

The Connection between Campaign Spending and Golden Parachute Jobs

At first glance, the situation of money in politics in the United States is very different from that in India or Brazil. It is certainly not the norm that US legislators enrich themselves to the tune of millions of dollars while in office. Of course, it happens on occasion, but the consequences tend to be swift and severe. For example, former Illinois Governor Rob Blagojevich was secretly recorded seeking to extract personal benefits from potential candidates to fill Barack Obama's Senate seat after he was elected president. Within weeks of announcing in graphic language that he would not "give it away for nothing," Blagojevich was arrested, impeached, and barred from ever holding public office in the state again. After two trials, he was convicted on seventeen charges and sentenced to fourteen years in prison.¹ Systematic investigations of US congresspersons have also found no evidence that their financial situation improves in an unusual manner *while* they are in office (e.g. Eggers and Hainmueller, 2013, 2014).²

Yet, US politicians *do* have opportunities to benefit financially from their position. The main difference from Brazil or India is that they can usually do so only *after* they leave office. As many as half of all members leaving Congress subsequently register as lobbyists (Lazarus, McKay, and Herbel, 2016). A similar percentage serve on one or more boards of directors (Palmer and Schneer, 2016). In fact, golden parachute jobs are so common that the satirical newspaper *The Onion* responded to the announcement that the devout Christian representative Michele

¹ He was pardoned in 2020 by President Trump.

² Eggers and Hainmueller (2013, 2014) show that members of Congress' stock portfolios do not grow in an unusual manner.

Bachmann would not seek reelection with an article titled “Michele Bachmann: ‘God Wants Me to Earn 7 Figures for a Lobbying Firm’.”³

Prior studies explain golden parachute jobs as a result of special interest groups’ demand for expertise or political connections (cf. de Figueiredo and Richter, 2014). And while this is certainly part of the story, it cannot explain why these jobs exist in the first place. The practice is almost unheard of in many other countries where expertise and connections are just as important as in the United States. What is more, in this chapter I show that there are large differences even *within* the United States in how common such employment is. In Chapter 3, I argued that golden parachute jobs should be seen as part of a larger interconnected system of money in politics. I reasoned that this form of money in politics is directly, and inversely, related to campaign spending, and that the relative importance of the two types is driven by a country’s legal and electoral campaign environments.

In this chapter, I test these claims using microlevel data from the United States, the setting of most past studies of both forms of money. In a first step, I examine the effect of the *legal environment* on the prevalence of campaign spending and golden parachute employment. Most prior research has focused on golden parachute jobs at the national level, where there is no variation in regulation.⁴ I address this limitation by analyzing state legislatures, the members of which are governed by different rules at different times. Using information from state disclosure agencies, I assemble the first dataset that permits a comparative analysis of golden parachute employment by tracking whether state legislators left office and subsequently took up a lobbying position. I also make use of the detailed campaign finance disclosure data that candidates in the United States must provide.

I exploit the fact that states differ in how stringent their campaign finance laws are, as well as their golden parachute restrictions. According to my argument, these regulations should affect not only the type of money they regulate but also have *second-order* consequences for the *other* form. That is, campaign finance regulation should impact golden parachute employment, and golden parachute restrictions should in turn affect the flow of campaign money into politics.

³ “Michele Bachmann: ‘God Wants Me to Earn 7 Figures for a Lobbying Firm’,” *The Onion*, May 29, 2013.

⁴ For an exception, see Cain and Drutman (2014).

To assess campaign finance laws, I analyze the consequences of a US Supreme Court ruling that introduced an exogenous change to the strictness of campaign finance regulation in some states, but not others. Consistent with the theoretical predictions detailed in Chapter 3, I show that states with more permissive campaign finance laws after the ruling, which resulted in an increase in campaign spending, saw fewer lawmakers taking up golden parachute jobs. Then I study the consequences of golden parachute rules for campaign finance. Many states have introduced “cooling off” laws that restrict how soon politicians can register as lobbyists after leaving office. Exploiting the differential timing of the changes to these laws, I show that such restrictions (1) reduce the probability that legislators will take up a golden parachute job and (2) lead to an increase in campaign contributions.

Finally, I analyze the effect of the *electoral campaign environment*. As I argued in Chapter 3, electoral competitiveness (or changes thereof) should influence how attractive staying in office is relative to leaving and taking up a golden parachute job. As I did for India in the previous chapter, I use the extent of redistricting as a shock to competitiveness. Consistent with my argument, I find that more extensive redistricting makes incumbents more likely to accept a golden parachute job.

5.1 MONEY AND POLITICS IN THE UNITED STATES

A 2015 poll revealed the extent to which Americans are fed up with the role that money plays in political campaigns: 84 percent of respondents said money had too much influence; 66 percent were convinced that wealthy people had more influence over elections than other Americans; and 55 percent thought that most of the time, politicians promote the policies of those who donated to their campaigns.⁵

US elections are notoriously expensive. In 2010, candidates for the House of Representatives first broke the billion dollar threshold. Expenditures have increased since then, to more than \$1.9 billion in 2020.⁶ In 2018, the average winner spent about \$2.1 million; the most expensive campaign of that cycle was that of Georgia Democrat Jon Ossoff, which cost just over \$30 million. Races for the Senate are even more costly. The average winner in 2018 spent about \$15.8 million. Rick Scott of Florida

⁵ “Americans’ Views on Money in Politics,” *New York Times*, June 2, 2015.

⁶ In the United States, this money typically comes mostly from campaign donors (external sources), or from candidates self-financing their campaigns.

set that year's record by spending \$83.5 million. Overall expenditures for Senate races reached a new record in 2020 of more than \$2.1 billion.⁷

As staggering as these figures are, they are only the tip of the iceberg. Spending on presidential campaigns easily eclipses the total outlays of races for the House and Senate. The 2020 presidential contest cost more than \$6.6 billion.⁸ In addition, there are ninety-nine state legislatures with many thousands of candidates, and even local races can involve substantial amounts of money. For example, the candidates in the 2017 mayoral race in Syracuse, New York, a city of less than 150,000 in population, spent more than \$1.3 million, a not atypical sum.⁹

On top of this direct spending by candidates, *indirect* election spending must also be taken into account. Private citizens, corporations, and unions can advertise for or against the election of candidates *independently* of politicians or parties. This kind of spending exploded after the Supreme Court ruled in 2010 that restrictions on such activities violated the right to free speech and were therefore unconstitutional (Spencer and Wood, 2014). In 2016, independent campaign spending exceeded \$1.5 billion for federal elections alone (Abdul-Razzak, Prato, and Wolton, 2020). Local races have also been affected. For example, outside groups spent almost \$15 million on the 2017 elections of the Los Angeles *school board*, translating to more than \$110 per vote cast.¹⁰

But campaign spending is not the only way in which money enters politics in the United States. Many politicians also take up golden parachute employment – often as lobbyists. For example, ex-House Majority Leader Dick Gephardt retired from Congress in 2005. He subsequently joined the lobbying firm DLA Piper before setting up his own practice in 2007. His roster of clients included companies like Goldman Sachs, Boeing, and Visa. In 2010, he billed them more than \$6.5 million. The same year, former Republican Representative Billy Tauzin earned more than \$11.5 million from his clients, setting a new record for ex-politicians.¹¹

While Gephardt and Tauzin are at the higher end of the earnings scale, their career paths are not exceptional. A comprehensive analysis

⁷ "Elections Overview," *Center for Responsive Politics*; "Election Trends," *Center for Responsive Politics*.

⁸ "2020 Election to Cost \$14 Billion, Blowing Away Spending Records," *Center for Responsive Politics*, October 28, 2020.

⁹ "Crowded Syracuse Mayor's Race Was Most Expensive in History at \$1.3M," *syracuse.com*, December 6, 2017.

¹⁰ "How L.A.'s School Board Election Became the Most Expensive in US History," *Los Angeles Times*, May 21, 2017.

¹¹ "The Trouble With That Revolving Door," *New York Times*, December 18, 2011.

by Lazarus, McKay, and Herbel (2016) shows that around a third of representatives and about half of senators leaving office find employment as lobbyists. Even Walt Minnick, the former congressman we met right at the start of this book who lamented how much time he had to spend raising campaign donations, started his own lobbying firm.¹²

Another lucrative path for former politicians is to join corporate boards of directors. In fact, senators and state governors are more likely to serve as a board member than to be registered as lobbyists (Palmer and Schneer, 2019). These positions are part time and come with a generous salary, on average about \$250,000 per year (Palmer and Schneer, 2016). For example, Dick Gephardt not only brought in millions through his lobbying activities, he also served as a director for Ford, Spirit Aerosystems, Dana, Centene, Embarq, and US Steel.¹³

The United States is thus the prime example of money in the form of campaign spending, and its politicians also often benefit financially from their position, although only after leaving it. So far, these two forms of money in politics have been treated separately. In this chapter, I study their connection.

5.2 GOLDEN PARACHUTE JOBS AS A TYPE OF MONEY IN POLITICS

Considering golden parachute jobs in the same framework as campaign contributions and self-enrichment in office is unusual. Indeed, the social science literature on the so-called revolving door has treated such jobs as a separate phenomenon. The main question it investigates is why corporations and interest groups hire former politicians (and their staffers) as lobbyists or in other functions such as members of their board of directors.

One line of studies argues that organizations that hire former politicians gain access to their expertise. People who have spent many years in politics are intimately familiar with the lawmaking process. In addition, many legislators develop subject-matter expertise in policy areas they specialize in. Several studies contend that corporations are more likely to hire legislators who have more knowledge (Salisbury et al., 1989; Heinz et al., 1993; Esterling, 2004; Parker, 2008). An alternative view is that former legislators not only gain expertise during their time in office, they also get to know many important people. Corporations may thus hire

¹² "Former Rep. Minnick Blazes His Own Trail," *The Hill*, June 5, 2012.

¹³ "Gephardt, Earley Join Ford's Board Of Directors," *Gephardt Group*, March 25, 2009.

ex-politicians because it allows them to tap into their network of connections. A growing body of studies finds support for this motivation (Hillman, 2005; Blanes i Vidal, Draca, and Fons-Rosen, 2012; Bertrand, Bombardini, and Trebbi, 2014; LaPira and Thomas, 2014, 2017).

While this debate has advanced our understanding of golden parachute employment, it has two characteristics that have insulated it from scholarly work on other forms of money in politics. First, it almost exclusively uses data from the United States, and more specifically Congress. Because this is a single institutional environment (or, if we want to be generous, two environments), the only variation available for analysis is *between* subjects: Why are some legislators hired while their colleagues are not? Prior studies have therefore taken it as a given that golden parachute jobs exist, and try to explain who gets one and who does not (and why). A second characteristic is that in trying to explain this variation, the focus has been on the employers' motivation – the *demand side* of golden parachute employment. Previous research has thus implicitly assumed that there is a pool of politicians willing to leave office, and that corporations hire those with the qualities that further their interests the most; the debate is about what exactly those qualities are.

This human capital account does not explain why golden parachute jobs exist in the first place. While it is true that there are plenty of politicians in the contemporary US Congress who are happy to take up private sector employment, this is not the case everywhere and at all times. Whereas Mayhew (1974) described members of Congress in the 1970s as behaving like single-minded seekers of reelection, Tennessee Representative Jim Cooper's view of his colleagues in 2012 was markedly different: "Serving the public used to be considered the highest calling; now, many see it as a stepping stone to lucrative lobbying careers."¹⁴ Demand-side arguments cannot easily explain why this change occurred, as presumably politicians in the past also had expertise and connections that were valuable to the private sector.¹⁵ The same is true for cross-sectional

¹⁴ "Cooper First to Sign 'No Lobbying' Pledge," *Jim Cooper Press Release*, September 17, 2012.

¹⁵ LaPira and Thomas (2017) explain the rise of politicians-turned-lobbyists in the United States with the increased demand for such services due to a decline in government capacity to address public problems and the increase in importance and coherence of political parties. However, these trends are unique to the US context. They do not translate to European countries, for example, where parties have become *weaker* over time (e.g. Dalton and Wattenberg, 2002; Van Biezen, Mair, and Poguntke, 2012), while golden parachute jobs have become much more common as well.

differences. In addition to the United States, golden parachute employment has been documented in places such as the United Kingdom, Ireland, and Germany (González-Bailón, Jennings, and Lodge, 2013; Dörrenbächer, 2016; Baturo and Arlow, 2018), but it is notably absent in many other countries (see Chapter 6). And as I show later in the text, there are even large differences between US states. Again, it seems unlikely that special interests need expertise or connections in some countries or states, but not in others.

In other words, while explanations based on politicians' expertise and connections have provided valuable insights into which lawmakers *within* a legislature are most likely to take up golden parachute employment, they have difficulty explaining differences *between* legislatures. Why do many legislators leave behind careers in public service to cash in through a private sector job in some contexts, while in others it rarely or never happens? In Chapter 3, I argued that to answer this question, we need to think about golden parachute jobs as part of the broader system of money in politics, and how it is influenced by the legal and electoral campaign environments. Here, I test this argument.

5.3 DATA ON GOLDEN PARACHUTE EMPLOYMENT IN US STATES

In this section, I introduce the data that for the first time makes it possible to study golden parachute jobs in a *comparative* manner. I do so by shifting from the national level, which has been the setting of almost all prior studies on the topic, to the subnational level. US state legislatures exhibit heterogeneity in both their legal and electoral campaign environments, which allows me to test the theoretical argument laid out in Chapter 3.

Collecting data on golden parachute employment is challenging (cf. Dal Bó and Finan, 2018). Lawmakers who leave politics return to being private citizens, so in many cases there is no public record of when they take up a new job. Some countries require former politicians to register their employment for a certain period of time after leaving office. However, these disclosure requirements usually only apply to a small number of high-level public officials and not to regular members of parliament (MPs) (cf. Weschle, 2021b). One can try to reconstruct lawmakers' career paths after leaving office using newspaper articles, internet searches, and the like. This works well for high-level officials who remain in the public spotlight, but is unlikely to be sufficient for analyzing MPs at the national or subnational level. Previous research using such an approach has indeed

focused on presidents, prime ministers, or central bankers (e.g. Adolph, 2013; Musella, 2015; Baturo and Mikhaylov, 2016; Dörrenbächer, 2016; Baturo, 2017).

To study golden parachute jobs for ordinary lawmakers, researchers have instead exploited disclosure requirements that apply to some other activity, but that as a byproduct reveal their employment. For example, publicly traded corporations have to publish the names of the members of their board of directors. By matching these names with lists of former lawmakers, one can obtain data on one way in which golden parachute employment manifests itself. This approach has been used for MPs in the United Kingdom (Eggers and Hainmueller, 2009; González-Bailón, Jennings, and Lodge, 2013) as well as for US senators and governors (Palmer and Schneer, 2016, 2019).

Another strategy is to exploit the fact that politicians-turned-lobbyists account for much golden parachute employment, particularly in the United States. Since lobbyists have to register, it is again possible to match their names with those of former legislators. This has been done for both the US Senate and the House of Representatives (Lazarus, McKay, and Herbel, 2016).

I follow the latter approach when looking at golden parachute employment in US states. While this does not capture all jobs, it likely covers the majority of them. Palmer and Schneer (2019) find that high-profile ex-politicians, such as cabinet members, governors, or senators, are more likely to join boards of directors than to become lobbyists. However, lower-profile ex-politicians, such as former members of the US House of Representatives, are much more likely to become lobbyists. It therefore stands to reason that former state representatives, who have an even lower profile, are unlikely to move into board of director positions.

I assemble new data that tracks whether state legislators left office and subsequently took up a lobbying position. Following the convention in the literature, I exclude Nebraska from the sample due to its unusual unicameral, non-partisan legislature. Information on lobbyists comes from the states' lobbyist registries for the years 2006–2013, compiled by the National Institute on Money in State Politics.¹⁶ Many individuals register to lobby at the state level: Their number ranges between 41,134 and 45,151 per year, and totals 112,190 unique individuals during the

¹⁶ See www.followthemoney.org.

observation period.¹⁷ The most lobbyists are registered in New York (4,801 lobbyists per year, on average), Arizona (3,559), and Florida (2,351). Wyoming (282), Maine (263), and Alaska (142) have the fewest. Information on state legislators comes from the State Legislative Election Returns dataset (Klarner, 2013; Klarner et al., 2013). Based on the elections to the lower chambers from 2006 to 2012, there were 8,349 state representatives in the 49 states (4,050 Democrats, 4,299 Republicans).

To link the legislator data with the lobbyist information, I use a two-step procedure that minimizes both false positive and false negative matches. First, I employ an algorithm that coarsely matches names between the lists.¹⁸ I allow the name matches to have considerable divergence to ensure that people who appear in the two lists with slightly different names are still linked. For example, it matches a former New Jersey representative who appears as “Kamin, Dick” in the election data, but who is registered as a lobbyist under “Kamin, C Richard.” This, of course, leads to a large number of false positives. For example, it matches “Kenny, Bernard F. Jr.” and “Flynn, Bernard M.” which clearly are not the same person. In a second step, I therefore check all matches manually. If there are doubts about a match, for example if it is a common name, I use supplementary information from internet searches to check whether they are indeed the same person.

This allows me to create a variable that captures how many legislators take up golden parachute jobs as lobbyists. The unit of observation is the legislator-election cycle. Since elections for state lower houses take place every two years, there are four time periods: 2006–2007, 2008–2009, 2010–2011, and 2012–2013. For each cycle, a politician is considered to have taken a golden parachute job if they leave office and register as a lobbyist in their state the same or the following year.¹⁹

The new data for the first time makes it possible to compare how common golden parachute employment is in each state. Figure 5.1 maps the

¹⁷ Definitions of what constitutes a lobbyist differ somewhat between states, as do the registration requirements (de Figueiredo and Richter, 2014). In the analysis, I account for any state-level differences, so they are not a threat to inference.

¹⁸ I use a measure based on the Levenshtein distance, which is a string metric for quantifying the difference between two sequences. Intuitively, it is the minimum number of insertions, deletions, or substitutions required to change one sequence into the other. To make them comparable, I use the following similarity measure: $1 - (d(s1, s2)/\max(A, B))$, where d is the Levenshtein distance function, $s1$ and $s2$ are the two strings, and A and B are their lengths.

¹⁹ Lazarus, McKay, and Herbel (2016) show that the overwhelming majority of golden parachute lobbyists at the federal level register within one year of leaving office.

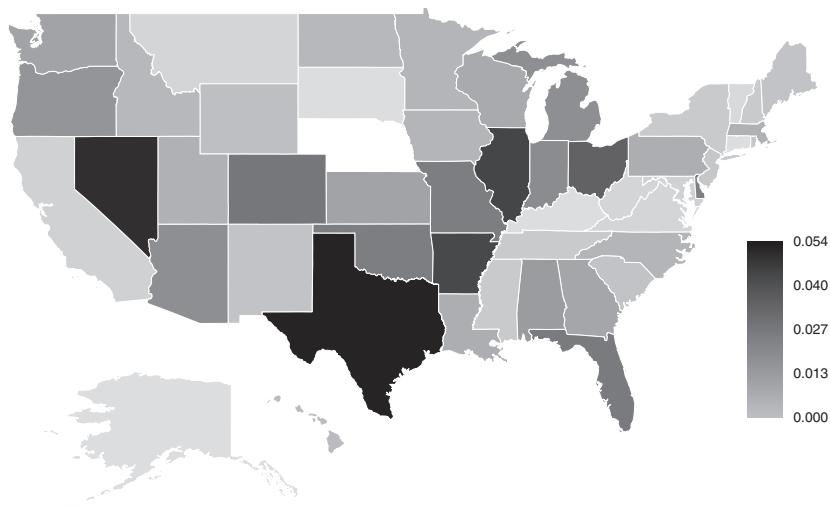


FIGURE 5.1 *Golden parachute employment in US state lower chambers, 2006–2013.* Proportion of representatives who leave office and register as lobbyists in the same or following year in each election cycle. Darker colors indicate a greater share. Nebraska (white) is excluded from the sample

proportion of representatives in each election cycle who leave office and subsequently register as lobbyists in each state. Darker shading indicates that golden parachute jobs are more common. The map reveals large differences between states.

The practice is most common in Texas, where on average 5.4 percent of sitting representatives take up a lobbying job in each election cycle. This may not sound like much at first. However, since an election cycle is only two years, it implies that more than a quarter of Texas representatives leave office and become lobbyists over the course of a decade. Also note that this is the probability *unconditional* on leaving office. Other studies and news stories often report the probability of becoming a lobbyist *conditional* on leaving office, which for the US Congress is between 30 and 50 percent. However, the unconditional probability at the federal level is roughly the same as in the state legislatures in which it is most common.²⁰

Other states in which movement from the state house to a lobbying position is roughly as common as at the federal level are Nevada (5.1

²⁰ Lazarus, McKay, and Herbel (2016) report that in 2008, roughly 60 of the 435 members departed the House of Representatives, so about 14 percent. Of these, around 40 percent subsequently registered as lobbyists. This implies an unconditional probability of 5.5 percent, which is roughly the same as in the Texas state legislature.

percent), Illinois (4.6 percent), and Arkansas (4.5 percent). There are four states in the data (Alaska, Connecticut, Kentucky, and South Dakota) in which no legislators registered as lobbyists in the year of leaving office or the next year during the period of observation.

5.4 CAMPAIGN FINANCE REGULATION AND GOLDEN PARACHUTE EMPLOYMENT

Is golden parachute employment part of the system of money in politics? If it is, I have argued that the *legal environment* should play an important role, since politicians arbitrage variation in how strictly the different forms are regulated. The crucial implication of this is that a change in regulation not only affects the type it applies to, it also has second-order consequences for other types. Here, I show that this is the case for *campaign finance regulation*.

The Impact of *Citizens United* on States' Campaign Finance Laws

The Supreme Court ruling that significantly changed American campaign finance started out as a relatively narrow lawsuit that challenged some provisions of the 2002 Bipartisan Campaign Reform Act (BCRA), also known as the McCain–Feingold Act. This law had imposed a number of restrictions on “electioneering communication,” which are TV or radio advertisements that target voters and refer to candidates for federal office, but do not explicitly advocate for or against their election. The BCRA mandated that such ads could not be funded by contributions from corporations or unions. It therefore imposed similar restrictions on electioneering communication as were already in place for “express advocacy” – campaign advertisements that directly advocate for or against the election of a specific candidate.

In 2008, the conservative nonprofit organization *Citizens United* produced a movie critical of Hillary Clinton and wanted to distribute it via TV on demand. Because the movie was classified as electioneering communication and had been financed by corporate donations, this was prohibited by the BCRA. The group thus challenged the ban on corporate and union spending on this type of communication, and the lawsuit made its way to the Supreme Court. After the oral argument, the court surprised litigants and observers by announcing that it would rehear the case. Instead of narrowly focusing on the provisions of the BCRA challenged by *Citizens United*, it would instead revisit “more than twenty

years of established campaign finance precedent” (Spencer and Wood, 2014, 327) and also review the ban on independent expenditures for express advocacy.

After the second oral argument, the court in early 2010 ruled with a narrow 5–4 majority that not only were the BCRA’s restrictions on electioneering communication unconstitutional but that those affecting express advocacy, which had been in place since 1973, were as well. This allowed corporations and unions to henceforth spend unlimited amounts of money on campaign advertisements, as long as this was done *independently* of candidates. In practice, however, the rules regulating the relationship between campaigns and organizations that carry out this independent spending, such as Super PACs or 501(c)(4) groups, are neither strict nor well enforced. For example, candidates can endorse and attend events organized by “their” Super PAC, which tend to be run by former staffers (Spencer and Wood, 2014; Dawood, 2015). There is also at least anecdotal evidence that politicians outsource much of their campaign activity to such groups, and that they explicitly solicit donations on their behalf.²¹

The Supreme Court ruling was met with widespread condemnation. President Obama alleged that it gave a “green light to a new stampede of special interest money in our politics.”²² A *New York Times* article stated that “[t]he Supreme Court has handed lobbyists a new weapon,”²³ and *Newsweek* called the ruling “the most serious threat to American democracy in a generation.”²⁴ The importance of the *Citizens United* decision was magnified three months later, when the US District Court for the District of Columbia additionally decided in *SpeechNow.org vs. FEC* that individuals as well as organizations were allowed to contribute unlimited amounts to groups that make independent expenditures. Taken together, these two rulings meant that lawmakers across the United States were suddenly confronted with dramatically looser campaign finance regulations.

The lawsuit in *Citizens United vs. FEC* only challenged federal law. However, the Supreme Court ruling by extension also applied to any

²¹ “Scott Walker, the John Doe Files and How Corporate Cash Influences US Politics,” *The Guardian*, September 14, 2016.

²² “Landmark Supreme Court Ruling Allows Corporate Political Cash,” *Reuters*, January 21, 2010.

²³ “Lobbyists Get Potent Weapon in Campaign Ruling,” *New York Times*, January 22, 2010.

²⁴ “Alter: The Roberts Court Radicals,” *Newsweek*, January 22, 2010.

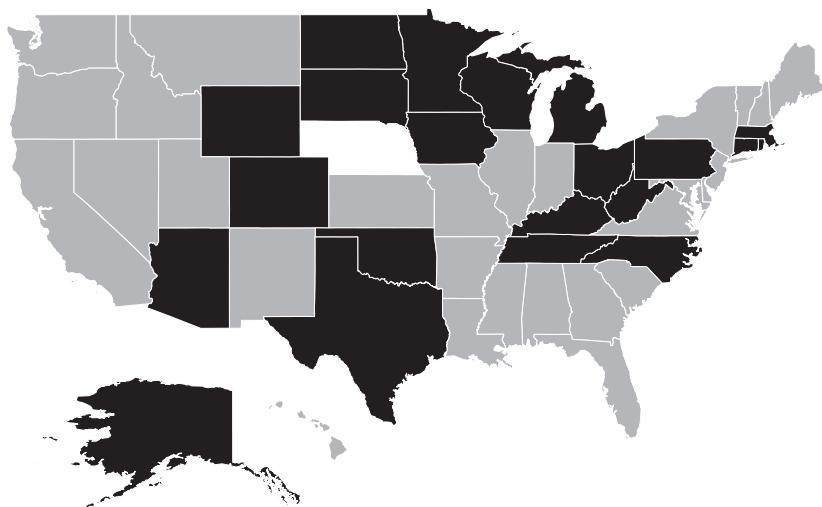


FIGURE 5.2 *Impact of Citizens United on the states.* States affected by the Supreme Court's *Citizens United* ruling in black; unaffected states in gray. Nebraska (white) is excluded from the sample

state laws that limited or prohibited independent expenditures by corporations or unions. In a stroke of good luck for social scientists, some states had laws on their books that were affected, whereas others never had any restrictions on independent campaign spending to begin with. Figure 5.2 shows the twenty-one states that were affected by the Supreme Court ruling. The relevant laws were either directly repealed by the legislatures or invalidated and not enforced by the states' campaign finance bodies. The other twenty-nine states did not have laws affected by the ruling.²⁵

This allows me to analyze the consequences of this ruling using a difference-in-differences design. I can examine how the variables of interest changed in states that were "treated" by the Supreme Court ruling compared to the "control group" of those that were not impacted. This makes it possible to isolate the effect of the change in campaign finance laws and separate it from secular trends in campaign expenditure and golden parachute employment.

²⁵ The results are robust to plausible alternative definitions of the set of treated states. For details, see Weschle (2021a).

Less Restrictive Campaign Finance Laws Increase Campaign Spending

The *first-order consequence* of the more permissive campaign finance regime after *Citizens United* should be an increase in campaign spending. And indeed, the ruling led to a stampede of money flowing into elections. In the pre-ruling 2008 election campaign, independent expenditures at the federal level totaled \$143.7 million. Four years later, post-ruling, they amounted to more than \$1 billion, about seven times as much. And in 2016, independent spending increased by a further 37 percent to \$1.38 billion.²⁶ This almost tenfold growth illustrates that there was a willingness to spend money on elections that had been kept in check by the laws the Supreme Court struck down (see also Hansen, Rocca, and Ortiz, 2015; Hansen and Rocca, 2019).

Of course, many other aspects of American politics changed from 2008 to 2016 that could have contributed to this trend. For example, wealthy individuals such as Charles and David Koch increased their political engagement, including through independent campaign spending (see e.g. Hansen, Rocca, and Ortiz, 2015; Mayer, 2016; Skocpol and Hertel-Fernandez, 2016). To address these potential confounding factors, prior studies have turned to state-level analyses, exploiting the fact that *Citizens United* only applied to some of them (see Figure 5.2).

Tracking independent campaign spending is difficult, as disclosure laws vary by state and many of them do not require even basic transparency.²⁷ Studies therefore have to work with subsets of states for which sufficient information is available. Spencer and Wood (2014) examine independent campaign spending in sixteen states between 2006 and 2010. They find that in 2010, it increased more where *Citizens United* struck down laws than in those where it had no impact. Abdul-Razzak, Prato, and Wolton (2020) compile data from eighteen states and extend the period from 2006 to 2012. They find that in states that were not affected by *Citizens United*, total outside spending increased by 5 percent from before the ruling to afterwards. In states that were affected and had restrictions struck down, outside spending rose by 54 percent. *Citizens United* had

²⁶ “Total Outside Spending by Election Cycle, Excluding Party Committees,” *Center for Responsive Politics*.

²⁷ In an assessment of the laws, the National Institute on Money in State Politics issued twenty-four states a failing grade. See “Scorecard: Essential Disclosure Requirements for Independent Spending, 2014,” *National Institute on Money in Politics*, December 3, 2014.

a larger impact on Republicans: independent spending on their behalf increased by 22 percent in unaffected states, and by 95 percent in affected states.

Prior studies have also found that this rise in campaign spending affected incumbents' reelection chances. Consistent with the asymmetric increase in spending for the two parties, the ruling led to an improvement in Republicans' electoral fortunes. In the lower chambers, removing the restrictions increased Republican candidates' chances of being elected by 3–4 percentage points (Klumpp, Mialon, and Williams, 2016; Petrova, Simonov, and Snyder, 2019; Abdul-Razzak, Prato, and Wolton, 2020). Note that this is the aggregate effect across all races, so it does not mean that only Republicans benefited from increased campaign spending on their behalf. As mentioned earlier, outside spending grew more in states affected by the Supreme Court ruling for *both* Democrats and Republicans, only more so for the latter (Abdul-Razzak, Prato, and Wolton, 2020).²⁸

Overall, then, loosening campaign finance regulation generated clear *first-order effects*: It increased campaign spending, especially for Republican Party candidates. If my argument is correct, the chain of events set in motion by *Citizens United* does not end here: The increase in campaign spending should be accompanied by a *decrease* in golden parachute employment, especially among Republicans. In the next section, I discuss the empirical approach that allows me to test whether such *second-order effects* indeed occurred.

Empirical Approach

Because *Citizens United* affected some states but not others, its impact on golden parachute employment can be estimated using a difference-in-differences design. The idea is to examine how such employment changed in states that were affected by the Supreme Court ruling (treatment group) compared to those that were not (control group). I compare the difference between the two before and after the intervention by estimating the following regression:

$$y_{ist} = \beta (\text{Ban}_s \times \text{Post-CU}_t) + \mu' Z_{st} + \lambda' X_{ist} + \gamma_s + \delta_t + \xi_{st} + \varepsilon_{ist}, \quad (5.1)$$

²⁸ These studies do not find that *Citizens United* had significant first-order effects on upper chambers, so I do not present those results here. Consistent with the previous findings, the ruling did not affect senators' propensity to become lobbyists (see Weschle, 2021a).

where i indicates a legislator, s a state, and t an election cycle. In the main specification, the dependent variable y_{ist} takes a value of one if a legislator leaves office and registers as a lobbyist in the same or the following year. I also estimate models with other dependent variables, which I discuss below. Because the decision of whether to leave politics and become a lobbyist is most relevant for legislators who are up for reelection, I restrict the analyses to incumbents who have reached the end of a legislative term and have to run again to stay in office.²⁹

Ban_s is a dummy variable indicating whether state s previously had a ban on independent corporate spending (see Figure 5.2), and $Post-CU_t$ is a dummy that takes the value of one for the post-*Citizens United* election cycles of 2010–2011 and 2012–2013. The quantity of interest is β . State and election cycle fixed effects are denoted by γ_s and δ_t . The former absorb any time-invariant differences between states, such as the size of the legislature or lobbyist registration laws. The latter absorb any common time shocks, such as national trends in partisan support.³⁰

There are a number of time-variant differences between states, which are denoted by Z_{st} . They are whether a state has a “cooling off” law that requires ex-politicians to wait for a specified period before they can register as a lobbyist, whether there are term limits, and whether the state has a public campaign finance system.³¹ A set of time-variant individual-level covariates is denoted by X_{ist} . They are the number of years a legislator has spent in office, whether his or her party controlled the legislature, and whether he or she held a speaker or party leadership position.

Difference-in-differences regressions require us to make a parallel-trends assumption. That is, we must assume that, absent *Citizens United*, the difference in the dependent variable between states that were affected by it and those that were not would have been the same after 2010 as it was before. To relax this stringent assumption, I include a set of state-specific linear time trends ξ_{st} (Angrist and Pischke, 2009).³² They account

²⁹ This results in a sample of 18,358 legislator-elections. The results are robust to including legislators who have not reached the end of a legislative term (see Weschle, 2021a).

³⁰ Together, the state and year fixed effects absorb the constituent terms of the interaction effect, Ban_s and $Post-CU_t$.

³¹ Sources: “Revolving Door Prohibitions,” *National Conference of State Legislatures*; “The Term-Limited States,” *National Conference of State Legislatures*; “Public Financing of Campaigns: Overview,” *National Conference of State Legislatures*.

³² There is evidence that the parallel-trends assumption may be violated when studying *Citizens United* (Klumpp, Mialon, and Williams, 2016; Petrova, Simonov, and Snyder, 2019; Abdul-Razzak, Prato, and Wolton, 2020). Therefore, the convention in all studies has been to include state-specific trends.

for secular trends that differ between states and may affect the dependent variable, such as their partisan composition or economic growth. Finally, ε_{ist} is the error term. Following the convention for difference-in-differences estimations with binary dependent variables, I use a linear probability model (see Angrist and Pischke, 2009). Parameter estimates are reported with robust standard errors clustered by state.

Less Restrictive Campaign Finance Laws Decrease Golden Parachute Employment

Table 5.1 displays the *second-order consequences* generated by removing campaign finance restrictions after *Citizens United*. It shows the results from twelve separate models that examine four dependent variables (rows) for three sets of observations (columns). In each case, I only provide the treatment effect and omit all controls for brevity.

First, I focus on the ruling's effect on all legislators combined (first column). In the first model, the dependent variable takes the value of one if a legislator leaves office and registers as a lobbyist in the same or the following calendar year. As predicted, the more permissive campaign finance regime had a clear and significant *negative* effect on politicians' propensity to take up a golden parachute job. Following the court ruling, legislators in the affected states were about 1.9 percentage points less likely to leave office and register as a lobbyist than they would have been otherwise. Given the baseline percentages in Figure 5.2, this effect is large in magnitude. Thus, the *first-order increase* in campaign spending after the relaxation of its regulation was accompanied by a *second-order decrease* in golden parachute employment.

The second and third panels in Table 5.1 examine the effect of more permissive campaign finance laws on the two different ways in which legislators can take up golden parachute employment: They can go straight into such a job without running for reelection (voluntary golden parachute), or they can take up a job after an election loss (insurance golden parachute).³³ For voluntary golden parachute employment, the dependent variable takes a value of one if the incumbent did not run for reelection at the end of their term (or resigned in the middle of it) and became a registered lobbyist in the year of leaving office or the next year. The effect of *Citizens United* is again negative, and its magnitude is similar to the one

³³ Paths 1 and 3 in Figure 3.1.

TABLE 5.1 *Effect of Citizens United on golden parachute employment.* Coefficients of treatment effect on the probability that a legislator will take up a golden parachute job (overall, voluntary, insurance) or retire without taking on a golden parachute job. Coefficients come from separate regressions. Controls not displayed

Golden Parachute	All	Republicans	Democrats
<i>Citizens United</i>	-0.019*** (0.007)	-0.027** (0.013)	-0.014** (0.007)
Voluntary Golden Parachute	All	Republicans	Democrats
<i>Citizens United</i>	-0.018** (0.007)	-0.023** (0.010)	-0.015* (0.008)
Insurance Golden Parachute	All	Republicans	Democrats
<i>Citizens United</i>	-0.002 (0.004)	-0.004 (0.007)	0.000 (0.006)
Non-Golden Parachute Retirement	All	Republicans	Democrats
<i>Citizens United</i>	0.041 (0.032)	-0.026 (0.043)	0.084* (0.045)

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. N = 18,358 for each model involving all legislators, N = 8,851 for Republican-only models, and N = 9,507 for Democrat-only models. All regressions include state and year fixed effects and state-specific time trends. State-level controls: Cooling off law, term limit law, public campaign finance. Individual-level controls: Years in office, chamber control own party, speaker or leader. Robust standard errors clustered by state in parentheses.

in the first regression, in which the dependent variable combined voluntary and involuntary golden parachute jobs. The Supreme Court ruling thus led to a decrease in the number of politicians who *voluntarily* left office to become lobbyists.

For the insurance path, the dependent variable takes a value of one if the incumbent ran for reelection at the end of their term and became a registered lobbyist the same year or the year after losing their seat. Here, the effect of *Citizens United* is close to zero, so it had no impact on politicians' propensity to become lobbyists after failing to win reelection. This null effect is not too surprising, since insurance golden parachute jobs can only be observed if an incumbent loses, and reelection rates in the

United States are typically over 90 percent. Thus, it is unlikely that the Supreme Court ruling would have much of an effect on the unconditional probability of using a golden parachute job as an insurance option.

Earlier, I cited evidence that the ruling increased independent campaign spending for both Democrats and Republicans, but more so for the latter (Abdul-Razzak, Prato, and Wolton, 2020). I therefore argued that we should see negative effects on golden parachute employment for legislators from both parties, but that the magnitude should be greater for Republicans. The second and third columns of Table 5.1 analyze the effect of *Citizens United* separately for the two major parties.

For overall golden parachute employment, *Citizens United* indeed had a larger effect on Republican representatives: they were 2.7 percentage points less likely to take up a golden parachute job in the states affected by the ruling than they would have been otherwise. For Democrats, the effect was negative and significant at conventional levels as well, but their probability only decreased by 1.4 percentage points. Thus, the impact of *Citizens United* on Democrats was only about half of the magnitude of what it was for Republican legislators.³⁴ The second panel shows that for both parties, the effect is driven by a drop in the number of incumbents who decided to take up a lobbying position instead of running for reelection. The third panel shows that there was no discernible effect on the share of legislators who resorted to their insurance option after an election loss. Overall then, and consistent with the theoretical predictions, the looser campaign finance laws had more of an effect on golden parachute employment among members of the party that benefited more from additional campaign spending.

Finally, the fourth panel shows the effect of *Citizens United* on retirement from office that is *not* followed by taking up a lobbyist position.³⁵ The point estimate for the pooled sample is positive and far from statistical significance. Looking at Republican legislators only, the point estimate is negative, but again statistically indistinguishable from zero. For Democrats, the effect is positive and significant at the 10 percent level, which indicates that they were more likely to leave office without getting a golden parachute job.

³⁴ Because each effect is somewhat imprecisely estimated, the difference between them is not statistically significant.

³⁵ The dependent variable takes a value of one if a legislator voluntarily left office and did not register as a lobbyist in the same or following year, and zero otherwise.

The Supreme Court ruling thus did not make legislators less likely to leave office in general; it only slowed down movement into golden parachute jobs. An interpretation consistent with this pattern is that it reflects heterogeneity in how important legislators are to special interests, and how much money they have access to as a consequence. In Chapter 3, I assumed that politicians can obtain a certain amount of money, and examined the various ways in which they use it. But of course, some politicians have more access to money than others.³⁶ For those with access to plenty of funding, removing restrictive campaign spending laws should affect whether they take up a golden parachute job. By contrast, legislators who are not on the radar of moneyed special interests benefit less from campaign spending and are unlikely to be offered a lucrative golden parachute job. Since restrictive campaign finance laws do not constrain them very much, making regulation more permissive should be of little consequence to such lawmakers.

And indeed, while we see a clear slowdown of movement into golden parachute jobs in both parties, this was not true for non-golden parachute retirement. Even among Republicans, who overall benefited more from the increased independent campaign spending, many legislators apparently did not expect enough of a shift in their electoral fortunes to warrant a change in their career paths. And a good share of Democratic incumbents seem to have anticipated that the looser campaign finance laws would disadvantage them, likely because they did not expect to benefit from greater spending.³⁷

Note that this interpretation contrasts sharply with the implication of the prevailing understanding of the Supreme Court ruling, which is that it opened the floodgates for overall spending on politics. If this were the case, and more campaign money was spent on behalf of everyone, all types of legislators would find the option of trying to stay in office more attractive. Instead, the ruling only resulted in a slowdown of golden parachute jobs, which provides evidence that more permissive campaign finance rules primarily lead to a “reallocation” of money: Incumbents with access to more money forgo a lucrative position as a lobbyist in favor of higher campaign spending on their behalf.³⁸ In other words, the

³⁶ See e.g. Fisman, Schulz, and Vig (2014); Fouirnaies (2018); Fouirnaies and Hall (2018); Weschle (2021b).

³⁷ In addition, given the overall partisan imbalance in spending increases, they might also have been more likely to run against a well-financed Republican.

³⁸ Note that this reallocation does not mean that the money has to come from the same financier in both scenarios.

view that looks at campaign spending in isolation and the system view I put forward in this book provide contrasting empirical predictions, and the evidence supports the latter.

Taken together, these results lend clear support for the argument that money in the forms of campaign spending and golden parachute jobs are part of a common system, and that their relative prevalence is influenced by the *legal environment*. Thus, more permissive campaign finance laws lead to a first-order increase in campaign spending, and trigger a second-order effect in the opposite direction by reducing the frequency with which incumbents leave office to take up employment in the private sector.

5.5 GOLDEN PARACHUTE REGULATION AND CAMPAIGN FINANCE

Given the growing prevalence of golden parachute employment in many countries, there have been increased attempts to rein in this practice by introducing additional legal restrictions. These regulations typically do not take the form of blanket bans on all private sector employment upon leaving office, as this would conflict with constitutionally guaranteed freedom of occupation provisions. Instead, they typically have a narrower target and prohibit former politicians from lobbying the legislature for a certain amount of time.

In this vein, many US states have introduced *cooling off laws* that mandate a waiting period, usually between six months and two years, before former politicians can register as lobbyists. According to the system argument put forward in this book, this should not only slow down golden parachute employment, it should also have second-order effects that increase campaign money. I now test whether this is indeed the case.

Cooling Off Laws in the US States

Figure 5.3 shows the twenty-one states that introduced cooling off laws by 2012.³⁹ California was the first state to implement such a law in 1991. Once regulation is introduced, it tends to not be reversed. The only exception to this is Ohio, whose law a court struck down in 2010.

³⁹ To determine the relevant dates of these policies, I used information on the laws provided in “Revolving Door Prohibitions,” *National Conference of State Legislatures*, and researched their history. Following the convention in the literature, I recorded the starting point as the year they first applied to legislators.

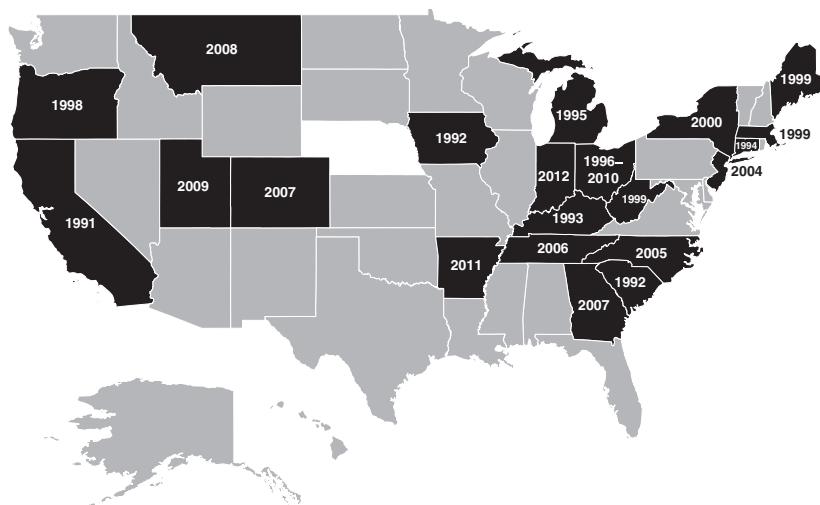


FIGURE 5.3 *Cooling off laws in the states.* States that introduced such laws in black, with year of introduction

I exploit the staggered introduction of the cooling off regulations in a difference-in-differences research design. I compare changes to golden parachute employment and campaign spending patterns in states that introduced or abolished such laws to those that did not. I first show that the laws do indeed have the intended first-order consequence: they reduce golden parachute employment. Then, I demonstrate that as a second-order consequence, they also change the pattern of campaign spending.

Cooling Off Laws Decrease Golden Parachute Employment

A waiting period makes it more difficult for a legislator to move into a lobbying position in the private sector. The straightforward first-order implication of such a regulation is that when states introduce a cooling off law, fewer politicians should accept golden parachute jobs.

My data on golden parachute employment in the states covers the years 2006 to 2013. Thus, I can test the first-order consequences of cooling off laws, exploiting the fact that a subset of states introduced them during that period,⁴⁰ and that one state (Ohio) rescinded its law. In the analysis of the consequences of *Citizens United*, one of the controls in Equation (5.1) was whether the state had a cooling off law. Table

⁴⁰ These states were Arkansas, Colorado, Georgia, Indiana, Montana, and Utah.

TABLE 5.2 *Effect of cooling off laws on golden parachute employment.* Coefficients of the effect of a law requiring a cooling off period on the probability that a legislator will take up a golden parachute job (overall, voluntary, insurance). Coefficients come from separate regressions. Controls not displayed

Golden Parachute	All	Republicans	Democrats
Cooling Off Law	-0.047* (0.025)	-0.063** (0.030)	-0.024 (0.031)
Voluntary Golden Parachute	All	Republicans	Democrats
Cooling Off Law	-0.049*** (0.018)	-0.062** (0.025)	-0.027 (0.026)
Insurance Golden Parachute	All	Republicans	Democrats
Cooling Off Law	0.001 (0.009)	-0.001 (0.011)	0.003 (0.007)

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. N = 18,358 for each model involving all legislators, N = 8,851 for Republican-only models, and N = 9,507 for Democrat-only models. All regressions include state and year fixed effects and state-specific time trends. State-level controls: Citizens United, term limit law, public campaign finance. Individual-level controls: Years in office, chamber control own party, speaker or leader. Robust standard errors clustered by state in parentheses.

5.2 displays the coefficients of this variable, which can be interpreted as difference-in-differences estimates.

The first panel shows the first-order impact of cooling off laws on overall golden parachute employment. As expected, these provisions have a negative effect on legislators' propensity to go into the private sector. The share of legislators who become lobbyists drops by almost 5 percentage points. The second and third columns show that this overall effect is mainly driven by Republican legislators, who are more likely to hold golden parachute jobs in the first place. Consequently, after cooling off requirements are introduced, Republican lawmakers are 6.3 percentage points less likely to become a lobbyist. The point estimate for Democrats is also negative, but smaller in size and not statistically significant.⁴¹

⁴¹ Because the cooling off laws changed during the period of observation in only seven states, the effects for Republicans and Democrats are somewhat imprecisely estimated. As a consequence, the difference between the two point estimates is not significant.

The second and third panels distinguish between the impact of cooling off laws on voluntary and insurance golden parachute employment. The overall effect is driven by changes to the former. The point estimates are about the same size as in the first panel. By contrast, the restrictions do not have a discernible impact on movement into the private sector after a lost election. Taken together, Table 5.2 clearly shows that cooling off laws have the expected *first-order consequences*.

Empirical Approach

But what about second-order effects? When restrictions on golden parachute employment are introduced, representatives who would have left office to become lobbyists are instead more likely to run for reelection, so they solicit campaign contributions when they otherwise would not have done so. In particular, because incumbents benefit from instrumental donations in a way that candidates in an open-seat election do not (cf. Fouirnaies and Hall, 2014), we should see an increase in campaign contributions when states introduce cooling off laws. Given that Republicans are more affected by those bans, the impact should be larger for them.

To test whether this is the case, I analyze campaign money in races for state lower houses between 1990 and 2012.⁴² I remove all donations from individuals and party organizations, as the underlying motivation for them is electoral or expressive, so my argument does not apply (see Chapter 3).⁴³ I estimate a difference-in-differences specification that uses the staggered timing of the introduction and abolition of the cooling off laws to identify their impact on donation patterns. The regression compares how donations change when states introduce (or abolish) waiting periods to states that do not alter their regulations. It takes the following form:

$$y_{dst} = \beta \text{ Cooling Off Law}_{st} + \mu' Z_{st} + \gamma_s + \delta_t + \xi_{st} + \varepsilon_{st} \quad (5.2)$$

I look at three dependent variables. The first two are the respective logged total amounts given to Republican and Democratic candidates in district d in state s for election t .⁴⁴ Given the larger drop in golden parachute employment for Republicans after cooling off laws are introduced, the effect is expected to be clearly positive for them. Democrats are less

⁴² Data were compiled by Bonica (2016).

⁴³ See Hall (2014) for a similar approach.

⁴⁴ I add 100 before taking the log.

affected by golden parachute regulation (see Table 5.2), so we would not expect a significant change in their campaign contributions. Finally, the third dependent variable is the *share* of money raised by Republican candidates in a district. This provides an easily interpretable effect of golden parachute regulation on campaign money.

The independent variable of interest is a binary indicator of whether a state had a cooling off law or not, and the coefficient of interest is β . As state-level controls in Z_{st} , I include whether the state had a system of public campaign finance, whether it had term limits, and whether the state bans corporate and union campaign spending (both direct contributions and indirect independent expenditures).⁴⁵ A set of state fixed effects is denoted by γ_s , and δ_t is a set of year fixed effects.

The claim for the exogeneity of the introduction of cooling off laws at the state level is certainly weaker than in the case of *Citizens United*. However, it is important to keep in mind that I am interested in the second-order consequences of these policies. Cooling off laws are unlikely to be passed in anticipation of changes in campaign money. Nevertheless, I again include a set of state-specific time trends ξ_{st} that can account for some differing trends between the states before such laws are passed. Parameter estimates are reported with robust standard errors clustered by state.

Cooling Off Laws Increase Campaign Spending

Table 5.3 reports the effect of cooling off laws on campaign money. As predicted, the value of donations given to Republicans significantly *increases* after restrictions on golden parachute employment are introduced. Republican candidates receive an average of \$32,000 before a cooling off law is introduced. Given the estimated coefficient, this is expected to increase to \$42,300 afterwards, which represents an increase of more than 30 percent. At the upper end of the campaign contribution distribution, the coefficient implies very large substantive effects: For a district in the 95th percentile of Republican campaign money, the expected effect is an increase from about \$137,750 to \$182,079, so almost \$45,000 more.

The first-order effect of cooling off laws on Democrats' likelihood of becoming lobbyists was small, so we would also expect a less pronounced impact on their campaign money. And indeed, the second column in

⁴⁵ Data for spending and contribution bans are taken from La Raja and Schaffner (2014).

TABLE 5.3 *Effect of cooling off laws on campaign money.* Coefficients of the effect of a law requiring a cooling off period on campaign money. Coefficients come from separate regressions. Controls not displayed

Campaign Contributions	<i>Log Amount Republicans</i>	<i>Log Amount Democrats</i>	<i>Share Republicans</i>
Cooling Off Law	0.279*** (0.115)	-0.102 (0.133)	0.045*** (0.013)

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. N = 35,996 for log amount models, N = 35,952 for share model. All regressions include state and year fixed effects and state-specific time trends. State-level controls: term limit law, public campaign finance, ban on union campaign contributions, ban on corporate campaign contributions, ban on union independent spending, ban on corporate independent spending. Robust standard errors clustered by state in parentheses.

Table 5.3 shows an effect that is not significantly different from zero. The point estimate is negative, but small in magnitude: The average amount of contributions to Democrats in a district is about \$35,000, which is expected to decrease to \$31,600 after the introduction of golden parachute restrictions.⁴⁶

Finally, the third column shows what this implies for the *share* of campaign money going to Republican candidates. A cooling off law leads to a 4.5-percentage-point shift toward them. Given that on average about 48 percent of donations are made to Republicans, the magnitude of this effect is substantial.

Table 5.3 thus yet again provides evidence that legal changes with respect to *any* form of money in politics generate a *second-order effect*: Making it more difficult for politicians to earn money after leaving office has downstream consequences for campaign money. Thus, for both campaign spending and golden parachute employment, the *legal environment* affects the form it regulates as well as the other type, again pointing to the importance of studying them as part of a common system.

5.6 REDISTRICTING AND GOLDEN PARACHUTE EMPLOYMENT

Finally, I investigate the impact of the *electoral campaign environment*. In the previous chapter, I demonstrated that incumbent politicians in India

⁴⁶ The difference in the effects for Republicans and Democrats is significant at the 1 percent level.

and Brazil make different decisions about whether to allocate resources to campaign spending or to engage in personal enrichment, depending on how competitive their reelection races are likely to be. The same argument should also apply to the choice between campaign spending and golden parachute employment. In particular, in Chapter 3 I hypothesized that lower electoral security makes incumbents more likely to take up a golden parachute job. Here, I test this proposition by using the extent of redistricting an incumbent was subject to following the 2010 census as a shock to their competitiveness.

The 2010 Redistricting Process

Members of the US House of Representatives and the lower state houses are elected in first-past-the-post constituencies. The size of these constituencies changes over time due to migration as well as differential birth and death rates. They are therefore adjusted every ten years following a population census. Within each state, the boundaries of the districts for both the federal and state legislatures are redrawn to be approximately equal in population size.

The more an incumbent's district is changed, the more the relationships and reputation he or she has accumulated over the years disappears. For example, former Illinois Representative Timothy Johnson described his thoughts upon finding out that he would lose many of his constituents due to his district being redrawn: "That is agony, I'll tell you. I thought: All these relationships! All these friendships! All this service!"⁴⁷

In some states, redistricting is performed in a non-partisan manner by commissions or courts, as it is in India. But in other states, legislatures are in charge of the redistricting, which can be conducted in a highly politicized manner. Following the 2010 census, parties in some states used fine-grained geographical data and sophisticated computer programs to create gerrymandered districts to increase their electoral chances (Chen and Rodden, 2015; Chen, 2017). However, this was not the case in many other states, and the overall effect of redistricting on partisan outcomes has been modest (Chen and Cottrell, 2016).

In addition, even if the redistricting procedure is highly politicized in some states, it is largely exogenous to most individual legislators. The idea behind gerrymandering is to advance one party by using "cracking and

⁴⁷ "Illinois Congressman Johnson May Be Thwarted in Bid to Call His Constituents," *Washington Post*, June 22, 2011.

packing” techniques (cf. Issacharoff and Karlan, 2004). For the former, boundaries are drawn such that supporters of the opposition party are scattered over many districts, and represent a minority in each. In the latter, supporters of the opposition are concentrated in one district where they form an overwhelming majority, leading to wasted votes that are missing in other districts. To the extent that the authors of the 2010 redistricting plans engaged in gerrymandering, they did so to benefit the chances of their party *as a whole*.

I focus here on how district changes affected incumbents’ *individual* career decisions. In state-level lower chambers, districts are relatively small and campaigns are based more heavily on personalities and personal contact than in federal races. Losing a large part of one’s constituents means losing relationships and goodwill accumulated over a period of years, even if the new district has a favorable demographic makeup. Because the redistricting plans are usually drawn up by a group of legislators with the support of outside expertise, and because the goal of gerrymandering is to benefit a party as a whole, individual legislators are unlikely to be able to influence how much of their district they get to keep.

Data on the Extent of Redistricting

To analyze the impact of redistricting on politicians’ career paths – particularly whether they take up a golden parachute job – I again use the data on state representatives and whether they leave office and register as a lobbyist. The one piece of new information needed is the amount of redistricting that each incumbent was exposed to. To create this variable, I adapt a procedure developed by Crespin (2005, 2010) for the state level.

I overlay the old and new district maps with census tract population data from 2010 and estimate how the population that resides in the old districts is distributed into the new ones.⁴⁸ To capture how much redistricting an incumbent is subject to, I calculate the minimum share of voters they lose following redistricting. For example, if 60 percent of the voters from an old district are moved to a new district A, 30 percent to a new

⁴⁸ At the state level, not all census tracts are wholly contained within districts. If they are not, I take the population that is fifteen and older and uniformly distribute it across a census tract, which allows me to approximate how the population of an old district was divided up into new districts. I use the population of fifteen and older rather than eighteen and older since census population data is released in five-year brackets.

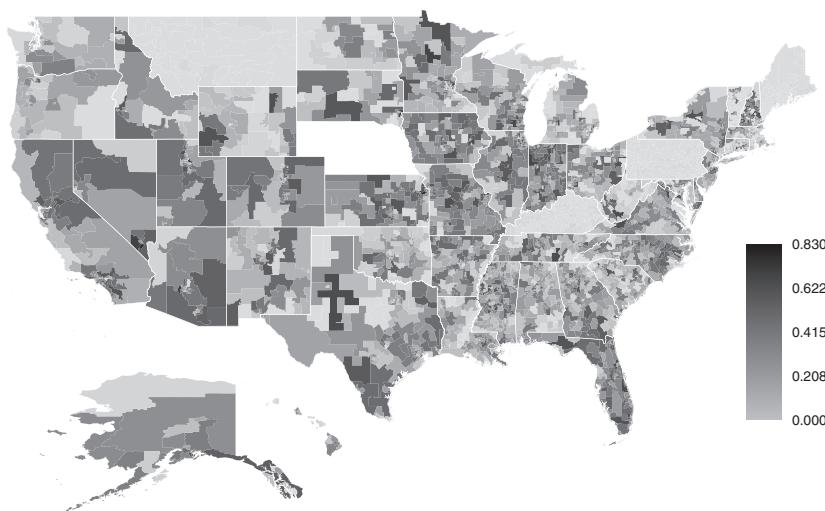


FIGURE 5.4 *Map of redistricting shares after 2010 census.* Darker colors indicate more redistricting. Nebraska (white) is excluded from the sample

district B, and the remaining 10 percent join a new district C, then the incumbent representing the old district can retain at most 60 percent of their constituents. Or, put the other way around, they lose at least 40 percent, resulting in a redistricting share of 0.4. The higher this number, the more an incumbent is affected by redistricting. If there are no changes to their constituency, the number is zero.

Figure 5.4 maps the extent of redistricting for the 2012 elections across 4,904 constituencies in forty-nine state lower houses (again excluding Nebraska). Around 27 percent had a redistricting share of 0.05 or less. The median district has a score of around 0.2, which indicates that half of the incumbents were able to keep up to 80 percent of their constituents. Some states experienced no or very little change, such as Kentucky, Maine, Montana, and Pennsylvania. These states did not implement new boundaries in time, in some cases because courts rejected the original plans, so elections were still held using the old districts. The most extensive redistricting occurred in the fast-growing state of Nevada, where the median state legislator lost about half of their constituents. Florida, Missouri, and Indiana follow at some distance. There is also considerable variation *within* states. For example, there are numerous black regions (indicating a lot of redistricting) right next to light gray regions (indicating little redistricting) in Texas, Minnesota, and West Virginia.

Empirical Approach

To estimate the effect of the extent of redistricting on golden parachute employment, I restrict the sample to the 2012–2013 election cycle. This was the first wave of elections conducted with the new boundaries. I estimate the following regression:

$$\text{logit}(\Pr(y_{is} = 1)) = \alpha + \beta \text{ Redistricting Share}_{is} + \mu' Z_s + \lambda' X_{is} + \varepsilon_{is}, \quad (5.3)$$

where i indicates a legislator and s a state. The dependent variable y_{is} again takes a value of one if a legislator leaves office and registers as a lobbyist in the same or the following year. Because this is a binary variable and I have a straightforward cross-sectional research design instead of a difference-in-differences approach, I use a logistic regression.

The main independent variable is the redistricting share displayed in Figure 5.4. A value of zero indicates that the district is exactly the same in 2012 as it was in previous elections. The higher the value, the larger the share of constituents an incumbent has lost. The coefficient of interest is β , which is expected to be positive.

State-level control variables, denoted Z_s , are whether the state had cooling off laws, term limits, and a public campaign finance system. Individual-level covariates, denoted X_{is} , are again the number of years a legislator has spent in office, whether his or her party controlled the legislature, and whether he or she held a speaker or party leadership position. Parameter estimates are reported with robust standard errors.

Note that Equation (5.3) does not contain state-level fixed effects. Thus, it estimates the effect of variation in redistricting within as well as between states. I also present results from models with state fixed effects (so α becomes α_s), which only analyze variation within states. Note, however, that this drastically reduces the number of cases, since it drops all legislators from states in which the 2012 districts remained unchanged.

More Redistricting Leads to More Golden Parachute Employment

What is the effect of being subject to more redistricting on legislators' propensity to take up a golden parachute job? Table 5.4 shows the coefficients for the redistricting share, estimated from separate models. The first panel displays the results from regressions without state fixed effects. The first column, which combines legislators from both parties, shows that there is a positive effect, which is significant at the 1 percent level. To ease interpretation of the non-linear logistic regression models, the first row of

TABLE 5.4 *Effect of redistricting on golden parachute employment.*
 Logistic regression coefficient of redistricting share on the probability
 that a legislator will take up a golden parachute job. Coefficients come
 from separate regressions. Controls not displayed

Without State Fixed Effects			
Golden Parachute	All	Republicans	Democrats
Redistricting Share	1.527*** (0.623)	1.351** (0.623)	1.854* (1.051)

With State Fixed Effects			
Golden Parachute	All	Republicans	Democrats
Redistricting Share	1.217 (0.822)	0.889 (0.822)	1.788 (1.590)

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. For models without fixed effects: N = 4,831 (all), 2,640 (Republicans), 2,191 (Democrats). For models with fixed effects: N = 2,793 (all), 1,176 (Republicans), 1,083 (Democrats). State-level controls (for models without state fixed effects): Cooling off law, term limit law, public campaign finance. Individual-level controls: Years in office, chamber control own party, speaker or leader. Robust standard errors in parentheses.

Figure 5.5 shows the predicted probability of golden parachute employment as a function of the redistricting share.⁴⁹ The first panel makes clear that the more constituents an incumbent loses, the more likely they are to accept a golden parachute job in the lobbying sector. The (unconditional) predicted probability rises from around 1 percent when a politician's district remains unaltered to around 3 percent at the sample maximum, so the effect of redistricting is substantial.

The second and third columns of the first panel in Table 5.4 analyze the impact on incumbents from the two parties separately. While the point estimate is slightly larger for Democrats than for Republicans, the estimation uncertainty is greater for them as well. The second and third panels of the first row of Figure 5.5 show that the substantial impact is roughly similar for both groups. Given that redistricting affected legislators in both parties at a relatively similar rate, this was to be expected.⁵⁰

The second panel in Table 5.4 displays the results from models with state fixed effects. Because their inclusion means that all legislators from

⁴⁹ For the predicted probabilities, control variables are set to the sample median.

⁵⁰ The average redistricting share is 0.26 for Republicans and 0.22 for Democrats.

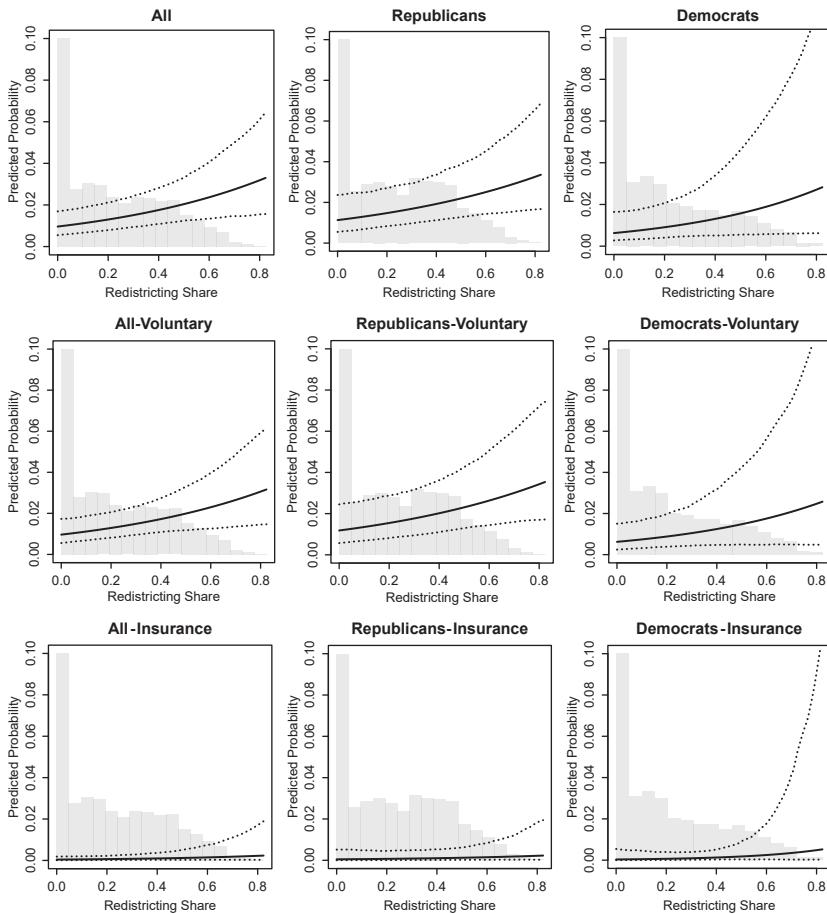


FIGURE 5.5 *Effect of redistricting on golden parachute employment.* Predicted probability of taking up a golden parachute job overall (top row), voluntarily (middle), and after a lost election (bottom) depending on the redistricting share. Point estimate and 95 percent confidence intervals. Density of the redistricting share in the background

states in which there was no redistricting are dropped, estimation uncertainty increases. However, the coefficients are similar to the models without state fixed effects.

Finally, I break down the effect by looking at the two paths of taking up golden parachute employment separately, focusing on a graphical presentation for brevity. The middle row of Figure 5.5 shows the predicted probabilities of leaving office voluntarily and subsequently registering as

a lobbyist, using the specification without state fixed effects. Redistricting clearly increases the probability of voluntary golden parachute employment, for all legislators together, as well as when examining Republicans and Democrats separately. The bottom row shows the effect of redistricting on insurance golden parachute employment.⁵¹ In all three models, the slope of the line is nearly flat. Given that we only observe insurance golden parachute employment if an election is lost, and reelection rates are high, this is not surprising. When estimating models for voluntary and insurance golden parachute employment with state fixed effects, the point estimates are similar, but again with larger confidence intervals due to the smaller number of cases.

Thus, politicians' likelihood of leaving office to take up golden parachute employment depends on their *electoral campaign environment* as well. If incumbents suffer a shock to their competitiveness, they are less inclined to run for reelection (and thus use campaign money), and instead are more likely to leverage their position for a lucrative private sector job.

5.7 SUMMARY

In this chapter, I have continued testing the theoretical argument laid out in Chapter 3. The focus was again on the connection between money used by politicians to improve their chances of staying in office and money used to improve their personal financial situation, only this time after leaving office. I have assembled a new dataset that for the first time makes it possible to track golden parachute employment in multiple legislatures, which made a comparative examination possible.

I provided evidence that the relative prevalence of campaign spending and golden parachute jobs is driven by the two key factors I have highlighted, and that a change in one form of money leads to second-order effects on the other. For the *legal environment*, I first used changes in campaign finance legislation to show that this affects how common golden parachute employment is. Second, I demonstrated that stricter regulation of golden parachute lobbying leads to changes in the pattern of campaign contributions.

Legislators' decisions about whether to take up golden parachute employment are also influenced by the *electoral campaign environment*.

⁵¹ Due to collinearity issues, I drop the following control variables for the model estimating the effect of redistricting on insurance golden parachute jobs for Democrats: public campaign finance, chamber control own party, speaker or leader. The results are similar when including these variables, although the sample size drops dramatically.

Exploiting variation in the amount of redistricting that incumbents experienced, I have demonstrated that they are more likely to take up golden parachute employment if their reelection chances are lower.

Taken together, these analyses show that campaign spending and golden parachute employment are directly connected to each other. Changes to one form lead to opposing changes in the other. This mirrors the findings from India and Brazil. Thus, these last two chapters have used microlevel data to demonstrate that the three main forms of money in politics are part of a common system, and that the legal and electoral campaign environments affect how politicians use money.

But does the argument also hold at a larger scale? Can it explain cross-national patterns in whether politicians use money primarily for self-enrichment, as campaign spending, or in the form of lucrative jobs? I examine this question in the following chapter.