

“Closing” the Revolving Door:

Assessing the effects of state-level restrictions on post-governmental employment on
legislator ideology and the insurance industry

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Abstract

This paper explores the effects of restrictions on the post governmental employment (PGE) of state legislators. Using a novel dataset I developed of state-level restrictions on legislator PGE, I employ a two-way fixed effects model to exploit differences in the timing of state PGE restrictions to test Hall's (2019) assertion that revolving door lobbyist positions are not a significant benefit of elected office. Further, following Fournaies and Fowler (2022), I consider the state-level regulatory climate towards property and casualty insurance as a measure of corporate influence on state government. Like past research of PGE restrictions on public utility commissioners (Law and Long 2012), I find mixed results – attempts to “close” the revolving door indeed increase legislator polarization, on average, contrary to Hall's hypothesis. However, while PGE restrictions might lower insurance premiums, this finding is complicated by the model's assumptions, and the weight of the evidence suggests PGE restrictions have null effect on insurance regulation. Overall, state lawmakers should consider policies to increase the benefits of holding elected office to ameliorate unintended consequences (i.e., heightened polarization) before attempting to “close” the revolving door.¹

Keywords: Political Selection, Revolving Door Laws, Lobbyists, State Legislators, Polarization

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Introduction

The First Amendment of the United States Constitution guarantees, in part, the right of the people to “petition the government for a redress of grievances.” The Association of Government Relations Professionals (a professional organization representing lobbyists) cites this clause of the First Amendment as the foundation for lobbyists’ advocacy activities. Yet despite this apparently auspicious Constitutional protection, lobbyists are often maligned as the “villains” within the “tale” of policymaking; lobbyists are portrayed as “dark influences” in the policymaking arena (Bitoni et al., 2025), and perhaps no lobbyists are as mistrusted as so-called revolving door lobbyists. By “revolving door lobbyists,” I refer to those individuals “who transition from governmental positions into lobbying for private entities” (Strickland 2020, 1). Simply put, revolving door lobbyists are former public officials who depart their positions of public trust and subsequently advocate for private clients.

The extent to which revolving door lobbying should be restricted by law – and to what extent those restrictions should apply to state legislators themselves – remain active political questions across the country. In February 2025, a Michigan lawmaker declared that his bill to strengthen the state’s revolving door laws was “an opportunity to show Michigan’s voters that the Legislature is not simply a stepping stone for career advancement” (Lobo 2025). In April 2025, Montana’s lower chamber voted to reject legislation that would “streamline” revolving door restrictions in the state and impose tougher regulations on former legislators (Larson). Whether state legislators restrict their own ability to become revolving door lobbyists thus remains unsettled across state legislatures.

In this paper, I principally examine the effect of revolving door laws on legislator ideology and political polarization. Specifically, I explore whether the adoption of state level

restrictions on the post-governmental employment (PGE) of state legislators causes legislatures (and individual legislators) to become more ideologically extreme. As increased ideological polarization paralyzes American legislatures and perturbs voters, identifying possible causes for this phenomenon has spawned a considerable corpus of political science literature (McCarty 2019).

Previous research suggests that prospective legislators conduct a cost-benefit analysis when deciding whether to run for public office – if the benefits of elected office (chiefly moving policy to your ideal point, but also a salary, prestige, etc.) outweigh the many costs (media attention, a lower salary, etc.), then prospective candidates will run for office (Hall 2019). Because their preferred policies tend to be further from the status quo, extremists often derive a greater benefit to holding office than moderates. Should the other benefits to holding office – like a legislator’s salary – are further reduced, then even fewer moderates will run, and we should expect partisan gridlock to worsen as more extremists are consequently elected (ibid.).

Besides shifting state policies or even their direct salary, the allure of the revolving door stands as another potential benefit of holding an elected office. As before, the revolving door refers to former government officials (in this case, former legislators) who subsequently become lobbyists (Strickland 2020).² Among all lobbyists, these revolving door lobbyists command the highest pay -- Bertrand, Bombardini, and Trebbi (2012) find that lobbyists with the most connections to their former colleagues are indeed paid the best, suggesting that revolving door lobbying may be an attractive career option for former legislators. Indeed, some candidates may

² Post-governmental employment (PGE) technically can have a broader definition – that is, PGE refers to *whatever* career a former government official pursues. Here, I refer to PGE restrictions and revolving door laws to both refer to restrictions of legislator’s ability to register as lobbyists (see data).

even be induced to run for office because of the allure of a future, lucrative lobbying position once they leave office and pass through the revolving door.

To “close” this revolving door, some states have enacted restrictions on post governmental employment (PGE) that proscribe former legislators from holding certain positions (generally as lobbyists) or representing certain clients. These are the sorts of laws recently endorsed by the state legislature in Michigan and defeated in Montana. To directly assess the causal effect of these PGE restrictions, I closely reviewed the legal corpora of all 50 states to create a novel dataset of the state-level restrictions on post governmental employment activities of former state legislators. Leveraging this new dataset, I explore the consequences of these state-level restrictions on the ability of former state legislators to register as lobbyists after leaving elected office. Using Shor-McCarty (2011) scores to assess individual legislators’ ideologies, I employ a two-way fixed effects model to directly estimate the average causal effect of these restrictions. If indeed the revolving door is an attractive benefit of elected office, then we expect that, once states act to “close” the revolving door, state legislators will become more extreme as fewer moderates are inclined to run for office.

Additionally, following Fourinaies and Fowler’s approach to campaign finance restrictions (2022), I consider whether PGE restrictions might actually serve their stated intention of reducing the sway of corporations over state government. If PGE restrictions do reduce the legislative clout of corporations, then we would expect regulatory measures from industries heavily regulated at the state level (like property and casualty insurance) to be less favorable to corporate stakeholders after a state restricts PGE. Essentially, this paper explores two potential outcomes of legislator PGE restrictions: do these so-called revolving door laws have unintended consequences on who wants to run for office (as measured by legislator ideology), and do they

actually serve their stated purpose of reducing corporate capture of state government (as measured by the regulatory climate towards property and casualty insurance). Little previous research has explored the direct effects of revolving door laws (Law and Long 2012), and no published studies specifically examine their effects on state legislators.

My findings suggest that revolving door laws indeed increase polarization, with perhaps a more robust effect among state senators than state representatives. Thus, contrary to Hall's ex ante assumption, the revolving door is indeed an attractive benefit of office and proscribing revolving door lobbying causes more extreme representatives to enter state legislatures. Additionally, some evidence indicates that adopting PGE restrictions may cause property and casualty insurance premiums to decrease, although this finding is less statistically robust. Further, taken in the context of several other null findings for the insurance industry, it appears that revolving door laws are most likely not serving their stated purpose of reducing corporate influence in government, at least insofar as that phenomenon can be measured in the insurance industry.

Ultimately, I conclude that states should be cautious in adopting PGE restrictions for state legislators – while these laws may indeed reduce the sway that corporations hold over lawmakers, they likewise make public office less attractive to more moderate candidates and worsen polarization. If states do adopt PGE restrictions, they should be paired with measures (like higher legislator salaries) to offset the deleterious side effects of “closing” the revolving door on state legislators.

Background

A great literature seeks to explain the increasing polarization of American legislators (see McCarty 2019, for a detailed review). Potential complementary (or competing) explanations for this phenomenon include external factors like gerrymandering, campaign finance restrictions, and whether moderates run for office (Hall 2019), a topic to which I return. Still other scholars attribute polarization to internal factors, like the decision-making of party members in Congress and the efforts of party leaders to whip their members by both setting the agenda and employing carrots and sticks to influence vote choice among legislators. Some scholars go so far as to dub the contemporary business of American legislators not as “Regular Order” but instead “Regular Disorder” (McCarty 2014, 1).

Hall attributes increased polarization to changes among the candidates who seek office. Hall adopts a rather parsimonious model: individuals only run for political office when the benefits of holding that office (e.g., compensation, or the opportunity to move policy towards their ideal points) exceed the cost of running for and winning office (e.g., negative publicity, a pay cut, etc.). Hall ultimately concludes that devaluing office discourages moderates from running because these moderates receive less benefit from shifting the status quo towards their ideal points compared to more ideologically extreme candidates. Yet he likewise finds that voters prefer more moderate candidates and that extremists face penalties in general elections; so, an increasingly extreme pool of candidates running for office ultimately leaves voters to suffer. Hall’s story of electoral selection suggests that the proper incentives are required to induce more high-quality (and likely more moderate) candidates to run for office.

Whatever the causes of increased polarization, this phenomenon is not merely limited to the U.S. House of Representatives and Senate, the settings where it is most frequently studied.

“Polarization of the whole set of candidates seeking state legislative office has risen dramatically over the past two decades” (Handan-Nader et al. 2024, 3), largely mirroring trends among national legislators. In fact, recent evidence suggests that increasingly polarized state legislators are among the most significant explanations for heightened polarization among members of Congress (Philips et al., forthcoming). That is, since many federal legislators began their political careers at the state level, as state legislators become more extreme ultimately federal legislators will become more ideologically extreme, too.

Further, given that “variations across the American states allow researchers to better understand” a host of political phenomena, including the effects of “institutional arrangements” (Lewis et al. 2015, 448), examining the impacts of restrictions on legislator post governmental employment at the state level will help elucidate the impacts of PGE restrictions at the federal level. That is, while it is difficult to credibly estimate the causal impacts of the uniform, federal restrictions on post-governmental employment, policy variation at the state level enables us to explore how institutional arrangements can affect trends in polarization at the state level, an exercise that could well inform scholarly efforts to understand national polarization, too.

Two often competing literatures seek to understand the value that lobbyists – and particularly revolving door lobbyists – provide to policymakers. By “revolving door lobbyists” I again refer to those “individuals who transition from governmental positions into lobbying for private entities” (Strickland 2020, 1). Revolving door lobbyists themselves often assert (Blanes i Vidal et al. 2012) that their value comes merely from their higher innate ability (Burger 2006) and expertise gained from their previous government experience. The second view towards revolving door lobbyists – more often held by the popular press and watchdog groups – asserts that “revolving door lobbyists are valuable because ‘Washington is all about connections’”

(Blanes i Vidal et al. 2012, 3731) and that these lobbyists are valuable not for their expertise per se but for their connections with and potential access to their former legislative colleagues (Strickland, 2019).

Some empirical research tends to support this more connections-based understanding of revolving door lobbyists. Blanes i Vidal et al. (2012) find that “lobbyists connected to US Senators suffer” a significant drop in their revenues once their connected Senator exits Congress, a drop which “persists in the long run” (3732). They likewise finding evidence that these connected ex-staffers are “less likely to work in the lobbying industry after their connected senators” depart office (ibid.), further strengthening the case that lobbying is, at least to a significant degree, valued not for the expertise that lobbyists bring to their clients but for the lobbyists’ connections with and access to incumbent legislators.

Likewise, while Bertrand, Bombardini, and Trebbi (2012) find evidence that there are some nonpartisan experts respected irrespective of party or previous experience, the weight of the empirical evidence suggests lobbyists with greater personal connections to policymakers command “the highest price” from clients (p. 391). Indeed, the balance of empirical evidence suggests that lobbyists are paid handsomely for their connections, rather than any unique expertise.

These supposedly well-connected, revolving door lobbyists frequently evoke consternation among both the popular press and public officials, with one American politician dubbing revolving door lobbying “obviously a problem, and it’s a big problem, and we were doing things about it” (Oprysko, 2024). When Florida lawmakers adopted the longest ban on revolving door lobbying in the United States (six years) in 2018, the lawmaker sponsoring the

bill called in necessary to counter “the perception, if not the reality of the revolving door” (Rosica, 2017).

Yet despite their apparent popularity in the popular press, few previous academic studies have directly examined the *effects* of these restrictions. Law and Long (2012) present perhaps the most robust exploration of these laws and their potential impacts on public utility commissioners and electricity prices. Utilizing a differences-and-differences design to exploit the staggered adoption of PGE restrictions across states, Law and Long find modest evidence that revolving door restrictions make utility regulators less beholden to large industrial interests, as measured by energy prices. Yet they likewise find these restrictions increase turnover among public utility commissioners, suggesting potential unintended consequences of PGE restrictions.

Similarly, few previous papers have sought to estimate the causal benefits that legislative offices might bestow onto incumbent lawmakers. Employing a regression discontinuity design to examine census data, Querubin and Snyder (2013) examined the lifetime wealth of members of Congress serving from 1850 to 1880. They found evidence of significant financial returns for lawmakers serving in the early 1860s but not for the rest of the period, a finding they attribute to opportunities for profiteering and inflated government spending during the American Civil War. Their finding thus suggests that legislative officeholders may indeed receive ‘fringe benefits’ from holding office, but only under certain circumstances.

Eggers and Hainmueller (2019) examine more generally whether holding elected legislative positions generate subsequent financial returns to officeholders. Using a regression discontinuity design to compare those British candidates who just barely win their parliamentary elections to those who narrowly come up short, they find that victorious members of parliament (MPs) indeed accrue significant additional wealth by the end of their lives, but only for those

from the Conservative Party. The authors interpret this result to mean that Labour MPs are more limited in their post-Parliamentary opportunities to work well-paying corporate benefactors, an obstruction absent for Conservatives. Ex ante, we might then imagine that, in the American context, legislators from the Democratic party derive less (or even no) financial benefit from holding office than their Republican counterparts.

Finally, Palmer and Schneer (2016) present the most comprehensive survey of the financial earnings of former legislators, creating a novel dataset of 1,200 public officials, principally former members of Congress and high-ranking executive officials, like cabinet secretaries and ambassadors. They find that former members of the House of Representatives are more likely to subsequently register as lobbyists, while former Senators and executive officials tend to serve on corporate boards; the latter, corporate positions tend to pay far better than the former. They attribute this in part to the fact that former members of the House of Representatives are less sought after than former Senators and consequently command lower wages. How this finding applies outside the federal context and to state legislators is not immediately apparent ex ante. Nevertheless, Palmer and Schneer's work makes clear that not all elected positions are 'created equal,' and that firms and lobbyists may value some incumbents more than others depending on the office they have held.

For his part, Hall (2019) is skeptical that the revolving door is a significant benefit to holding office, claiming that "it is not at all clear that [the revolving door] conveys nearly enough value to induce people to run for office, particularly those already in high-paying occupations" (76). But Hall provides scant empirical evidence to support this assertion. Indeed, the three most common occupations of state legislators are attorneys, business owners, and educators (Makse 2019). Federal data suggests the median attorney earns \$145.7 thousand, while

National Education Association data suggests that the average, experienced teacher earns about \$70 thousand annually. While comparable data for “business owners” is lacking, Eggers and Hainmueller British politicians with a greater background in business received the largest financial benefit from winning their parliamentary election (2019). In contrast, previous work finds the median salary of a revolving door lobbyist of \$376.16 thousand (Ban et al., 2019), far above the earnings of these other professions. While Ban et al.’s figure is for former federal revolving door lobbyists, if state-level revolving door lobbyists tended to earn just half the amount of their federal counterparts, their salaries would still comfortably exceed the median attorney’s. Even this quick review of wages cautions against adopting Hall’s untested assumption that lobbying is an unattractive career for former state legislators.

I empirically test Hall’s assumption that the lure of the revolving door is not an attractive career for would-be state legislators. If, as Hall claims, diminishing the benefits of holding elected offices induces fewer moderates to run for office, then we would expect more political polarization among legislators in states after they adopt restrictions on post governmental employment. This finding would suggest both that registering as a revolving door lobbyist is an attractive career for former state legislators and that restricting former legislator’s ability to register as lobbyists might inadvertently diminish the attractiveness of elected office. While prior research (e.g., Law and Long, 2012) has considered the effect of revolving door laws on the political selection of executive officeholders (public utility commissioners), and other researchers (e.g., Eggers and Hainmueller, Palmer and Schmeer) have examined whether legislators derive some financial benefit from elected office, this paper is the first to specifically examine whether the revolving door is an attractive benefit of office for state legislators.

Rent-Seeking and Insurance Lobbyists

Beyond only the behavior of the state legislators, I expand my analysis to consider potential impacts on revolving door legislation on the policies that states adopt. Following Fournaies and Fowler (2022), I consider the property and casualty insurance industry as a setting which is likely especially affected by such legislation. Property and casualty insurance is regulated almost exclusively at the state level (Brining 1992); indeed, the federal government definitively ceded oversight of insurance to the states 80 years ago with the passage of the McCarran-Ferguson Act (Weller 1978). This arrangement has culminated in what McCray dubbed “virtually exclusive regulatory control” by state governments over property and casualty insurance, with the federal government playing almost no role in regulating insurance (1993, p.33). This historical regulatory landscape led Brining to conclude that “the insurance industry has for more than a hundred years enjoyed a system of friendly regulation by state authorities,” which allows legislators to extract rents from insurance companies (in the form of campaign contributions, etc.). We might also imagine that legislators are able to extract revolving door lobbying positions from insurance companies as another benefit of their regulatory oversight. This arrangement consequently causes higher insurance prices (and more onerous coverage requirements) for consumers. That is, because state legislators rely on insurance companies for campaign contributions and, possibly, future employment, the legislators provide a more favorably regulatory climate for the property and casualty insurance companies.

Whatever the relationship between insurance companies and state lawmakers, it remains that property and casualty insurance is one of the few policy areas where states retain “virtually exclusive” regulatory oversight. Likewise, some authors (e.g., Brining 1992) suggest that insurance companies enjoy a very friendly regulatory landscape at the state level. So, we might expect any policy changes designed to limit the power of corporations over lawmakers (for

instance, the adoption of revolving door laws) would consequently lead to a less favorable regulatory environment for property and casualty insurance. That is, if lawmakers are less enticed to support insurance company priorities (because insurance companies can no longer offer revolving door jobs as a ‘carrot’ to lawmakers), then insurance companies may suffer when states adopt PGE restrictions.

Fournaies and Fowler (2022) consider a similar research design and setting, but their independent variable of interest is an index of state-level campaign finance restrictions. They find that more stringent campaign finance laws ultimately have little impact on state insurance policies. As lobbying and campaign contributions are generally regarded as co-occurring complements (Dahm and Porteiro, 2008), we expect that some restrictions on lobbying (specifically, restricting the ability of legislators to eventually become lobbyists) will have similar effects as Fournaies and Fowler’s campaign finance index. That is, the mostly null finding for campaign finance restrictions suggests that revolving door laws likely are not serving their intended purpose of reducing corporate influence over policy.

Fournaies and Fowler’s perhaps surprising finding (2022) is partially congruent with Law and Long’s seminal study of revolving door restrictions on public utility commissioners (2012). Law and Long found that PGE restrictions indeed caused electricity prices to decrease, but only for industrial consumers – residential and commercial electricity customers received no benefits from the laws. Law and Long concluded that PGE restrictions may indeed diminish corporations’ hold over regulators, but not in every case. Thus, given both Law and Long’s mixed findings and Fournaies and Fowler’s conclusion that campaign finance restrictions have little effect, it seems likely that adopting revolving door laws for state lawmakers does not lead to a less favorable state-level regulatory environment for property and casualty insurers.

By considering each state's regulatory climate towards property and casualty insurance, we can assess not only whether revolving door laws effect who becomes a state legislator but also whether revolving door laws change the policies that the legislators themselves adopt. If we detect evidence that revolving door laws are causing states to adopt less favorable policies towards property and casualty insurers, then this would suggest that revolving door laws are serving their intended purpose and reducing the regulatory capture of state legislators. If, on the other hand, we detect null effect, then revolving door laws may not actually reduce the extent to which state legislators are clients of insurers, if at all. Just as Law and Long (2012) examined electricity prices as a proxy for corporate capture of state public utility commissioners, in this paper I employ Fournaies and Fowler's (2022) insurance framework to examine the extent to which state legislators are captured by corporate interests.

Data

Legislator Ideology

To measure legislator ideology, I employ the scores developed previously by Shor and McCarty (2011) and used extensively in political science research. The latest readily available iteration of these Shor-McCarty scores covers all state legislatures from all states over the period from 1993 through 2016. Shor and McCarty employ a novel approach allowing estimation between legislatures across chambers, states, and time by developing a single, unidimensional score for all legislators incorporating legislator responses to the NPAT survey, roll call data, and ratings assigned to legislators by interest groups (e.g., the Sierra Club and the National Rifle Association). Legislators with unidimensional left-wing ideologies receive negative scores, while legislators with conservative ideologies receive positive scores (generally ranging from -1.5 to

1.50); a legislator with a 0 score would, in expectation, be perfectly moderate. The Shor-McCarty dataset contains 24,716 unique state legislators from all 50 states over the period 1993 to 2016 from both state upper and lower chambers.

For the 2022 cycle, the Council of State Government finds that approximately 94% of Democratic and 97% of Republican state legislative incumbents were reelected, and incumbents won 67% of all state legislative races that cycle. Of course, these data come a bit after the period covered by the Shor-McCarty scores, and it is possible that incumbents are reelected more (or even less) frequently now than they were from 1993-2018. Like Congress, it appears that, much more often than not, incumbent state legislators are reelected.

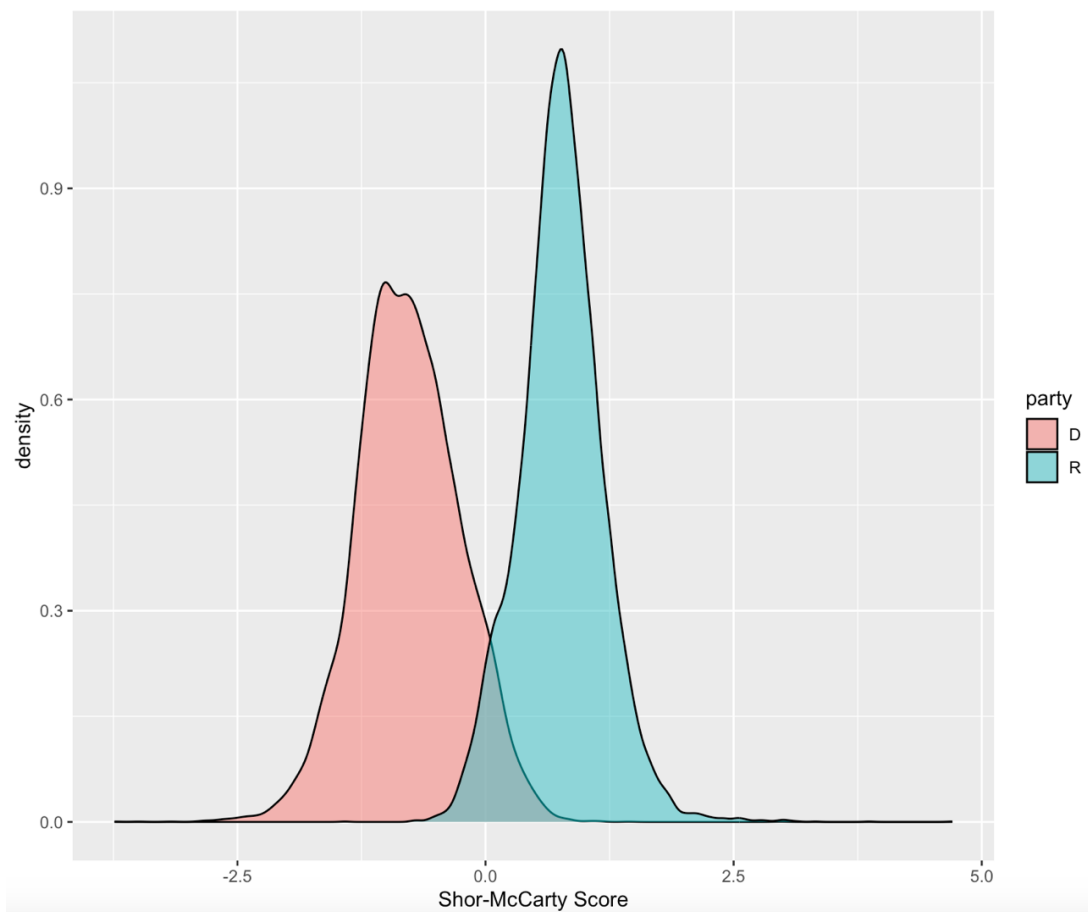


Fig. 1: Density plot of Democratic and Republican legislators' Shor-McCarty Scores, 1993-2016.

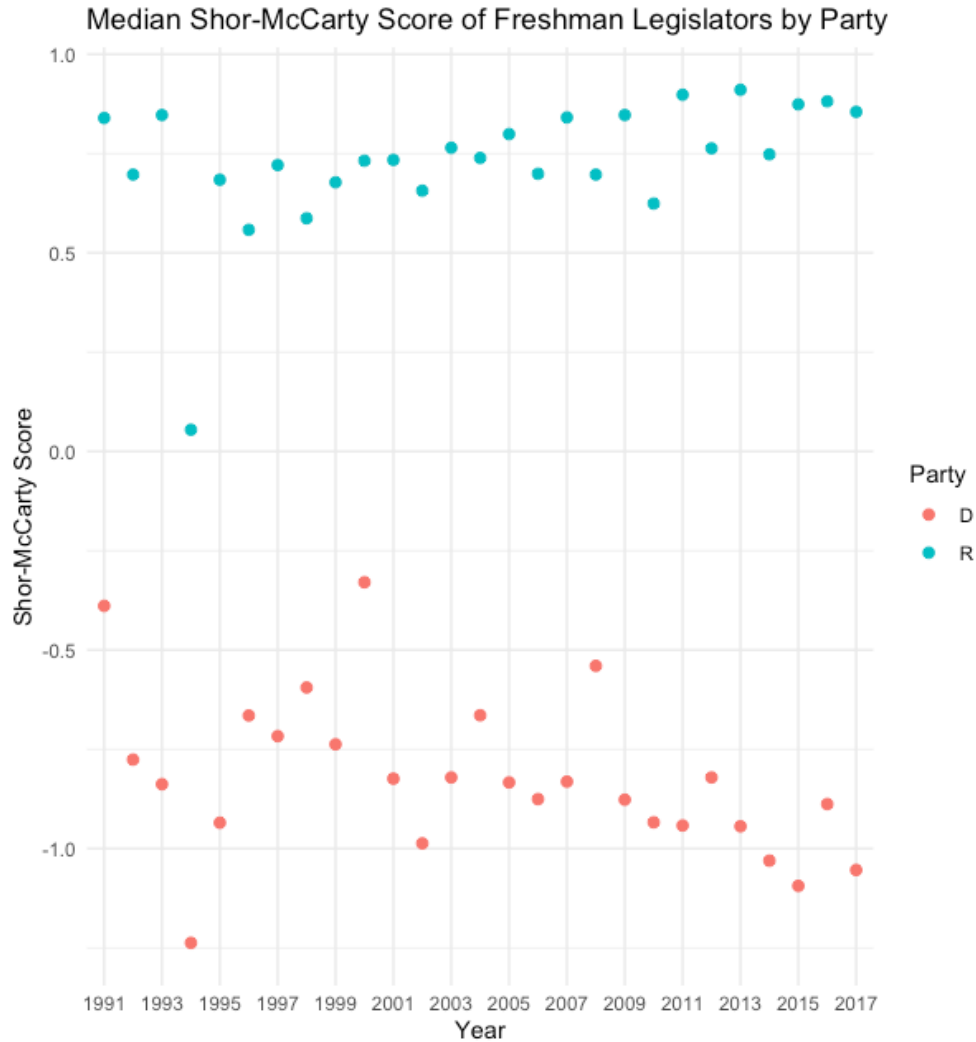


Fig. 2: Median freshman (newly elected) legislator Shor-McCarty from each party in each year.

As expected, Figure 1 reveals that, across the dataset, Democratic legislators tend to receive negative (i.e., unidimensional liberal) Shor-McCarty scores, while Republican legislators tend to receive positive (i.e., conservative) scores. There is slight overlap between the Democratic and Republican legislators in Fig. 1, perhaps a surprising finding given the seemingly ever-worsening partisan polarization of the present (McCarty 2019). Note however that Fig. 1 records the distribution of all 24,548 Democratic and Republican state legislators in the dataset across all years from 1993 to 2018; it does not provide any insights into how that

distribution has shifted; that is, whether the partisan gap has ‘worsened’ over that period, or whether the distribution of legislators’ ideologies has changed over time.

Indeed, as a quick visual treatment of Fig. 2 demonstrates, the median freshman legislator elected each year from each party has, on average, become more extreme. That is, Fig. 2 depicts the ideology of the median freshman legislator elected in each year from each party across the United States, irrespective of state. Where Fig. 1 (as a density plot) was unable to illustrate whether legislator ideology has changed over time, Fig. 2 suggests that polarization has indeed worsened among state legislators, with Republican freshman legislators tending to become more conservative and Democratic freshman legislators tending to become more liberal over the period.

To be clear, this paper does not seek to measure legislator ideology; Shor and McCarty have already essentially accomplished that task. Rather, I assess whether restrictions on post-governmental employment increase *polarization* among legislators. Thus, I consider three complementary measures of polarization for each chamber of the state legislator (state senate and state house of representatives): the ideology (measured by the Shor-McCarty score) of the median Democrat, the ideology of the median Republican, and the difference of these values. To develop these measures of polarization, I utilized ‘R’ Version 4.4.2 (“Pile of Leaves”) to filter legislators by state, year, chamber, and party, and aggregate those data to find the median legislator for each state, year, chamber, and party. Then, I subtracted the median Democrat’s Shor-McCarty score from the median Republican’s to find the difference between the parties, the principal measure of polarization employed here.

If adopting revolving door restrictions indeed causes heightened polarization, then we would expect to see the Shor-McCarty scores decrease for the median Democrats and increase

for the median Republicans, and subsequently the difference between the median party members would increase. Together, these six measures – median Democratic ideology, median Republican ideology, and the difference in these medians, for each chamber – are the “Outcome_{it}” identified as the dependent variable in Equation 1 and Equation 2, below. Of course, we can imagine many other reasonable measures of polarization – perhaps the difference between the mean (rather than median) legislators, or some measure of legislator deviation from a 0 (moderate) Shor-McCarty score. While time and brevity prevented these other measures from being considered here, future researchers would do well to examine alternative dependent variables to confirm the results of this study.

These ideology scores are not without limitations, however. Principally, Shor and McCarty produce a single score for each legislator throughout their career in elected office. It is quite conceivable that legislators (especially long-serving legislators) adjust their ideologies once in office. Any such temporal shifts for individual legislators are unrecorded by the single Shor-McCarty scores. Likewise, Shor and McCarty do not make the inputs underlying their scores (e.g., roll call data) readily available to other researchers, so there is no opportunity to use any different measure of ideology that they collected (for instance, NRA scores.)

Because each legislator receives a single score for their entire career, any changes recorded in the medians are solely because of new members joining the legislature, not because existing legislators adjust their ideologies. This feature, in fact, represents an advantage for my work – because I test (indirectly) whether revolving door laws change who wants to run for elected office, any changes in the median legislator ideologies will exclusively be a result of different candidates entering office, allowing a more direct assessment of the hypotheses here.

Post Governmental Employment Restriction Dataset

To construct a novel dataset of post-governmental employment (PGE) restrictions on state legislators, I consulted the survey of state restrictions on post-governmental employment of public utility commissioners developed by Law and Long (2012). Likewise, I reviewed the summaries of state restrictions on PGE created by the National Conference of State Legislatures (NCSL 2025).

Most significantly, I reviewed the legal corpora of each state – often beginning my search with the citations provided by the NCSL – to confirm whether a state has revolving door laws that apply to legislators. This required careful analyses of each state’s codes. Ex ante, we might believe that the NCSL’s survey of state legislative codes would be sufficient, but this proved not to be the case. Given the nature of the two-way fixed effects model employed here, the date in which the PGE restrictions were adopted by each state is of course essential for the model, yet the NCSL did not collect this information for state legislators. Law and Long did provide related information (i.e., the date in which the restriction was implemented), but they surveyed PGE restrictions on public utility commissioners (executive employees), not state legislators (2012). Thus, I reviewed the legal codes of each state to confirm whether the PGE restrictions explicitly applied to state legislators rather than merely some executive employees of state agencies.

Tennessee’s Code provides a straightforward and typical example of a proscription on lobbying that applies to state legislators:

No member of the general assembly, elected official in the executive branch, member of the governor's cabinet, or cabinet level staff within the governor's office shall be a lobbyist during the twelve-month period immediately following departure from such office or employment. (TN §3-6-304(1))

This statute is clear and unambiguous – no state legislators (that is, “member of the general assembly”) may “be” a lobbyist during the twelve-month period immediately after their departure from elected office. And fortunately, the legislative history of the law provides its date

as well – “Acts 2006.” So, a review of the pertinent legal statutes for Tennessee both clearly verifies that the state restricts legislator PGE (prohibits them from becoming lobbyists) and provides the date in which the restriction took effect. This close review of statutes was an integral part of data collection because, through my examination of pertinent statutes, I found that even some statutes that the NCSL claimed “closed” the revolving door on state legislators nevertheless granted exemptions. Specifically, the NSCL indicated that Maryland and Wisconsin have both ‘closed’ the revolving door on their state legislators, yet my review of the pertinent statutes found, in fact, that the language specifically exempted state legislators. Consider the following example from the Wisconsin General Code, Chapter 19.45:

Except in the case where the state public office formerly held was that of legislator....:

(a) No former state public official, for 12 months following the date on which he or she ceases to be a state public official, may, for compensation, on behalf of any person other than a governmental entity, make any formal or informal appearance before, or negotiate with, any officer or employee of the department with which he or she was associated as a state public official within 12 months prior to the date on which he or she ceased to be a state public official.

As the emphasized text of the law indicates, former state public officials are prohibited from representing other entities for compensation (i.e., working as a lobbyist), “except in the case where the state public office formerly held was that of legislator...” This statutory language quite plainly exempts state lawmakers from the revolving door law. I found that such self-serving exemptions are not uncommon in statutes; as previously stated, even the NCSL incorrectly identified two states as having revolving door laws when in fact state legislators were exempted from the proscriptions. Essentially, my data collection on state revolving door laws followed a three-step process: I began with the initial categorization Law and Long (2012) employed for public utility commissioners, then I consulted the National Conference of State Legislatures database for assistance identifying the pertinent legal citations, and, ultimately, I

carefully reviewed the legal codes of all 50 states to determine whether their legal codes proscribed legislators from working as lobbyists after leaving office and verified the date in which those restrictions took effect.

After this close review of the pertinent statutes from all 50 states, I categorized a state as restricting its legislators' PGE if former state legislators were prohibited from registering as lobbyists or working as lobbyists for *any* length of time after leaving office, although most restrictions are for one or two years. Those laws had to both clearly apply to state legislators *and* prohibit them from working or registering as lobbyists, as opposed to a more general prohibition on conflicts of interest for former public employees. I coded states as 1 if they met these requirements (explicitly prohibiting former state legislators from registering as lobbyists for any length of time) and 0 otherwise.

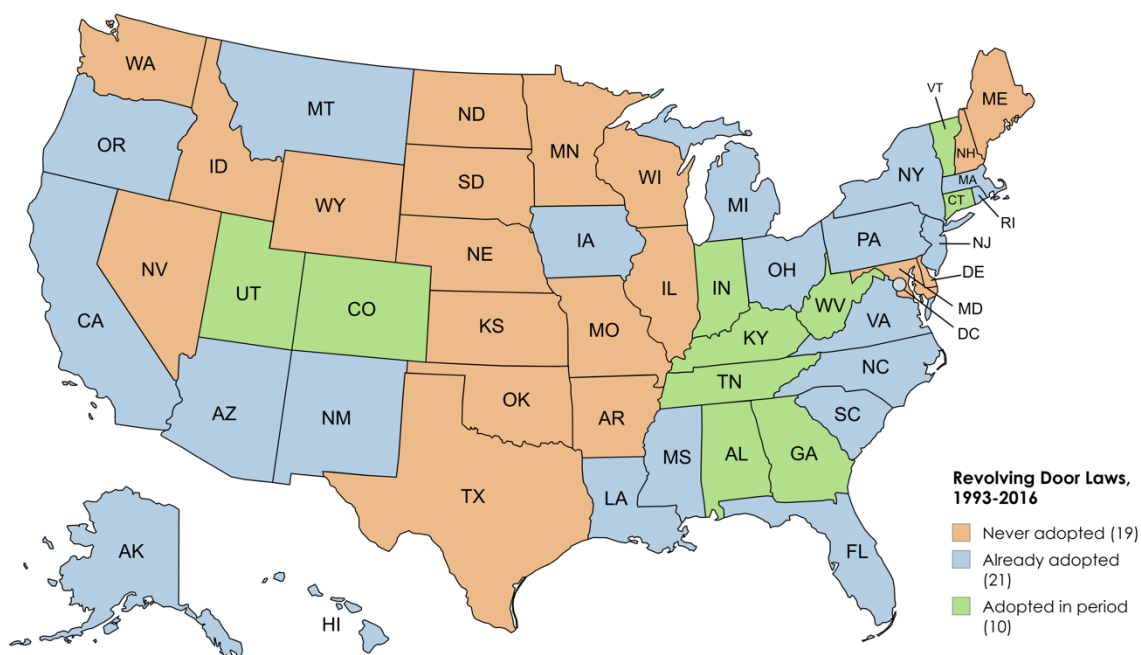


Fig. 3: Adoption of revolving door laws by state, 1993-2016.

Ultimately, for the period in question (1993 to 2016), I find that 19 states never adopted revolving door laws, 21 states had already adopted revolving door laws, and ten states adopted revolving door laws during the period. Additionally, no states repealed their revolving door laws over the period. See Appendix A for a detailed treatment of each state's revolving door laws, and the citation for the pertinent statute from each state's legal codices. In general, there does not appear to be a clear partisan or regional pattern to the laws' adoption over the period: northern, more 'liberal' states like Vermont and Connecticut both adopted laws over the period, as did southern states like Alabama and Tennessee and Western states, including Colorado and Utah.

Insurance Performance

Fourinaies and Fowler (2022) collect several metrics to assess the success of property and casualty insurers at the state level, I utilize here: insurance premiums, the state tax rate on those premiums, statutory minimum automobile insurance, and guaranty fund net assessment. They extend data collection beginning in 1970 – for the purpose of this paper, I utilize data beginning in 1993 (please see Appendix B for summary statistics of each insurance measure).

Perhaps the most straightforward and direct measure are premiums; specifically, the property and casualty premiums, per capita, in adjusted 2017 dollars. In about half of states, insurers must receive prior approval before increasing their rates; in other states, regulators generally “retain authority to disprove rates” that do not further competition (NAIC, 2011). State legislators play a key role in this process – they oversee the state insurance departments and grant statutory authority to regulators (ibid.). As Fourinaies and Fowler (2022) concede, simply considering premiums per capita does not reveal information about whether the insurers are actually profitable. Nevertheless, it is reasonable to believe that insurers would prefer to assess higher premiums.

Relatedly, Fournaies and Fowler examine the premium tax rates in each state from 1966 to 2017, which they calculate simply by dividing the total taxes paid by the total value of premiums written. Indeed, Fournaies and Fowler report that insurers' government relations teams "devote significant attention" (2022, p.4) to these tax rates and would strongly prefer that these tax rates be as low as possible. Thus, if revolving door laws indeed diminish the corporate capture of state legislators, we would expect to see this tax rates increase.

Automotive insurance is required in 48 of 50 states by the end of the period explored by Fournaies and Fowler, and insurers would generally prefer that the statutory minimum for automotive insurance is higher (such that they receive more business). To produce a measure of the minimum required automotive insurance, Fournaies and Fowler generate the inflation-adjusted sum of the bodily injury and property damage requirements (which are often required separately). We might then expect that implementing PGE restrictions would cause a decrease in this minimum automotive coverage requirement.

Finally (for this paper), Fournaies and Fowler consider the annual guaranty fund net assessment levied in each state. Insurance companies pay these assessments annually to "guaranty associations" which are "non-profit organizations created by state statute for the purpose of protecting insurance policyholders from financial losses and delays in claims payments due to the insolvency of an insurance company" (NAFA, 2017). Essentially, guaranty funds serve as insurers of last resort to pay claims should a property and casualty insurer become unable to meet its obligations to policyholders. Licensed insurers doing business in a state pay an annual assessment to support the guaranty fund. These annual assessments, then, indicate the financial health of the insurance industry in each state (in a state with many insolvent insurers, the assessment will be higher.) Indeed, in some instances insurers receive a payment (negative

assessment) from the fund. Insurance executives confirmed to Fourirnaies and Fowler (2022) that this annual assessment is a good measure of the overall regulatory environment in a state. In states with a favorable regulatory environment, then, we expect the average annual assessment to be low (or even negative.) And if revolving door laws serve their stated purpose and reduce the sway of corporations over policy, then the guaranty fund assessment should increase after the PGE restrictions are adopted.

Methods

To directly consider the causal impact of restrictions on post-governmental employment, I employ a two-way fixed effects model accounting for state and year fixed effects. The principal model employed throughout this paper is:

$$Outcome_{it} = \beta * Revolving_{it} + \gamma_i + \delta_t + \epsilon_{it} \quad (1)$$

Where the $Outcome_{it}$ is a measure of legislators' ideology or the partisan gap between parties for state i in year t ; PGE is a dichotomous variable for whether the state restricts post-governmental employment of former state legislators (or not); and gamma and delta control for unit (state) and time (year) fixed effects, respectively. In expectation, this regression yields an unbiased estimate, β , for the average causal effect of whether a state adopts restrictions on the post governmental employment of state legislators, assuming the parallel trends assumption holds. That is, as the existence of revolving door laws varies by state and year, the model implicitly controls for all unobserved covariates between states and across time that could bias an estimate of the effect of revolving door laws.

To assess the impact of revolving door laws on the insurance industry (and, in expectation, measure whether these laws are serving their stated purpose), I utilize a two-way

fixed effects model identical to Equation 1, above, but where the dependent variable.

“Outcome_{it}”, is now different measures of each state’s regulatory climate for property and casualty insurance. These measures are explained in greater detail in the Data section below.

Parallel Trends Assumption

It is not possible to assess the parallel trends assumption directly – parallel trends is, of course, an assumption of legislators’ behavior in the hypothetical counterfactual where their state did not restrict post governmental employment. That is, we assume that legislators in states that adopted these PGE restrictions would behave (in terms of their ideologies) the same as their peers in states that did not adopt PGE restrictions in this period but for the effect of the treatment (PGE restrictions). While this assumption can never be tested directly, we may nevertheless attempt to assess the validity of parallel trends by considering the pre-treatment trends of legislator ideology. That is, if legislators in treated and untreated states exhibit the same trends in their polarization *before* receiving the treatment, then this supports the assumption that parallel trends would hold true in the unobservable counterfactual where the treated states never adopted the PGE restrictions.

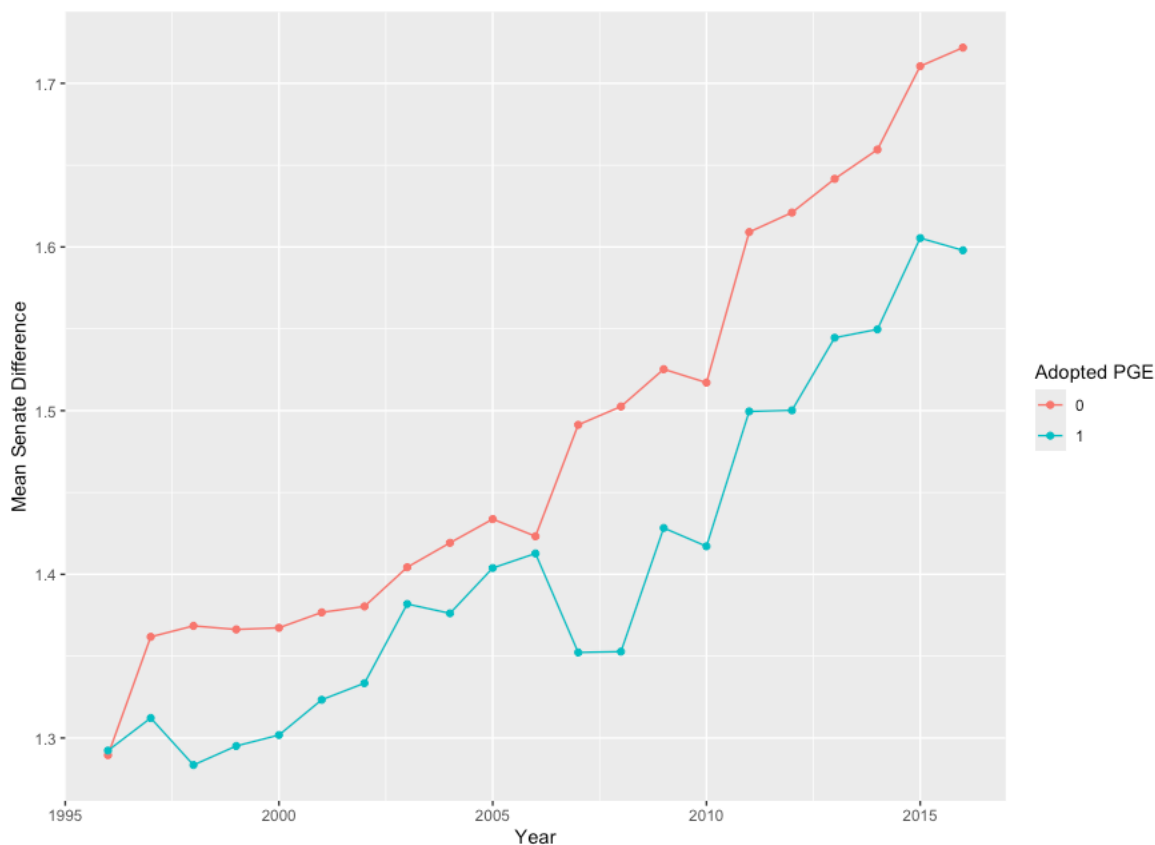


Fig. 4: Average Difference in the Mean Democratic and Republican State Senator's Ideology, 1996-2016.

Figure 4 above provides some preliminary visual evidence that the parallel trends assumption may hold, at least for state senators. Note that the graph begins in 1996 as less than half the Shor-McCarty ideology data is available before that year. The two lines plot the mean difference between the mean Democratic and mean Republican state senator in each state's upper chamber. On average, the difference between the mean Democratic and mean Republican state senator was 1.6 on the Shor-McCarty scale in treated states and about 1.71 for untreated states in 2015. This number is essentially a measure of polarization – higher mean differences indicate that the average Democrat and average Republican are ideologically farther apart on the unidimensional Shor-McCarty ideology scale.

Of course, that polarization appears to be slightly worse in untreated states does not disprove our hypothesis that adopting PGE restrictions, on average, worsens polarization – we are interested in understanding whether PGE restrictions cause polarization to worsen relative to the unobservable counterfactual where the treated states did not adopt the restrictions. That is, we hypothesize that polarization would be even less intense in the treated states, but for the fact that they adopted PGE restrictions. Arguably the key finding from Fig. 4 is that, in both treated and untreated states, polarization has worsened consistently between 1993 to 2016, and that it has worsened at roughly the same rate. That is, both trend lines move in the same direction and with roughly the same slope, inspiring some confidence that, despite technically being unobservable, the parallel trends assumption indeed holds for this analysis.

The exception is the year 2007, where the treated and untreated lines sharply diverge. Yet 2007 was a banner year for PGE restrictions, with three states (Colorado, Georgia, and North Carolina) adopting revolving door laws that year, and Tennessee adopting the restrictions the year before, in 2006. It is thus unsurprising that we would observe the treated and untreated trends change rather significantly when several states adopted the treatment. Admittedly, this is an essentializing analysis -- simply ‘looking at’ two aggregate lines is not an especially empirical assessment of the pre-treatment trends.

To more rigorously assess these pretreatment trends, I therefore created a dummy, dichotomous leading variable “Pre” that takes a value of 1 beginning the year immediately *before* state “i” adopts the treatment and remains 1 for all subsequent years.

$$Outcome_{it} = \beta_1 * Revolving_{it} + \beta_2 * Pre_{it} + \gamma_i + \delta_t + \epsilon_{it} \quad (2)$$

Thus, Equation 2 above has the exact specifications as Equation 1, but with the added pre-treatment dummy variable. If the pre-trends are parallel, as we expect, then the coefficient on

the pre-treatment dummy, β_2 , should equal zero. Further, the coefficient of interest, β_1 , should not change. In this ideal case (β_2 equals zero, and β_1 is unchanged), then the pre-treatment trends in the year immediately before a state adopts the treatment are parallel, and we can be more confident in our belief that the parallel trends assumption holds. In contrast, a positive (negative) sign on β_2 would suggest that the partisan gap had already begun to worsen (improve) in treated states *before* they received the treatment (relative to untreated states) and would consequently weaken our belief that the parallel trends assumption holds. A positive sign, in particular, would inspire concern that the model perhaps overestimates the increase in polarization attributable to the adoption of revolving door laws.

Of note for all these analyses is the staggered timing of PGE adoption between states. Given the nature of the two-way fixed effects model, the coefficient we derive from these equations thus assumes homogeneity in the treatment effect both between units and across time – that is, we assume that the effect of revolving door laws on state legislators and insurance outcomes is the same regardless of the year the restriction is adopted or the state in which it is adopted. Ex ante, it is difficult to know the extent to which this assumption is reasonable; however, it seems likely that Kentucky's revolving door law in 1993 did not have the identical effect on legislators' ideology as did Vermont's in 2015. Therefore, the causal estimates derived here are in fact the *weighted average* of the effect of a state adopting restrictions on legislator PGE across the period (1993 to 2016) and the units (the ten treated states). So, the estimate coefficient on β_1 , for example, should not be interpreted as the effect of revolving door laws on legislator ideology for every state that adopts PGE restrictions (which requires the assumption of homogeneous treatment effects across states and years), but rather as the average effect of PGE restrictions over the period and across the treated states.

Findings

Legislator Ideology

Conducting the regression described in Equation 1 above with the various measures of legislator ideology as our dependent variable of interest yields the following results:

	<i>Dependent variable:</i>	
	sen_difference (1)	house_difference (2)
treated	0.079*** (0.029)	0.087*** (0.021)
Constant	1.154*** (0.072)	0.906*** (0.052)
Observations	1,023	1,033
R ²	0.894	0.945
Adjusted R ²	0.886	0.941
Residual Std. Error	0.158 (df = 950)	0.116 (df = 960)
F Statistic	111.708*** (df = 72; 950)	228.996*** (df = 72; 960)
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01	

Table 1: Impact of PGE restrictions on the difference between the median state senator and median state representative.

	<i>Dependent variable:</i>			
	dem_sen_median (1)	rep_sen_median (2)	dem_house_median (3)	rep_house_median (4)
treated	-0.031 (0.021)	0.048** (0.019)	-0.068*** (0.015)	0.019 (0.014)
Constant	-0.680*** (0.053)	0.474*** (0.048)	-0.331*** (0.038)	0.575*** (0.035)
Observations	1,023	1,023	1,033	1,033
R ²	0.915	0.895	0.954	0.951
Adjusted R ²	0.908	0.887	0.950	0.947
Residual Std. Error	0.117 (df = 950)	0.105 (df = 950)	0.085 (df = 960)	0.077 (df = 960)
F Statistic	141.530*** (df = 72; 950)	111.875*** (df = 72; 950)	274.470*** (df = 72; 960)	259.325*** (df = 72; 960)
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01			

Table 2: Impact of PGE restrictions on the Shor-McCarty scores of the median Democratic and Republican legislator in each chamber.

Six outcomes measuring ideology are explored: the ideology of the median Democrat and Republican in each chamber (state house and senate), and the difference between the median Democrat and Republican in each chamber. So the “sen_difference” dependent variable in Table 1, for example, is the difference between the Shor-McCarty scores of the median Democrat and the median Republican member of a state’s upper chamber. The main results of interest – the difference between the median Democrat and median Republican in each chamber – is reported in Table 1. In both chambers, the partisan gap (i.e., polarization) between the median Democrat and median Republican increases significantly, with a slightly greater substantive effect for members of the lower chamber. Between 1993 and 2016 (the period of interest), the polarization between the median members in each chamber increased by about 0.02 per year, on average. The findings from table 1 above (a 0.08-point polarization increase in state senates and a 0.09 increase in the lower chambers, on average) suggests that adopting revolving door laws increases the political polarization by the same amount as two to three electoral cycles. That is, adopting revolving door laws “speeds up” polarization by about four to six years, on average.

When considering the median member of each chamber and each party and not the difference between the parties, however, the results are less clear, as reported in Table 2. There are heterogeneous treatment effects both between and within chambers and parties. Senate Democrats seem unaffected by the revolving door laws, but house Democrats are especially affected; the reverse is true for senate and house Republicans. In each case, however, the signs point in the expected direction – Democrats are becoming more liberal in their ideologies (negative), Republicans are becoming more conservative (positive), and the differences between the median party members in each chamber is increasing. And, for four of the six examined measures, the p values are clearly beneath the traditional threshold (0.05) for statistical

significance. Together, this evidence suggests that, on average, revolving door laws cause state legislators to become more extreme and worsen political polarization.

Assessing Pre-Treatment Trends

	<i>Dependent variable:</i>			
	Difference in Mean House Members		Difference in Mean Senators	
	(1)	(2)	(3)	(4)
treated	0.087*** (0.021)	0.033 (0.045)	0.079*** (0.029)	0.121* (0.065)
pre		0.059 (0.043)		-0.045 (0.063)
Constant	0.906*** (0.052)	0.902*** (0.052)	1.154*** (0.072)	1.156*** (0.072)
Observations	1,033	1,033	1,023	1,023
R ²	0.945	0.945	0.894	0.894
Adjusted R ²	0.941	0.941	0.886	0.886
Residual Std. Error	0.116 (df = 960)	0.116 (df = 959)	0.158 (df = 950)	0.158 (df = 949)
F Statistic	228.996*** (df = 72; 960)	226.077*** (df = 73; 959)	111.708*** (df = 72; 950)	110.129*** (df = 73; 949)

Note: *p<0.1; **p<0.05; ***p<0.01

Table 3: Implementing Equation 2, that is, including the additional pre-treatment variable.

To assess the robustness of this finding – that revolving door laws worsen polarization – I next include a pre-treatment dummy variable, as described by Equation 2, to check whether polarization between Democratic and Republican legislators had already begun to worsen in the year immediately before states adopted these revolving door laws. Thus, finding a positive coefficient on the “pre” variable would suggest that polarization had indeed worsened before states implemented PGE restrictions, suggesting that the laws themselves are likely not the cause of the worsening polarization.

From this analysis, we see that difference in ideologies between the median state senate Democrat and Republican are robust to the inclusion of the pre-treatment dummy variable. In fact, for state senators, the estimated average treatment effect is now larger (0.121) than before

the inclusion of the pre-treatment dummy variable (0.08), and it remains near significant (p is between 0.1 and 0.05). Thus, it appears that the pre-treatment trend among state senators was nearly parallel, increasing our confidence that the parallel trends assumption holds for this group. And since the sign on the “pre” variable for state senators is negative, it is possible our initial analysis underestimated the extent to which revolving door laws worsen polarization.

In contrast, the result for the state lower chambers does not inspire much confidence – the derived coefficient on the pre-treatment dummy is, in fact, larger than on our treated variable, indicating that state representatives in treated states become more ideologically extreme *before* they receive the treatment, relative to untreated states. Because the pre-treatment trends for state representatives are not parallel immediately before the treatment, it seems much less likely that the parallel trends assumption holds for the state lower chambers, which should lead us to question whether the coefficients we obtained previously may be interpreted causally. It thus seems likely that Equation 1 overestimates the effect of adopting revolving door law legislation of ideology for state representatives, but perhaps not for state senators.

This mixed finding is curious – it is not immediately clear why the pre-treatment trends appear to be generally parallel for state senators but not for state representatives. One potential explanation for this phenomenon relates to the nature of state senate (versus state house of representatives) elections. In almost every state outside New England, state senators (like their federal counterparts) are elected in staggered classes, and most serve four-year terms.³ Perhaps because they serve only two-year terms, state representatives are better able to anticipate the upcoming adoption or implementation of revolving door laws and consequently retire before the laws are adopted. This would explain why sign on the “pre” coefficient is positive, which

³ In New England (and New York), state senators serve for two years. For a detailed treatment, see Ballotpedia.

indicates that polarization had already begun to worsen more in treated states (among their state senators) than in untreated states. Alternatively, as described later (see Discussion), perhaps state representatives are less ‘attractive’ lobbyists to firms and so are not recruited to the same extent as state senators. Thus, state representatives would be less affected by revolving door laws.

As described previously, the parallel trends assumption is unobservable, so we can never know with certainty whether it holds in this (or any other) case. By considering the trends between the treated and untreated groups immediately before the treatment is adopted, however, we attempt to work around this problem and provide some insight into the robustness of this model. The results of these regressions suggest that we should be especially cautious of interpreting the results for state lower chambers, but that we should feel more confident that our estimated average causal effect on state senators is rather unbiased.

Insurance Industry

Next, I examine the extent to which adopting revolving door laws affects the property and casualty insurance industry, as previously described. Recall that, if revolving door laws indeed serve intended purpose and reduce the influence of corporate interests on state government, then we would expect insurance premiums (premiums) to decrease, insurance assessments (assessments) to increase, automotive insurance minima (automin) to decrease, and the tax rate levied on insurance companies (taxrate) to increase, as described by Fournaies and Fowler (2022).

	<i>Dependent variable:</i>			
	premiums (1)	assessments (2)	automin (3)	taxrate (4)
treated	-116.935*** (27.873)	0.044 (1.516)	0.557 (1.862)	0.001 (0.001)
Constant	2,180.216*** (48.683)	14.284*** (2.595)	176.877*** (3.321)	0.037*** (0.002)
Observations	1,150	1,028	1,176	1,150
R ²	0.819	0.241	0.812	0.710
Adjusted R ²	0.807	0.186	0.799	0.690
Residual Std. Error	165.623 (df = 1077)	8.452 (df = 958)	11.545 (df = 1103)	0.007 (df = 1077)
F Statistic	67.640*** (df = 72; 1077)	4.408*** (df = 69; 958)	65.953*** (df = 72; 1103)	36.555*** (df = 72; 1077)

Note: *p<0.1; **p<0.05; ***p<0.01

Table 4: Estimated average effect of revolving door laws on the insurance industry.

Overall, the evidence of an impact on the property and casualty insurance industry is mixed. Only one tested measure – the average premiums assessed – shows a statistically significant change caused by the adoption of state-level restrictions on legislator post governmental employment. I find that adopting restrictions on post governmental employment causes property and casualty insurance premiums to decrease by \$116.94, on average across states and years. Fourinaies and Fowler (2022) find that the interquartile range for assessed premiums for all states (across all measures years) ranges \$1126 to \$1760, and the average premium is about \$1480. The \$116.94 reduction detected here is thus substantively relevant – essentially, adopting restrictions on post governmental employment reduces the average insurance premium by about 7.9%.

The three remaining measures tested here – guaranty fund net assessment, statutory minimum automobile insurance, and the state tax rate levied on insurance profits – show no substantive or statistically significant results. This suggests that restricting revolving door lobbying for state legislators has no readily discernible effect on those measures. Thus, the findings for insurance are mixed: only one of four outcome measures shows any substantive or significant response to the adoption of revolving door laws, although the insurance premiums do

move in the direction we would expect if revolving door laws indeed reduce corporate influence over legislators.

Assessing Parallel Trends for Insurance

As for the ideologies of state legislators, I rerun the two-way fixed effects model for the insurance outcomes, this time with the inclusion of a dichotomous pre-treatment ‘dummy’ variable that takes the value 1 beginning the year immediately before a state implements revolving door laws. If the parallel trends assumption likely holds (that is, if the pre-treatment trends between treated and untreated states are parallel), then we expect this pre-treatment dummy variable will have statistically insignificant coefficients near zero.

	<i>Dependent variable:</i>			
	premiums (1)	assessments (2)	automin (3)	taxrate (4)
treated	-86.867 (56.606)	-0.269 (3.224)	-0.536 (3.764)	0.002 (0.002)
pre	-34.083 (55.841)	0.350 (3.188)	1.237 (3.701)	-0.0002 (0.002)
Constant	2,183.599*** (49.012)	14.252*** (2.613)	176.758*** (3.342)	0.038*** (0.002)
Observations	1,150	1,028	1,176	1,150
R ²	0.819	0.241	0.812	0.710
Adjusted R ²	0.807	0.185	0.799	0.690
Residual Std. Error	165.671 (df = 1076)	8.457 (df = 957)	11.550 (df = 1102)	0.007 (df = 1076)
F Statistic	66.680*** (df = 73; 1076)	4.341*** (df = 70; 957)	64.999*** (df = 73; 1102)	36.021*** (df = 73; 1076)

Note:

*p<0.1; **p<0.05; ***p<0.01

Table 5: Average impact of PGE restrictions on property and casualty insurance outcomes, with the inclusion of a pre-treatment dummy variable to assess the pre-treatment trends.

With the dichotomous pre-treatment ‘dummy’ variable included in our regression (as in Equation 2), the supposedly significant effect detected for insurance premia disappears. It appears that the average property and casualty insurance premium had already begun to decrease in treated states in the year immediately before those states adopted revolving door law restrictions. Given that none of the other three insurance outcomes showed any significant effect

before the addition of the pre-treatment dummy variable, it is possible that our initial, significant finding for insurance premia was merely a false positive. That is, of four tested measures, that one briefly appeared significant does not especially support the conclusion that revolving door laws have any discernible effect on the insurance industry.

Discussion

Overall, the principal finding of this paper – restrictions on post governmental employment cause state legislators to become more ideologically extreme – is consistent with Law and Long ‘s seminal study of the impact of revolving door laws on public utility commissioners (2012). In that setting, Law and Long found that revolving door laws had several unintended and deleterious consequences on the characteristics of public servants, including both decreased average tenure and average prior experience for public utility commissioners. I detect related unintended consequences here – revolving door laws cause state legislators to become more ideologically extreme, on average. For state senators, this finding is rather robust. Further researcher working with additional data – for instance, details about the average tenure or experience of state legislators – should aim to explicitly replicate Law and Long’s findings that PGE restrictions increase turnover and decrease experience among government employees. Indeed, if PGE restrictions cause state legislators to spend less time in office or induce less experienced public servants to seek office, this would confirm my finding that revolving door laws have negative consequences on state legislators.

Further, my findings challenge Hall’s (2019) ex ante assumption that the revolving door does not convey “nearly” enough value to induce would-be candidates enter the race for elected office. The difference in the median Democrat and Republican members of both chambers of the

state legislature increased both significantly and substantively, on average. If we allow the parallel trends assumption to hold (and I posit it likely does, at least for state senators), our two-way fixed effects model shows that this increased (average) polarization is *caused* by the adoption of revolving door laws. By Hall's own cost-benefit model, because partisanship has worsened after states restrict PGE, we should conclude that the revolving door induces some (moderate) candidates to run for office. Once states act to restrict the revolving door, they remove one benefit (incentive) of running for office, and, because extreme candidates tend to benefit more from running for office in general (because they would like to move policy further to their ideal points), we are left with only more extreme candidates running for office and, consequently, more extreme legislators. The effect, as measured here, is heightened polarization among elected legislators, and, presumably, its resultant gridlock and legislative disfunction. Because we find revolving door laws cause greater polarization, on average, by Hall's model we should conclude that the revolving door law is itself an attractive benefit of elected office. Once that benefit is removed, some (mostly moderate) candidates will no longer run for office, and consequently more partisan extremists are elected to state legislatures.

These findings provide additional context to the previous studies of the value gained from holding elected office. Considering the careers of British members of Parliament, Eggers and Hainmueller (2019) found that MPs from the Conservative Party derived a much greater benefit from their time in office than their Labour colleagues, a phenomenon the authors attributed to the Conservative Party's closer ties to business. Given this, we might expect *ex ante* that Republicans would become much more extreme after the repeal of revolving door laws than their Democratic counterparts. However, this is not the case – the parties seem to be roughly equally affected by the treatment (see Table 2, above). That Democrats and Republicans seem to be

equally affected by revolving door laws suggests that neither party is clearly the “corporate” favorite, unlike Eggers and Hainmueller’s conclusions (2019) from the United Kingdom.

Alternatively, this finding could suggest that the firms who would hire revolving door lobbyists are somehow “savvier” in the United States than in the UK; perhaps firms gain an advantage by hiring former legislators from both parties (additional access, better insider knowledge, etc.). If this were the case, then we would find (as we do here) that Democrats and Republicans tend to be equally affected when the revolving door is ‘closed’ by law. Nevertheless, my finding suggests that, unlike their British counterparts, American legislators themselves are affected by the revolving door irrespective of their partisan affiliations.

Relatedly, this finding might inspire confidence that both state legislators and firms are law abiding. That is, once states adopt revolving door laws, it is, in theory, possible for former legislators to work as lobbyists merely by shirking reporting requirements or otherwise obscuring the true nature of their work. Because we detect some average treatment effect for revolving door laws, however, this suggests that once a state adopts these laws its legislators abide by the proscriptions. Indeed, while past research has indicated that lobbying and bribery serve similar purposes for firms – eliciting more favorable policy from policymakers (see, for example, Harstand and Svesson, 2011) -- these findings suggest that firms are not shifting their strategies to provide bribes to former legislators, for example. Additional research could confirm this finding by examining the (often publicly available) lists of registered lobbyists in each state and considering if PGE restrictions cause a decrease in the number of registered revolving door lobbyists. If this exercise showed that revolving door laws cause fewer former legislators to become lobbyists, this would confirm our suspicion that legislators (and firms) are indeed law abiding and do not continue to work as illegal revolving door lobbyists.

Additionally, further research in this area should aim to replicate Palmer and Schneer's (2016) finding that (federal) representatives are much more likely to register as corporate lobbyists than their Senate counterparts, who were more likely to become (much better paid) corporate directors. Palmer and Schneer attributed this difference to the greater "desirability" of senators relative to representatives – in large part because there are fewer senators, Palmer and Schneer believed that they were in greater demand (and, consequently) received more pay than representatives. They suggest that lobbying is thus a less prestigious (and, granted, much lower paid) position than serving on a corporate board of directors. Unexplored here is whether the same holds true at the state level – are state senators less likely to register as lobbyists than state representatives? We could imagine, because state legislatures tend to be much lower profile than their federal counterparts (and there are many more state legislators), that in fact the reverse is true: perhaps former state senators are more likely to register as lobbyists, and state representatives are consequently left with an even lower paid, less prestigious positions after leaving office. If this hypothesis were true, then it would suggest that state senators are more affected by restrictions on PGE than state representatives. And indeed, the finding here for state senators is rather more robust than state representatives. So perhaps state senators are more affected by PGE restrictions because they were more likely to register as lobbyists *ex ante*. In any case, further research into the profile of state lobbyists is needed to explore the relationship between legislative experience and future lobbying activities.

Besides an examination of whether revolving door laws have unintended consequences on state legislators (i.e., discourage moderates from seeking office), I likewise test whether revolving door laws serve their intended purpose (i.e., reducing corporate influence over state policy). Aside from perhaps the average property and casualty insurance premiums, I find little

evidence that revolving door laws affect the insurance industry. As discussed previously, there are also serious inferential issues for insurance premiums – it appears that premiums had already begun to decline substantially immediately before the adoption of the revolving door laws.

These findings (for the measure of corporate influence over policy) are generally inconsistent with Law and Long’s research into electricity prices following the adoption of PGE restrictions for state public utility commissioners (2012). In their context, Law and Long found that revolving door laws caused public utility commissions to lower the allowed electricity rates charged to industrial (but not residential or commercial) users to decrease, indicated that revolving door laws were serving their intended purpose and reducing corporate (i.e., utility) influence over policymaking.

Granted, in Law and Long’s setting, assessing the causal relationship between PGE restrictions and consumer electricity prices is much more straightforward because public utility commissioners often directly set electricity prices. Unlike public utility commissioners, state legislators do not directly set insurance premiums, nor do they regulate premium changes (a prerogative often under the remit of insurance commissioners or another state agency.) So, perhaps one reason we do not observe a more significant relationship between state insurance regulation and revolving door laws is because legislators simply lack the authority to directly exert influence over insurance regulation. Indeed, Fourinaies and Fowler (2022) find that the states with the most significant evidence of corporate influence in their insurance markets are those with elected insurance commissioners. Perhaps insurance commissioners and state insurance departments actually are beholden to the allure of the revolving door, and we do not detect an effect for state legislators because legislators play little role in regulating insurance. If directly elected state insurance commissioners indeed exert significant influence independent of

state legislators, then it is not surprising that insurance regulations are set independently of state legislators. Simply, the direct link between state legislators and insurance premiums is much weaker than the direct, causal relationship Law and Long considered between public utility commissioners and electricity prices, so it is not especially surprising that we likely detect no significant impact of PGE restrictions.

Despite these reservations, property and casualty insurance remains one of the best settings to examine whether revolving door laws serve their stated purpose of reducing corporate influence over policymaking. As Fourinaies and Fowler noted, property and casualty insurance is “arguably the industry most affected by state regulations” (2022). If we detect null results for PGE restrictions impacting property and casualty insurers, as we do here, it is difficult to imagine any industry where we *could* detect results. That is, if property and casualty insurers are the most heavily regulated at the state level but they suffer no apparent regulatory effects from the closure of the revolving door, then it is possible that PGE restrictions do not significantly impact corporate interests at the state level. Thus, PGE restrictions appear to come up short of their noble intentions – I find no evidence that they reduce corporate influence over policymaking, yet PGE restrictions do worsen partisan polarization.

Policy Implications

These findings suggest that states should be more hesitant to adopt restrictions on their legislators’ post governmental employment. While there is little evidence that revolving door restrictions reduce the extent to which legislators serve the interests of corporate interest groups and lobbyists, the evidence appears rather robust (for state senators, at least) that PGE restrictions cause more ideologically extreme legislators to enter office. If we view a wide partisan gap (and the gridlock it often induces) as troublesome in our legislatures and

governments, then we should hesitate to support PGE restrictions on state legislators. By removing the incentive of the revolving door, states have inadvertently made their legislatures less attractive to would-be officeholders.

Yet if we follow Hall's cost-benefit model of officeholding (2019), states could ameliorate the negative impacts of closing the revolving door (which, we have shown, is an apparent benefit of elected office) by increasing some other more explicit benefit of holding elected office. As Hall argues, prospective officeholders will seek office only when the collective benefits of running for (and ideally winning) elected office outweigh its many costs. If we accept that the revolving door is a benefit of holding office, at least for some moderate, would-be candidates, then we could compensate for that lost incentive by increasing the other benefits of holding office – for example, by paying legislators higher salaries.

Essentially, if legislators were paid higher salaries while they held office, the benefits of holding elected office would again increase in the eyes of prospective candidates, and more moderate candidates would consequently seek office. The exact amount of the pay increase required to “make up” for the missing revolving door income is not explored further here, although we can imagine that an economically inclined model could indeed compute a monetary value of the compensation needed to account for the lost, future revolving door income that entices moderate candidates into running for office.

Past research indeed suggests that higher legislator pay changes the nature of who seeks elected office (McCormick and Tollison, 1978), with higher paid legislatures attracting more “blue-collar” professions (like teachers and nurses) than the lowest paid legislatures, which tend to be dominated by officeholders who are already well-off (i.e., lawyers). Thus, by increasing legislator pay, states could retain the (potential) advantages of restricting legislator PGE (less

corporate influence over legislation, if such a benefit even exists) while nevertheless reducing the costs of PGE restriction (more extreme legislators). And sufficiently high legislative salaries could have the added benefit of inducing would-be candidates from historically underrepresented backgrounds (i.e., nonlawyers) to seek office.

Alternatively, rather than compensating legislators for the lost benefit of the revolving door, states could also work to lower the costs of holding elected office. Hall cites myriad factors, including the slog of campaigning and the specter of heightened press attention, as significant costs to seeking office. Hall specifically identifies candidates' needs to fundraise for their campaigns as "a key driver of the costs of running for office" (2019, p. 64). Many of these costs – the need to fundraise or press scrutiny – are seasonal and related directly to elections themselves. Perhaps lengthier terms, then, would reduce these costs for state legislators. That is, if state legislators served four- or six-year terms (instead of, most frequently, two-year terms), then perhaps the costs of holding office would be reduced and we would once again see more moderate candidates seeking office. While 'closing' the revolving door may have reduced the benefits for running for office, extending the length of legislative terms could proportionally reduce the costs of running for office, once again returning to a more moderate equilibrium.

Unfortunately, empirical evidence for this hypothesis is mixed. Exploiting a natural experiment in the term lengths of Argentinian Congress members, Bó and Rossi (2011) find that legislators assigned shorter terms tend to shirk more legislative responsibilities. They attribute this finding to the "distraction" of campaigning, supporting the notion that campaigning indeed increases the costs of holding elected office. Yet Gaines, Nokken, and Groebe (2012), exploiting a very similar natural experiment in term lengths among Texas and Illinois's state senators, find a null effect, at least in terms of the characteristics of the campaigns themselves. That is,

assessing a wide variety of behaviors, Gaines et al. find little difference at all between the behavior of legislators who must contest two versus four-year terms.

Nevertheless, if we adopt Hall's (reasonable) assertion that fundraising and campaigning are among the most significant costs of running for office, then policies that reduce those costs, including lengthier terms, are likely worthwhile. Since we have demonstrated that the revolving door is a benefit of holding office whose removal causes more extremists to enter state legislatures, state policymakers should explore measures like lengthier legislative terms to lower the costs of running for office and, ideally, induce more moderates to once again seek office. Indeed, policymakers should consider a combination of the above policy suggestions – higher legislative salaries to increase the benefits of office and longer terms to decrease the costs – to induce more moderates to seek office and consequently reduce partisan polarization.

Conclusion

Leveraging a novel dataset of state restrictions on legislator post governmental employment, I find evidence that PGE restrictions – attempts to close the so-called revolving door between legislators and lobbyists – cause polarization to worsen, on average. These effects are rather robust, at least for state senators. In contrast, I find little evidence that revolving door laws reduce corporate influence on policymaking, as measured within the property and casualty insurance landscape. These findings suggest that PGE restrictions may not serve their intended purpose, yet they still cause more extremists to enter state legislatures.

If we take seriously concerns that America's federal and state legislatures have ground to a halt due to partisan gridlock, then we must critically examine the causes of worsening polarization. As V.O. Key opined in *The Responsible Electorate* nearly a century ago: "If the

people can choose only from among rascals, they are certain to choose a rascal.”⁴ Subscribing to Hall’s cost-benefit model of who seeks elected office at the state level (2019), it follows that revolving door laws remove an important benefit of elected office from prospective, moderate candidates. Consequently, we are left only with a pool of more extreme candidates – ‘rascals’ who ultimately produce more polarized state legislatures.

Additional research should closely examine the details of this revolving door induced polarization. By aiming to replicate findings like Palmer and Schmeer’s – that representatives and senators behave differently after leaving office – and Eggers and Hainmueller’s – that conservatives tend to benefit more from elected office – at the state level, future work could shed further light onto the exact nature of PGE restrictions’ effects on state legislators. Ultimately, if states adopt revolving door laws, as most have, policymakers should adopt measures to increase the other benefits or reduce the costs of running for office. Doing so is needed to induce more moderate candidates to run for office and consequently reduce worsening polarization among state legislators. Ultimately, as Phillips et al. note, increased polarization among state legislators is one of the leading causes of worsening polarization at the national level (forthcoming). By identifying and ameliorating causes of polarization in state legislatures, including from revolving door laws, policymakers can consequently reduce polarization on the national scale.

⁴ As cited in Hall 2019, p. 22.

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Appendix A: State Level Restrictions on Post Governmental Employment

Table 4: Summary of state-level PGE restrictions

State	Adopted	Restriction Length	Citation	Notes
Alabama	1996	2 years	Code of Alabama Section 36-25-13	
Alaska	Pre 1970	1 year	Alaska Constitution Art. II, Section 5; date from “phone conversation with AK attorney general’s office” (Law and Long)	
Arkansas	Never	N/A	AR Code 21-8-102 does not include legislators	
Arizona	1974	1 year	Confirmed in Ariz. Rev. Stat. Ann. § 38-504, Law and Long date: "Phone conversation with AZ attorney general's office"	
California	1980	1 year	Gov. Code section 87406, added in 1980	
Colorado	2007	2 years	Amendment 41 adopted 2007 by voters	
Connecticut	1995	1 year	Sec. 2-16a, effective for those taking office in 1995.	
Delaware	2017	1 year	Chapter 58, Title 29 of the Delaware Code, Section 5837	
Florida	2022	6 years	Chapter 78-426 Laws of Florida, updated by voters	Toughest in country
Georgia	2007	2 years	Georgia Code section 21-5-75	
Hawaii	1972	1 year	HRS Section 84-18	
Idaho	Never	N/A		
Illinois	Never	N/A		
Indiana	2011	1 year	Ind. Code Ann. § 2-7-5-7	Code contains effective date
Iowa	1992	2 years	IA Code § 68B.5A	Adopted after the Public Trust Scandal
Kansas	Never	N/A		
Kentucky	1993	2 years	Kentucky Code 6.757	
Louisiana	1980	2 years	LA RS 42:1121, enacted 1980	Applied to “elected officials”
Maine	2020	1 year	Me. Rev. Stat. tit. 1, § 1024	

Maryland	Never	N/A	Md Gen Provis § 5-504 (a) (1), specifically exempts legislators	NCSL disagrees.
Massachusetts	1978	1 year	Mass. Gen. Laws Ann. chapter. 268A, § 5	Subsection e effects the “general court”
Michigan	Pre 1970	Varies	Mich Comp Laws Section 4.416(a)	Depends on whether legislator resigns in office
Minnesota	Never	N/A		
Mississippi	1989	1 year	MS Code § 25-4-105 (2024)	Date from Law and Long, correspondence with state officials
Missouri	2018	2 years		Stricken down by federal court
Montana	1977	1 year	“Ethical Requirements for Public Officers and Public Employees,” MC § 2-2-105(3)	Legislative History shows adoption in 1977
Nebraska	Never	N/A		
Nevada	Never	N/A	NRS 281A.550	Legislators omitted from ethics law
New Hampshire	Never	N/A		
New Jersey	1971	1 year	NJ Rev Stat § 52:13C-21.4	
New Mexico	1978	1 year	N.M. Stat. Ann. § 10-16-8	
New York	1987	2 years	Public Officers Law § 73(8)(a)	
North Carolina	2007	6 months	§163A-308	
North Dakota	2019	2 years	N.D. Const. art. XIV	Approved by voters 2018
Ohio	1974	1 year	Ohio Rev. Code Ann. § 102.03	Date unclear: from Law and Long’s “correspondence with state official”
Oklahoma	Never	N/A		
Oregon	1987	1 year	Or. Rev. Stat. Ann. § 244.045	Date in legislative history

Pennsylvania	1979	1 year	65 Pa. C.S.A. § 1103	
Rhode Island	1976	1 year	R.I. Gen. Laws § 36-14-4(e)	
South Carolina	1992	1 year	S.C. Code Ann. § 2-17-15	Legislative history provides date
South Dakota	Never	N/A		
Tennessee	2006	1 year	TN §3-6-304(l)	
Texas	Never	N/A		
Utah	2009	1 year	Utah Code: Title 67, Chapter 24, Section 103	
Vermont	2015	1 year	See Vt. Stat. Ann. tit. 2, § 262.266	
Virginia	1987	1 year	Va. Code Ann. § 30-103	Legislative history has date
Washington	Never	N/A		
West Virginia	2011	1 year	HB 2464, 2011	
Wisconsin	Never	N/A	Legislators specifically exempted by §19.45(8)	NCSL incorrect on this
Wyoming	Never	N/A		

Appendix B: Summary Statistics of Measures

Table 5: Fourinaies and Fowler (2022) summary statistics

Measure	Min	Q1	Median	Mean	Q3	Max
<i>Premiums</i>	433.2	1126.4	1496.9	1480.2	1760.1	3736.8
<i>Auto Minimum</i>	27.07	67.43	88.57	99.25	124.28	315.68
<i>Premium Tax Rate</i>	.00003	.02918	.03584	.03654	.04300	.10769
<i>Guaranty Assessed</i>	-42.043	0.0	0.0	2.943	3.105	229.691

