers previously served in state legislatures. See, for example, Manning (2017), as well as earlier editions.

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 discuss motivations for their passage in Section 3.)

We collect new data on the passage of revolving door laws across states over time and exploit their staggered timing of adoption to identify the causal effect of revolving door restrictions. We do so 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 (2021a) 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.

As a preliminary step, we construct a novel link between state legislators and post-office lobbying careers by matching legislators to lobbying registrations from `Followthemoney.org`, an aggregator of data on money in state politics. This data linkage is a key contribution of the paper: it allows us to directly characterize which politicians go on to become lobbyists. Our model yields sharp predictions for these cross-sectional patterns. Politicians who become lobbyists should be ideologically less extreme and higher ability—reflecting weaker non-pecuniary motivations to remain in office and stronger outside options. Consistent with this prediction, candidates who eventually register as lobbyists tend to be more moderate, a pattern that holds for independents, Republicans, and Democrats alike.[2] Politicians-turned-lobbyists are also more likely to hold an advanced degree and to have attended a more selective undergraduate institution.

[2]This finding aligns with Egerod and Tran (2023), who document that former Members of Congress with strong partisan voting records are less likely to sit on corporate boards.

We then turn from these descriptive correlates to the policy consequences of revolving door restrictions. Post-2006 (the earliest year in which lobbying data is available for all states), about 12% of legislators eventually become lobbyists. Revolving door laws substantially reduce these transitions, especially within the cooling-off period: the probability that a former legislator registers within one year of leaving office falls by 3.9 percentage points (a 64% decline relative to the 6.1% pre-treatment mean), while the probability of registering within two years falls by 5.4 percentage points (a 57% decline relative to the 9.4% pre-treatment mean).

We next investigate the consequences of revolving doors for legislator entry and exit, and find support for both predictions of our model. For the entry margin, we find that, on average, 0.14 fewer new candidates enter relative to a mean entry rate of 0.92, 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 increases by 3.7 percentage points on a baseline of 78%. The decline in entry may also account in part for the relatively large increase in incumbent win rate, which increases by 4.4 percentage points relative to a baseline of 73%. The decline in entry also contributes directly to the increased likelihood that a candidate stands unopposed, which increases by 9.4 percentage points from an already-high 31 percent. For incumbents, there is an increase of 8.7 percentage points in the probability of standing unopposed on a baseline of 28 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 using AI Large Language Models. While our results on quality are mixed, we find clear support for the predictions on ideology. Most notably, entry of independents declines more after 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) and documented by Handan-Nader et al. (n.d.) on the increased polarization of the candidate pool in U.S. state politics.

Most directly, our work contributes to the body of research that studies the causes and consequences of revolving door employment and regulations.[3] 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).[4] 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. 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. The recent work of Brancaccio and Kang (2025) is one of the few attempts at studying the direct policy-making consequences of revolving door restrictions. By exploiting staggered state-level re-

[3]The revolving door is just one means through which interest groups may try to influence legislators. In this sense, our work links to a much larger literature that explores access via campaign contributions, lobbying, and other means; see Fouirnaies and Hall (2014); Bertrand et al. (2014, 2020), among many others.

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

forms that limit post-government employment of officials involved in municipal bond issuance, they show that these rules lead to lower complexity in bond design, consistent with reduced underwriter influence in public finance decisions.

We emphasize that our intention is not to evaluate the overall welfare effects of restrictions that reduce lobbying, which depend also on whether lobbying itself leads to policies that improve or harm social welfare – as the preceding paragraph highlights, this question has been well-studied and the weight of the evidence points toward social harm. Our purpose is instead to understand a distinct dimension that is also an input into evaluating the effects of lobbying restrictions through their impact on political selection. We thus study a heretofore undocumented consequence of such laws, and our findings suggest a cost that needs to be weighed against the potential benefits that lobbying restrictions have for policy formulation.

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 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, which has largely gone unstudied until now.[5]

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

[5]By studying the effect of revolving door laws on the labor supply of politicians, we also speak to a large literature in labor economics that studies 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).

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. 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 executive officials in Italy. Their main finding is that revolving door restrictions *increase* the education of politicians. While we do not observe any such pattern (and our theoretical framework predicts the opposite), several institutional distinctions may explain these differences. First, they focus 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 versus state) and system (parliamentary versus presidential) likely creates different selection mechanisms and career incentives.

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 3 describes the datasets we employ. In Section 4 we present the difference-in-differences model we will use in our empirical analysis. Section 5 provides our empirical results, and Section 6 concludes.

## 2 Illustrative Model

In this section, we provide a framework to illustrate the implications of revolving door rules on political selection. The model shows that the passage of revolving door restrictions will lead to the following:

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 legislators' 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.[6] 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

[6] In a similar spirit, Mattozzi and Merlo (2008) feature an environment in which there are private returns to holding office, though in their context the returns are driven by the fact that political careers deliver signals about ability in the private sector.

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 that 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 where 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. Panel (a) shows the relevant combinations in the absence of any revolving door opportunities, which are simply those with sufficiently high ideological concerns to offset the loss of private earnings – the darker purple-shaded region. Panel (b) adds 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 restrictions 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 expect a relative decline in the prevalence of higher-ability candidates. (Note, though, that there is some ambiguity in the overall prediction of how the composition of candidates changes, since the highest-quality ideological candidates continue to run for office, i.e., those the upper-right part of the figure.)

Turning to the decision to stand for reelection, we 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 de-

cided 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 among incumbents. 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 running a campaign. In equilibrium, this should reduce the impact of revolving door laws on exit (holding the cost of a campaign) 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 potential 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.

# 3 Data and Descriptive Statistics

## 3.1 Data

**Revolving Door Statutes** 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.[7] State revolving door statutes differ in details—most notably in which officials they cover and the length of the cooling-off period—but their basic structure is highly uniform: they impose a post-office waiting period before former legislators can lobby. Cooling-off periods range from as little as six months to as long as two years (Florida adopted a six-year ban in 2022, but that falls outside our sample). We therefore treat the date of passage as the relevant policy shock and do not further distinguish across statutory variants, provided they apply to legislators. In practice, this simplification is especially natural because most states converge on a standard one-year ban (82 percent), leaving limited scope—and little empirical payoff—for differentiating treatment intensity by the length of the cooling-off period.

Appendix Figure A1 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 A2, which suggests little obvious geographic or socioeconomic predictors of the presence of such a law or its passage date; we will examine in more detail whether the passage of such a law or its date of passage is correlated with state attributes in Section 4.

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.

**Election Outcomes** 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.[8] Each observation in the data refers to an individual candidate who ran for state legislative

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

[8] 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 November 21, 2025.

office.

**Candidate and Legislator Attributes** We use a range of sources to capture candidates' and 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.[9] 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. For robustness, we also replicate our analyses using NP-ideology scores developed by Shor and McCarty (2011), based on Nokken and Poole (2004), which are a widely-used variant on the classic DW-Nominate score of ideology calculated using roll call votes. The major advantage of CF scores relative to the roll-call-based measure 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 were then processed using ChatGPT to extract and organize the candidate's work history and educational experience. We ran this process on 130,123 unique candidates. The resulting output identified 73,882 candidates (56.8%) with identifiable work history information and/or education information available in the extracted text. 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 correct 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

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

assigned an incorrect age to 16. These findings suggest a relatively high degree of accuracy (84%) and also that false positives may be a bigger concern than false negatives.[10] 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 A2).

We use the resulting dataset to generate two education-based measures of politicians' human capital, motivated by past work which finds that better-educated leaders are more productive legislators (Fisman et al., 2015) and are associated with higher economic growth (Besley et al., 2011). 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 52,511 candidates, or approximately 62% 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" undergraduate institutions as those with either (1) a below median acceptance rate or (2) an above median 25th percentile SAT score.

Finally, 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

[10] As an alternative approach to assessing the quality of our matches, we utilize the dataset of Carnes and Hansen (2022), which provides biographical information on state lawmakers who held office in 2021 and 2022, manually compiled from legislative and campaign websites, as well as other online sources. Even via this manual search of recent legislators, year of birth could be identified for fewer than 60% of legislators. For the legislators that appear in both their data and ours, we can readily compare age and highest degree obtained. As shown in Table A1, there is a perfect match in approximately 75% of observations for age, and 86% for highest degree.

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.

**Lobbying Activity** 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.[11] 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 (e.g., Eric versus Erica). With this match in hand, we then link legislators to the registered lobbyist data for the state they represented, which is where they are most likely to lobby.[12] 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.[13]

**Additional State-Level Controls** In our main analysis, we also include additional state-level time-varying controls. Most importantly, we include annual legislative salary data from the Book of the States to control for the 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 years and for states with missing annual salary data, we supplement with data from Bowen and Greene (2014). We further include additional data on GDP per capita and population from the Bureau of Economic Analysis (BEA), minority and urban population population shares from the US 1990 Census, Democratic vote share data from the

[11] 14 states have some lobbyist information pre-2006, but all states have lobbyist information post-2006.

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

[13] False negatives can arise from names with low string similarity, but can be identified as common nicknames or misspellings, such as "Richard" versus "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.

MIT Election Lab, state-level corruption data provided by Glaeser and Saks (2006) and newspaper circulation data collected by Djourelova et al. (2021).

## 3.2 Descriptive Evidence

We provide summary statistics for candidates and legislators in state legislative elections in Table 1, at the district–election level (panel (a)) and at the candidate-election level (panel (b)). At the district–election level, an incumbent runs 78% of the time. 74% of contests feature an incumbent winning, but conditional on running, incumbents win 95% of the time. The mean number of candidates is 1.81, and there is relatively little turnover, with 0.9 new entrants and 0.8 exiters. Finally, the rate of uncontested elections is quite high at 32%. 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.[14] At the candidate-election level, women account for 21% of candidate-cycles; among observations with race data, 90% of candidates are White. Party representation is close to parity (47% Democrats, 44% Republicans) with 9% independents. Measured human capital is high: 34% of candidates hold an advanced degree. The typical politician accrues substantial service, with mean tenure of 7.8 years (median 6.8, max 56). Electoral results are consistent with the strong incumbent advantage and high rate of single-candidate elections we observe in our data: the average vote share is 55%, and the unconditional win rate is 57%. Involuntary exits are rare (2%). Finally, individuals who ever appear as lobbyists are observed in 8% of candidate-cycles.[15]

### 3.2.1 Who Becomes a State Legislator?

We investigate descriptively who becomes a state legislator. Our data allow us to characterize for the first time the attributes of both candidates and elected officials of the universe of U.S. state legislatures for an extended period of time. We start by providing summary statistics comparing state candidates and elected legislators to members of the U.S. Congress in Appendix Table A3. We find that state candidates/legislators are similar to national legislators in terms of age, averag-

[14] The frequency of single-candidate elections also can help to account for the relatively modest increase in sample size when we look at all candidates in panel (b) rather than elected legislators.

[15] 55% of candidates also appear in the legislator sample. *Female* has 2.3% of observations missing because of gender-ambiguous first names or initial-based names; *White* is missing for 38.7% of observations because race information is only identified for a sub-sample of the legislator data, and *Adv. Degree* is missing for 37.9% of observations because our search algorithm did not turn up any matches.

ing about 43-47 years old. They are also comparable in gender composition, with the U.S. Congress having slightly more male legislators compared to state legislatures. While state and national legislatures are similar in terms of race, there are notable differences by ethnicity. 6% of elected congress members are of Hispanic origin, whereas this number is only 3% for state legislators. The composition of Democrats versus Republicans is similar across state and national legislatures. However, there are significantly more independent candidates and legislators at the state-level, 15% and 2% respectively, compared to only 0.2% of elected members of the U.S. Congress. Similarly, there are substantial 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, respectively). 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, state candidates are less likely to appear as lobbyists than state legislators, at 6% and 10% respectively, and both are far less likely to register as lobbyists relative to the 22% of U.S. Congress members that appear as lobbyists, as reported in Palmer and Schneer (2019).

#### 3.2.2 Who Becomes a Lobbyist?

After the fuzzy match, we identify 5,960 unique individuals registered as lobbyists, which constitutes approximately 6% of all candidates in our data. However, we make several important qualifications to the interpretation of this figure. First, this represents a lower bound on the true lobbying rate, since these are the cases that we were able to match to the FollowTheMoney database. Furthermore, given we only observe lobbying activity post-2006 for most states, a large fraction of politicians we study are very unlikely to show up because they exited politics relatively early in the sample. Table A4 documents how often state-level politicians appear in the lobbying registries and the transition rates at different points of their careers. Consistent with lobbying being primarily a post-office path for officeholders, observed lobbying rates are substantially higher among elected legislators than among non-elected candidates: 10% versus 6% in the full sample, and 12% versus 7% when restricting to candidates who ran post-1998. For certain states, such as Arizona and Nevada, lobbying rates are as high as 20% (Figure A3). More generally, observed lobbying rates vary markedly across states, a pattern that could plausibly be related—at least in part—to cross-state differences in revolving door rules (e.g., the presence and stringency of cooling-off periods), alongside other features of state political economies

and the size of the local lobbying sector. If we focus only on lobbying rates for legislators post-exit, we find that this rate is 5% in the full sample and 8% in the post-1998 sample, reflecting the fact that those who ran later are more likely to have observed lobbying activity in the post-2006 period. If we further restrict the denominator only to legislators that we observe ever exiting politics, these shares increase, respectively, to 6% and 11%.

Finally, while these observed transition rates might seem modest, it is important to note that what is relevant for the decision to enter politics is prospective politicians' *beliefs* about their likelihood of earning lobbying income. It is thus plausible that a larger fraction of politicians think they may act as lobbyists later in their careers, and to the extent that this is viewed as a particularly desirable post-office occupation, overconfidence (which may be particularly prominent among political leaders, Moore and Bazerman, 2022) may amplify candidates' beliefs that they themselves will be able to earn lobbying income.

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.

Figure 2 examines 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. 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. Finally, the patterns in panels (e) and (f) suggest that non-lobbyists have more extreme ideologies among both elected legislators and non-elected candidates, with slightly more pronounced differences in the distribution of ideologies for candidates. 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 A5 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. Closely related (since in our model ability maps directly into outside earnings), in Table A6, we show that, for all groups, pre-office earnings of lobbyists are slightly higher than for non-lobbyists, based on occupation-level salary data from the Bureau of Labor Statistics.

Finally, we use data from the National Survey of State Lobbyists (SSL) as a representative benchmark to compare with our own lobbying data.[16] The SSL reports that approximately 5% of lobbyists surveyed are former state legislators, which closely matches the percentage of candidates/legislators we identify as lobbyists in the SLERs dataset. The SSL also provides information on the frequency of lobbying activity by industry/topic area, which allows us to inspect the distribution of lobbying activity across sectors in Appendix Table A7. Former legislators lobby in similar industries to non-legislators, with some modest differences: former legislators lobby more on education and less on tax issues.[17]

## 4 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-

[16]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).

[17]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.

in-difference specification:

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

$Y_{ist}$ are the outcomes of interest, measured at the district-election-year level. $Treated_{st}$ is an indicator variable that takes the value of 1 if a revolving door law is in place in state $s$ in election-year $t$. The specification includes two sets of fixed effects. State fixed effects $\alpha_s$ absorb time-invariant differences across states, such as political culture, institutional design, or baseline competitiveness. Calendar-year fixed effects capture shocks that affect all states in a given year, such as nationwide political cycles or federal reforms. We allow calendar-year fixed effects to be interacted with an indicator for whether a state belongs to the never-treated group, $\delta_{gt}$. This ensures that never-adopting states, while serving as part of our comparison group, are permitted to follow their own arbitrary trajectory over time. This is important because never-treated states may differ systematically from eventual adopters in both levels and trends; by letting them evolve flexibly, we avoid attributing such differences to the effect of revolving door laws.[18] Standard errors are clustered at the state level, allowing for serial correlation over time.[19]

To investigate pre-trends, as well as the dynamic evolution of the treatment effect, we also estimate a non-parametric event-study specification:

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

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

Our main coefficients of interest, $\beta$ and the set of $\beta_k$'s, recover the average treat-

[18] While at first glance the inclusion of these interacted fixed effects may appear non-standard, they are in fact relatively common in recent work on staggered adoption designs (see, e.g., Goodman-Bacon, 2021; Sun and Abraham, 2021b). Our approach can be understood as a discrete version of group-specific time effects: it ensures that treatment effects are identified by comparing adopters before and after reform to appropriate counterfactuals, rather than relying on the assumption that never-adopting states follow the same trend as adopters.

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

ment 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.[20] Our dynamic specification allows us to inspect the plausibility of the first two assumptions. For the third assumption, we proceed in two ways. First, we estimate the weights attached to each of the 2x2 comparisons across states and periods, as recommended by de Chaisemartin and D'Haultfœuille (2020). We find that all the ATTs receive non-negative weights, thus suggesting that our TWFE estimates are not biased by the presence of negative weighting. 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 (2021a).

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). Using newspapers, we identify 8 states were Revolving Door laws were passed in response to political scandals.[21] 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 have 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. Using a conservative definition as any law enacted with comprehensive ethics legislation, we identify 17 states as having passed revolving door laws as part of broader ethics legislation (44% of all Revolving Door laws passed).

Both of these features of reform complicate the interpretation of our results: po-

[20] 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, 2021a).

[21] This definition includes prominent scandals involving specific state legislators, and excludes laws passed in response to more general and non-specific concerns of unethical behavior.

litical 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. While the passage of revolving door restrictions as a direct response to scandals is relatively rare in the post-1998 sample period (3 of 17 states), laws passed as part of broader ethics legislation are more common (9 of 17). However, as we document in Section 5, there are no significant differences in the estimated effects for states with ethics scandals or concurrent ethics reform, suggesting that it is revolving door restrictions themselves that drive the effects we document.

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 the log of population and GDP per capita from the Bureau of Economic Analysis, 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. We include 1996 newspaper circulation data from Djourelova et al. (2024) as a measure of local newspaper presence, which could affect the ability of voters to learn about revolving door lobbyists, and an indicator for the presence of active corporate political contribution bans in the 1990s, adapted from data in La Raja and Schaffner (2014). Lastly, we include an indicator for the presence of legislative term limits, which is collected from the National Conference of State Legislatures.[22] We investigate whether pre-treatment covariates (fixed at 1990-2000 values) predict the passage of revolving door laws. In Appendix Table A8, we find no significant differences in means between the ever-treated and never treated states

[22]https://www.ncsl.org/about-state-legislatures/the-term-limited-states, last accessed December 12, 2025.

for most covariates, except that treated states have slightly higher Democratic vote share and are less likely to have corporate contribution bans in place. In Appendix Table A10 we test whether state-year covariates are correlated with treatment status, and find no clear patterns when accounting for state and year fixed-effects.[23] Overall, while some time-invariant factors are correlated with the adoption of a revolving door law, namely the presence of contribution bans, there is little evidence that state-year correlates predict the timing of law adoption. We discuss the robustness of our results in Section 5, where we control for corporate contribution and other confounders that may be correlated with revolving door law adoption.

# 5 RESULTS

## 5.1 Revolving Door Transitions

As a natural starting point, we examine the extent to which revolving door restrictions reduce the number of former legislators that register as lobbyists after leaving office. An important caveat to make in this analysis is that, in order to "walk through the revolving door," politicians have to decide to leave office. As emphasized in our conceptual framework, however, the exit margin is itself affected by revolving door "treatment," thus making our analysis conditional on an endogenous outcome. With this caveat in mind, we present our results in Table 2. We find clear evidence that revolving door laws substantially reduce short-run transitions: the probability that an ex-legislator registers within one year falls by 3.9 percentage points, a 64% decline relative to the pre-treatment mean of 6.0%., and the probability of registering within two years falls by 5.4 percentage points (57% relative to a 9.4% pre-treatment mean). The effect on longer-term exit is smaller (-2.2 percentage point, with a 14.2% pre-treatment mean) and imprecisely estimated. Taken together, the estimates indicate that cooling-off rules meaningfully delay entry into lobbying – there is a sharp decline in the probability of registering as a lobbyist in the first one to two years, without clear evidence of eliminating the transition altogether over longer horizons. This pattern is consistent with our model in which cooling-off periods depress the immediate continuation value of lobbying after office, inducing would-be lobbyists to remain in office longer or dissuading would-be lobbyists from entering politics.

[23] Appendix Table A10 excludes newspaper circulation since it is only available for a subset of all election-year observations (1994-2010). We also exclude corporate contribution bans since this has little to no variation in the post-1998 period.

## 5.2 Entry, Exit and Competitiveness of Elections

Next, we examine how the passage of revolving door laws impacts political entry and exit, using the framework of Section 4.[24]

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 set of 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 3 presents our main results, with the regression analog in Table 3, column 2. 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 sizable effect: the results in Table 3 suggest that revolving door laws cause a significant drop of 0.14 in new candidates, relative to a sample mean of 0.92.[25]

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 that 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 candidates will enter politics with the intention of staying in office.[26]

Figure 4 and column 5 of Table 3 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

[24] We plot the aggregate rates of exit and entry across our sample period in Appendix Figure A5 for reference.

[25] 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.

[26] 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.

rerun rate is 78% – and the passage of a revolving door law is associated with a further 3.7 percentage point increase. Given the decline in entry, it is unsurprising that the impact on reelection is even higher – a 4.4 percentage point increase relative to an unconditional mean incumbent reelection rate of 73% (Table 3, column 3, and Figure 5). This is consistent with stricter revolving doors laws reducing the returns to leaving office, and thus making incumbents more likely to remain in office rather than pursue the outside option of lobbying.

Finally, Figure 6 and column 1 of Table 3 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 reduced 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 7 shows the effect of revolving door laws on the probability of having an unopposed (single-candidate) election. We find that post-reform, the probability of a single-candidate election is 9.4 percentage points higher (Table 3, column 3), compared to the pre-reform period. We observe a similar pattern for unopposed incumbents in Figure 7, and Table 3, column 6. This is a very large impact when compared to the sample mean of just under 31% unopposed races.

We conclude this section by discussing the magnitudes of our estimated effects, and compare them to existing work on the responsiveness of political labor supply to changes in monetary incentives. In our data, the introduction of a cooling-off period reduces the number of new entrants by about 0.14 (a 15% decline relative to pre-treatment mean, and 11% of a standard deviation) and increases the probability of incumbents seeking re-election by 3.7 percentage points (a 5% increase relative to the pre-treatment mean and 9% of a standard deviation). Based on current compensation figures from the Book of the States, a state legislator earns around $40–50,000 per year, with a non-lobbying outside option of about $125,000 based on our data on pre-legislative earnings reported in Table A6. While it is harder to find estimates of lobbying earnings, based on disclosure data available in a sample of states, as well as figures reported on Glassdoor, we estimate that a former politician who later becomes a lobbyist can earn up to $200,000-300,000.[27] A one-year cooling off period

[27]For example, a Center for Public Integrity report from 2006 found that ex-legislators working as lobbyists in Texas earned between between $256,000 and $494,000 in client fees in 2005 ("Statehouse Revolvers," Kevin Bogardus, October 12, 2006). While current estimates from Glassdoor put median state lobbyist earnings at $90,000, the higher end of the distribution is above $200,000, and we

thus implies a loss of about 100% relative to the outside wage, or 200% relative to the average legislator's annual wage. For comparison, Ferraz and Finan (2009) show that in Brazilian municipal elections a 50% increase in legislator wages increases political competition by 22% relative to the mean. Fisman et al. (2015) similarly find that doubling the pay for Members of the European Parliament raises the probability of incumbents running for re-election by 40%, and the number of parties contesting elections by 60% of a standard deviation. In this sense, the magnitudes of our entry and exit effects are quite comparable, if not smaller, when compared to those in the existing literature, once we account for the size of the change in financial incentives induced by cooling-off laws.

## 5.3 Robustness

### 5.3.1 Alternative Time Window

Since, as previously described, we may obtain data on electoral outcomes only for 1968 onward, in our main analysis 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. In Appendix Figure A4, we show that the patterns we document in our main results are very similar if we use a much more inclusive definition of all states that passed laws 1975 or later.

### 5.3.2 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 (2021a), which is specifically designed to account for such heterogeneity in staggered difference-in-differences settings. Appendix Figure A6 shows the results using this alternative estimation method. The coefficients maintain similar patterns of significance and magnitude as in our primary speci-

surmise that former legislators turned into lobbyists would likely be in the latter range, given their high degree of relevant political experience. Indeed, Blanes I Vidal et al. (2012) show that former congressmen-turned-into-lobbyists earn over twice as much as other federal lobbyists.

fications, 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 de Chaisemartin and D'Haultfœuille (2020). 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.

#### 5.3.3 Alternative Control Group

When estimating Sun and Abraham (2021a), 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 Figure 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.

#### 5.3.4 Accounting for Other Policy Changes

A potential concern is that our identification strategy may be confounded by other reforms that occur 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. Naturally, given the small number of state-level units in our analysis, adding these additional (potentially correlated) reforms could be viewed as suffering from bad control problems, but with that caveat we include what we see as the most relevant changes to state-level governance during our sample period.

Compensation for elected officials varied considerably across states during our

sample period; we test the robustness of our results by controlling for changes in politicians' salaries. Appendix Table A11 reproduces the analysis in Table 3 but 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 legislatures in campaign finance regulation and contribution limits. Appendix Table A12 reproduces the analysis in Table 3, but including controls for changes in campaign contribution restrictions. Our estimates remain statistically significant and similar in magnitude across all outcomes with the exception of *Incumbent Runs*, which is marginally smaller and no longer significant ($p < 0.11$). Overall, this suggests that campaign finance reforms are not driving our findings. Lastly, we do not find any heterogeneity in the effects between states with and without campaign contribution bans, as shown in Table A17.

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. We present analyses that account for term limit changes in Appendix Table A13. It does appear that term limits themselves impact entry and exit decisions, an interesting result in its own right. The inclusion of this control has a modest impact on the entry and exit margins of all candidates that we document in our main results, with the latter no longer significant ($p < 0.12$) Since, even after accounting for term limit changes, we find that entry is significantly affected, it is thus unsurprising that we still find significant effects on single-candidate elections and incumbents running unopposed. However, while the coefficient on *Treated* is still positive in predicting incumbents running, it is much attenuated and does not approach statistical significance. As such, these results in our main analysis should be interpreted with somewhat more caution.

We show the results when including all confounds in Appendix Table A15. While the effects on uncontested elections remain consistent, the effects on entry, exit and incumbent re-election are attenuated, which mainly comes from the inclusion of

term limits rather than salary and corporate contribution bans (Appendix Table A14). While term limits are uncorrelated with the adoption and timing of revolving door laws as shown in Appendix Tables A8-A10, the presence of such laws does matter for the interpretation of our main results. Finally, when we look at heterogeneity by the presence of term limits, we find that the results are much stronger for non-term limited states. We also note that most states have no term limits in place - only 20% of state-years observed in our data have active term limits in place.

#### 5.3.5 Bootstrapped Standard Errors

All standard errors in our analysis are clustered at the state level, which includes 28 state-clusters with the post-1998 sample restriction. To address potential concerns resulting from the small number of clusters, we replicate our analysis using a nonparametric bootstrap estimation.[28] The results, shown in Appendix Table A16, show that our main estimates remain statistically significant, indicating that our findings are robust to concerns related to small cluster sizes and are not driven by downward-biased standard errors.

#### 5.3.6 Heterogeneity by motivation for reform

As we noted earlier, in many cases, revolving door laws were passed either as part of broader ethics legislation or in response to ethics scandals. In Appendix A21 and A22, we present results that allow for heterogeneity by whether revolving door restrictions were passed in response to ethics scandals or as part of broader ethics reforms, respectively. In neither case do we observe any significant difference in the treatment effects for these subgroups. Particularly when combined with the fact that the passage of revolving door restrictions makes distinct predictions for how we expect incumbents to respond, relative to the effects of scandal or ethics reform more generally, this suggests that the patterns we document in our main results are caused by revolving door restrictions rather than these other factors that may have led to reform.

[28] Nonparametric bootstrap estimation is performed using the *bootstrap* command in Stata, with 200 replications.

## 5.4 Who Responds? The Role of Salary and Legislative Professionalization

A key mechanism in our model is that revolving door laws change the expected returns to holding office by reducing the value of a potentially important post-legislative earnings opportunity. Thus, the model implies that the impact of revolving door restrictions should be especially pronounced in environments where (i) post-office opportunities are more valuable, or (ii) politicians are more likely to be on career paths where such opportunities are salient. While we cannot observe the value of lobbying opportunities directly across all states and cohorts, we use two institutional characteristics that are strongly associated with the professionalization of state politics—legislator salaries and whether the legislature is full- or part-time—as proxies for the extent to which politics is a career-oriented vocation and the relevance of outside options. The results appear in Appendix Tables A18 and A19, respectively.

We emphasize that the model does not deliver a clear prediction for how these characteristics should interact with revolving door laws. On the one hand, higher legislative pay can dampen the marginal effect of a revolving door restriction: if the compensation from office is already high, politicians may rely less on post-legislative monetization, so restricting lobbying represents a smaller proportional loss in the overall payoff from a political career. Similarly, full-time service could strengthen attachment to holding office, making incumbents less responsive to changes in post-office opportunities. Alternatively, professionalization may amplify the effect of revolving door restrictions. High-salary and full-time legislatures typically attract candidates with greater qualifications and more lucrative outside options, for whom post-legislative career restrictions represent a more significant constraint. Moreover, full-time service often reduces the feasibility of maintaining a parallel career while in office, making the post-office path (including lobbying) more central to the expected career benefits of holding office.

Consistent with the latter logic playing a more dominant role, we find that revolving door laws have substantially stronger effects in high-salary and full-time legislatures. We interpret this pattern as suggestive that revolving door opportunities are more economically relevant—and thus more affected by legal restrictions—in more professionalized settings. In other words, the environments where politics is most plausibly a career, and where legislative experience is likely to be most marketable, are also the environments where restricting post-legislative lob-

bying has the greatest consequences for political selection and competition.

## 5.5 Who Responds? 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. On the other hand, 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 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 4 and 5 for entry of new candidates and exit versus rerun decisions respectively.[29] Throughout, the regression estimates for high-quality candidates – whether entry, exit, or the rerun decision – are too imprecisely measured and thus provide no clear evidence of a significant differential impact on higher-quality politicians from the passage of revolving door laws; this could result from the reduced sample size, or noisiness in our proxies for quality; it is also worth reiterating that we had no clear prediction for the exit margin, which could again account for the lack of any consistent estimates.

That said, the dynamic event studies in Appendix Figure paint a more coherent picture: for all three proxies, the point estimates for entry become increasingly negative in the post-reform period, consistent with a gradual decline in higher-quality entrants. The slow timing might thus contribute to explaining the lack of significance in the tables above that provide an average effect across all periods. In terms

[29]Event plots corresponding to the analyses presented in this section may be found in Appendix Figures A8-A11.

of magnitudes, for example, the average point estimate for post-reform election cycles for entry of candidates with advanced degrees converges to approximately -0.05, which is roughly 35% of the mean entry rate for such candidates; for attending a high-quality college, the implied effect is even larger: -0.05 relative to a dependent variable mean of 0.078. For comparison, for the full sample, the point estimates converge to approximately -0.2, which is just above 20% of the full sample entry rate. A similar gradual pattern appears for overall exits among higher-quality politicians in Appendix Figure .

Turning to ideology, we again consider the differential effects for entry (Table 6) and exit/rerunning (Table 7). 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 -0.040 in column 2 is 10.8% when scaled by the base rate, as compared to the overall impact of 15.4% 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, -0.0625, and much larger when scaled by the relatively low base rate of independent entry ($0.0625/0.135 = 46.3\%$). In the final column of Table 6 we provide results based on the CF ideology 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 ($0.0650/0.177 = 36.7\%$).

For exit and rerun decisions, recall that we expect to see a relative increase in *moderates* for both exiters and incumbents that rerun.[30] Focusing first on the exit decision, we find a disproportionate effect on relatively moderate candidates. Compared to the overall treatment effect on exit of 16.2% (0.131/0.81), for independents and moderates the point estimates imply treatment effects of 45.8% and 33% respectively. The results on incumbents rerunning are more mixed: while the treatment ef-

[30]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.

fect for moderates is relatively large (1.4% versus 4.7% 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: there is a disproportionate decline in independents and moderates who run for office and a decline in the exit of moderates. The reduced number of 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. [31]

## 6 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 larger reduction in entry of 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

[31] 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.

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-Election 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 | 0.47 | 0.00 | 0.50 | 0.00 | 1.00 | 141,254 |
| 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 | 0.34 | 0.00 | 0.47 | 0.00 | 1.00 | 87,372 |
| 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 |
| Lobbyist | 0.08 | 0.00 | 0.28 | 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-election 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 3. Advanced degree information is generated through the online biographical candidate data collection described in Section 3.

TABLE 2: Revolving Door Laws and Revolving Door Lobbying

| | (1) Lobby $\leq$ 1 year | (2) Lobby $\leq$ 2 years | (3) Lobby Post |
|---|---|---|---|
| Treated | -0.0386** | -0.0540** | -0.0217 |
| | [0.0172] | [0.0207] | [0.0181] |
| DepVarMean (Pre-T) | 0.0606 | 0.0942 | 0.142 |
| Observations | 8772 | 7504 | 8772 |
| R-sq | 0.0298 | 0.0329 | 0.0433 |

*Notes:* This table shows the effect of Revolving Door laws on post-office lobbying activity. We run our main regression specification on a candidate-level dataset and restrict attention to elected legislators with observed exits in the data. We also restrict to legislators who appear in the data post-1998 to account for missing/incomplete lobbyist information in pre-2002 years. Columns 1 and 2 shows the effects of treatment on lobbying less than 1 year, within 1 year or within 2 years of leaving office, respectively. Column 3 shows the effects of treatment on lobbying at any point post-exit. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level.
* $p < .1$, ** $p < .05$, *** $p < .01$.

TABLE 3: 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 | -0.131*** | -0.139** | 0.0937*** | 0.0868** | 0.0367* | 0.0438** |
| | [0.0380] | [0.0531] | [0.0336] | [0.0354] | [0.0204] | [0.0200] |
| DepVarMean (Pre-T) | 0.822 | 0.922 | 0.311 | 0.276 | 0.777 | 0.731 |
| Observations | 78239 | 78239 | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0824 | 0.0912 | 0.159 | 0.136 | 0.0271 | 0.0291 |

*Notes:* This table shows the results of our main specification in Section 4. 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. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level.
* $p < .1$, ** $p < .05$, *** $p < .01$

## TABLE 4: Revolving Doors Laws and Entry for Higher-Quality Candidates

| | (1) All | (2) Adv. Degree | (3) High Quality Edu | (4) Future Federal |
|---|---|---|---|---|
| Treated | -0.139** | -0.0268* | -0.0182** | -0.00836*** |
| | [0.0531] | [0.0135] | [0.00835] | [0.00257] |
| DepVar Mean | 0.900 | 0.142 | 0.0783 | 0.0118 |
| DepVar SD | 0.872 | 0.377 | 0.282 | 0.108 |
| Observations | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0912 | 0.0331 | 0.0268 | 0.00661 |

*Notes:* The table above shows the effect of revolving door laws on entry of higher-quality candidates. Column (1) shows the effect of treatment on the total number of new candidates in a given district-election. Columns (2) and (3) show the treatment effect on the number of new candidates with an advanced degree and higher quality undergraduate institutions, respectively. Column (4) shows the treatment effect on the number of new candidates who run for federal office in future periods. Advanced degree information and undergraduate quality indicators are generated through the online biographical candidate data collection described in Section 3. Data on former state legislators running for federal elections comes from Phillips et al. (2024). Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level. * $p < .1$, ** $p < .05$, *** $p < .01$

## TABLE 5: 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 | -0.131*** | -0.0159 | -0.00797 | -0.00579** |
| | [0.0380] | [0.00983] | [0.00842] | [0.00238] |
| DepVar Mean | 0.807 | 0.129 | 0.0725 | 0.00897 |
| DepVar SD | 0.794 | 0.353 | 0.268 | 0.0944 |
| Observations | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0824 | 0.0195 | 0.0194 | 0.00529 |

**Panel B: Incumbent Runs**

| | (1) All | (2) Adv. Degree | (3) High Quality Edu | (4) Future Federal |
|---|---|---|---|---|
| Treated | 0.0367* | 0.00664 | 0.0135 | -0.00652 |
| | [0.0204] | [0.0154] | [0.0178] | [0.00584] |
| DepVar Mean | 0.779 | 0.214 | 0.155 | 0.0261 |
| DepVar SD | 0.415 | 0.410 | 0.362 | 0.159 |
| Observations | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0271 | 0.0277 | 0.0775 | 0.0112 |

**Panel C: Incumbent Wins**

| | (1) All | (2) Adv. Degree | (3) High Quality Edu | (4) Future Federal |
|---|---|---|---|---|
| Treated | 0.0438** | 0.0108 | 0.0183 | -0.00592 |
| | [0.0200] | [0.0152] | [0.0181] | [0.00563] |
| DepVar Mean | 0.736 | 0.204 | 0.148 | 0.0256 |
| DepVar SD | 0.441 | 0.403 | 0.355 | 0.158 |
| Observations | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0291 | 0.0264 | 0.0751 | 0.0112 |

*Notes:* The table above shows the effect of revolving door laws on exit of higher-quality candidates. Panel A, column (1) shows the effect of treatment on the total number of exiters in a given district-election. Columns (2) and (3) show the treatment effect on the total number of exiters with advanced degrees and with higher quality education, respectively. Column (4) shows the treatment effect on the total number of exiters who run for federal office in future periods. Panel B and C, column (1) shows the effect of treatment on the probability of running for re-election and winning re-election, respectively. Panel B and C, columns (2) and (3) show the treatment effect on the probability of running for and winning re-election for incumbents with advanced degrees and with higher quality undergraduate education, respectively. Panel B and C, column (4) show the treatment effect on the probability of running for and winning re-election for incumbents who run for federal office in future periods. Advanced degree information and undergraduate quality indicators are generated through the online biographical candidate data collection described in Section 3. Data on former state legislators running for federal elections comes from Phillips et al. (2024). Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level. * $p < .1$, ** $p < .05$, *** $p < .01$

TABLE 6: Revolving Doors Laws and Entry for More Moderate Candidates

| | (1) All | (2) Democrat | (3) Republican | (4) Independent | (5) Moderate |
|---|---|---|---|---|---|
| Treated | -0.139** | -0.0403 | -0.0334 | -0.0625*** | -0.0650** |
| | [0.0531] | [0.0312] | [0.0337] | [0.0187] | [0.0250] |
| DepVar Mean | 0.900 | 0.374 | 0.367 | 0.135 | 0.177 |
| DepVar SD | 0.872 | 0.507 | 0.507 | 0.417 | 0.409 |
| Observations | 78239 | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0912 | 0.0714 | 0.0761 | 0.0942 | 0.0934 |

*Notes:* This table shows the effect of revolving door laws on entry for candidates with differing levels of ideology. Column (1) shows the effect of treatment on the total number of new candidates in a given district-election. Columns (2)-(4) show the treatment effect on the total number of democratic/republican/independent new candidates, respectively. Column (5) shows the treatment effect on the total number of new candidates with moderate ideology. Information on candidate party affiliation comes from the Klarner SLERs Database. Prior lobbyist activity is determined by matching to the FollowTheMoney database, as described in Section 3. 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). Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level. * $p < .1$, ** $p < .05$, *** $p < .01$

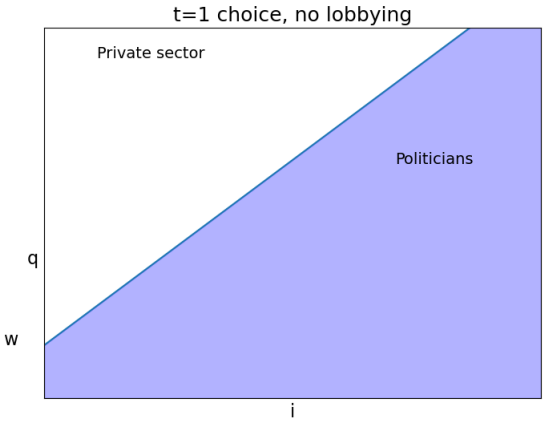
TABLE 7: Revolving Doors Laws and Exit for more Moderate Politicians

| | | | | | |
|---|---|---|---|---|---|
| **Panel A: Number of Exiters** | | | | | |
| | (1) All | (2) Democrat | (3) Republican | (4) Independent | (5) Moderate |
| Treated | -0.131*** | -0.0348 | -0.0378 | -0.0586*** | -0.0452** |
| | [0.0380] | [0.0249] | [0.0265] | [0.0160] | [0.0188] |
| DepVar Mean | 0.807 | 0.347 | 0.332 | 0.128 | 0.137 |
| DepVar SD | 0.794 | 0.494 | 0.497 | 0.402 | 0.365 |
| Observations | 78239 | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0824 | 0.0384 | 0.0541 | 0.0854 | 0.123 |
| **Panel B: Incumbent Runs** | | | | | |
| | (1) All | (2) Democrat | (3) Republican | (4) Independent | (5) Moderate |
| Treated | 0.0367* | -0.0342 | 0.0721* | 0.000185 | 0.0438* |
| | [0.0204] | [0.0492] | [0.0381] | [0.00140] | [0.0216] |
| DepVar Mean | 0.779 | 0.411 | 0.361 | 0.0137 | 0.299 |
| DepVar SD | 0.415 | 0.492 | 0.480 | 0.116 | 0.458 |
| Observations | 78239 | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0271 | 0.0543 | 0.0487 | 0.318 | 0.155 |
| **Panel C: Incumbent Wins** | | | | | |
| | (1) All | (2) Democrat | (3) Republican | (4) Independent | (5) Moderate |
| Treated | 0.0438** | -0.0331 | 0.0774* | 0.000448 | 0.0480** |
| | [0.0200] | [0.0488] | [0.0378] | [0.000686] | [0.0216] |
| DepVar Mean | 0.736 | 0.388 | 0.341 | 0.0103 | 0.290 |
| DepVar SD | 0.441 | 0.487 | 0.474 | 0.101 | 0.454 |
| Observations | 78239 | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0291 | 0.0542 | 0.0500 | 0.316 | 0.150 |

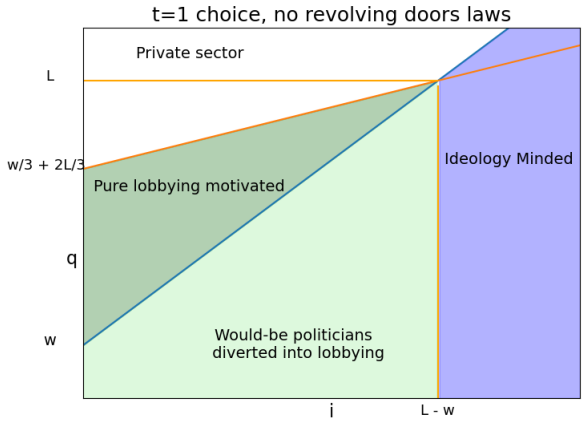
*Notes:* This table shows the effect of revolving door laws on exit for candidates with differing levels of ideology. Panel A, column (1) shows the effect of treatment on the total number of exiters in a given district-election. Panel, columns (2)-(4) show the treatment effect on the total number of democratic/republican/independent exiters, respectively. Panel A, column (5) show the treatment effect on the total number of exiters with moderate ideology. Panel B and C, column (1) shows the effect of treatment on the probability of running for re-election and winning re-election, respectively. Panel B and C, columns (2)-(4) show the treatment effect on the probability of running for and winning re-election for democratic/republican/independent incumbents, respectively. Panel B and C, column (5) show the treatment effect on the probability of running for and winning re-election for moderate incumbents who run for federal office in future periods. Information on candidate party affiliation comes from the Klarner SLERs Database. Prior lobbyist activity is determined by matching to the FollowTheMoney database, as described in Section 3. 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). Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level. * $p < .1$, ** $p < .05$, *** $p < .01$

Figure 1: Model Illustration

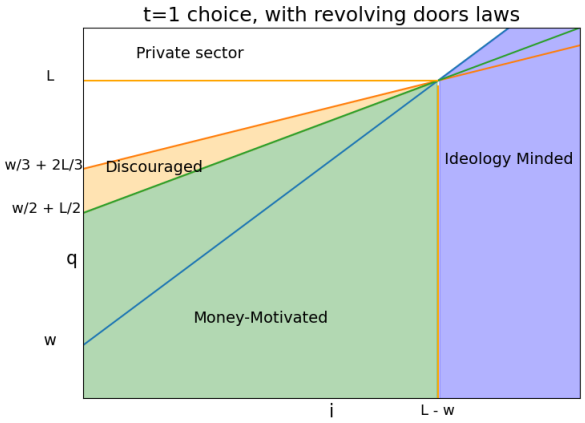
(a) No Lobbying



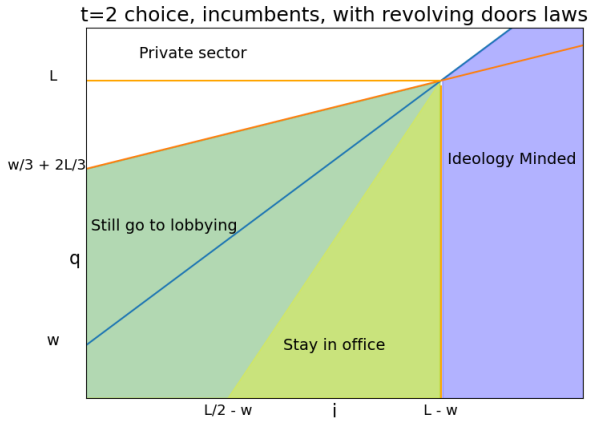

(b) Lobbying, No Laws




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

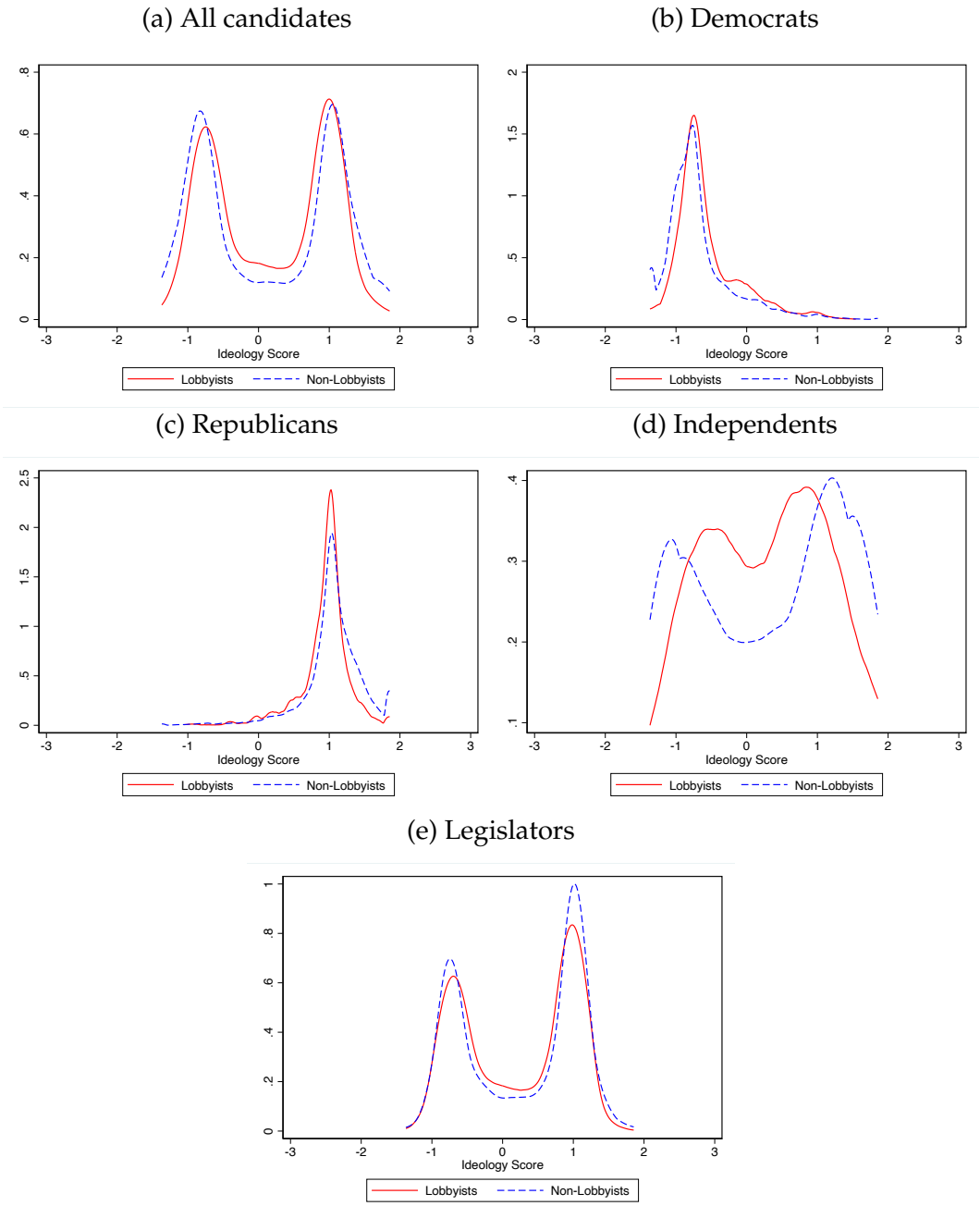



(d) Lobbying, with Laws, $t = 2$



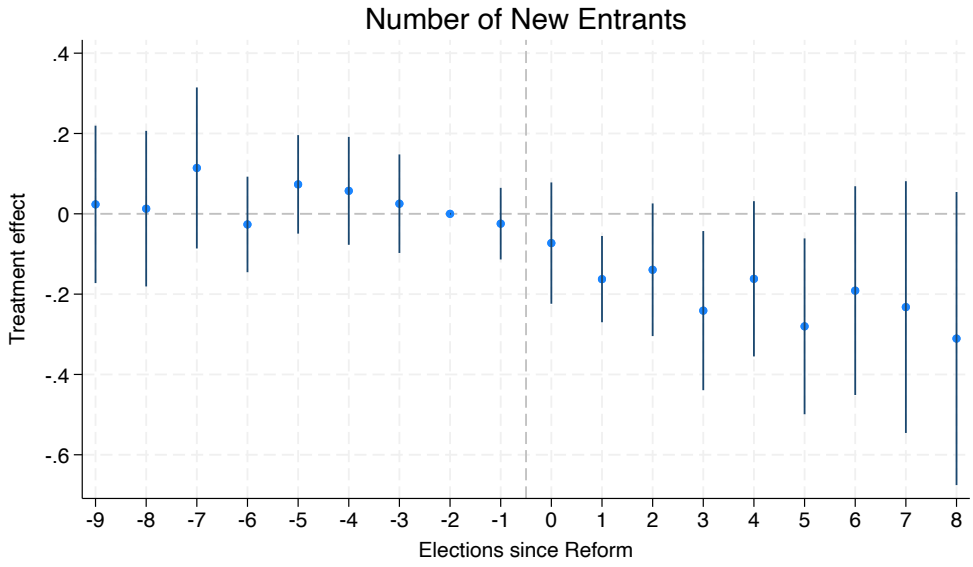

*Notes:* The figure above provides intuitions for the key implications of our conceptual framework as described in Section 2. Panels (a)-(c) illustrate the implications of revolving door laws at $t = 1$. Panel (a) shows the division of private and public sector labor without lobbying. Panel (b) shows 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: Ideology by Lobbying Status and Political Affiliation



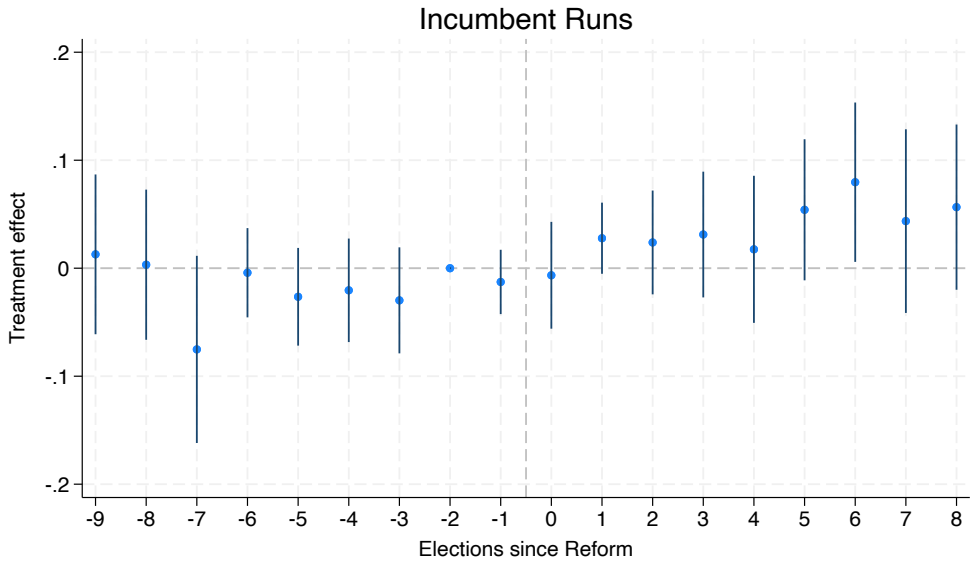

*Notes:* The figure above shows the distribution of ideology across candidates by lobbying status and party affiliation. Data on candidate-level ideology come from Bonica (2014), where negative values indicator more liberal ideologies and positive values indicate more conservative ideologies. Lobbying status is determined from the matching process described in Section 3. 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. Panels (e) shows the distribution of ideology for lobbyists vs. non-lobbyists for elected legislators only.

Figure 3: Revolving Door Laws Deter Entry into Politics



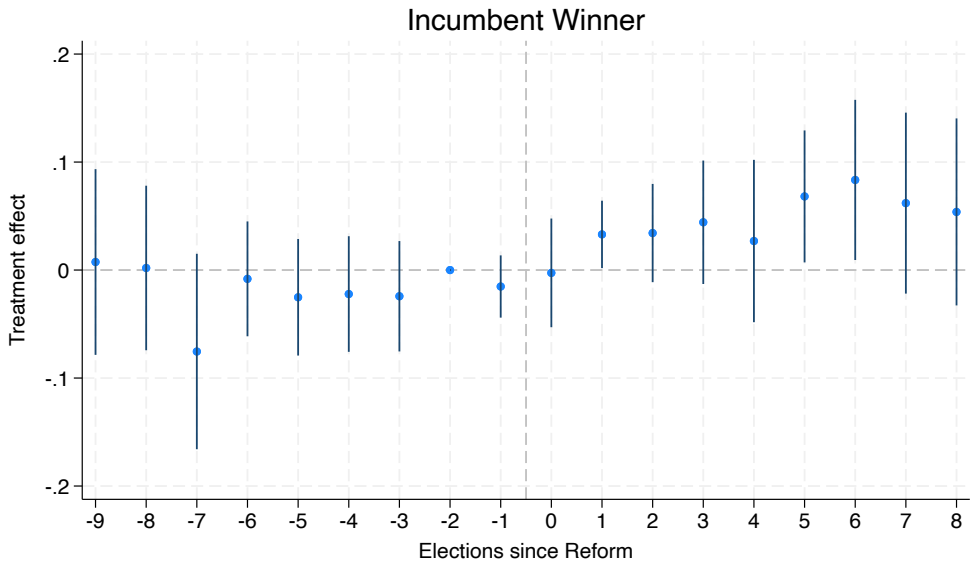

*Notes:* The figure shows the dynamic difference-in-differences estimates of the causal effect of revolving door laws on the total number of candidates that run for the first time. Relative time reflects the number of elections before and after the passage of the law (calculated using term length). New entrants are defined as those who appear for the first time in the election data. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level.

Figure 4: Revolving Door Laws Increase the Probability of Re-Running



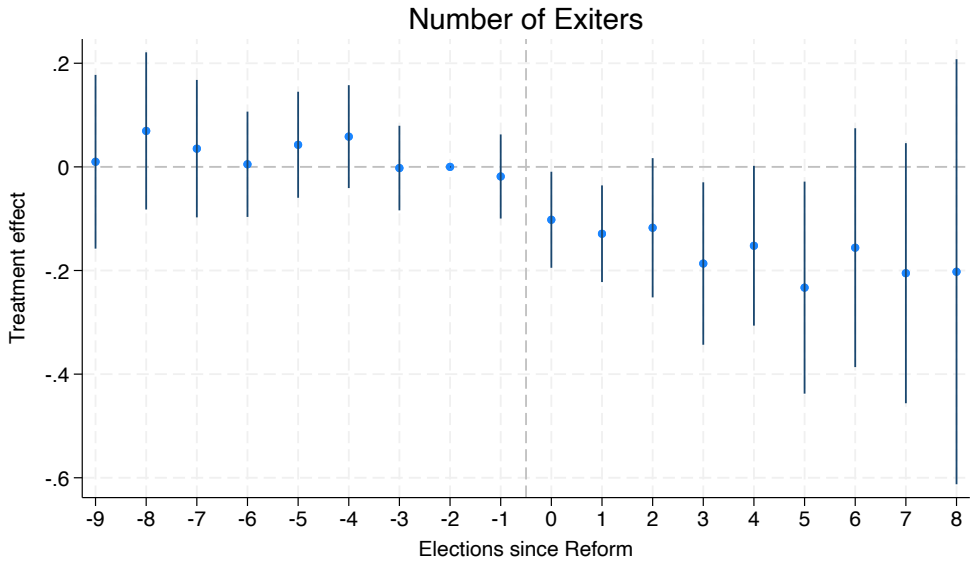

*Notes:* The figure shows the dynamic difference-in-differences estimates of the causal effect of revolving door laws on the probability of candidates running for re-election. Relative time reflects the number of elections before and after the passage of the law (calculated using term length). Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level.

Figure 5: Revolving Door Laws Increase the Probability of Incumbent Winning Re-election



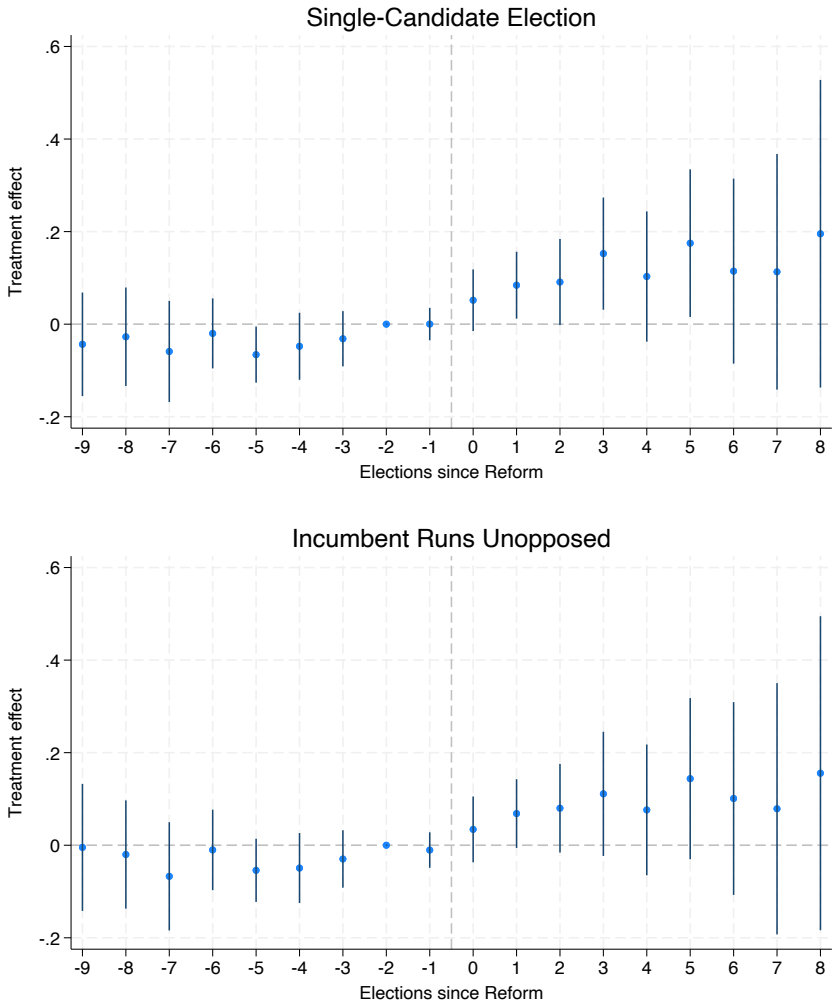

*Notes:* The figure shows the dynamic difference-in-differences estimates of the causal effect of revolving door laws on the probability of the incumbent candidate winning re-election. Relative time reflects the number of elections before and after the passage of the law (calculated using term length). Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level.

Figure 6: 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. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level.

Figure 7: 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 top panel shows the effects of revolving door laws on the probability of unopposed elections (where the number of candidates is equal to one). The bottom panel shows the effect of revolving door laws on the likelihood of an incumbent standing unopposed in an election. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level.

# APPENDIX FOR ONLINE PUBLICATION

TABLE A1: Bio Scraping: Comparison with 2021-22 State Legislators Dataset (Carnes and Hansen, 2022)

| | Age First Ran | Highest Degree |
|---|---|---|
| Comparable Obs | 942 | 2,090 |
| Matched Obs | 709 | 1,798 |
| Percent Matched Obs (%) | 75.27 | 86.03 |

*Notes:* This table provides a summary of comparisons between the biographic data in 2021-22 Duke Legislator Data (Carnes and Hansen, 2022) and the biographic scraping for the SLERS Dataset. Row 1 shows the number of comparable non-missing observations present in each dataset for the age and highest degree variables, respectively. Row 2 shows the total number of matched observations with non-missing observations. Row 3 shows the percent of match rate relative to the total number of comparable observations.

TABLE A2: 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 |

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

TABLE A3: Summary Statistics for Candidates/Legislators at the State vs. National Level

| | *State Legislators* | | *National Legislators* | |
|---|---|---|---|---|
| | Candidates | Elected | Candidates | Elected |
| Average Age | 44.11 | 42.79 | n/a | 47.73 |
| Male (%) | 77.62 | 79.03 | 81.67 | 83.32 |
| Democrat (%) | 44.01 | 51.14 | 45.17 | 49.29 |
| Avg. Ideology (Democrat) | -0.67 | -0.52 | -0.70 | -0.58 |
| Republican (%) | 43.33 | 48.31 | 50.74 | 51.47 |
| Avg. Ideology (Republican) | 1.04 | 0.96 | 1.05 | 1.02 |
| Independent (%) | 14.86 | 2.25 | 5.49 | 0.19 |
| Avg. Ideology (Independent) | 0.33 | 0.29 | 0.23 | -0.1 |
| Black (%) | 8.05 | 8.09 | n/a | 8.55 |
| Hispanic (%) | 2.88 | 2.93 | n/a | 6.11 |
| Undergrad Degree (%) | 62.34 | 70.12 | n/a | 93.30 |
| Advanced Degree (%) | 33.75 | 35.62 | n/a | 73.21 |
| Lobbyist (%) | 5.56 | 10.15 | n/a | 22.22 |
| Total (N) | 107,310 | 34,937 | 5,991 | 1,055 |

*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 A4: Lobbying Rates for Candidates and Legislators

| | | *Elected Legislators* | | |
|---|---|---|---|---|
| | All | Pre/Post-Office | Post-Office | Post-Office, Exiters |
| All Years | 0.06 | 0.10 | 0.05 | 0.06 |
| Obs | 107310 | 35672 | 35672 | 29175 |
| Post-1998 | 0.07 | 0.12 | 0.08 | 0.11 |
| Obs | 56005 | 20501 | 20501 | 14507 |

*Notes:* This table shows the lobbying rates for candidates/legislators, where lobbyists are identified through the matching process described in 3. Rows 1 and 3 show the lobbying rates across all years and restricting to post-1998 elections, respectively. Column 1 shows the overall lobbying rate for all legislators/candidates, Columns 2-4 show the lobbying rates among elected legislators. Column 2 shows the overall lobbying rate among legislators. Columns 3 and 4 shows the rate of legislators who lobby after leaving office unconditional and conditional on exiting, respectively.

TABLE A5: Quality Indicators for Lobbyists and Non-Lobbyists by Party Affiliation

| | Lobbyists | | | Non-Lobbyists | | |
|---|---|---|---|---|---|---|
| | Dem | Rep | Indep | Dem | Rep | Indep |
| Adv Degree (%) | 0.42 | 0.36 | 0.39 | 0.37 | 0.30 | 0.31 |
| High Quality Undergrad Edu (%) | 0.42 | 0.37 | 0.34 | 0.41 | 0.38 | 0.38 |
| 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 3. Advanced degree information and undergraduate quality are collected through biographical candidate data collection described in 3. Data on former state legislators running for federal elections is constructed from Phillips et al. (2024).

TABLE A6: Pre-Office Occupational Salary by Lobbying Status

| | Lobbyists | | | Non-Lobbyists | | |
|---|---|---|---|---|---|---|
| | Dem | Rep | Indep | Dem | Rep | Indep |
| BLS Occupational Salary ($) | 128,832 | 127,922 | 121,450 | 123,076 | 121,242 | 115,793 |
| Obs | 2144 | 1967 | 330 | 22557 | 22440 | 5738 |

*Notes:* This table shows the average pre-office salary 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 3. Pre-office salary data are constructed as follows: We first collect work history information from the biographical data scraped as described in Section 3. We then use LLM to categorize work histories into different occupational groups which correspond with the categories provided by the BLS, using the BLS-provided occupational salary as a proxy for pre-office earnings. Candidates with multiple occupations identified in the first step are the assigned the highest salary among their listed occupations.

TABLE A7: 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 | 0.14 | 0.09 | 0.19 | 0.15 |
| Education | 0.34 | 0.50 | 0.34 | 0.35 | 0.33 | 0.36 | 0.38 | 0.34 |
| Environment | 0.38 | 0.43 | 0.38 | 0.40 | 0.36 | 0.31 | 0.45 | 0.45 |
| Infastructure | 0.40 | 0.46 | 0.40 | 0.42 | 0.39 | 0.34 | 0.51 | 0.44 |
| Labor/Employment | 0.30 | 0.25 | 0.31 | 0.34 | 0.28 | 0.29 | 0.39 | 0.30 |
| Legal/Criminal Justice | 0.26 | 0.28 | 0.26 | 0.25 | 0.26 | 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 | 0.23 | 0.31 | 0.24 | 0.24 |
| Social Welfare | 0.56 | 0.57 | 0.58 | 0.58 | 0.55 | 0.63 | 0.53 | 0.52 |
| Other Issues | 0.26 | 0.12 | 0.27 | 0.26 | 0.25 | 0.28 | 0.24 | 0.31 |
| Obs | 1259 | 72 | 1132 | 517 | 742 | 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 A8: Difference in Pre-1998 Covariates: Treated vs. Control States

| | Treated | Control | Diff in Means | Std. Diff | P-Value |
|---|---|---|---|---|---|
| Log Pop (1996) | 1.29 | 0.90 | 0.38 | 0.334 | [0.305] |
| Log Inc. Per Capita (1996) | 5.84 | 5.82 | 0.02 | 0.203 | [0.575] |
| % Urban (1990) | 66.34 | 63.76 | 2.58 | 0.281 | [0.492] |
| % Minority (1990) | 19.69 | 15.77 | 3.92 | 0.258 | [0.437] |
| State Dem. Vote Share (1996) | 0.62 | 0.57 | 0.04 | 0.315 | [0.337] |
| SD State Dem. Vote Share (1990-1996) | 0.23 | 0.22 | 0.01 | 0.238 | [0.538] |
| Incumbent Vote Share (1990-1996) | 0.74 | 0.70 | 0.03 | 0.250 | [0.425] |
| Corruption Rate (1996) | 0.25 | 0.26 | -0.01 | -0.050 | [0.910] |
| Corp Contrib Ban (1990-2000) | 0.28 | 0.64 | -0.36 ** | -0.714 | [0.037] |
| NP Circulation (1996) | 0.23 | 0.24 | -0.01 | -0.127 | [0.679] |
| Term Limits (1990) | 0.42 | 0.36 | 0.05 | 0.099 | [0.757] |
| Salary (Scaled 1998 $) (1996) | 1.09 | 1.01 | 0.08 | 0.433 | [0.217] |
| High Salary Indicator (1990-2000) | 0.56 | 0.09 | 0.46 *** | 1.526 | [0.000] |
| Full Time Indicator (1990-2000) | 0.17 | 0.18 | -0.02 | -0.049 | [0.911] |

*Notes:* The table above shows the difference in pre-1998 covariates between treated vs. control states in our main sample. Columns 1 and 2 show the covariate means for the treated and control states, respectively. Column 3 shows the difference in means from a bivariate regression of ever treated on each covariate, using robust standards. Column 4 represents the standardized difference in means coefficient, relative to the standard deviation of the control group. Column 5 represents the p-value for the difference in means coefficient. State-year level covariates come from the sources described in Section 3. * $p < .1$, ** $p < .05$, *** $p < .01$

TABLE A9: Predictors of Laws' Adoption, Cross-Section

| | Indicator(Ever Passed RD Law) |
|---|---|
| Log Pop (1996) | 2.83 |
| | [2.695] |
| Log Inc. Per Capita (1996) | 5.57 |
| | [3.976] |
| % Urban (1990) | 0.06 |
| | [0.094] |
| % Minority (1990) | -0.19 |
| | [0.148] |
| State Dem. Vote Share (1996) | 15.55 |
| | [16.571] |
| SD State Dem. Vote Share (1990-1996) | 40.78 |
| | [32.519] |
| Incumbent Vote Share (1990-1996) | -10.44 |
| | [18.819] |
| Corp Contrib Ban (1990-2000) | -5.34 |
| | [3.715] |
| Corruption Rate (1996) | 0.34 |
| | [2.260] |
| NP Circulation (1996) | -4.37 |
| | [5.422] |
| Term Limits (1990-2000) | -0.22 |
| | [0.843] |
| Salary (Scaled 1998 $) (1996) | -8.68 |
| | [10.162] |
| Full Time Indicator (1990-2000) | -2.41 |
| | [2.135] |
| Obs | 27 |

Standard errors in brackets

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

*Notes:* The tables above show probit regression of 1990s state-level covariates on an indicator for Revolving Door. The dependent variable is a binary indicator for whether a given state ever passed a Revolving Door law, 0 otherwise. State-level covariates come from the sources described in Section 3. When available, 1996 values are included in the regression. For census data (urban and minority population share), we use values from 1990 decennial census. For corruption rate, we include average state-level corruption from 1976-2002. We include an indicator for the presence of corporate contribution bans at any point in the 1990s, as well as an indicator for the presence of term limits in 1996. Lastly, we include average newspaper circulation in states as of 1996. Robust standard errors are in parenthesis. * $p < .1$, ** $p < .05$, *** $p < .01$

TABLE A10: Predictors of Laws' Adoption, Over Time

| | No FE | Year FE | State FE | Year + State FE |
|---|---|---|---|---|
| Log (Pop) | 0.07** | 0.07** | -0.14 | 0.05 |
| | [0.031] | [0.032] | [0.402] | [0.434] |
| Log(Inc Per Capita) | 0.30** | 0.15 | 0.08 | -0.28 |
| | [0.127] | [0.203] | [0.174] | [0.401] |
| % Urban | -0.00 | -0.00 | 0.03** | 0.02* |
| | [0.001] | [0.002] | [0.014] | [0.013] |
| % Minority | 0.00 | -0.00 | 0.01 | -0.01 |
| | [0.002] | [0.002] | [0.013] | [0.018] |
| State Dem. Vote Share (%) | -0.42 | -0.39 | -0.26 | -0.39 |
| | [0.281] | [0.343] | [0.401] | [0.533] |
| SD State Dem. Vote Share (%) | 0.25 | 0.39 | 0.22 | 0.41 |
| | [0.336] | [0.372] | [0.387] | [0.453] |
| Incumbent Vote Share (%) | 0.41* | 0.38 | 0.03 | 0.18 |
| | [0.228] | [0.270] | [0.349] | [0.408] |
| Per-Capita Corruption Rate | 0.04 | 0.04 | 0.00 | 0.01 |
| | [0.057] | [0.063] | [0.045] | [0.046] |
| Term Limits Indicator | -0.08** | -0.07 | -0.06* | -0.06 |
| | [0.038] | [0.043] | [0.035] | [0.039] |
| Salary (Scaled 1998 $) | -0.09 | -0.07 | -0.07 | -0.07 |
| | [0.055] | [0.053] | [0.065] | [0.062] |
| High Salary Indicator | 0.02 | 0.02 | 0.64 | 0.32 |
| | [0.036] | [0.034] | [0.653] | [0.691] |
| Full Time Indicator | -0.07 | -0.06 | 0.26 | -0.13 |
| | [0.088] | [0.093] | [0.935] | [0.998] |
| Obs | 425.00 | 425.00 | 425.00 | 425.00 |
| Adjusted $R^2$ | 0.16 | 0.19 | 0.36 | 0.36 |

Standard errors in brackets

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

*Notes:* The table above shows the linear regression of state year-level covariates on treatment status. The dependent variable is a binary indicator equal to 1 if a Revolving Door law is in place for a given state-year, 0 otherwise. Column (1) includes no fixed-effects, column (2) includes only year fixed-effects, column (3) includes only state fixed-effects, and column (4) includes both state and year fixed-effects. State-year level covariates come from the sources described in Section 3. Standard errors are clustered at the state level. * $p < .1$, ** $p < .05$, *** $p < .01$

TABLE A11: 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 | -0.104** | -0.120** | 0.0863** | 0.0802** | 0.0387* | 0.0438** |
| | [0.0378] | [0.0505] | [0.0324] | [0.0341] | [0.0189] | [0.0188] |
| Change in Salary | -0.0459 | -0.0463 | 0.0311 | 0.0436 | 0.00602 | 0.0143 |
| (Scaled 1998 Dollars) | [0.0384] | [0.0470] | [0.0363] | [0.0297] | [0.0214] | [0.0201] |
| DepVarMean | 0.790 | 0.872 | 0.335 | 0.297 | 0.786 | 0.743 |
| Observations | 68573 | 68573 | 68573 | 68573 | 68573 | 68573 |
| R-sq | 0.0856 | 0.0933 | 0.158 | 0.134 | 0.0268 | 0.0286 |

*Notes:* The table above shows the results of our main specification in Section 4, controlling for changes in real legislator wages (scaled to 1998 dollars) as described in Section 3. Regressions are run at the district-election level, and outcome variables are constructed from the Klaner 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 for a given district. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level. * $p < .1$, ** $p < .05$, *** $p < .01$

TABLE A12: Revolving Doors and Political Selection, Controlling for Corporate Contribution Bans

| | (1) Number of Exiters | (2) Number of New Entrants | (3) Single-Candidate Election | (4) Incumbent Runs Unopposed | (5) Incumbent Runs | (6) Incumbent Winner |
|---|---|---|---|---|---|---|
| Treated | -0.130*** | -0.139** | 0.0985*** | 0.0906** | 0.0341* | 0.0430** |
| | [0.0361] | [0.0519] | [0.0324] | [0.0351] | [0.0194] | [0.0194] |
| Contrib. Ban Active | -0.0124 | -0.00337 | -0.0502 | -0.0398 | 0.0277 | 0.00865 |
| | [0.0819] | [0.0971] | [0.0405] | [0.0311] | [0.0197] | [0.0235] |
| DepVarMean | 0.807 | 0.900 | 0.325 | 0.287 | 0.779 | 0.736 |
| Observations | 78239 | 78239 | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0824 | 0.0912 | 0.159 | 0.136 | 0.0272 | 0.0291 |

*Notes:* The table above shows the results of our main specification in Section 4, controlling for the presence of corporate finance contribution bans as described in Section 3. Regressions are run at the district-election level, and outcome variables are constructed from the Klaner 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 for a given district. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level. * $p < .1$, ** $p < .05$, *** $p < .01$

TABLE A13: 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 | -0.0787** | -0.0754 | 0.0695** | 0.0660** | 0.0115 | 0.0161 |
| | [0.0362] | [0.0490] | [0.0311] | [0.0319] | [0.0170] | [0.0166] |
| Term Limits in Place | 0.199*** | 0.241*** | -0.0917** | -0.0786** | -0.0958*** | -0.105*** |
| | [0.0421] | [0.0508] | [0.0335] | [0.0358] | [0.0239] | [0.0238] |
| DepVarMean | 0.807 | 0.900 | 0.325 | 0.287 | 0.779 | 0.736 |
| Observations | 78239 | 78239 | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0856 | 0.0952 | 0.161 | 0.137 | 0.0298 | 0.0320 |

*Notes:* The table above shows the results of our main specification in Section 4, controlling for the presence of state legislative term limits as described in Section 3. Regressions are run at the district-election level, and outcome variables are constructed from the Klaner 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 for a given district. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level. * $p < .1$, ** $p < .05$, *** $p < .01$

TABLE A14: Revolving Doors and Political Selection, Controlling for Salary and Corporate Contribution Bans

| | (1) Number of Exiters | (2) Number of New Entrants | (3) Single-Candidate Election | (4) Incumbent Runs Unopposed | (5) Incumbent Runs | (6) Incumbent Winner |
|---|---|---|---|---|---|---|
| Treated | -0.104*** | -0.121** | 0.0891*** | 0.0827** | 0.0374* | 0.0432** |
| | [0.0363] | [0.0498] | [0.0317] | [0.0340] | [0.0184] | [0.0186] |
| Change in Salary | -0.0478 | -0.0476 | 0.0368 | 0.0473* | 0.00344 | 0.0133 |
| (Scaled 1998 Dollars) | [0.0393] | [0.0463] | [0.0306] | [0.0234] | [0.0205] | [0.0197] |
| Contrib. Ban Active | 0.00892 | 0.0165 | -0.0451 | -0.0392 | 0.0224 | 0.0109 |
| | [0.0993] | [0.108] | [0.0507] | [0.0394] | [0.0160] | [0.0165] |
| DepVarMean | 0.790 | 0.872 | 0.335 | 0.297 | 0.786 | 0.743 |
| Observations | 68573 | 68573 | 68573 | 68573 | 68573 | 68573 |
| R-sq | 0.0857 | 0.0933 | 0.158 | 0.134 | 0.0269 | 0.0286 |

*Notes:* The table above shows the results of our main specification in Section 4, controlling now for salary and corporate campaign contribution bans as described in Section 3. Regressions are run at the district-election level, and outcome variables are constructed from the Klaner 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 for a given district. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level. * $p < .1$, ** $p < .05$, *** $p < .01$

TABLE A15: Revolving Doors and Political Selection, All 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 | -0.0568 | -0.0657 | 0.0641** | 0.0615* | 0.0139 | 0.0178 |
| | [0.0355] | [0.0484] | [0.0298] | [0.0309] | [0.0150] | [0.0156] |
| Term Limits in Place | 0.185*** | 0.215*** | -0.0972*** | -0.0824** | -0.0914*** | -0.0985*** |
| | [0.0399] | [0.0483] | [0.0311] | [0.0344] | [0.0227] | [0.0229] |
| Change in Salary | -0.0478 | -0.0476 | 0.0368 | 0.0473* | 0.00343 | 0.0133 |
| (Scaled 1998 Dollars) | [0.0347] | [0.0373] | [0.0311] | [0.0248] | [0.0172] | [0.0148] |
| Contrib. Ban Active | -0.0185 | -0.0156 | -0.0306 | -0.0269 | 0.0360** | 0.0255*** |
| | [0.0779] | [0.0833] | [0.0404] | [0.0321] | [0.0138] | [0.00842] |
| DepVarMean | 0.790 | 0.872 | 0.335 | 0.297 | 0.786 | 0.743 |
| Observations | 68573 | 68573 | 68573 | 68573 | 68573 | 68573 |
| R-sq | 0.0886 | 0.0966 | 0.160 | 0.135 | 0.0294 | 0.0312 |

*Notes:* The table above shows the results of our main specification in Section 4, controlling now for salary, corporate campaign contribution bans, and term limits as described in Section 3. Regressions are run at the district-election level, and outcome variables are constructed from the Klaner 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 for a given district. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level. * $p < .1$, ** $p < .05$, *** $p < .01$

TABLE A16: Revolving Doors and Political Selection, Bootstrapped Standard Errors

| | (1) Number of Exiters | (2) Number of New Entrants | (3) Single-Candidate Election | (4) Incumbent Runs Unopposed | (5) Incumbent Runs | (6) Incumbent Winner |
|---|---|---|---|---|---|---|
| Treated | -0.131** | -0.139** | 0.0937** | 0.0868** | 0.0367* | 0.0438* |
| | [0.0565] | [0.0693] | [0.0405] | [0.0417] | [0.0220] | [0.0227] |
| DepVarMean (Pre-T) | 0.822 | 0.922 | 0.311 | 0.276 | 0.777 | 0.731 |
| Observations | 78239 | 78239 | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0824 | 0.0912 | 0.159 | 0.136 | 0.0271 | 0.0291 |

*Notes:* The table above shows the results of our main specification in Section 4, applying a nonparametric bootstrapping to the estimation of standard errors. Regressions are run at the district-election level, and outcome variables are constructed from the Klaner 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 for a given district. Regressions include state and year x never-treated fixed effects. * $p < .1$, ** $p < .05$, *** $p < .01$

TABLE A17: Revolving Doors and Political Selection, Heterogeneity by Corporate Contribution Bans

| | (1) Number of Exiters | (2) Number of New Entrants | (3) Single-Candidate Election | (4) Incumbent Runs Unopposed | (5) Incumbent Runs | (6) Incumbent Winner |
|---|---|---|---|---|---|---|
| Treated | -0.129*** | -0.136** | 0.0921*** | 0.0711*** | 0.0282 | 0.0426** |
| | [0.0371] | [0.0507] | [0.0232] | [0.0173] | [0.0183] | [0.0206] |
| Treated × | -0.00215 | -0.00662 | 0.0144 | 0.0440 | 0.0133 | 0.000980 |
| Contrib. Ban Active | [0.0821] | [0.0865] | [0.0804] | [0.0726] | [0.0152] | [0.0164] |
| DepVarMean | 0.807 | 0.900 | 0.325 | 0.287 | 0.779 | 0.736 |
| Observations | 78239 | 78239 | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0824 | 0.0912 | 0.159 | 0.136 | 0.0272 | 0.0291 |

*Notes:* The table above shows the results of our main specification in Section 4, inspecting heterogeneity by the presence of state-level corporate contribution bans. Data on contribution bans come from the sources described in Section 3. Regressions are run at the district-election level, and outcome variables are constructed from the Klaner 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 for a given district. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level. * $p < .1$, ** $p < .05$, *** $p < .01$

TABLE A18: 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 | -0.0590 | -0.0663 | 0.0224 | 0.0293 | 0.0236 | 0.0254 |
| | [0.0405] | [0.0532] | [0.0263] | [0.0223] | [0.0216] | [0.0235] |
| Treated × | -0.119* | -0.120* | 0.117** | 0.0947** | 0.0216 | 0.0303* |
| High Salary | [0.0601] | [0.0698] | [0.0456] | [0.0451] | [0.0154] | [0.0172] |
| DepVarMean | 0.807 | 0.900 | 0.325 | 0.287 | 0.779 | 0.736 |
| Observations | 78239 | 78239 | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0830 | 0.0917 | 0.161 | 0.137 | 0.0271 | 0.0292 |

*Notes:* The table above shows the results of our main specification in Section 4, 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. 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 for a given district. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level. * $p < .1$, ** $p < .05$, *** $p < .01$

TABLE A19: 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 | -0.0792* | -0.0823 | 0.0379 | 0.0302 | 0.0322* | 0.0421** |
| | [0.0429] | [0.0487] | [0.0285] | [0.0207] | [0.0186] | [0.0189] |
| Treated × | -0.206*** | -0.225*** | 0.221*** | 0.224*** | 0.0181 | 0.00669 |
| Full-Time | [0.0473] | [0.0572] | [0.0274] | [0.0225] | [0.0149] | [0.0186] |
| DepVarMean | 0.807 | 0.900 | 0.325 | 0.287 | 0.779 | 0.736 |
| Observations | 78239 | 78239 | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0834 | 0.0922 | 0.163 | 0.139 | 0.0271 | 0.0291 |

*Notes:* The table above shows the results of our main specification in Section 4, 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. 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 for a given district. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level. * $p < .1$, ** $p < .05$, *** $p < .01$

TABLE A20: Revolving Doors and Political Selection, Heterogeneity by Presence of Legislative 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 | -0.0973*** | -0.105* | 0.0874** | 0.0826** | 0.00985 | 0.0178 |
| | [0.0343] | [0.0513] | [0.0352] | [0.0369] | [0.0177] | [0.0188] |
| Treated × | 0.0721 | 0.116* | -0.0697 | -0.0645 | 0.00631 | -0.00643 |
| Term Limits in Place | [0.0633] | [0.0650] | [0.0581] | [0.0464] | [0.0236] | [0.0189] |
| DepVarMean | 0.807 | 0.900 | 0.325 | 0.287 | 0.779 | 0.736 |
| Observations | 78239 | 78239 | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0858 | 0.0955 | 0.162 | 0.138 | 0.0299 | 0.0320 |

*Notes:* The table above shows the results of our main specification in Section 4, inspecting heterogeneity by whether the state has legislative term limits in place. Regressions are run at the district-election level, and outcome variables are constructed from the Klaner 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 for a given district. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level. * $p < .1$, ** $p < .05$, *** $p < .01$

TABLE A21: Revolving Doors and Political Selection, Heterogeneity by Presence of Political Scandals

| | (1) Number of Exiters | (2) Number of New Entrants | (3) Single-Candidate Election | (4) Incumbent Runs Unopposed | (5) Incumbent Runs | (6) Incumbent Winner |
|---|---|---|---|---|---|---|
| Treated | -0.139*** | -0.148** | 0.0981** | 0.0914** | 0.0416* | 0.0480** |
| | [0.0402] | [0.0569] | [0.0383] | [0.0392] | [0.0231] | [0.0223] |
| Treated × | 0.0583 | 0.0668 | -0.0333 | -0.0342 | -0.0360 | -0.0313 |
| Political Scandal | [0.0457] | [0.0501] | [0.0606] | [0.0423] | [0.0389] | [0.0372] |
| DepVarMean | 0.807 | 0.900 | 0.325 | 0.287 | 0.779 | 0.736 |
| Observations | 78239 | 78239 | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0824 | 0.0913 | 0.159 | 0.136 | 0.0272 | 0.0292 |

*Notes:* The table above shows the results of our main specification in Section 4, inspecting heterogeneity by whether the Revolving Door law was passed in response to a political scandal. Regressions are run at the district-election level, and outcome variables are constructed from the Klaner 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 for a given district. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level. * $p < .1$, ** $p < .05$, *** $p < .01$

TABLE A22: Revolving Doors and Political Selection, Heterogeneity by Presence of Broader Ethics Reform

| | (1) Number of Exiters | (2) Number of New Entrants | (3) Single-Candidate Election | (4) Incumbent Runs Unopposed | (5) Incumbent Runs | (6) Incumbent Winner |
|---|---|---|---|---|---|---|
| Treated | -0.130** | -0.130* | 0.0876** | 0.0657** | 0.0376* | 0.0522*** |
| | [0.0585] | [0.0644] | [0.0348] | [0.0247] | [0.0197] | [0.0186] |
| Treated × | -0.00132 | -0.0165 | 0.0105 | 0.0364 | -0.00145 | -0.0144 |
| Ethics Reform | [0.0890] | [0.0939] | [0.0769] | [0.0695] | [0.0172] | [0.0208] |
| DepVarMean | 0.807 | 0.900 | 0.325 | 0.287 | 0.779 | 0.736 |
| Observations | 78239 | 78239 | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0824 | 0.0912 | 0.159 | 0.136 | 0.0271 | 0.0291 |

*Notes:* The table above shows the results of our main specification in Section 4, inspecting heterogeneity by whether the Revolving Door law was passed as part of a broader ethics reform. Regressions are run at the district-election level, and outcome variables are constructed from the Klaner 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 for a given district. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level. * $p < .1$, ** $p < .05$, *** $p < .01$

TABLE A23: Revolving Doors and Political Selection, Heterogeneity by Lobbying Intensity

| | (1) Number of Exiters | (2) Number of New Entrants | (3) Single-Candidate Election | (4) Incumbent Runs Unopposed | (5) Incumbent Runs | (6) Incumbent Winner |
|---|---|---|---|---|---|---|
| Treated | -0.0605 | -0.0715 | 0.0218 | 0.0340 | 0.0376 | 0.0423* |
| | [0.0393] | [0.0488] | [0.0273] | [0.0207] | [0.0233] | [0.0221] |
| Treated × | -0.147** | -0.141** | 0.150*** | 0.110* | -0.00177 | 0.00319 |
| High Num. Lobbyists | [0.0537] | [0.0676] | [0.0507] | [0.0576] | [0.0173] | [0.0205] |
| DepVarMean | 0.807 | 0.900 | 0.325 | 0.287 | 0.779 | 0.736 |
| Observations | 78239 | 78239 | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0833 | 0.0920 | 0.162 | 0.137 | 0.0271 | 0.0291 |

*Notes:* The table above shows the results of our main specification in Section 4, inspecting heterogeneity by average number lobbyists within each state. Regressions are run at the district-election level, and outcome variables are constructed from the Klaner 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 for a given district. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level. * $p < .1$, ** $p < .05$, *** $p < .01$

TABLE A24: Revolving Doors and Political Selection, Heterogeneity by Per-Capita Lobbying Intensity

| | (1) Number of Exiters | (2) Number of New Entrants | (3) Single-Candidate Election | (4) Incumbent Runs Unopposed | (5) Incumbent Runs | (6) Incumbent Winner |
|---|---|---|---|---|---|---|
| Treated | -0.148*** | -0.162** | 0.108** | 0.0994** | 0.0412* | 0.0487** |
| | [0.0473] | [0.0632] | [0.0437] | [0.0460] | [0.0223] | [0.0228] |
| Treated × High Num. Lobbyists Per-Capita | 0.0606 | 0.0852 | -0.0525 | -0.0466 | -0.0164 | -0.0179 |
| | [0.0709] | [0.0792] | [0.0656] | [0.0601] | [0.0199] | [0.0254] |
| DepVarMean | 0.807 | 0.900 | 0.325 | 0.287 | 0.779 | 0.736 |
| Observations | 78239 | 78239 | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0825 | 0.0914 | 0.159 | 0.136 | 0.0271 | 0.0291 |

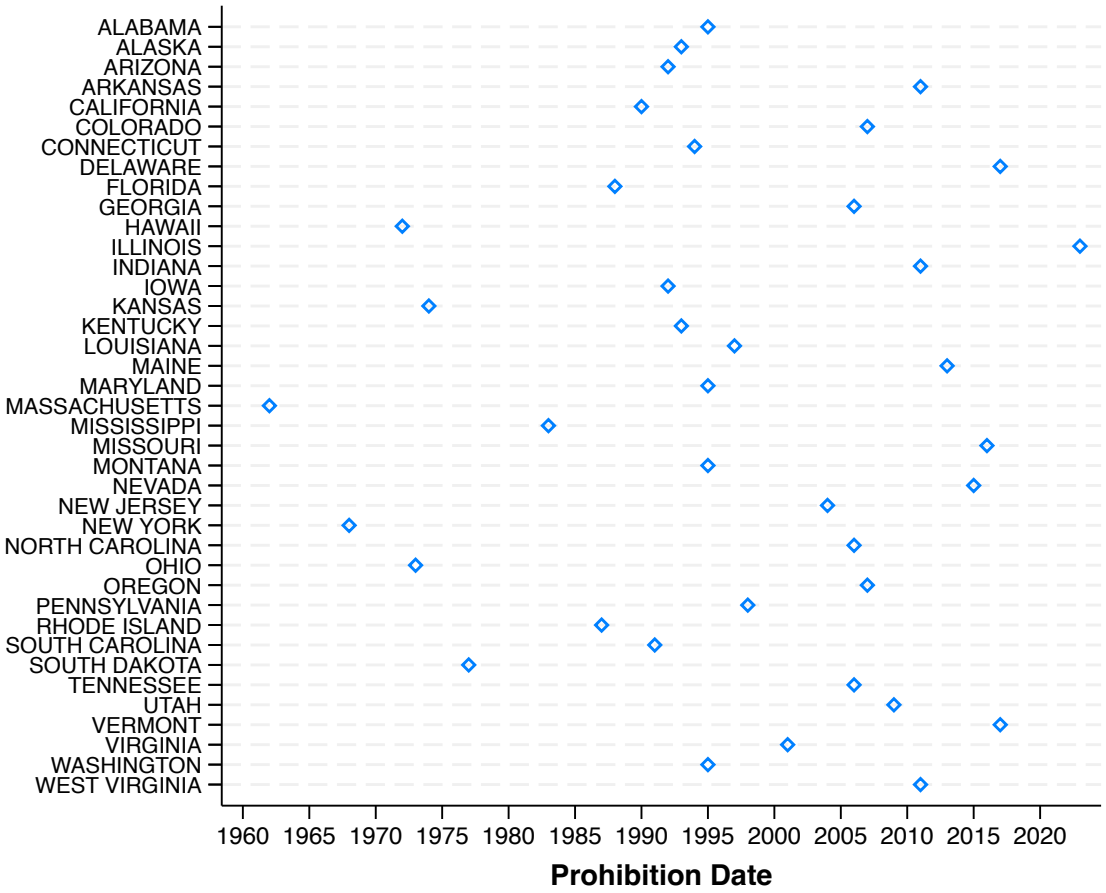
*Notes:* The table above shows the results of our main specification in Section 4, inspecting heterogeneity by average number lobbyists per-capita within each state. Regressions are run at the district-election level, and outcome variables are constructed from the Klaner SLERs Database. Number of Exiters (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 for a given district. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level. * $p < .1$, ** $p < .05$, *** $p < .01$

TABLE A25: Revolving Doors and Political Selection, Heterogeneity by Number of Lobbying Clients

| | (1) Number of Exiters | (2) Number of New Entrants | (3) Single-Candidate Election | (4) Incumbent Runs Unopposed | (5) Incumbent Runs | (6) Incumbent Winner |
|---|---|---|---|---|---|---|
| Treated | -0.0860** | -0.104** | 0.0357 | 0.0450** | 0.0453* | 0.0525** |
| | [0.0344] | [0.0460] | [0.0257] | [0.0193] | [0.0239] | [0.0232] |
| Treated × | -0.0934 | -0.0729 | 0.120** | 0.0864 | -0.0177 | -0.0179 |
| High Num. Clients | [0.0666] | [0.0797] | [0.0568] | [0.0605] | [0.0176] | [0.0210] |
| DepVarMean | 0.807 | 0.900 | 0.325 | 0.287 | 0.779 | 0.736 |
| Observations | 78239 | 78239 | 78239 | 78239 | 78239 | 78239 |
| R-sq | 0.0828 | 0.0914 | 0.161 | 0.137 | 0.0271 | 0.0291 |

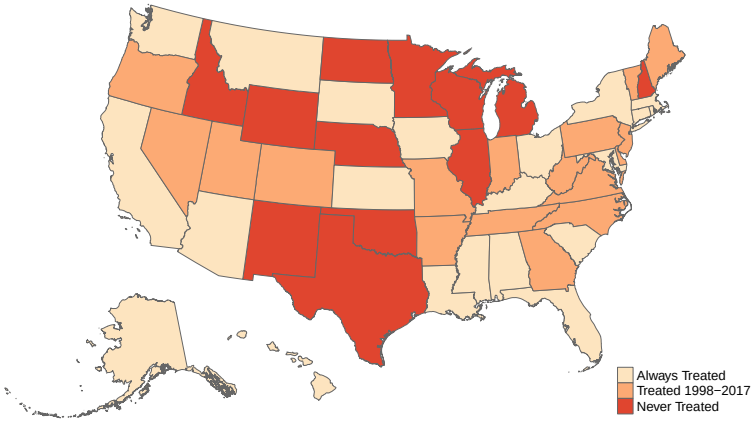
*Notes:* The table above shows the results of our main specification in Section 4, inspecting heterogeneity by average number lobbying clients within each state. Regressions are run at the district-election level, and outcome variables are constructed from the Klaner SLERs Database. Number of Exiters (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 for a given district. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level. * $p < .1$, ** $p < .05$, *** $p < .01$

Figure A1: Revolving Door Prohibition Date by State



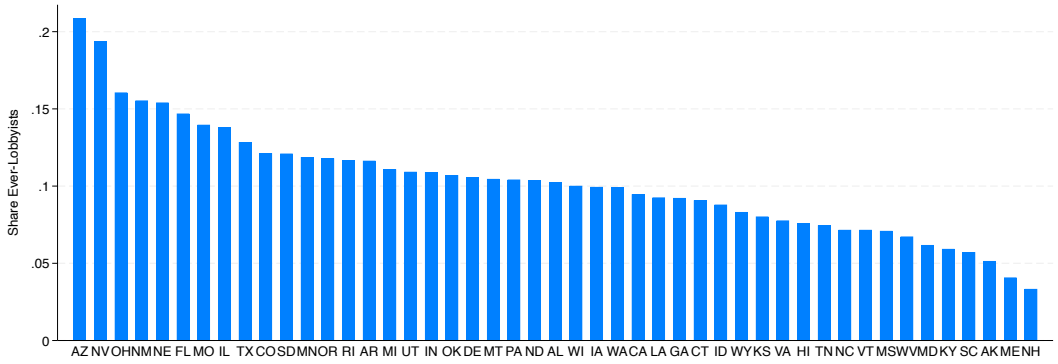

*Notes:* This figure shows the passage date of revolving door prohibition laws by state. Information on revolving door legal codes and legislative histories come from National Conference of State Legislatures (NCSL), WestLaw and HeinOnline databases. *Note*: For the 10 states without prohibitions, (1) 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 A2: Presence and Timing of Revolving Door Laws



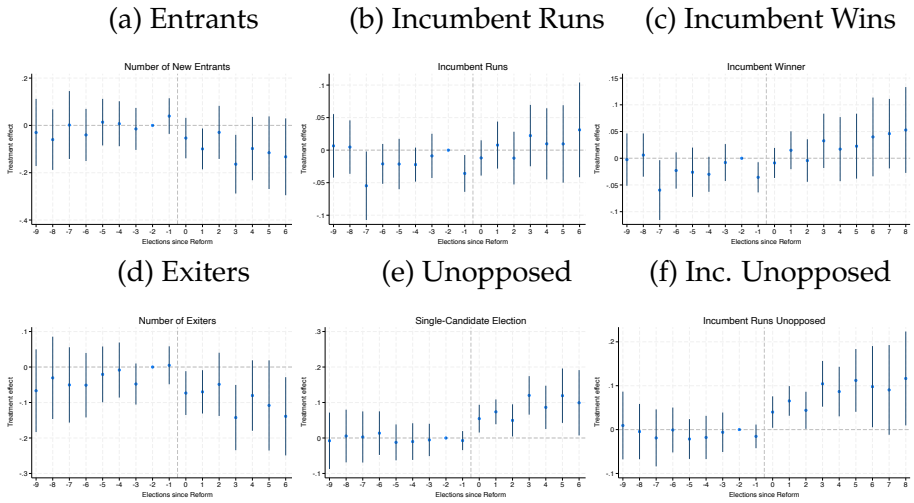

*Notes:* The map above shows the presence and timing passage of Revolving Door Laws by State, as it refers to the treatment of state legislators. Timing of law adoption is determined by the procedure described in Section 3. States shaded in yellow are always-treated states which adopted their first law affecting state legislators pre-1998. States shaded in orange adopted their first law affected state legislators between 1998-2017 (the sample of main analysis). States shaded in red are never-treated 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 and (3) states which adopted laws after the sample period (post-2017).

Figure A3: Lobbying Rates by States among State Legislators



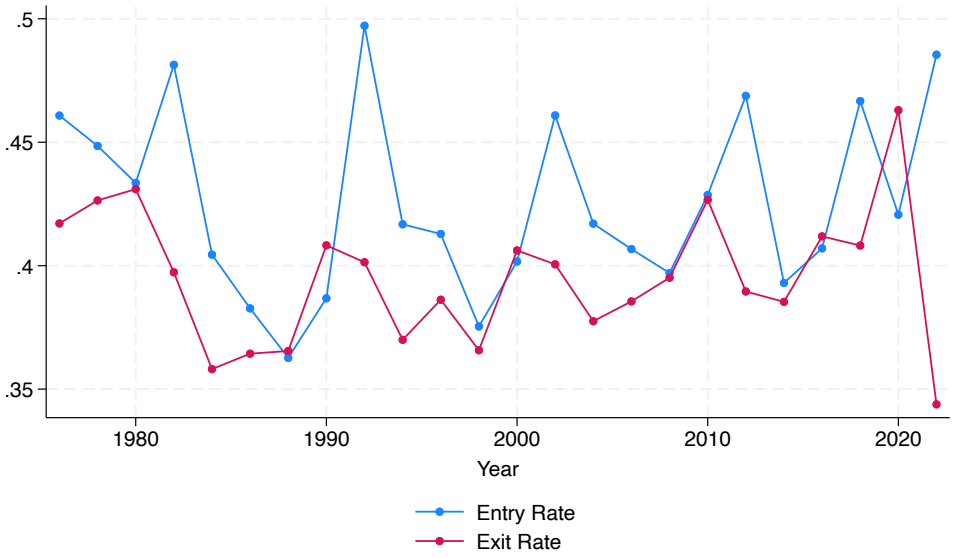

*Notes:* The figure shows the percentage of state legislators identified as former or future lobbyists by state. This only includes legislators who are identified as ever-winning an election to state legislature, and excludes non-elected candidates. Lobbying status is determined by the fuzzy matching process described in Section 3.

Figure A4: Revolving Door Laws and Political Selection, Event Studies: Post-1975 Sample



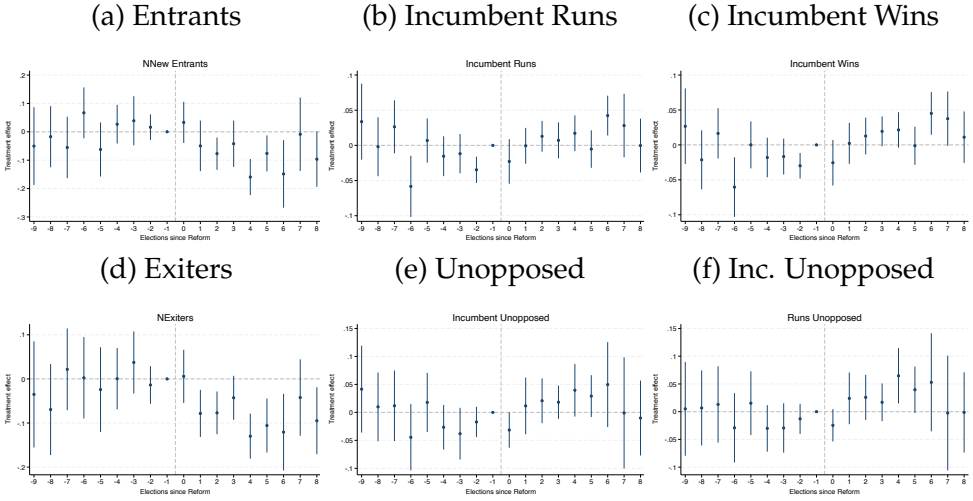

*Notes:* The figure shows the effect of revolving door laws on political selection, extending our sample to include all observations post-1975. Panel (a) shows the dynamic treatment effects on the total number of candidates that run for the first time. Panels (b) and (c) show the effects on the probability of candidates running for and winning re-election, respectively. Panel (d) show the effects on the total number of legislators that leave politics. Panel (e) shows the effects on probability of an uncontested election, and panel (f) shows the effects on probability of the incumbent running unopposed. Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level.

Figure A5: Average District-Level Entry and Exit Rates: 1976-2022



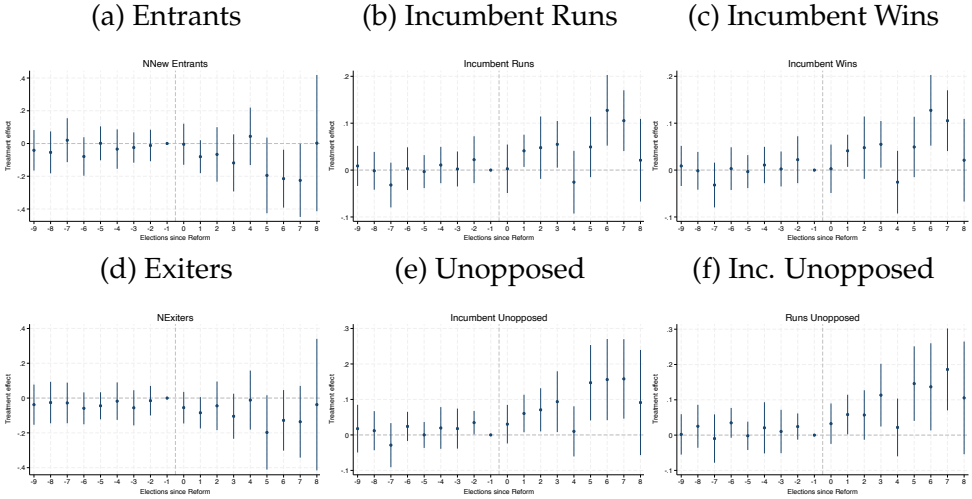

*Notes:* The figure shows the aggregate rates of entry and exit among candidates for state legislature from 1976-2022. Exit and entry rates are defined at the district-election level from the Klarner SLERS database as the number of new exiters/entrants divided by the total number of candidates in a given district. Rates are then averaged across all districts in two-year election periods (to account for variation in the timing of elections across states).

Figure A6: Revolving Door Laws and Political Selection, Event Studies: Sun & Abraham (2021)




*Notes:* The figure shows the effect of revolving door laws on political selection, using the interaction weighted estimator as in Sun and Abraham (2021a). Panel (a) shows the treatment effect on the total number of candidates that run for the first time. Panels (b) and (c) shows the probability of candidates running for and winning re-election, respectively. Panel (d) shows the total number of legislators that leave politics. Panels (e) shows the probability of an uncontested election, and panel (f) shows the probability of the incumbent running unopposed. Regressions include state and year fixed effects. Standard errors are clustered at the state-level.

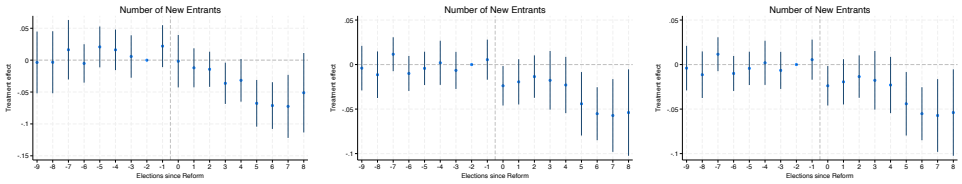
Figure A7: Revolving Door Laws and Political Selection, Event Studies: Stacked Design




*Notes:* The figure shows the effect of revolving door laws on political selection, using the alternative controls approach described in Section 5. Panel (a) shows the treatment effect on the total number of candidates that run for the first time. Panels (b) and (c) shows the probability of candidates running for and winning re-election, respectively. Panel (d) shows the total number of legislators that leave politics. Panels (e) shows the probability of an uncontested election, and panel (f) shows the probability of the incumbent running unopposed. Regressions 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

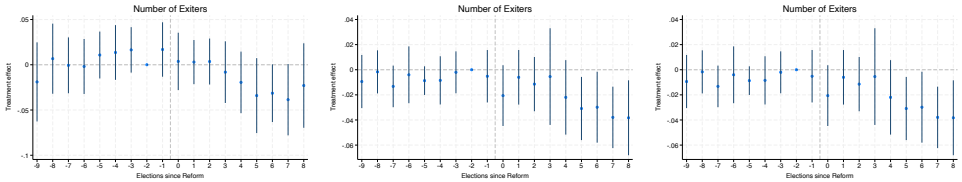
(a) Adv. Degree (b) High Quality Edu (c) Future Federal



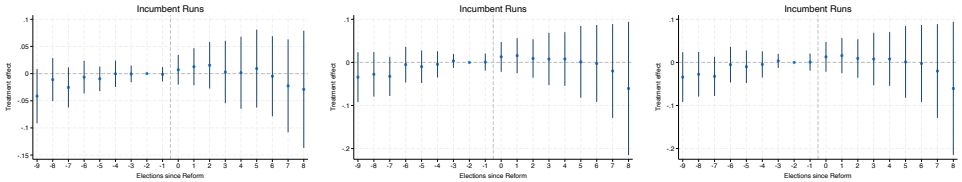

*Notes:* The figures show the effect of revolving door laws on the total number of candidates that run for the first time, broken down by measures of quality. Advanced degree information and undergraduate quality are generated through the online biographical candidate data collection described in Section 3. Data on former state legislators running for federal elections is constructed from Phillips et al. (2024). Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level.

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

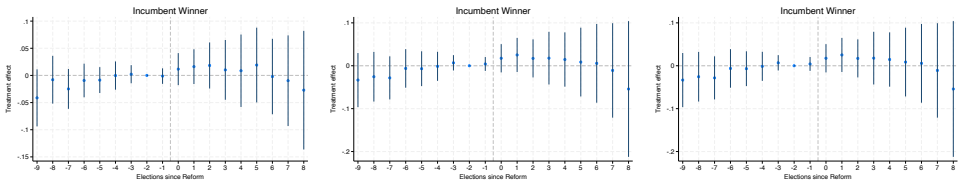
(a) Adv. Degree (b) High Quality Edu (c) Future Federal




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

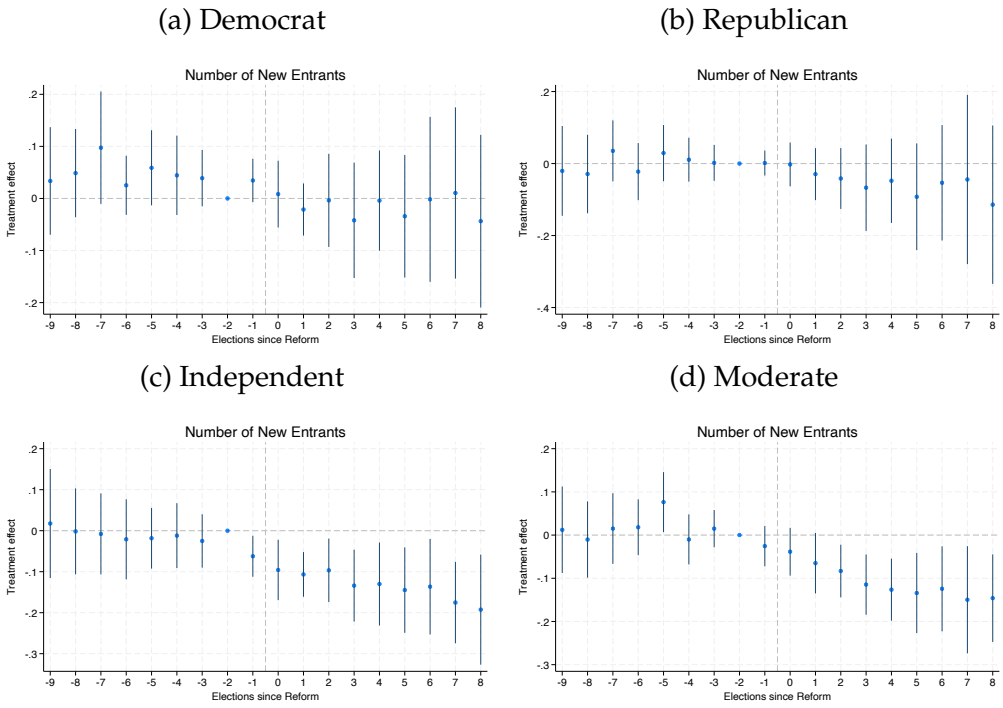



(g) Adv. Degree (h) High Quality Edu (i) Future Federal



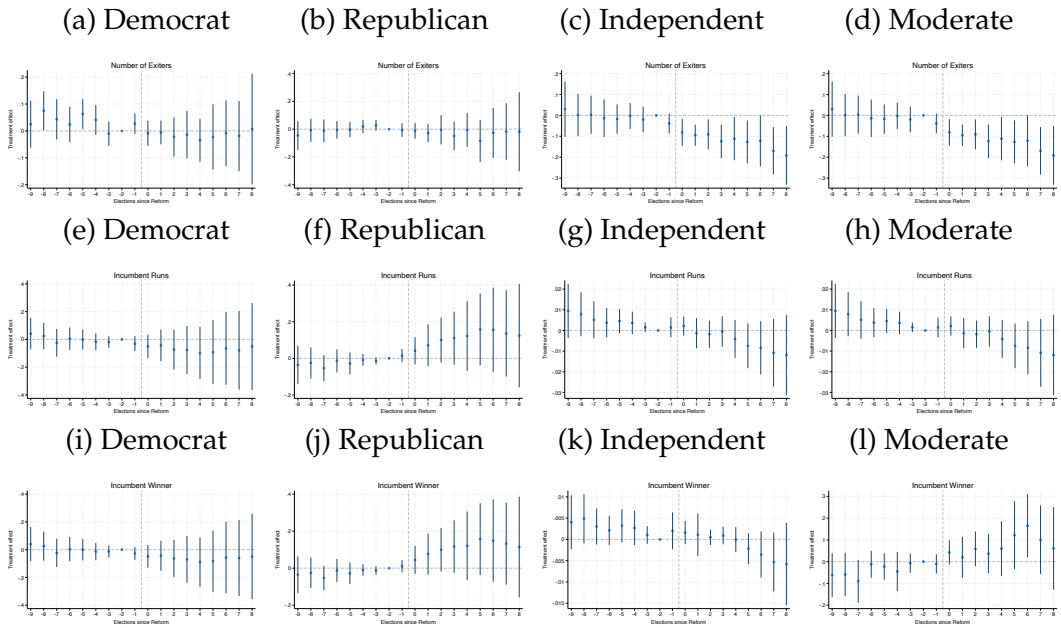

*Notes:* The figures above show the effect of revolving door laws on the exit and incumbent re-election, broken down by measures of quality. The first row shows the effects on overall exit, the second row shows the effects on the probability of incumbent re-running for election, and the third row shows the effects on incumbent re-election. Advanced degree information and undergraduate quality are generated through the online biographical candidate data collection described in 3. Data on former state legislators running for federal elections is constructed from Phillips et al. (2024). Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level.

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




*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). Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level.

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




*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). Regressions include state and year x never-treated fixed effects. Standard errors are clustered at the state-level.