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Essays on Campaign Finance and Political Power

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Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it). The copyright of this thesis rests with the author. Quotation from it is permitted, provided that full acknowledgement is made. This thesis may not be reproduced without the prior written consent of the author. I warrant that this authorisation does not, to the best of my belief, infringe the rights of any third party.

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Statement of conjoint work

I confirm that Chapter 3 was jointly co-authored with Andrew B. Hall (Stanford University), and I contributed 50% of this work. Regarding the new datasets collected for the analyses presented in Chapters 4 and 5, I can confirm that the following people helped me convert PDF files into Excel files: Elysa Geras, Saurabh Sharma, and Janet Dungog.

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Abstract

This thesis is concerned with the influence of campaign finance on the interplay between political power and electoral competition in the United States and the United Kingdom. The thesis considers both the donation and expenditure sides of campaign finance: In the context of U.S. state and federal legislative elections (1980-2014), I study how political power affects the allocation of campaign contributions, and in the context of U.K. House of Commons elections (1885-2010), I examine how campaign spending restrictions affect political power via electoral behavior. The three papers which make up the construct of the thesis answer the following questions:

(i) What is the financial value of incumbency status, and who generates it?

(ii) Who values legislative agenda setters, and why do they do so?

(iii) What are the electoral consequences of statutory limits on campaign expenditure?

I argue that campaign donors make their contributions to powerful politicians in exchange for access to the policy-making process, and that the power of these politicians is sustained, at least in part, due to these contributions.

In the first paper, I document that U.S. incumbent legislators enjoy sizeable financial advantages compared to challengers, and I demonstrate that this advantage is the result of donations from access-seeking industries. In the second paper, I show that U.S. legislators who are institutionally endowed with agenda-setting powers are given special treatment by campaign donors. I document that donors with vested economic interests in regulatory policy place great value on agenda-setting legislators – in particular when institutions provide these legislators with the authority to block new legislation. In the final paper, I study the consequences of campaign spending limits in the context of U.K. House of Commons elections. I show that unrestrained spending reduces electoral

competition, promotes professionalized campaigns, and benefits incumbents and center-right parties.

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Chapter 1

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Chapter 2

Introduction

A free and fair election process to select public officials is the defining feature that separates representative democracy from other forms of government. Through periodic elections citizens are given the opportunity to select, according to their free opinion, the most qualified public officials, in terms of talent, experience, social skills, policy preferences, etc., to represent the will of the people. Elections may also serve to incentivize public officials to exert effort on behalf of their constituents once they are elected.

In the United States and the United Kingdom, as well as in most other Western democracies today, elections are generally free and fair in the sense that the administration of the electoral process is not inherently corrupt and that citizens are not systematically disenfranchised or prohibited from running for public office on arbitrary grounds. De jure, all citizens officially enjoy the same basic constitutional rights and are politically equal according to the principle of *one man, one vote*.

De facto, however, political power and influence are by no means uniformly distributed among members of society. As discussed at length by classical elite theorists, such as Michels (1915) and Pareto (1935), economic and political capital tend to be concentrated in the hands of a few.

In the traditional pluralist view of representative democracy, political inequality may not necessarily be a major cause for concern. According to Dahl (1961: p. 89), the distribution of power in the U.S., and, by extension, other modern liberal democracies, can be characterized by a "pattern of dispersed rather than cumulative inequalities" in which every societal group "has access to some resources that it can exploit to gain influence". The decentralized nature of liberal democracies ensures that the advantages

enjoyed by a group in one societal sphere will be offset by the disadvantages in another sphere. All groups have the freedom to voice their concerns; if an issue is important enough for citizens to care about strongly, they will unite and jointly put pressure on the political system (Bentley 1908; Truman 1951).

The pluralist belief that all groups in society have access to some political influence, albeit through different channels, rests on the assumption that all vital common interests can and will be voiced through the pressure system. This idea was theoretically critiqued by Olson (1965) in his discussion of collective action problems faced by large groups in which everybody has a common interest in a public good, but no one is willing to incur the cost of providing it. Similarly, the pluralist understanding of democracy was empirically critiqued by Schattschneider (1960). In perhaps the most famous quote in the literature on organized interests, Schattschneider claims that,

> The flaw in the pluralist heaven is that the heavenly chorus sings with a strong upper-class accent. (Schattschneider 1960: p. 35)

Schattschneider's main argument is that the pressure system is biased in favor of groups representing business interests and of the well-off, and against groups representing broader public interests and disadvantaged citizens (Schlozman, Verba, and Brady 2012).

This thesis extends the literature on political power and special interest group politics building on the work of Schattschneider (1960). To use his metaphor, the goals of this thesis are, at the very broadest level, to characterize who sings in the heavenly chorus, and to understand why some voices ring out more clearly than others. The ambition is to improve our understanding of the interaction of special interest groups with the insiders and outsiders of political elites. In the next sections of this introduction, I outline how I intend to implement this in practice.

2.1 Campaign Finance and Political Elites

Special interest groups may try to seek influence in many different ways. The set of possible activities ranges from irreproachable actions, such as drafting reports, and producing objective, impartial analyses of policy consequences, to lobbying via various networks and contributing to political campaigns; from the grey area of helping policy

makers to draft and amend policy proposals, to outright corrupt and illegal activities, such as buying votes on a quid pro quo basis.

In this thesis, I focus on campaign finance. Campaign finance is by no means the only way by which groups attempt to influence policy, but it is a tangible and quantifiable resource that elected public officials appear to value. In all likelihood, campaign contributions represent only the visible tip of an iceberg of attempts to influence public policy, suggesting that any documented effect of campaign finance may represent only a lower bound on the true attempts at influence.

In the 2014 electoral cycle in the U.S., legislative candidates running for office at the state and federal levels raised more than 1.3 and 2.1 billion U.S. dollars, respectively.[1] In 2014, political parties at the national level in the U.K. raised more than 65 million British pounds, and it may be presumed that this amount increased in the months leading up to the general election in 2015. Individual candidates and local party organizations solicited considerable amounts, as well.[2]

As dryly noted by Lewis (1998), such amounts are not raised in bake sales. Rather, most of that money is donated by wealthy individuals, firms, labor unions, and interest groups. Legislators apparently place great import on these contributions: They spend hours on the phone soliciting donations and participating in other fundraising activities, and generous campaign donors are given privileged access when scheduling meetings with them (Kalla and Broockman 2015).

Whether or not campaign finance poses a fundamental threat to representative democracy depends on the reason donors give money to political campaigns, and what consequences their contributions may have. On the one hand, it might be cause for concern if special interest groups, who represent partial, unbalanced or extreme policy preferences relative to the median voter, receive favourable political treatment by elected officials in exchange for their campaign contributions at the expense of broader public interests. The severity of the issue would be compounded if the advantages generated by these donations systematically reduced electoral competition and thus limited the opportunity for constituents to electorally punish poorly performing public officials.

[1] Source (federal): http://www.followthemoney.org/show-me?y=2014&f-core=1&f-fc=1&c-exi=1. Source (state): http://www.followthemoney.org/show-me?y=2014&f-core=1&f-fc=2&c-exi=1

[2] http://www.independent.co.uk/news/uk/politics/generalelection/general-election-2015-explained-who-finances-the-parties-who-gets-the-most–and-how-much-does-the-campaign-cost-10186008.html

On the other hand, campaign contributions to powerful legislators would be less of a concern if the favored politicians were elected to office or appointed to their leadership positions simply because they were more qualified than other candidates.

2.2 Research Question and Main Argument

To evaluate the quandary around the normative issues outlined above, one needs to accurately understand how campaign finance influences the interplay between political power and electoral competition. The purpose of this thesis is to provide a solid empirical foundation upon which such an evaluation can be made. The fundamental research question that I address in the thesis is:

> How does political power affect the distribution of campaign contributions, and how does campaign spending affect the distribution of political power through electoral competition?

To answer such a wide and comprehensive question in a precise and meaningful way, I narrowly define and operationalize the key concepts.

The main focus in this thesis is on formal political institutions. When I refer to the distribution of political power, it denotes the allocation of political privileges associated with official positions. Obviously, power has many different faces, and, while the approach taken in this thesis does not account for political power exercised through informal institutions, it does reflect an important dimension of power and can be applied in a tractable, simple way. In particular, I am interested in how political power is distributed among insiders and outsiders of the elite along two dimensions: Within the group of candidates running for office, I examine the difference between incumbents and challengers; within the group of incumbent legislators, I am interested in the difference between rank-and-file legislators and legislators, such as party leaders, committee chairs, and majority party members, who are institutionally endowed with certain parliamentary agenda-setting privileges.

Similarly, when I study how campaign spending affects electoral competition, my main focus is on a political institution: The statutory legal maxima on candidate campaign expenditure. Campaign spending may affect the distribution of power in a variety of ways, but instead of trying to answer the question in general terms, I narrowly study how ceilings on total campaign spending affect electoral behavior.

This discussion leads to three specific questions that I address in Chapters 3, 4 and 5, respectively:

(i) What is the financial value of incumbency status, and who generates it?

(ii) Who values legislative agenda setters, and why do they do so?

(iii) What are the electoral consequences of statutory limits on campaign expenditure?

Geographically, I focus exclusively on the U.S. and the UK. Besides the obvious value of studying each of these countries in its own right, I have chosen them as cases in part due to the availability of necessary data, and in part because key aspects of the campaign finance institutions in these countries have been implemented throughout the democratic countries of the world. The disclosure requirements in the U.S. enable me to study the inflow of money from donors to candidates (the contributions side examined in Chapters 3 and 4), whereas the U.K. disclosure rules allow me to scrutinize the outflow (the expenditure side explored in Chapter 5). Of course, there are many systemic differences between the countries' political institutions, but I do not claim that the findings pertaining to one country can be generalized to the other. Rather, I study the U.S. and U.K. political institutions as separate entities and only discuss the extent to which any conclusions can be generalized.

The main argument presented in this thesis is simple: Campaign donors contribute money to the insiders of powerful political elites in exchange for access to the policy-making process, and the power of this elite is sustained, at least in part, because of these contributions.

I empirically substantiate both the inflow and outflow sides of this argument. First, I establish that campaign contributions flow to the insiders of powerful political elites in Chapters 3 and 4. In these chapters, I study campaign finance in the context of U.S. federal and state legislatures. In Chapter 3, I document that incumbent legislators enjoy sizeable financial advantages compared to their challengers, and I demonstrate that this advantage is, to a certain degree, a result of donations from access-seeking industries. In Chapter 4, I show that legislators, such as party leaders, committee chairs, and majority party members, who are institutionally endowed with agenda-setting powers, are given special treatment by campaign donors relative to that given to other elected legislators. I document that donors with vested economic interests in regulatory policy place great

value on agenda setters – in particular when institutions allow these legislators to block new legislation. Second, in Chapter 5, I turn to the claim that campaign finance helps to sustain the power of political elites. In this chapter, I study the electoral consequences of campaign spending limits in the context of U.K. House of Commons elections. I show that unrestrained spending reduces electoral competition, and benefits incumbents and parties, who have notable connections to businesses and wealthy individuals.

2.3 New Data

I base my empirical claims on data from various sources, as well as from the development of two new datasets. The campaign finance data pertaining to U.S. elections, upon which the claims in Chapters 3 and 4 are based, was obtained from two sources: The data on campaign contributions in U.S. state legislative elections (1990-2010) was collected by the non-partisan organization The National Institute on Money in State Politics (NIMSP) via `www.followthemoney.com`. This data, which is a compilation of candidate filings to authorities overseeing state-level campaign finance regulations, contains information on donations to candidates in legislative races across all 99 chambers. The data on campaign contributions in U.S. House elections (1980-2014) was obtained from the non-partisan organization Center for Responsible Politics (CRP) via `www.opensecrets.org`, as well as from official records from the Federal Election Commission.

To assess the value of legislative agenda setters in Chapter 4, I collected a new dataset on all committee chairs and vice chairs of all standing and joint committees from 1990 to 2010 across all 99 state legislative chambers, as well as information on the following leadership positions: Speaker of the House, President of the Senate, President Pro Tempore, party whips, and floor and caucus leaders from both parties. The primary sources were the quarterly-published State Yellow Books (e.g. Leadership Directories 2014) and the web sites of various state legislatures. Based on name, party and legislative district, I matched each committee chair, vice chair and party leader to the unique state legislator identifier in the ICPSR dataset 34297 (Klarner et al. 2013). This will enable future researchers to use the data in studies of committee and party leadership in state legislatures. The number of leadership observations during the period 1990-2010 totals approximately 30,000.

Studying campaign finance in the U.K. context proved to be more difficult. Whereas U.S. campaign finance data extends back to the 1980s and 1990s at the federal and state levels, respectively, and can easily be downloaded online, campaign finance data pertaining to U.K. House of Commons elections has only been published in a digital format since 2001. However, candidates have filed, and have documented with receipts, their campaign expenses since the election in 1885 in accordance with *The Corrupt and Illegal Practises Prevention Act 1883*. This act requires that all candidates running for a seat in the House of Commons must disclose detailed information on campaign spending immediately after each general election. The dataset introduced and referenced in Chapter 5 is based on these filings. Using archival material from the House of Commons, I collected the longest spanning and most detailed dataset on campaign finance ever collected: The dataset covers the spending of 99.7% of all candidates running for seat over the past 130 years, in other words, it provides information on more than 61,000 individual candidate-years. I match each return in the archival material to the electoral returns collected and used in a series of papers by Eggers and Spirling (2014*a*,*b*,*c*). This will allow future researchers to link the campaign finance data to information on candidates and constituencies, including data on the political careers and parliamentary speeches given by ministers and MPs.

2.4 Methodological Perspectives

The argument presented in this thesis claims causality: Political power and spending limits *cause* campaign donors and candidates to behave in certain predictable ways. However, assessing the causal effect of political power on the allocation of campaign contributions, as well as determining the electoral consequences caused by spending limits, are not trivial empirical matters.

The characteristics of powerful legislative candidates are apparently different from those of other candidates, and some of those characteristics may, presumably, be correlated with the ability to attract campaign contributions. It seems plausible, for example, that incumbents possess some unique qualities that help them to win elections, but those same qualities may also explain their ability to attract campaign contributions. Similarly, legislators may be appointed to committee leadership positions because they are particularly knowledgeable or have a policy-relevant network within a specific field, but

this, in turn, may also help them to solicit contributions. Selection issues like these will most likely induce bias in naïve comparisons across candidates and other simple cross-sectional studies of political power and campaign finance. Likewise, campaign spending limits are not randomly assigned across constituencies, but determined by underlying constituency characteristics that may also affect electoral competition.

The ideal way to study the effect of political power on campaign finance would be to randomly assign politically powerful positions to candidates, and the optimal way to study the electoral consequences of spending limits would be to randomly assign different limits across constituencies. Luckily, from the perspective of a citizen, neither of these approaches are practically feasible. Instead, I approach the randomization ideal using natural experiments. The idea is to identify causal effects by exploiting exogenous shocks that induce quasi-random variation in the treatment variables.

In Chapter 3, I use a regression discontinuity design to identify the causal effect of incumbency on campaign contributions. I exploit that incumbency status is almost as if randomly assigned in two-party races where the outcome of the previous election was determined by a razor-thin margin. In Chapter 4, I employ a difference-in-difference design to identify the causal effect of institutional agenda-setting power on campaign contributions. The basic idea is to compare within-legislator changes in campaign contributions before and after the appointment to a leadership position, while differencing out common changes affecting all legislators within a given year. In Chapter 5, I use an instrumental variables approach to identify how spending ceilings affect electoral behavior. In the first step of a 2SLS analysis, I exploit reforms of the spending limit formula and shocks to formula inputs to predict within-constituency changes in spending limits. In the second step, I estimate the impact of these predicted changes on electoral behavior.

The remainder of this thesis is structured as follows: The main body of the thesis comprises Chapters 3, 4, and 5. These three chapters constitute three building blocks that, taken together, form a coherent argument. However, each of the chapters is composed of one paper, authored as a self-contained piece of research, and, as such, the chapters can, in principle, be read as independent, self-standing articles. Finally, in the Conclusion in Chapter 6, I integrate the findings from the three preceding chapters and discuss the implications our understanding of the role of money in politics.

Chapter 3

The Financial Incumbency Advantage

In this article, we use a regression discontinuity design to estimate the causal effect of incumbency on campaign contributions in the U.S. House and state legislatures.[1] In both settings, incumbency causes approximately a 20–25% increase in the share of donations flowing to the incumbent's party. The effect size does not vary with legislator experience and does not appear to depend on incumbent office-holder benefits. Instead, as we show, the effect is primarily the result of donations from access-oriented interest groups, especially donors from industries under heavy regulation and those with less ideological ties. Given the role of money in elections, the findings suggest that access-oriented interest groups are an important driver of the electoral security of incumbents.

[1]This chapter is co-authored with Andrew B. Hall (Stanford University). Both authors contributed equally to the paper.

3.1 Introduction

Incumbents possess many advantages over challengers in U.S. elections. The overall "incumbency advantage" might reduce the incentives for incumbents to exert effort on behalf of their constituents, or it might not, depending on its sources. If it is the result of voters rewarding incumbents for effective representation, then it need not diminish these incentives. On the other hand, if the advantage of incumbents stems from other factors not directly linked to how they represent their constituents, it may well distort their calculus while in office. In this paper we connect the advantage of incumbents to the role of money in elections, and we trace incumbents' financial advantage back to the behavior of interest groups who desire access to those in office.

A large literature in political science studies the electoral advantage of incumbents (e.g., Ansolabehere and Snyder 2002; Ansolabehere and Snyder Jr 2004; Cox and Katz 1996; Erikson 1971; Gelman and King 1990; Hirano and Snyder 2009), but our knowledge of its sources remains incomplete. A separate literature studies the connections between incumbents and access-oriented interest groups, offering theoretical motivations for the ways interest groups support incumbents, both financially and otherwise (Baron 1989; Hall and Deardorff 2006; Hall and Wayman 1990; Snyder 1990, 1992). Finally, a third literature studies the links between campaign contributions and electoral outcomes and suggests that, on the whole, receiving and spending more money boosts vote share (e.g., Erikson and Palfrey 2000; Gerber 1998, 2004; Green and Krasno 1988; Jacobson 1978, 1990) and can help "scare off" opponents (Box-Steffensmeier 1996; Goodliffe 2005). In this paper, we connect these three literatures. We show that incumbency causes a large increase in campaign contributions, i.e., that there is a large financial incumbency advantage that precedes, and helps generate, the electoral incumbency advantage we observe. We demonstrate that access-oriented interest groups create a large fraction of this financial incumbency advantage, and are thus an important driver of—and beneficiary of—the electoral incumbency advantage.

Incumbents substantially out-raise challengers, on average, across all U.S. legislatures (e.g., Ansolabehere and Snyder 2000; Hogan 2000; Krasno, Green, and Cowden 1994; Jacobson 2009; Magee 2012; Moncrief 1992). But this does not necessarily imply that incumbency, *per se*, delivers a financial advantage. Much of the observed advantage might instead stem from the fact that incumbents differ from challengers in many

unobserved ways, including in their underlying characteristics and those of the districts in which they tend to sit.[2] Simply comparing the average receipts of incumbents and challengers cannot separate the pre-existing differences between incumbents and challengers from the differences that result from occupying political office. This is equally true when investigating the kinds of donors that give to incumbents and challengers; although interest groups favor incumbents with their donations,[3] this could be driven in large part by the preference of strategic donors for different kinds of candidates and different types of districts, rather than because these donors care about access to office *per se*. We must investigate alternative evidence.

We use a regression discontinuity design (RDD)[4] (e.g., Lee 2008) in U.S. House and state legislative elections to estimate the financial incumbency advantage, i.e., the increase in contributions caused by the [as if] random assignment of party incumbency.[5] We present evidence that party incumbency causes a substantial increase in campaign contributions (approximately a 20–25% jump in the share of total contributions), and we investigate the donor groups responsible for this pattern. We carry out tests that show that strategic interest groups direct money to incumbents in exchange for access (and not for some of the other reasons often put forward), and we show that access-oriented interest group donors account for approximately two-thirds of the overall causal financial incumbency advantage. Moreover, interest groups representing industries that are heavily regulated or that underwent fundamental changes in their regulatory environment (e.g. energy, technology, healthcare and transportation), are more likely to coordinate and target incumbents.

The paper is organized as follows. First, we explain our empirical strategy. Second, we briefly describe the datasets we use to study the financial incumbency advantage. Third, we present our results and use subgroup analyses to discuss potential causal mechanisms. Finally, we conclude with a short discussion.

[2] Ansolabehere and Snyder (2000) addresses this problem of causality by using a "sophomore surge" estimator. However, resulting estimates are likely to be somewhat downward-biased because of reversion to the mean (e.g., Gelman and King 1990).

[3] This can be calculated using FEC data and National Institute On Money in State Politics data. Also, see for example Ansolabehere and Snyder (2000).

[4] Our results do not depend substantively on the choice of incumbency-advantage estimator.

[5] We focus on campaign contributions rather than expenditures, although the two are inevitably highly correlated.

3.2 Empirical Strategy

A simple comparison of incumbent and challenger campaign receipts does not estimate the causal effect of incumbency on campaign receipts, for obvious reasons. Those who win an election are not directly comparable to those who do not. In addition, incumbents may be strategic in their decision to run for reelection. In a pooled analysis, moreover, unobserved differences across districts with open elections, those with uncontested incumbents, and those with incumbents running against challengers will be confused with the effects of incumbency. Forms of bias like these threaten most estimates of incumbency advantages. RDDs provide a solution to these selection problems by focusing on close elections in which incumbency is "as if" randomly assigned to either the Democratic or Republican party (see Imbens and Lemieux 2008; Lee 2008). If the campaign contributions donated to the party in the next election cycle in districts it barely won differ systematically from the donations the party receives in districts it barely lost, this difference can be attributed to the impact of incumbency under weak conditions.[6]

More formally, the RDD estimator is defined as

$$\tau_{RDD} = lim_{v \downarrow c} E[Y_{it+1}(1)|V_{it} = v] - lim_{v \uparrow c} E[Y_{it+1}(0)|V_{it} = v] \quad (3.1)$$

$$= E[Y_{it+1}(1) - Y_{it+1}(0)|V_{it} = c] \quad (3.2)$$

where V_{it} is the forcing variable which determines whether or not treatment is assigned (if $v_{it} \geq c$ treatment is assigned). In the present setting, this forcing variable is the Democratic vote share winning margin, i.e., the difference between the Democratic share of the two-party vote and 50%, the necessary vote percentage required to win office. When this variable is above zero, the district is "treated" with a Democratic incumbent. $Y_{t+1}(1)$ is the potential outcome at time $t+1$ if unit i is treated and

[6]The random assignment of incumbency at time t ensures that the districts that receive a Democratic incumbent and those that receive a Republican incumbent have incumbents of equal quality as long as Democrats and Republicans in close elections are, on average, of equal quality. This is the sense in which candidate quality is accounted for. At time $t+1$ we do not want to constrain candidate quality. If challengers are lower in quality in response to the random assignment of incumbency, this is part of the causal effect of interest. It is downstream of the treatment. In addition, any fixed difference in average quality between Democrats and Republicans would not affect our results since this would only shift the intercepts at the discontinuity and not the gap that estimates the treatment effect.

$Y_{t+1}(0)$ is the potential outcome at time $t+1$ if unit i is not treated. In the present setting, the outcome variable is a measure of Democratic campaign donation receipts in the next electoral cycle, and each unit is a district. The identification of the treatment effect rests on the key assumption that $E[Y_{i,t+1}(1)|V_t = v]$ and $E[Y_{i,t+1}(0)|V_t = v]$ are continuous in v. In other words, the assumption states that the only variable that "jumps" at $v = c$ is the assignment to treatment – all other relevant variables must be continuous around the cutoff such that for an arbitrarily small bandwidth around the discontinuity, barely winners and barely losers are not systematically different from one another in any way except for their treatment status.

Recent work has challenged the validity of the RDD assumption in the context of the post-war U.S. House (Caughey and Sekhon 2011; Grimmer et al. 2012; Snyder 2005), presenting evidence that barely winners and barely losers are systematically different from each other. In particular, they show that incumbents appear to have an advantage in extremely close elections. However, Eggers et al. (2015) presents evidence and arguments that this apparent sorting is likely the result of a fluke, which can be controlled for econometrically, and not a violation of the RDD assumption. Moreover, this sorting does not occur in state legislative elections, where there is strong evidence for the validity of the RDD (Eggers et al. 2015). To be especially prudent, we also go beyond Eggers et al. (2015) in checking for validity in state legislative elections specifically. The Appendix reports an expansive battery of balance tests that find no evidence of sorting. We also show that our results survive a variety of robustness checks, including the use of covariates and the use of the "donut" RDD, and we focus primarily on comparisons *across* RDD estimates, which would remove any fixed bias from sorting even if it did exist. In addition, all empirical results are robust to the use of alternate incumbency advantage estimators.[7]

To explore the overall financial incumbency advantage, we use OLS to estimate RDD equations of the form

$$Y_{i,t+1} \quad = \quad \beta_0 + \beta_1 D_{it} + f(V_{it}) + \epsilon_{i,t+1} \tag{3.3}$$

where $Y_{i,t+1}$ measures the Democratic Party's share of all donations in district i in

[7]In particular, we have replicated the main analysis using the Gelman-King estimator. Results are substantively similar.

election $t+1$.[8] This includes all donations from individuals and groups, excluding candidate self-financing and party committee funds.[9] The variable D_{it} is a binary variable indicating the "treatment," i.e., a Democratic victory in district i in election t, and $f(V_{it})$ represents a function of the forcing variable, the Democratic vote share winning margin in district i in election t.[10] The purpose of this function is to extrapolate to the discontinuity by accounting for the relationship between vote share and campaign donations. We might expect, for example, that Democratic vote share in election t and the Democratic share of campaign donations in the campaign for election $t+1$ are positively associated within the bandwidth, an association that biases observational studies of the financial incumbency advantage.[11] In the estimates presented below, we employ local linear regression, fitting a linear relationship between the running variable and the outcome variable within a small bandwidth and allowing the slope to vary on either side of the discontinuity (Imbens and Lemieux 2008). However, the Appendix shows that the results are highly stable across many bandwidths and many specifications of the forcing variable. Finally, $\epsilon_{i,t+1}$ represents the disturbance term.

Two features of the RDD are worth mentioning. First, the RDD estimator is necessarily *local*. Our estimated effects only speak, directly, to districts with close elections. It is possible, for example, that incumbency causes a bigger increase in campaign contributions in safe districts, where incumbents might be expected to have longer time horizons and interest groups have more to gain from access.[12] Nevertheless, the effect

[8] In cases in which the subsequent election is uncontested (which is rare since the initial election was so close), the incumbent is credited with receiving 100% of donations, a fact which is literally true because there are, indeed, campaign donations even in uncontested elections. All estimated results are robust, however, to the exclusion of uncontested elections.

[9] This definition is not necessary to find the results, but is in keeping with previous literature (Snyder 1992).

[10] The vote share winning margin is defined as $V_{it} = vtsh_{it} - 50\%$ where $vtsh_{it}$ is the Democratic Party's share of the votes received by Democrats and Republicans in district i in election t (in percentage points).

[11] At larger bandwidths, we might suspect that this relationship inverts. Once a candidate is particularly safe, she may receive fewer donations. Again, this justifies the use of small bandwidths. It also suggests the value of using a higher-order polynomial of the forcing variable, to account for possible non-linearities in the relationship between the forcing variable and the outcome, a strategy we pursue in the Appendix.

[12] In such districts, there is no random variation in incumbency, and thus no unbiased way to assess the effects of incumbency.

of incumbency in competitive districts is, by itself, extremely important. Competitive districts are those where incumbency status is likely to change, so the districts in our sample comprise the districts most likely to be exposed to the effects we are studying. Competitive districts are also those where campaigns are most salient (since both parties have a chance of winning), making our focus on the financial effects of incumbency in competitive races natural. As a result we are not overly concerned about the local nature of the RDD estimator.

Second, the RDD estimator captures what is sometimes called the "party" incumbency advantage, because the winning candidate at time t is not required to run again at time $t+1$ and may be replaced by a new co-partisan. As such we must be cautious in couching all of our findings in terms of both the party and the individual (for further discussion, see Erikson and Titiunik 2012; Fowler and Hall 2013). To be clear, our estimates reflect the advantage in campaign contributions that accrues to the candidate running in election $t+1$ when her party – either represented by herself or a predecessor – held the office in the previous cycle. However, the party component of this overall advantage, i.e., the amount of the advantage not accruing to the individual legislator but instead to any candidate running from her party, is estimated to be zero in state legislatures (Fowler and Hall 2013). As a result we have reason to believe the effects we estimate reflect the personal incumbency advantage exclusively. Either way, the estimated effects are meaningful, as they point to the reaction of different donor types to random changes in the identity of the party that controls a given seat in the legislature.

3.3 Data

To examine the financial incumbency advantage, we employ a large dataset on U.S. House elections from 1980 – 2006 and state legislative elections from 1990 – 2010.

For data on state legislative election returns, we use ICPSR dataset 34297 (Klarner et al. 2013). The dataset runs from 1967-2010, however, we only use elections from 1988 on in order to match the elections to data on campaign finance.[13] Raw data on state legislative campaign donations comes from The National Institute on Money in

[13]The first campaign finance observations are in 1990. Given our empirical setup, our first election observations occur in 1988, so that we can observe how barely winners and barely losers in 1988 collect campaign contributions in their next election cycle.

State Politics (http://www.followthemoney.org). This financial dataset provides donation records for all state political races from 1990 – 2010 at the level of the individual donor. We keep all records pertaining to state lower or upper house general election races, and discard all others. We are also able to separate individual and interest group donors by the name formatting that The National Institute on Money in State Politics uses.[14] In addition, we use The National Institute on Money in State Politics codings to categorize donations into industry categories. The main categories are: Agriculture, Business, Construction, Energy, Finance, Government, Healthcare, Ideological, Labor, Lawyers/Lobbyists, and Transportation. These categories come from state disclosure requirements. We merge this financial data with the election dataset using the year, state, chamber (upper or lower) and district number.[15]

The data on campaign spending at the federal level is provided by the United States' Federal Election Commission and consists of information disclosed according to the Federal Election Campaign Act (FECA). FECA requires all candidates running for the U.S. House to disclose all contributions made by political action committees (PACs) and individual contributions worth more than $200. The analysis is based on the Post-Election Cycle Summary Data Files which contains summary information about all candidates who ran for a seat in the House from 1980 – 2006. [16] In addition, we use the Federal Election Commission's categorization of contributors. The main categories are: Corporate PACs, Labor PACs, Unconnected PACs, Trade, Health and Membership PACs (hereafter "THM"), Cooperative PACs, and individuals. Data on federal election results and seniority are obtained from the replication dataset for Caughey and Sekhon (2011). For details, see the online data Online Appendix to that paper.[17]

Our main outcome variable—the Democratic party's share of total contributions—is simply constructed by dividing the total amount donated to the Democratic party

[14] In personal correspondence with The National Institute on Money in State Politics, we have confirmed that they intentionally never use commas in interest group names, so that researchers can separate individual from group donors.

[15] The merge is imperfect due to discrepancies in district naming conventions between the two datasets (e.g., in New England states with named and numbered districts), but where possible we have corrected these errors. Such errors should reduce statistical power, but do not present a problem in the estimates presented below.

[16] The data can be downloaded from `http://www.fec.gov/finance/disclosure/PostCycleSummaryDataFiles.shtml`

[17] `http://sekhon.berkeley.edu/papers/RDOnlineAppendix.pdf`

in a given election cycle in each district by the total amount donated to both the Democratic and Republican party in that district for that cycle. Similarly, we construct the Democratic party's share of contributions from a particular donor group (e.g. labor) by calculating the total amount donated to the Democrats by the group and dividing it by the total amount that the group donated to both parties.[18] In order to compare absolute amounts across years, we adjust the data to constant 1990 prices using the standard Consumer Price Index published by Bureau of Labor Statistics.[19]

We construct our running variable as the difference between Democratic percentage of the two-party vote at time t and 50, i.e., the distance between the Democratic vote share and electoral victory. To do so, we drop any elections at time t in which a third party secures either the highest or second highest vote total. Finally, we also exclude observations in which the outcome variable is measured after a redistricting period. So, for example, close elections for most U.S. House districts in 1990 are not used because the donations received for the next election cycle (1992) occur in new districts.[20]

3.4 Results

Figure 3.1 illustrates how incumbency affects the Democratic Party's share of total contributions. The forcing variable, the Democratic win margin, is divided into 1 percentage-point bins, and each dot represents the average of the Democratic Party's share of total contributions *in the next electoral cycle*.[21] There is a clear jump in campaign receipts just at the cutoff; as the vote share winning margin approaches 0 from below, the Democratic Party's share of total contributions in the next electoral cycle approaches between 35–40%, and as the winning margin approaches 0 from above, the share of contributions approaches 60% of the total contributions.[22] At both the fed-

[18] We do not consider any donations made to third party candidates in constructing this ratio.

[19] The Consumer Price Index can be downloaded from `http://www.bls.gov/data/#prices`. Series ID: CUUR0000SA0.

[20] For the House, we use information on off-cycle redistrictings collected and organized for Caughey and Sekhon (2011). For the state legislatures, we assume all redistrictings occur in years ending in '2' before the 1990s. For subsequent years we use redistricting information collected, and generously provided by, Carl Klarner.

[21] Binning reduces noise but does induce bias in the discontinuity. We never bin the data for any of our statistical analyses.

[22] As one would expect, the party's share of subsequent campaign contributions is increasing in the party's vote share (more qualified candidates attract both more votes and campaign contributions).

eral and state level, incumbency causes a substantial jump in the party's share of total contributions in the reelection campaign. Interestingly, the effect size is quite similar in both settings.

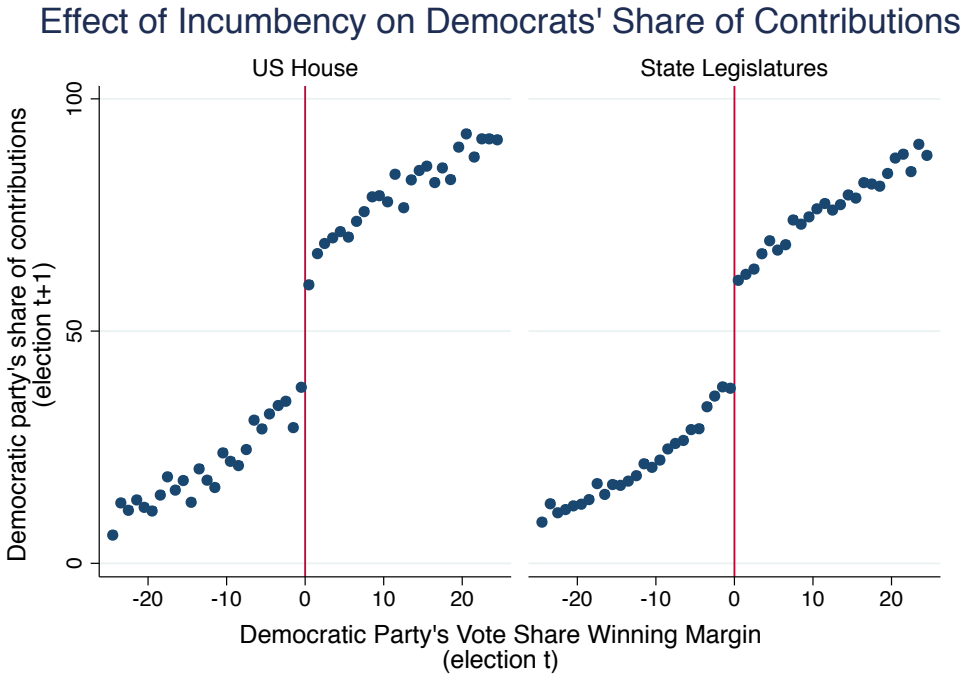


Figure 3.1 – Binned averages of the Democratic share of contributions in the subsequent electoral cycle. When the Democratic party crosses the threshold from barely losing to barely winning the election, its share of the next campaign's contributions increases dramatically.

The statistical analysis is consistent with the figure. Table 3.1 presents the main results from the RDD analysis using local linear regression to estimate Equation 3.3. Specifically, we include a linear term of the running variable estimated separately on each side of the discontinuity, using a variety of bandwidths as specified in the table. On average, incumbency causes approximately a 20 to 25 percentage-point jump in the Democratic Party's share of contributions both at the federal and state level.[23] Incumbency also has a substantial impact on the *level* of contributions. On average, incumbency approximately causes a $275,000 jump in contributions in U.S. House elections and a $28,000 jump for incumbents in state legislatures (measured in constant 1990 dollars).[24]

[23] The only point estimate outside of this range is in the U.S. House with a 1 percentage-point bandwidth. We suspect this estimate is slightly smaller (17%) only because of increased sampling variability. This estimate uses the smallest sample size of any of the six reported.

[24] Table A.1 in the appendix shows the estimated effect on total contributions.

Table 3.1 – RDD Results: Effect of Incumbency on the Democratic Party's Share of Total Contributions, $t+1$

	Dependent Variable: Democratic share of total contributions, $t+1$.					
	(1)	(2)	(3)	(4)	(5)	(6)
Democrat Win (time t)	17.15*	20.62*	24.94*	23.05*	22.37*	20.04*
	(6.55)	(4.46)	(3.50)	(3.59)	(2.06)	(1.60)
Constant	37.69*	37.88*	35.71*	38.69*	37.68*	39.20*
	(4.11)	(2.97)	(2.24)	(2.61)	(1.48)	(1.12)
Observations	108	329	568	815	2421	4020
Level	Federal	Federal	Federal	State	State	State
Bandwidth Pct.	1	3	5	1	3	5

NOTE: Robust standard errors in parentheses. All models are estimated using OLS with a linear specification of the forcing variable estimated separately on each side of the discontinuity. * $p < 0.05$.

In American legislatures, there is a substantial and causal financial incumbency advantage. The literature on the impact of campaign spending on electoral outcomes suggests that money translates into votes, although the exact conversion rate is up for debate. If money can be converted into votes then the electoral incumbency advantage may stem, in part, from this financial advantage. Crude back-of-the-envelope calculations in which the dollar estimates obtained in this paper are multiplied by the estimates of the effect of incumbents' campaign spending on vote shares from the literature suggest that the financial incumbency advantage can account for up to approximately 1.4 and 6 percentage point increases in the incumbent's vote share in U.S. House and state legislative elections, respectively.[25] To understand *why* incumbents and their parties have both of these advantages, then, we need to understand who generates the financial incumbency advantage. What kinds of donors support incumbents and their parties,

[25] In Table A.2 in the appendix, we evaluate the importance of the financial incumbency advantage for electoral outcomes by relating our estimates to estimates of the impact of campaign spending on vote shares from the literature. The crude idea is simply to multiply the dollar estimates obtained in this paper with the estimates of the effect of incumbents' campaign spending on vote shares from the literature. While the financial advantage occurs *prior* to the electoral advantage, it is still possible that the knowledge of the electoral advantage in part drives donors to favor the incumbent's party. That is to say, when considering two outcomes from a single randomized treatment, the randomization cannot buy us the identification on the relationship between the two outcomes. To make progress, later we will consider estimates of the mapping between money and votes.

and what factors vary this support? The next section probes these questions in order to uncover the sources of the financial incumbency advantage.

3.5 Sources of the Financial Incumbency Advantage

Correlationally, incumbents in both federal and state legislatures have a sizable financial advantage (e.g., Ansolabehere and Snyder 2000; Hogan 2000; Krasno, Green, and Cowden 1994; Jacobson 2009; Magee 2012; Moncrief 1992). The existing literature proposes many mechanisms behind this "financial incumbency advantage." We focus on the three popular theories to which our data can speak. Strategic contributors might use campaign donations to buy access to the political system, making them more likely to target incumbents because they have a higher probability of being in office after the next election (Snyder 1990; Cassie and Thompson 1998). Incumbents might exploit the direct benefits of being in office to attract contributors, e.g., the franking privilege (Herrnson 1992; Levitt and Wolfram 1997). Or, incumbents might become more skilled at fundraising and might be able to build valuable connections to potential contributors while in office (Cho and Gimpel 2007; Squire and Wright 1990). We test each of these theories in turn, and we find strong support for an access-oriented theory of the financial incumbency advantage. We find less support for theories that rest on in-office experience or office-holder benefits.

Testing Theories of Interest Group Access

The first explanation is based on the differing attitudes of contributors towards incumbents and challengers. Strategic interest groups ought to invest in the political campaigns that give them the highest return in terms of political benefits. Access-motivated interest groups — groups that care more about access to the political system than ideology — will invest more in incumbents' campaigns, for a variety of reasons. Donations to incumbents may grant immediate access to those in office. Perhaps more importantly, given the presence of an electoral incumbency advantage, incumbents are likely to stay in office for a long time, providing a higher return to investment for interest groups.

Snyder (1992: 17) presents a strong argument for why strategic interest groups should target incumbents with their contributions:

> "...contributors must develop a relationship of mutual trust and respect with officeholders in order to receive tangible rewards for contributions. A contributor cannot simply buy a congressman's vote on an important bill with a $5,000 campaign donation. Large donations over several elections, however, together with intelligent, informative discussions about matters of concern to the contributor, may eventually yield considerable benefits."

As Snyder (1992) shows, the desire for access differentiates these strategic groups from other donors, like individuals and ideological groups, who care instead about supporting the electoral bids of candidates with particular ideological positions. We might therefore expect to observe differential responses to incumbency by strategic groups and other donors, respectively.

If the jump in campaign contributions is caused by "investor" or access-motivated interest groups who focus their contributions on incumbents, one would expect to observe a difference in the treatment effect for different subgroups of contributors. More specifically, the effect on contributions from access-motivated or investor interest groups should be greater than the effect for ideologically-motivated or consumption-based donors. Snyder (1990) shows that individuals and non-connected PACs can be seen as consumption contributors because they contribute to promote a certain cause or ideology, while PACs associated with corporations, unions, THM and cooperatives can be seen as "investor contributors" because they tend to support candidates financially in exchange for access to the political system in the event that the candidate is elected.

To investigate this theory, we estimate equations of the form

$$Dem\ Share_{ij,t+1} = \beta_0 + \beta_1 D_{it} + \beta_2 Investor_j + \beta_3 (D_{it} Investor_j) + f(V_{it}) + \epsilon_{i,t+1} \quad (3.4)$$

where $Investor_{ijt}$ is a dummy variable taking the value 1 if donor j is an investor group and all other variables are defined as before. At the federal level, we follow Snyder (1990) and define consumer contributors as individuals and non-connected PACs, and investor contributors as every other donating interest group. At the state level, we apply a similar classification and define consumer contributors as individuals and ideological interest groups, and investor contributors as every other donating interest group. As Equation 3.4 implies, we reshape the data such that we have two observations for each district-year: one observation pertaining to the Democratic Party's share of investor contributions and the other pertaining to the Democratic Party's share of consumer contributions.

In columns 1 and 2 in Table 3.2, we formally test whether there is heterogeneity in the treatment effect across contributions donated by investor and consumer contributors at the federal level and the state level, respectively. The relevant quantity of interest is β_3 from Equation 3.4. For simplicity, we report results using a 5 percentage-point bandwidth with local linear regression for this and all subsequent analyses. However, as suggested by Table 3.1, all results are highly stable across bandwidths and specifications. The positive and statistically significant coefficients on the interaction terms in both columns demonstrate that party incumbency has a greater impact on campaign contributions for access-motivated contributors. While consumer-motivated donors in the U.S. House contribute 16.57 percentage points more of all their donations to the incumbent party in the next election cycle, access-oriented interest groups—"investors"—increase the percentage of donations they direct to the incumbent party by 40.52 points ($16.57 + 23.95 = 40.52$). The effect for investors is more than 2.4 times as large as for consumer contributors. This difference is even larger in state legislatures, where the effect for investors is nearly 32 percentage points ($9.52 + 22.03 = 31.55$), 3.3 times larger than the effect for non-investors.

This is not the same as saying that access-motivated contributors donate more, on average, to incumbents, which could be driven in part by these contributors preferring, e.g., higher quality candidates – rather, this is evidence that access-motivated contributors change their contribution patterns based purely on incumbency status in a way other donors do not.

In the Appendix, we calculate the difference in these effects in terms of overall dollars, rather than in percentages. In the U.S. House, investors direct $165,700 more to the Democratic party after it wins a close election. The overall financial incumbency advantage in levels in the U.S. House is $275,600, as the first column of Table 3 shows. Access-oriented interest groups are therefore responsible for roughly 60% of the financial incumbency advantage in the House. In state legislatures, this relationship is even more pronounced. Here, the investor-specific effect is roughly $20,000, comprising 71% of the financial incumbency advantage.

To evaluate this causal mechanism further, we obtain industry-specific estimates of the financial incumbency advantage by reestimating Equation 3.3 (the baseline specification from the previous section) using industry-specific outcome variables. Figure 3.2

presents the estimated RDD effects for the different types of contributors at the state and federal levels, respectively. The general pattern at both the federal and state level appears to be the same: the effect is smaller for consumer contributors (non-connected PACs/ideological groups and individuals) than for investor contributors.

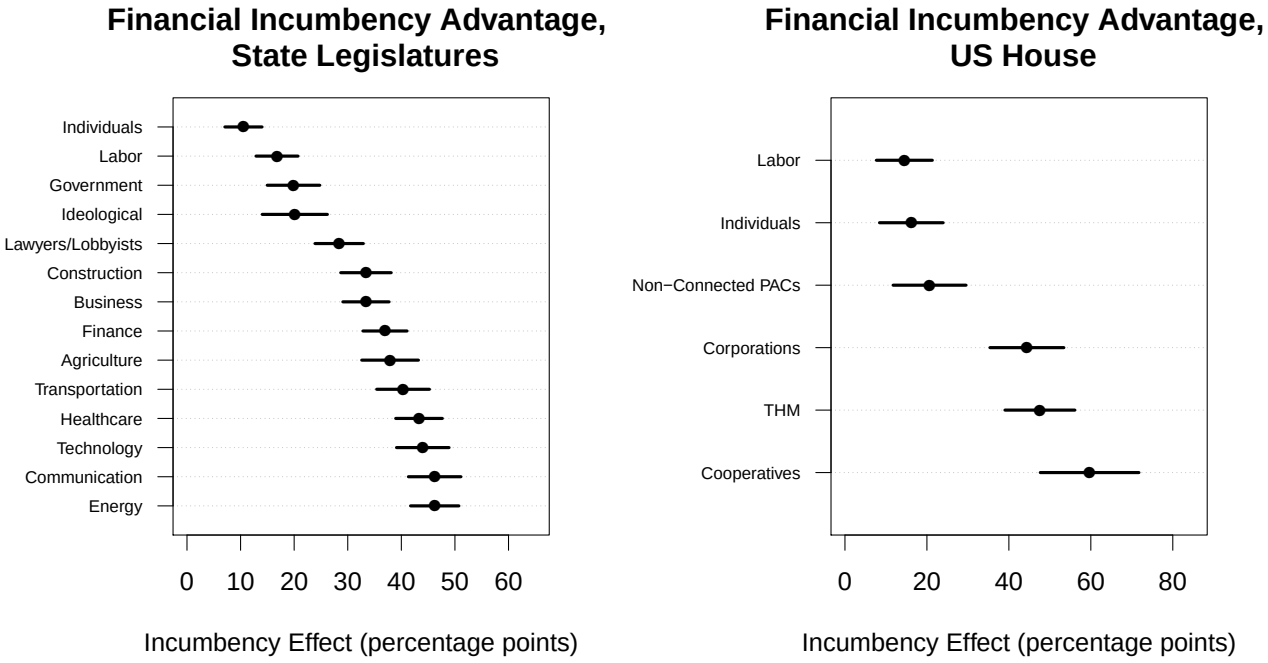


Figure 3.2 – Sensitivity of different interest group industries to incumbency. Estimates are from separate RDDs where the outcome is the share of contributions at $t + 1$ from that industry that flow to the incumbent in the subsequent electoral cycle.

The access hypothesis also implies that, among strategic interest groups, those with less ideological leanings ought to respond more strongly to incumbency. Labor unions, though perhaps strategic, are also deeply linked to the Democratic party. Therefore, they are unlikely to shift donations to the Republican party, even if the Republican candidate wins election. Corporations, on the other hand, are less beholden to one party, and so should switch donations between the two parties more readily. The estimates presented in Figure 3.2 are consistent with this story: the jump in campaign contributions to the Democratic party after a Democratic win is greater for corporate contributions (approximately 50–70%) than for labor contributions (approximately 20%). Ideological groups in state legislatures – groups that are formed for a single issue – likewise are somewhat insensitive to incumbency because of their ideological leanings. Less ideological groups, who require access to state government in order to further their policy goals, exhibit a high degree of sensitivity.

Industries that are highly regulated, and industries that underwent significant changes in their regulatory environment during the studied period, such as energy, technology, healthcare, and transportation (Joskow 2005; Kearney and Merrill 1998), appear to be particularly inclined to channel their resources towards incumbents in state legislatures. In other words, the industries that have the most to win or lose from new government regulation are the same industries that seem to coordinate and target their contributions towards incumbents. Though the classifications are coarser at the federal level, the same general pattern is observed. Strategic interest groups still value access to the incumbent, even when the incumbent is of the opposite party – evidence of the sheer importance of access.[26]

Testing Theories of Office Holder Benefits

The second explanation for the financial incumbency advantage is based on the benefits associated with being in office. Under this explanation, direct office-holder advantages "that House members use to attract electoral support [...] also help them to raise campaign money. Nonincumbents possess none of these advantages" (Herrnson 1992: p. 862). The franking privilege makes direct-mail solicitation easier for incumbents (Herrnson 1992; Jones and Hopkins 1985) and traveling benefits give incumbents an advantage in attracting new contributors via personal visits (Shaw and Gimpel 2012), to pick two examples. It is easier for a member of the U.S. House to exploit these direct benefits in her own congressional district than outside it (Fenno 1978). For example, the franking privilege can only be used to send mail to addresses in an incumbent's own congressional district, and in-state fundraising activities can more easily be disguised as expenses related to a Representative's district office than out-of-state fund-raising activities.[27] These observations generate a clear prediction for this theory.

If the observed jump in incumbent-party campaign receipts is caused by incumbents

[26]This "access" could take the form of donors asking for favors, but it could equally result from incumbents holding interest groups over a barrel, demanding donations in exchange for favorable political actions. These opposite (but not mutually exclusive) possibilities should be investigated further.

[27]Members of Congress have not been able to send mass mail outside their districts since 1992 when the provision that previously permitted this was ruled unconstitutional. See for example Glassman (2007). In regressions not reported in the paper, we have tested for in-state vs. out-of-state effects before and after 1992. Donors do appear slightly more sensitive to the geographical distinctions after 1992, but the differences are not statistically significant.

who use direct office-holding benefits to attract more campaign contributions, then we would expect the jump in campaign contributions coming from home-state contributors to be far greater than the jump in campaign contributions coming from out-of-state contributors.[28] Indeed, were office-holder benefits the only cause of the financial incumbency advantage, we might imagine out-of-state donors to ignore incumbents almost completely.

To test this, we estimate the equation

$$Dem\ Share_{ij,t+1} = \beta_0 + \beta_1 D_{it} + \beta_2 Home\_State_{ijt} + \beta_3 (D_{it} Home\_State_{ijt}) + f(V_{it}) + \epsilon_{i,t+1} \tag{3.5}$$

where $Dem\ Share_{ij,t+1}$ is the share of donations in district i flowing to the Democratic party in election $t+1$ from either in state ($j = 1$) or from out of state ($j = 0$). The variable $Home\_State_{ijt}$ is an indicator variable taking the value one if donor j is in district i's state.[29]

The fourth column in Table 3.2 shows how the financial incumbency advantage varies across home-state and out-of-state contributions.[30] The coefficient of interest is β_3 from Equation 3.5, the interaction of the treatment with the home state indicator. As the results show, we cannot reject the null hypothesis that in-state and out-of-state contributors respond to incumbency in the same manner. Furthermore, the point estimate on β_3 is small, and out-of-state donors, who presumably do not gain from office-holder benefits, still respond extremely strongly to incumbency. Indeed, incumbency causes a 33.52 percentage-point gain in the share of all out-of-state donations a candidate receives, despite the fact that out-of-state donors are unlikely to gain from the incumbent's office-holder benefits.[31]

[28] We use the distinction between home-state vs. out-of-state contributors instead of in-district vs. out-of-district contributors for a practical reason: postal codes cut across congressional districts but they do not across state lines. This means that we can always identify the state of a contributor but not always the congressional district. To avoid any misclassification, we focus on whether contributions are coming from the same state as the Representative or not.

[29] Contributions from individuals are excluded from this analysis because FEC's data do not contain the addresses of all individuals who donated.

[30] In order to examine the heterogeneity across in-state and out-of-state contributors, we reshape the data such that we have two observations for each district-year: One observation pertaining to the Democratic Party's share of in-state contribution and the other pertaining to the Democratic Party's share of out-of-state contributions.

[31] We cannot conclude from this test that office holding benefits do not matter at all; the home-state

Table 3.2 – Heterogeneity in Treatment Effect Across Locations, Type of Contributors, and Seniority of Candidate.

	Dependent Variable: Democratic share of total contributions, $t+1$.			
	(1)	(2)	(3)	(4)
Democrat Win (time t)	16.57*	9.52*	36.53*	27.02*
	(3.04)	(1.40)	(5.04)	(4.06)
Investor Contribution × Democrat Win (time t)	23.95*	22.03*		
	(2.79)	(1.28)		
Investor Contributor	-1.89	-7.06*		
	(1.99)	(0.88)		
Home State Contribution × Democratic Win			6.43	
			(4.22)	
Home State Contribution			-9.37*	
			(2.96)	
Previously Held Office × Democrat Win				-5.05
				(4.02)
Previously Held Office				-4.59
				(3.19)
Constant	36.03*	42.48*	30.31*	38.72*
	(2.04)	(0.98)	(3.27)	(2.80)
Observations	1136	7967	713	568
Level	Federal	State	Federal	Federal

NOTE: Standard errors in parentheses. The reported standard errors are the maximum of robust and robust standard errors clustered by elections. All models are estimated using OLS with a linear specification of the forcing variable estimated separately on each side of the discontinuity. The outcome variables in all models are the Democratic party's share of total contributions at $t+1$ in percentage points. All models are estimated based on a 5 pct. bandwidth. * $p < 0.05$.

Testing Theories of Experience

The third explanation is based on experience: "Fundraising aptitude is cultivated and learned, not inborn" (Squire and Wright 1990; Cho and Gimpel 2007: p. 255). Over time politicians learn certain skills and acquire knowledge that facilitate fundraising, and challengers are disadvantaged because "this knowledge is often limited to incumbents" (Cho and Gimpel 2007: p. 255). For example, political networking is probably more effective in Washington D.C. than in a small congressional district far from the Capitol. This means that compared to a challenger, it is easier for an elected politician who spends a lot of time in Washington D.C. to develop a network of important lobbyists, interest groups, corporations and individuals that could contribute to her campaign. If the jump in campaign contributions is driven by incumbents who gradually become more experienced and acquire skills, contacts, etc. over the period of time they are in office, we would expect heterogeneity in the treatment effect across first-time incumbents and more experienced incumbents.

To test this, we estimate the equation

$$\begin{aligned} \textit{Dem Share}_{i,t+1} = \beta_0 + \quad \beta_1 D_{it} \quad & + \beta_2 \textit{Held\_Office\_Before}_{it} + \\ & \beta_3 (D_{it} \textit{Held\_Office\_Before}_{it}) + f(V_{it}) + \epsilon_{i,t+1} \end{aligned} \tag{3.6}$$

where all variables are defined as before and $\textit{Held\_Office\_Before}_{it}$ is a dummy indicating that the Democratic candidate in district i at time t previously held a seat in the U.S. House. The coefficient of interest is β_3, the interaction that tests whether the effect is larger for previous incumbents. Again, we use a 5 percentage-point bandwidth around the discontinuity.

The results presented in column 4 in Table 3.2 do not support the seniority hypothesis, either. If seniority were driving the jump in campaign contributions, we would expect a positive and statistically significant coefficient on the interaction term, β_3 from Equation 3.7, which directly tests the prediction that the effect should be larger

effect is, of course, still large and positive. It is possible that home-state donors are responding to office holding benefits while out-of-state donors are responding to other factors that in-state donors do not care about. But we can certainly rule out that office holder benefits are the only large driver of the financial incumbency advantage, since donors continue to reward incumbency even when they cannot plausibly receive these benefits.

for those who have held office before. However, the coefficient is *negative*, and we cannot reject the null hypothesis that it is zero.

Carrying this idea further, we can also test for the effect of differing levels of seniority on the financial incumbency advantage. To do so, we restrict the sample to only elections in which: an incumbent is running for reelection at time t and, at time $t+1$, either the same incumbent is running again having gained reelection at time t, or, if she lost at time t, her replacement is running as an incumbent at time $t+1$.[32] To put this another way, the "treatment" group is the set of districts in which the senior incumbent wins reelection at time t, and the "control" group is the set of districts in which a new junior incumbent wins election at time t. The outcome variable is the incumbent share of all donations at time $t+1$ – either the senior incumbent's share, if the district is treated, or the junior incumbent's share, if the district is a control district.

Thus we estimate equations of the form

$$Inc\ Share_{i,t+1} = \beta_0 + \beta_1 Inc\_Winner_{it} + f(S_{it}) + \epsilon_{i,t+1} \tag{3.7}$$

where $Inc\ Share_{i,t+1}$ is the share of all contributions that go to the incumbent running for reelection in election $t+1$ in district i. The variable Inc_Winner_{it} is an indicator variable that takes the value one when district i reelects the incumbent candidate in election t. The function $f(S_{it})$ represents the function of the (new) running variable, the incumbent's vote share (rather than the Democrat's vote share like before). Again, we use a 5 percentage-point bandwidth around the discontinuity.

We estimate this equation first using all incumbents. In this case, the question being tested is: does an incumbent who wins *reelection* receive more money than a candidate running as an incumbent for the first time? This is a different question from the original RDD, in which we compared outcomes when one party had incumbency status vs.

[32] Typically selecting on the decision to run again induces bias in the RDD. Here, however, this selection occurs both in the treated districts (selecting on the senior incumbent running again) and in the control districts (selecting on the junior incumbent running again). As long as senior incumbents and junior incumbents do not differ in this form of selection bias, the estimates will be unbiased. Even if they do, the comparison across estimates will not be biased so long as the difference in the selection effect is constant across levels of seniority among the senior incumbents. What is more, even if there is such differential selection bias, it will bias us towards finding higher effects at higher levels of seniority, which is not what we find.

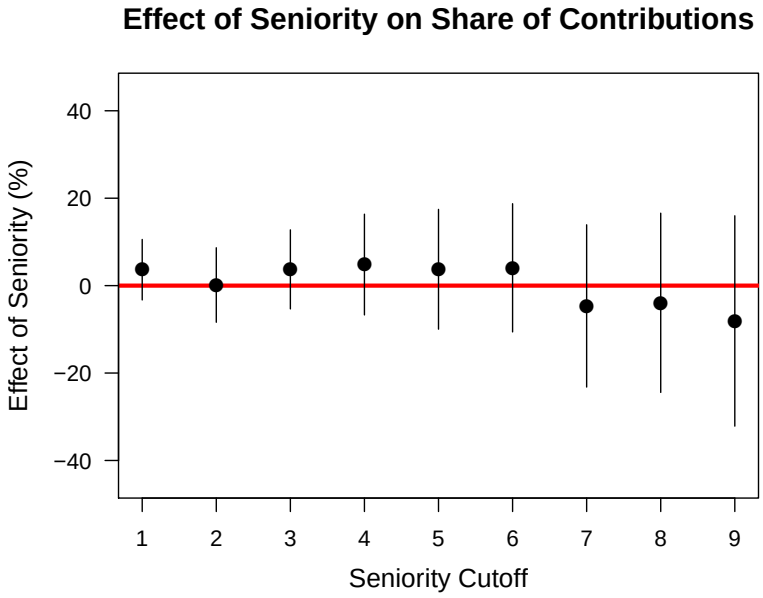


Figure 3.3 – Testing the experience hypothesis. The financial incumbency is flat across levels of incumbent seniority. Each point is an estimate from a separate RDD using the indicated seniority cutoff.

when the other party had incumbency status. In that case, the "treated" districts had Democratic incumbents and "control" districts had Democratic challengers. In this case, "treated" districts have representatives who have served at least one previous term while "control" districts have first-time incumbents.

Next we subset the data to only incumbents with at least two terms of service at time t who run for reelection at time $t+1$, and then three, and so on.[33] In each case, we are testing the question: when an incumbent with x or more terms of seniority wins reelection, does she get more money than a first-time incumbent running for reelection? Under the experience hypothesis, the advantage to the incumbent should increase across these estimates.

Figure 3.3 plots the estimated effect across levels of seniority. For each level of seniority on the horizontal axis, we reestimated the RDD using only elections in which

[33] When comparing *across* RDD estimates, we are performing a somewhat-observational study. Seniority is, of course, not randomly assigned. Nevertheless, we suspect the comparison is informative. It is hard to propose a source of selection bias that would make the effect appear flat across levels of seniority. For example, incumbents in close elections may be lower quality than other incumbents. This would not flatten out the effects across levels of seniority unless incumbents in close elections became increasingly low quality over time.

one incumbent with at least that level of seniority was running.[34] If experience is valuable, we should see the effect increase across levels of seniority. Instead, the line is flat. Indeed, the effect is very small even for one-term incumbents, suggesting that almost all of the gain in campaign contributions occurs in the very first reelection campaign.[35]

3.6 Conclusion

There is a large financial incumbency advantage in U.S. legislatures. Depending on the value of money for electoral security, this advantage may explain a significant portion of the electoral incumbency advantage. But this advantage does not come equally from all donors. Instead, while individuals and ideological interest groups support candidates based on a variety of other factors, access-motivated interest groups coordinate intensively on incumbents. As a result, access-motivated interest groups generate approximately two-thirds of the financial incumbency advantage.

Uncovering the sources of the financial incumbency advantage also informs theories of representation. We find that in-state donors respond to incumbency in the same manner as out-of-state donors, suggesting that incumbents do not provide tailored benefits to local groups through the use of their office, and we find even stronger evidence that the financial advantage of incumbents does not depend on how long they have served in the legislature. Although office benefits and seniority are still no doubt important factors in other parts of the political process—and indeed may still help create the electoral incumbency advantage—their connection to campaign finance is limited.

On the other hand, strategic interest groups are highly sensitive to incumbency. Even among these groups, the more regulated and less ideological account for a larger share of the advantage. By investing in incumbents over time, these interest groups are able to create long-lasting connections that can pay off in a variety of unobservable ways. While such an access advantage has long been understood from a theoretical

[34] We drop the 33 cases in the data in which two incumbents run against each other.

[35] The logic is as follows. When we compare Democratic winners to Democratic losers, we see that incumbency, overall, causes a large increase in donations. When we then compare repeat-incumbents to first-time incumbents, we see that the effect is near zero. Logically, then, the largest increase must be between the time a candidate runs as a challenger and the first time she runs again as an incumbent.

perspective (e.g., Olson 1965; Schattschneider 1960), its precise magnitude, along with its basis in the modern American campaign finance system, has been obscure.

The electoral incumbency advantage may provide elected officials with poor incentives if it insulates them from reelection concerns and thus leads them to exert less effort on behalf of their constituents. However, it is also possible that the electoral advantage we observe is simply the result of incumbents behaving "well" and being rewarded by voters, in equilibrium. The financial incumbency advantage we uncover is consistent with the former view. Strategic interest groups with particular goals—like the policy desires of regulated industries—coordinate to support incumbents financially. This financial support offers incumbents an amount of electoral security independent from the actions they take on behalf of their constituents if, as seems likely, strategic interest groups differ from constituents in their preferences for policy and other forms of government activity.

Chapter 4

Who Values Agenda Setters in American Legislatures?

Theories of legislative politics claim that agenda-setting institutions affect policy. Yet, data limitations have prevented scholars from assessing the conditions under which agenda control is valuable to legislators and groups seeking influence on policy. I collect a new dataset on more than 20,000 committee chairs, vice chairs and party leaders in the state legislatures (1990-2010) and link it to detailed information on campaign donations. Using a difference-in-difference design, I assess the price that firms and groups are willing to pay for access to committee and party leaders and document how it varies across industries and institutions. I show that firms are particularly sensitive to agenda setters regulating their industry. Consistent with theories of legislative organization, chairs are shown to be more valuable when committees can block bills, and party leaders more valuable when controlling the bill referral process. Finally, the value of party leaders has increased dramatically in recent years.

4.1 Introduction

Campaign donors devote substantial resources to candidates running in legislative races. In the 2014 electoral cycle, donors contributed more than 1.3 and 2.1 billion dollars to legislative candidates in federal and state elections, respectively.[1] Legislators apparently place great import on these donations: They spend hours every day on the phone soliciting contributions and participating in other fundraising activities, and campaign donors are given privileged access when scheduling meetings with legislators (Kalla and Broockman 2015).

Given the substantial amounts spent on legislative campaigns and the efforts legislators exert to attract their share of donations, it is somewhat surprising that most studies fail to show that campaign contributions have any influence on how legislators behave (Ansolabehere, de Figueiredo, and Snyder 2003; Grenzke 1989; Eggers and Hainmueller 2013; Langbein 1993; Wright 1990). According to the amassed documentation of roll-call votes, legislators are not more likely to vote in favor of bills benefiting their financial supporters. In a review of this literature, Ansolabehere, de Figueiredo, and Snyder (2003: p. 114) conclude:

> Overall, PAC contributions show relatively few effects on voting behavior. In three out of four instances, campaign contributions had no statistically significant effects on legislation or had the "wrong" sign – suggesting that more contributions lead to less support.

This puzzling pattern begs the question: If campaign contributions do not influence voting behavior, what, if anything, do contributors get in return for their financial support?

In this paper, I provide new empirical evidence suggesting that donors use campaign contributions to secure access to legislators with institutional control over the legislative agenda. On the basis of new data, I argue that donations are not used to buy floor voting coalitions, but, rather, are carefully targeted towards key legislators who may have the power to influence bills in the pre-floor stage of lawmaking.

The idea that agenda control is a valuable asset, clearly, is not new: Agenda-setting power is one of the core concepts in political science; extensive theoretical literature

[1] Source (federal): `http://www.followthemoney.org/show-me?y=2014&f-core=1&f-fc=1&c-exi=1`. Source (state): `http://www.followthemoney.org/show-me?y=2014&f-core=1&f-fc=2&c-exi=1`

is devoted to the topic, and in American politics in particular. However, in contrast to the substantial theoretical literature on agenda setting, and in contrast to the extensive empirical literature on the financial advantages enjoyed by incumbents relative to challengers (Fouirnaies and Hall 2014), our empirical understanding of the way that agenda-setting powers affect the allocation of campaign contributions is relatively limited. The lack of evidence reflects that the turnover rate for party and committee leaders is relatively low and that the institutional variation is almost non-existing at the federal level. Limited variation makes it difficult to assess by whom and under what conditions agenda setters are deemed valuable.

To get empirical leverage, I exploit the rich variation in the 99 state legislative chambers. The analyses focus on the two most important institutional assets identified in the theoretical literature on legislative organization – party leadership and committee chairmanship (Aldrich 1995; Cox and McCubbins 2005; Shepsle and Weingast 1987; Wilson 1956). I collected a new dataset of more than 20,000 observations on the identity of every chairman and vice chairman of all standing and joint committees, as well as of every minority and majority party leader across the state legislatures for each year during the period 1990-2010.

Disentangling the value of agenda control from confounding factors is not a trivial matter. Committee and party leader positions are clearly not randomly assigned, and naïve comparisons across legislators only yield causal effects under very strong, implausible assumptions. To address various selection issues, I exploit the panel structure of the data by implementing a simple difference-in-difference design. The basic idea is to compare the contributions that flow to an individual legislator before and after they attain a party- or committee-leader position while differencing out general trends across all legislators in the chamber.

The results reveal three important patterns. First, sensitivity to agenda control varies substantially among donors, and the most sensitive industries are the ones that are licensed or regulated at the state level, such as the insurance industry. In particular, industries value access to committee leaders by whom they are primarily regulated, more than they value other agenda setters. Second, the campaign-finance value of party leaders increases when they control the process of referring bills to committees, and the value of committee leaders increases when institutional rules permit committees to

block bills from reaching the chamber floor. Thirdly, while the campaign-finance value of committee leaders has remained relatively stable over the studied period, I show that the value of party leaders has grown substantially in recent years, suggesting that party leaders may have become more powerful.

The paper proceeds as follows. First, I briefly review the existing literature to discuss the motivation for donating money to committee and party leaders. Second, I introduce the new dataset and describe the identification strategy. Then, I present the main results and the results disaggregated by industries. In the two next sections, I analyze the institutional conditions under which committee and party leaders are deemed most valuable. After that, I show how the effects have changed over time. Finally, I conclude.

4.2 Institutional Assets and Campaign Finance

Scholars have long been interested in the market for public policy (McCormick and Tollison 1981; Peltzman 1976). Many studies of American politics have focused on how legislative institutions shape the distribution of political capital among legislators and how interest groups respond to this distribution.

In a seminal study, Denzau and Munger (1986) use a formal model to examine how legislative productivity affects campaign contributions. They study the interaction between vote-maximizing legislators, welfare-maximizing voters, and special interest groups seeking influence in order to derive a supply price for public policy. A key result is that a legislator's public policy supply price depends on their productivity, which in turn is determined by their portfolio of parliamentary rights. In equilibrium, interest groups will naturally target legislators endowed with important parliamentary rights because it is less costly for these legislators to influence public policy.

In this paper, I focus on two groups of legislators who, according to the literature, are particularly productive, enjoying institutional privileges that enhance their ability to influence public policy: Party leaders and committee chairs.

In the literature concerning legislative organization in American politics, *party leaders* are considered to be central agents, and one of the central debates revolves around the question of whether party leaders influence public policy (Aldrich 1995; Cox and McCubbins 1986, 1993, 2005; Rohde 1991), or whether they are superfluous agents with

no political influence (Krehbiel 1992, 1993, 2010). The theory of *Conditional Party Government* developed by Rohde (1991) and Aldrich (1995) conceptualizes parties as a floor-voting coalitions. The strengths and weaknesses of parties are determined by the distribution of preferences within and between the two major parties. The primary claim is that the willingness of legislators to delegate power to party leaders is conditional on interparty polarization and intraparty homogeneity in preferences. The theory of the *Cartel Party* developed by Cox and McCubbins (1986, 1993, 2005) conceptualizes parties as procedural coalitions. In this theory, party strength is explained by the institutional rules and procedures that allow the majority-party leaders to control the political agenda. The institutional setup in American legislatures endows majority-party leaders with certain privileges, such as the ability to appoint committee chairs, refer bills to committees, and to schedule the legislative agenda. Party leaders use negative agenda control to prevent bills that are opposed by a majority of legislators *within* the majority party from reaching the chamber floor. In return, junior party members do not vote against the party leadership on important procedural bills.

Consistent with these partisan theories, scholars have documented that lobbyists, journalists and legislators themselves perceive party leaders as more productive than other members of the legislature (Miquel and Snyder Jr 2006), and studies have shown that PACs also value party leadership positions in the US House (Ansolabehere and Snyder Jr 1998).

Committee chairmen are perceived as crucial agents in the legislative process by most scholars. Most of the legwork in American legislatures is undertaken by committees. The committee system allows members to gain knowledge and expertise in their respective fields via a division of labor and specialization in a variety of policy areas. Committee chairs are particularly important because they organize most aspects of the committees' work: Among other things, committee chairs schedule meetings, set the agenda, invite interest groups to participate in hearings, and draft and amend bills, also deciding if and when they are to be reported to the floor. Not surprisingly, committee chairs are amongst the most productive legislators as measured by the number of sponsored bills reported to the floor and passed by the legislature (Cox and Terry 2008; Frantzich 1979), as well as by opinion surveys of legislators and lobbyists (Miquel and Snyder Jr 2006). Committee and subcommittee chairs are also crucial players in the process of

distributing of public outlays (Berry and Fowler forthcoming).

Several empirical studies have been conducted on the campaign-finance value of committee membership and leadership. These studies have shown that at the federal level lobbyists and donating interest groups value assignments to powerful committees (Bertrand, Bombardini, and Trebbi 2014; Grier and Munger 1986, 1991; Grimmer and Powell 2013; Munger 1989; Stratmann 1998; Romer and Snyder Jr 1994; Vidal, Draca, and Fons-Rosen 2012) and to committee leadership positions (Ansolabehere and Snyder Jr 1998). Many studies posit that committee chairs are particularly valuable to organized interests who operate within the purview of the committee, but due to the limited variation in committee leadership it has been difficult to substantiate these claims.

To sum up, there are theoretical reasons for believing that interest groups value agenda-setting assets such as committee- and party-leader positions. Empirically, however, we know relatively little about the interest groups who value these assets and the conditions under which they do so. The intent of this paper is to fill the gap that exists in the empirical literature. In the next section, I outline how I intend to do this and describe the empirical strategy employed in the paper.

4.3 New Data on Committee and Party Leaders in the State Legislatures 1990-2010

To assess how groups value of committee and party leaders, I collected a new dataset on the identity of committee chairs, vice chairs and legislative leaders in the state legislatures. The dataset covers all standing and joint committees from 1990 to 2010 across all 99 state legislative chambers, as well as information on the following party leadership positions: Majority Leader, Minority Leader, Speaker of the House, President of the Senate, President Pro Tempore, party whips, and floor and caucus leaders from both parties.

The primary source is quarterly editions of *The State Yellow Book* published during the period 1990-2010 (Leadership Directories 2014). In cases where the relevant information in these volumes is missing or in other ways incomplete, I supplement it with information collected from archival material, such as legislative minutes and proceedings, obtained from state legislative archives.

I start by producing a list of all legislators in a given session. For each legislator in a given year, I construct two vectors in which the elements are the recorded names of any committee on which they served as chairman or vice chairman, and a third vector containing any leadership positions held during the session. Based on name, party and district number (or district name), I then link the information pertaining to each legislator in a given session to the unique candidate identifier and election year in the ICPSR dataset 34297 (Klarner et al. 2013). This will enable other researchers to easily use the data in future studies of committee and party leadership in state legislatures.

Table 4.1 – # Legislator-session Observations by state and chamber, 1990-2010.

State	Leader	Chair	Rank-and-file	State	Leader	Chair	Rank-and-file
AK	63	215	239	AL	36	280	523
AR	97	155	957	AZ	122	288	490
CA	119	413	468	CO	110	156	558
CT	318	359	1193	DE	73	225	239
FL	126	444	870	GA	128	545	1687
HI	210	213	225	IA	198	270	798
ID	90	239	742	IL	228	439	848
IN	163	278	809	KS	87	273	1050
KY	90	235	865	LA	20	167	368
MA	138	251	1611	MD	127	110	891
ME	90	165	1605	MI	178	300	850
MN	167	329	1313	MO	91	484	1225
MS	18	348	504	MT	107	218	927
NC	136	683	881	ND	73	157	750
NE	7	110	127	NH	178	291	3771
NJ	195	264	581	NM	67	192	608
NV	131	117	278	NY	257	615	1242
OH	132	263	761	OK	234	321	695
OR	144	189	417	PA	72	594	1614
RI	185	153	1010	SC	45	172	1204
SD	178	181	691	TN	119	194	843
TX	31	449	1222	UT	100	189	606
VA	54	233	913	VT	87	236	1477
WA	194	307	723	WI	89	405	662
WV	99	306	765	WY	90	155	524
				Total	6091	14175	44220

The data is reorganized such that each row correspond to a legislator, i, in a given session, t. During the period 1990-2010, we observe approximately 6,000 party leaders, 14,000 committee chairs and 44,000 rank-and-file members. Table 4.1 reports how these

legislator-session observations are distributed across states.

The next step is to link the data on legislators to information on campaign contributions donated to each legislator, i, during the period of each session, t. The data on campaign contributions in US state legislatures is obtained from the non-partisan organization The National Institute on Money in State Politics (NIMSP) via `www.followthemoney.com`. This data, which is based on candidate filings to authorities overseeing state-level campaign finance regulations, contains information on donations to candidates in legislative races across all 99 chambers. Using categorizations of donors by NIMSP, I sum up donations based on types of donors to the level of 69 industries. Since my main interest in this paper is how firms and industry organizations respond to agenda setters, I exclude donations from individuals, ideological donors as well as unions. Furthermore, to ensure that the contribution variables do not conflate money flowing to an individual candidate's campaign with fundraising on behalf of the party, I exclude all donations to leadership PACs.

Based on state, party, district, candidate name and election year, I connect the campaign finance data with the committee and leadership data. To minimize merging errors stemming from minor differences in the spelling of candidate names in the two datasets (e.g. matching "William Hanson" and "William Hansen"), I calculate the Jaro-Winkler distance, a measure of similarity of strings, between the names of the candidates in the two datasets and match the most similar name strings within a given district, party and year.[2] In the rare cases where a candidate appears in the Klarner et al. (2013) dataset, but not in the NIMSP data, I code the candidates' contributions as zero.[3] For some states, the campaign finance data extends back to 1990, whereas for others it does not start until the middle or late 1990s. Table B.1 in the Appendix shows, state by state, the period for which data on campaign contributions is available as well as the total number of observations in the final sample. To ensure comparability across years, I adjust all campaign contributions to 2014 constant prices using a standard Consumer Prices Index.[4] Table 4.2 reports the basic summary statistics of some of the

[2]For details on the Jaro-Winkler calculations, see Winkler (1990) and `https://en.wikipedia.org/wiki/Jaro%E2%80%93Winkler_distance`

[3]When possible, I consult the original campaign finance files to confirm that the candidate in question did not receive any campaign contributions

[4]The Consumer Price Index can be downloaded from the website `http://data.bls.gov/`. Series Id: CUSR0000SA0

key variables used in the analysis.

To analyze how the campaign-finance value of committee and party leaders vary across institutional settings, I also collect data on the institutional rules governing the bill referral process. This information is primarily obtained from annual editions of *Book of the States* published by the non-partisan organization The Council of State Governments during the period 1990-2010 (Council of State Governments 1990). For each chamber, I record whether the authority to refer bills to committees primarily rests with the Speaker of the House, the President of the Senate, the President Pro Tem, the Majority Leader, a Committee on Referrals, the Rules Committee, or if the authority is delegated to anyone else. After linking this data to the dataset on committee and party leaders, I create a dummy equal to one if a given legislator is responsible for referring bills to committees. In the chambers where the authority is delegated to a committee (Committee on Rules, Committee on Referrals), I code the variable 1 for the chair of that committee, and zero otherwise.

Data on institutional rules governing whether or not committees can reject reporting bills to the chamber is obtained from Anzia and Jackman (2013). To construct an indicator for majority-party status, I use the data on the partisan control of state governments that have been collected and used in a series of papers by Klarner (2003).[5] Information on limits on campaign contributions as well as the Squire Index of professionalization come from Barber (N.d.)'s replication data.

In Table 4.2, I provide basic summary statistics for key variables used in the analysis.

4.4 Empirical Strategy: A Difference-in-Difference Design

In this section, I outline how I use campaign contributions to quantify how different groups value the institutional powers of committee and party leaders. From a methodological perspective, the main challenge is to isolate the institutional value from other characteristics of committee and party leaders.

First, I define the key treatment variables of interest. In the main analyses, I focus on two dummies indicating whether a given legislator, i, is assigned a committee chair or party leader position in a given session, t. The chairman treatment variable is defined

[5]The data can be downloaded from the following website: `http://www.indstate.edu/polisci/klarnerpolitics.htm`

Table 4.2 – Summary Statistics. The table reports the descriptive statistics on key variables used in the analyses.

	Lower Chambers		Upper Chambers	
	Mean	St.Dev.	Mean	St.Dev.
Chair	0.20	0.40	0.44	0.50
Leader	0.07	0.26	0.19	0.39
Industry Total	34377.14	98715.84	62499.43	1.2e+05
Agriculture	643.25	3147.91	1104.11	4630.63
Commerce	3193.15	15854.11	5573.36	19722.54
Communications	1236.40	6771.72	1841.17	6742.45
Construction	1515.97	6549.28	2731.37	8444.76
Defense	20.31	226.24	41.35	366.50
Energy	1971.85	7326.21	3167.26	9486.51
Finance	4316.69	17902.18	7126.05	20576.61
Health	3575.06	13745.00	6468.26	17543.29
Manufacturing	344.68	2296.15	646.53	9493.08
Transportation	1180.66	4243.67	2011.19	5468.58
Observations	30093		6996	

NOTE: The reported statistics are calculated based on legislator-years. Standard deviations are reported in parentheses.

in Equation 4.1.

$$\text{chair}_{it} = \begin{cases} 1 & \text{if } \textit{legislator}_i \text{ chairs any committee at time } t \\ 0 & \text{otherwise.} \end{cases} \tag{4.1}$$

Similarly, the party leader treatment is defined as

$$\text{leader}_{it} = \begin{cases} 1 & \text{if } \textit{legislator}_i \text{ is majority leader, minority leader, speaker,} \\ & \text{president, president pro tem, or whip at time } t \\ 0 & \text{otherwise.} \end{cases} \tag{4.2}$$

If the committee and party leaders positions were assigned to a random subset of legislators, a simple comparison of means would yield the average causal effect of the treatments. In the absence of a randomized experiment, a simple comparison of contributions would not, in all likelihood, reflect the causal effect. Committee chairs and party leaders differ in many systematic ways that could have an influence on campaign contributions. For example, high-quality legislators are more likely to serve as leaders, and,

presumably, these qualifications also help them to attract campaign contributions. In this example, a simple OLS regression based on cross-sectional data would overestimate the true causal effects.

To deal with selection problems like this, I implement a simple panel difference-in-difference design exploiting the fact that I observe each legislator over multiple sessions. The idea is to compare the money that flows to an individual legislator before and after they attain a committee- or party-leader position while differencing out general trends in contributions in a given year. This design washes out all of the time-invariant characteristics of a legislator (quality, party, basic ideology, charisma, etc.) as well as shocks that are common to all legislators (trends in campaign contribution patterns, mid-term effects etc.). Although the difference-in-difference design is by no means as ideal as a randomized experiment, it does capture the causal effect based on assumptions that are much weaker than those employed in the simple cross-sectional design, and there are good reasons to believe that these assumptions are, in fact, justified in the current setting.

The difference-in-difference design identifies the average causal effect under the key assumption that the treated legislators would have followed the same trend as the non-treated legislators in the absence of treatments. This assumption is likely to be satisfied in the current setting because legislators cannot self-select into the treatment groups: Appointments to party- and committee-leader positions are determined by many factors that an individual legislator could not possibly manipulate single-handedly. Variation in committee- and party-leader status is typically induced by changes in majority party or by senior legislators who retire. These are factors are very difficult for an individual legislator to control.

Based on the panel dataset, which I described in detail in the previous section, I use OLS to estimate the following baseline equation

$$log(1 + contributions_{it}) = \alpha_i + \delta_t + \beta_1 chair_{it} + \beta_2 leader_{it} + \mathbf{x}_{it}\theta + \varepsilon_{it}, \qquad (4.3)$$

where $contributions_{it}$ measures the campaign contributions allocated to legislator i during session t; $chair_{it}$ and $leader_{it}$ are the dummy variables defined in equations 4.1 and 4.2, respectively; α_i denotes legislator-fixed effects that control for time-invariant char-

acteristics of a legislator; δ_t represents state-year fixed effects that control for common shocks among legislators in a given year; $\mathbf{x}_{it}$ is a vector of additional covariates; finally, ε_{it} is the error term. β_1 and β_2 are the coefficients of interest.

A priori, there are good reasons to believe that the identifying assumptions are satisfied in this specific setting, but a few concerns still need to be addressed. First, one might be concerned about selection into the sample caused by retiring legislators. Campaign contributions are only observed for legislators who decide to run for re-election between session t and $t+1$. For obvious reasons one cannot observe campaign contributions to retired candidates, and this might be a cause for concern if the decision to retire is correlated with the treatments and related to campaign donations. Imagine, for example, that following a shift in majority power, some of the legislators who previously served as committee chairs decide to retire. If the legislators who retire are the ones who expect to receive few contributions without the chairman status, the sample will be weighted towards the better performing legislators inducing a downward bias in the chair coefficient. While this story seems plausible and less of a concern because it biases against the expected findings, it is not possible to characterize, more generally, the magnitude or direction of the bias without applying additional assumptions about the motivations for retirement. To address this selection concern, I estimate the effects using an alternative specification in which the unit of analysis is the district rather than the individual legislator and report these estimates in Table B.3 in the Appendix. The results from these statistical analyses are very consistent with the findings presented in the paper, suggesting that the retirement selection issue is not a major cause for concern.

Second, one might worry about trending legislators. Imagine, for example, that legislators gradually become more skilled at fundraising and that senior legislators are more likely to get promoted to party and committee leadership positions. Since seniority would be correlated with the treatments and a predictor of the outcome, the parallel-trends assumption would be violated inducing bias in the estimated coefficients. I deal with the potential trending concerns in several ways. In the statistical analysis I include a vector of additional covariates, $\mathbf{x}_{it}$, that allows for certain types of trending behavior. The vector includes seniority dummies and a majority-party indicator. Furthermore, I test whether the treatment assigned at time t can predict the outcomes at time $t < 0$

and report these results in Figure B.2. These results suggest that the pre-treatment effects are statistically indistinguishable from zero.

4.5 Main Results: The Value of Committee and Party Leadership

The main results are presented in Table 4.3. The results from the baseline model without additional covariates, presented in column 1, indicate that committee and party leader positions are highly valuable institutional assets. On average, attaining a party leader position causes a boost in campaign contributions in the magnitude of 0.32 log points, whereas a committee chair position leads to a 0.26 log-point increase in contributions. The results are highly statistically significant.

In the second model, I relax the common trends assumption by including seniority dummies and a majority-party indicator. The estimated effects of committee chairs and party leaders are almost identical to the results presented in column 1 suggesting that trending, at least with respect to seniority and majority status, is not a major cause for concern.

In model 3, I add a vice chair dummy to the regression in order to explore whether industries narrowly target committee chairs or funnel money towards ranking committee members more generally. The coefficient on the vice chair dummy is positive but only marginally significant. In terms of magnitude, the coefficient on the chairman variable is approximately 5 times larger than the coefficient on the vice chair variable. This could indicate that firms and interest groups place great value on the procedural privileges enjoyed by the committee chairman, while they care less about access to senior committee members per se.

In column 4, I report the results from a model in which I explore how the value of the chair positions depends on the type of committee. I add a dummy indicating whether the legislator chaired one of the committees that are often assumed to be among the most powerful and prestigious: Rules, Ways & Means, Appropriations, and Finance. The results indicate that chairs of less prestigious committees experience a boost of approximately 0.2 log points when they are appointed to lead the committee, whereas being appointed to chair one of the most the most prestigious committees causes a boost in campaign contributions of approximately 0.48 log points (0.2+0.28). The difference

Table 4.3 – Effect of Committee- and Party-leader Positions on Industry Contributions. Party and committee leaders are deemed valuable by firms and interest groups.

	log(1+Industry Contributions$_{it}$)			
	(1)	(2)	(3)	(4)
Leader	0.32**	0.31**	0.31**	0.31**
	(0.04)	(0.04)	(0.04)	(0.04)
Chair	0.26**	0.24**	0.25**	0.20**
	(0.03)	(0.03)	(0.03)	(0.03)
Vice			0.05†	0.05
			(0.03)	(0.03)
Chair of Top Committees				0.28**
				(0.07)
Majority Member		0.06	0.05	0.05
		(0.03)	(0.03)	(0.03)
Observations	31397	31089	31089	31089
Legislators	9170	9097	9097	9097
Legislator Fixed Effects	✓	✓	✓	✓
State-Year Fixed Effects	✓	✓	✓	✓
Seniority Fixed Effects		✓	✓	✓

NOTE: All models are estimated using OLS. Standard errors are clustered on legislators and are reported in parentheses.

is substantial in magnitude and statistically significant.

In Tables B.4 and B.3 in the Appendix, I show that the results are robust when measuring the outcome in percent of total donations instead of logged donations, as well as when the difference-in-difference estimator is implemented at the level of the district instead of the individual legislator. In the next section, I examine in details how different industries value access committee and party leaders.

4.6 Regulated Industries Most Sensitive to Agenda Setters

To explore how sensitivity to agenda-setting institutions vary across groups, I subset the campaign finance data by donating industries and examine how these industries respond to changes in institutional power. In particular, I compare how much money an industry, j, donates to a legislator, i, before and after he attains an institutional asset at time t. Using OLS, I estimate equations of the form:

$$log(contribution_{ijt} + 1) = \alpha_i + \delta_t + \beta_{1,j} chair_{it} + \beta_{2,j} leader_{it} + \mathbf{x}_{it}\theta + \varepsilon_{it}, \qquad (4.4)$$

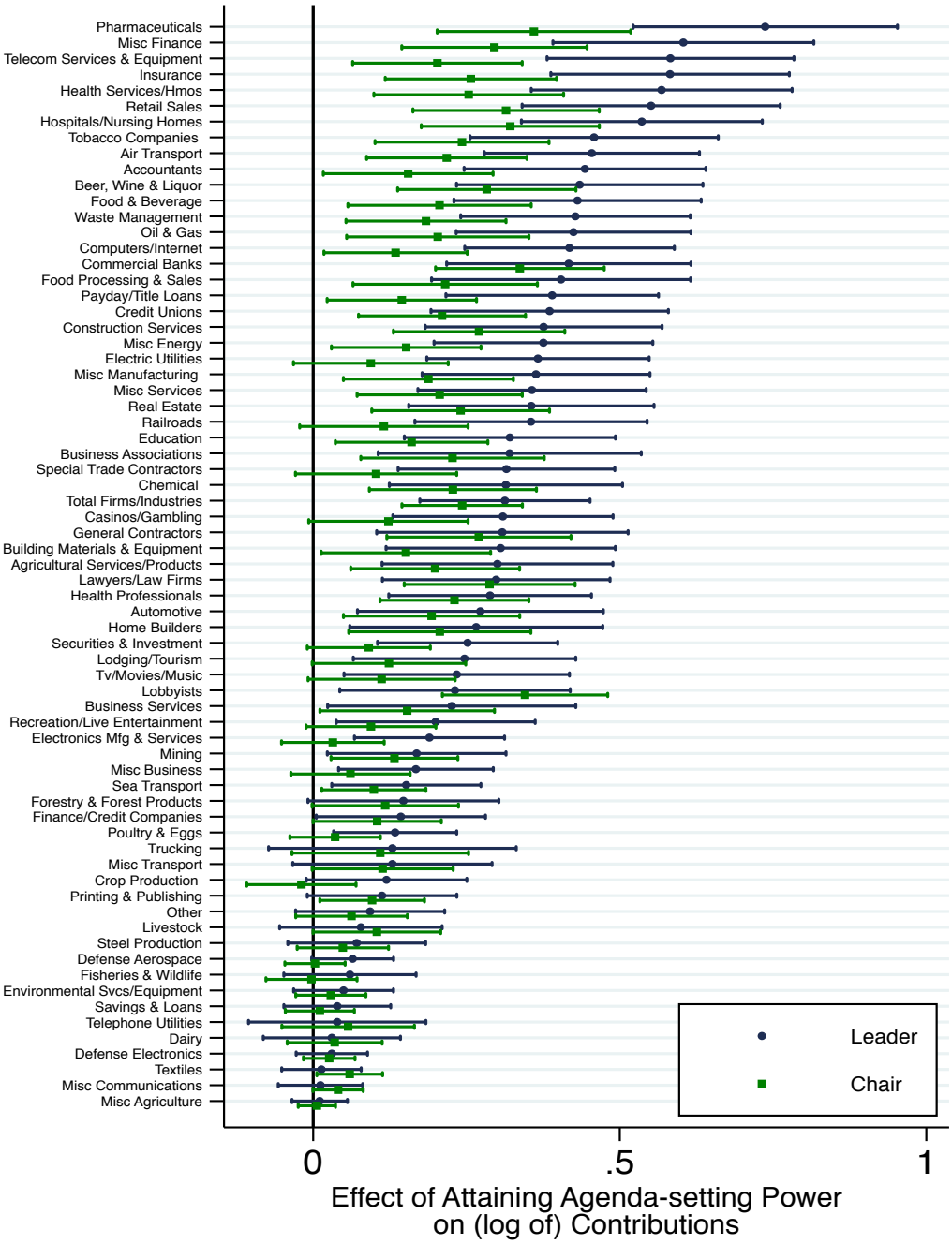
where $contribution_{ijt}$ is the money donated to legislator i by industry j at time t; α_i and δ_t represent legislator- and state-year-fixed effects; $chair_{it}$, and $leader_{it}$ are the variables defined in Equations 4.1, and 4.2, respectively; $\mathbf{x}_{it}$ is a vector of additional covariates including seniority dummies and majority-party status. The estimates of $\beta_{1,j}$, $\beta_{2,j}$, indicate the average value, as measured by campaign donations, that industry j assigns to committee chairs and party leaders, respectively. In Figure 4.1, I report the estimates for different industries.[6]

The figure illustrates that the sensitivity to leaders and committee chairs varies considerably across industries. Some industries appear to be very sensitive to agenda-setting institutions, while firms in other industries only change their donations marginally when a legislator rises to power.

Industries that are heavily regulated at the state level appear to be very sensitive to agenda setters. The insurance industry is perhaps the most notable example of an

[6]To deal with multiple-testing concerns, I report Bonferroni-adjusted confidence intervals.

Figure 4.1 – Effect of Attaining Committee- and Party Leader Positions on (log of) Contributions by Industry. Industries that are regulated at the state level, such as insurance, health care, and energy, are more sensitive to state legislative agenda setters than industries that are less dependent on political decisions at the state level, such as defense.



NOTE: The x-axis shows the estimated effect of attaining a position of party leader and committee chair on log(1+contributions) from the respective industry indicated on the y-axis. The bars indicate the 95%-confidence interval. To deal with multiple-testing issues, the confidence intervals are adjusted using Bonferroni corrections taking into account that I run the regressions for each of the 69 industries, i.e. the lower bounds are calculated the following way: point estimate $-\Phi^{-1}(1-0.05/69)\times$ standard error.

industry which is primarily regulated at the state level. On average, firms and interest groups in the insurance industry increase their contributions by approximately 0.6 log points and 0.25 (or equivalently $e^{0.6} - 1 \approx 80\%$ and $e^{0.25} - 1 \approx 28\%$) when a legislator attains a leadership or a committee chair position, respectively. Similarly, firms and interest groups in heavily regulated sectors such as health and energy also appear to be very sensitive to agenda setters when allocating campaign contributions.

The state legislatures are responsible for administrating occupational licensing in a number of industries (Kleiner and Krueger 2010). For example, the licensing of pharmacists, accountants, lawyers, health professionals is organized at the state level in all 50 states (Summers 2007). According to the estimates reported in Figure 4.1, these groups are noticeably sensitive to agenda setters. This finding is consistent with the hypothesis that interest groups representing licensed industries are "raising funds from members to lobby the state legislature, particularly the chairs of appropriate committees" (Kleiner and Krueger 2010: p. 678).

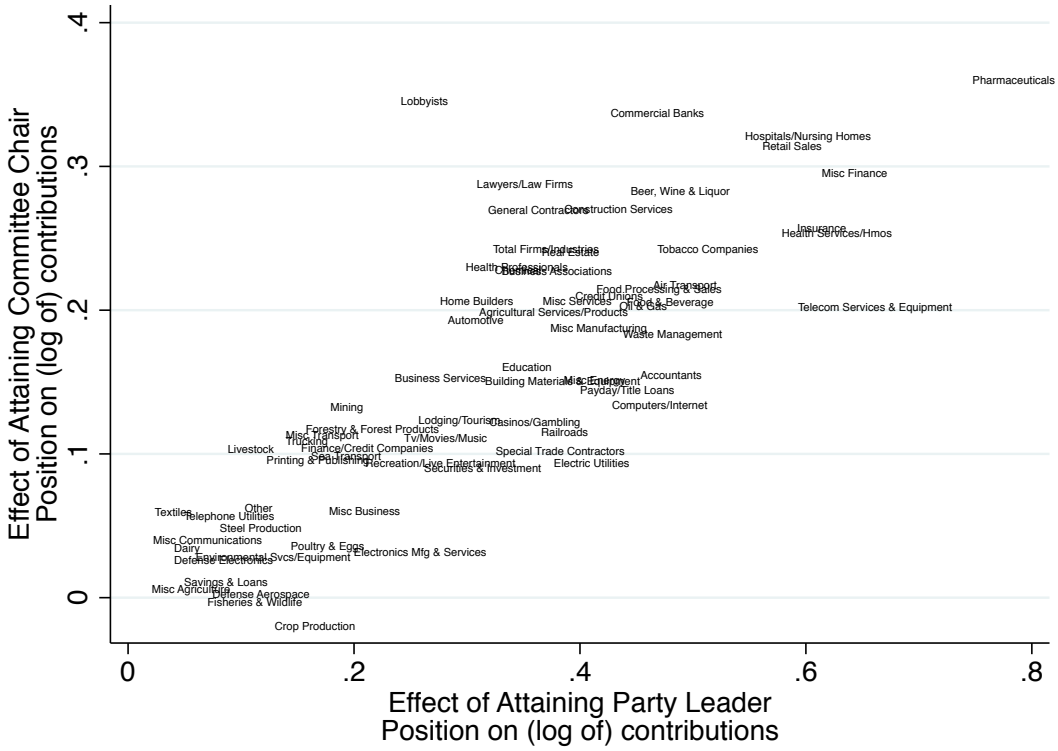
In contrast, the other end of the sensitivity spectrum is dominated by firms and interest groups that are less affected by state-level policies, such as industries in the Defense sector.

Do industries perceive donations to committee chairs and party leaders as investments in substitutable or complimentary assets? In Figure 4.2, I explore this question more closely by plotting an industry's estimated committee-chair sensitivity against its sensitivity to party leaders. The figure reveals a very clear positive trend indicating a strong positive correlation between sensitivity to committee and party leaders. This could suggest that donating firms and interest groups value committee- and party-leader positions as complementary assets, perhaps because successful lobbying involves control over both the bill referral process and the committee stages of lawmaking. In the next sections, I explore these questions more thoroughly.

4.7 Donors with Vested Interests Value Chairs Who Can Block Bills

The variation across industries reported in Figures 4.1 and 4.2 could suggest that firms and interest groups are more likely to target party and committee leaders in the state legislatures when subject to state-level legislation and regulation. Although suggestive,

Figure 4.2 – Industries Value Committee- and Party-Leader Positions as Complementary Assets. Industry evaluations of committee and party leaders are strongly positively correlated, suggesting that industries perceive the two types of leader positions as complementary assets rather than as substitutes.



NOTE: The x- and y-axis report the estimated effects of attaining positions of party and committee leaders, respectively, on log of contributions from the relevant industry.

the results do not make it clear whether industries funnel money towards agenda setters in an attempt to influence public policy.

To better understand the conditions under which firms and interest groups seek access to agenda setters, I test two things in this section:

1. Are industries regulated by an issue-specific committee more sensitive to the chair of that particular committee relative to other committee chairs?

2. Does the sensitivity of donors with interests in a given committee increase when institutional rules allow committees to prevent bills from reaching the chamber floor?

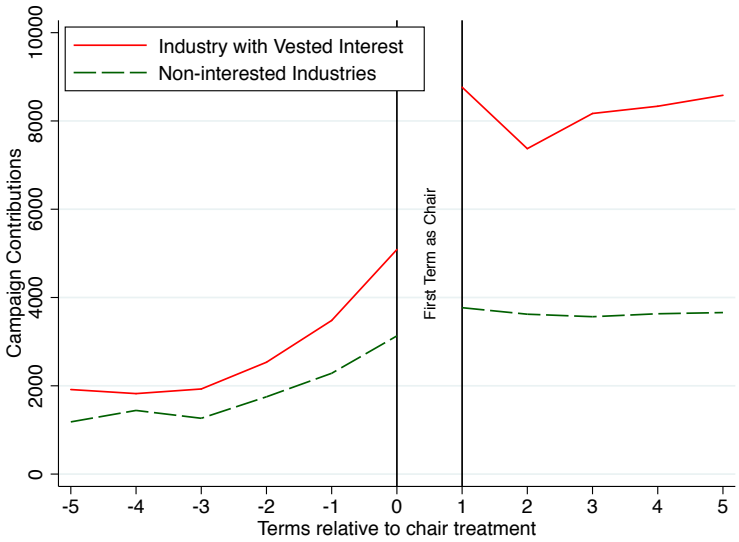
I use donations from non-interested industries as a benchmark for evaluating whether industries with vested economic interests in a particular committee are particularly sensitive to the chairman of that committee. I restrict the sample to ten industries for which the industry-committee mappings are fairly clear and meaningful in most states:

Agriculture, Energy, Insurance, Banking, Health, Transportation, Military Affairs, Construction, Education, and General Business. I reshape the data such that each row is uniquely identified by a legislator (i), committee/industry (j) and time (t).

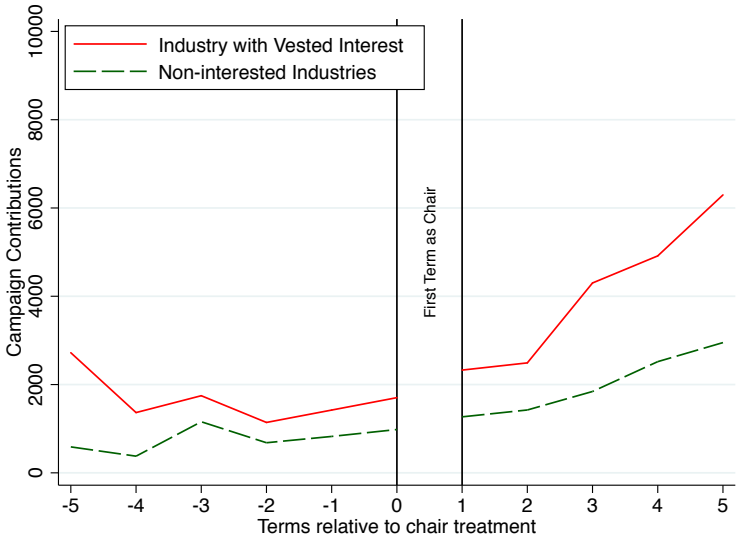
First, I present a simple graphical difference-in-difference analysis. I define the treatment group as legislators who, at some point in their careers, were appointed to chair one of the committees listed above. For each legislator in the treatment group, I define a variable, t, which measures the terms relative to the change in committee-chair status. This means that the legislator is not chairing the committee when $t \leq 0$; the legislator is serving as chair when $t > 0$. I can now calculate the average contributions flowing to legislators from the treated sector in both the pre- and post-treatment period. Further, I construct a control group using donations to the same legislator from the remaining, non-interested industries. More specifically, I calculate the average contributions flowing from across all other industries to a legislator and match each of these control observations with the legislator in a given year. Thus, the treatment group consists of donations from industry j to legislator i before and after their appointment to committee chair, whereas the control group consists of the average donations from all other industries to legislator i in a given year.

Figure 4.3 – Effect of Attaining Chair Position on Donations from Industries with Vested Interests. Committee chairs are given special treatment by industries that are within the purviews of the respective committees. The effect is most pronounced when institutional rules allow committees to prevent bills from reaching the chamber floor.

(a) Committees Can Block Bills



(b) Committees Can't Block Bills



The graphical results are presented in Figure 4.3. I split the sample according to the institutional rule governing whether or not committees can block bills from reaching the chamber floor. The panels illustrate how chair-status affects money flowing from industries that operate within the purview of the committee in question to the chair of that committee. The vertical lines indicate the cutoff separating pre and post-treatment period. The solid lines represent average donations from firms that are regulated by the

legislator when $t > 0$, whereas the dashed lines represent money flowing to the same legislator from industries that are not regulated by the committee in question.

In panel (a), I report the findings for the chambers in which committees have the authority to block legislation from reaching the chamber floor. We see that the two groups more or less follow the same common trend in the pre-treatment period: The donations from the interested and non-interested industries roughly follow each other prior to the term of the legislator's assignment to the chair position. By simply eyeballing the graph, it is clear that legislators experience a substantial boost in contributions from the industries they oversee immediately after they attain the industry-relevant committee-chair position. This could suggest that industries may seek access to chairs in an attempt to influence public policy to their advantage.

In panel (b), I report the results from the same exercise, but based on data from the chambers in which committees cannot block bills from reaching the chamber floor. In the periods before the legislator is appointed to the chair position, we see again that, on average, donations from the affected and unaffected industries run relatively parallel to one another, suggesting that it is not unreasonable to assume that the treatment and control groups would have followed the same trends in the absence of treatment. In the terms following the appointment to the chair position, we observe that donations from the interested industries increase. Although there is a visible effect on donations from industries with vested interests in the committee, the increase is moderate compared to the substantial jump in panel (a).

Overall, the graphical evidence suggests that industries with vested interests in particular committees target the chairs of those committees, and that this donation pattern is most pronounced when institutional rules allow committees to kill bills at the committee stage of lawmaking.

To further understand how the connection between regulated industries and committees affects the allocation of campaign contributions, I conduct a statistical analysis with an additional control group. In this difference-in-difference-in-difference design, I use donations from the industry in question to other legislators as an additional control group. More specifically, I estimate the following model using OLS based on the data described above:

$$log(1 + contributions_{ijt}) = \alpha_{ij} + \gamma_{it} + \delta_{jt} + \beta_1 chair_{ijt} + \varepsilon_{it}, \tag{4.5}$$

where $contributions_{ijt}$ measures the donations that flow to legislator i from industry j at time t; α_{ij} represents legislator-industry fixed effects that capture all time-invariant legislator-industry factors, such as prior work experience in the industry, basic policy preferences relating to industry regulation, etc.; γ_{it} denotes legislator-year fixed effects that wash out all characteristics of a legislator in a given year which affects all industries in the same way, such as leadership positions, majority status, as well as legislator trends, etc.; δ_{jt} indicates industry-year fixed effects that control for industry-specific trends over time.

The results are presented in Table 4.4. In the first column, we see that that the statistical results confirm the graphical analysis. The positive and statistically significant coefficient indicate that, relative to other industries, those that are regulated by a specific committee funnel substantially more money towards the chair of that committee compared to the industry's donations to the legislator before he attained the chair position. The magnitude suggest that, on average, an industry-relevant chair position causes a 0.27 log-point boost in donations from the industry to the promoted legislator, or approximately a 31% increase ($e^{0.27} - 1 \approx 0.31$).

Next, I explore how regulated industries respond towards industry-relevant committee vice chairs. The committee vice chair plays an important role in many state legislative committees: The vice chair helps the chair to schedule and organize the work of the committee, is typically the second-ranking majority party member, and is often the next in line to chair the committee upon the retirement of the chair. Accordingly, one would expect that these positions would also be deemed valuable by industries that are motivated to influence the political agenda. To test this, I add a vice-chair indicator to the estimated equation and present the results from this analysis in column 2. The coefficient on the vice chair dummy is also positive and statistically significant. In terms of magnitude, a vice chair position, on average, causes a 0.09 log-point increase in contributions from industries with economic interests in the committee relative to other unaffected industries. Presumably, this reflects the fact that vice chairs have some power to influence the political agenda, but not as much as committee chairs.

In column 3-8, I turn to the prediction that committee chairs are deemed more

valuable by regulated industries when institutional rules allow committees to block bills from reaching the chamber floor. Specifically, I augment Equation 4.5 by including an interaction between the chair indicator and a dummy equal to one if committees control whether to report bills to the parent chamber.[7] The coefficient on the interaction term is positive and statistically significant suggesting that industries value chairs more in chambers where the committee can decide whether or not to report a bill to the chamber.

Of course, one has to be careful when interpreting the results since the legislative institutions are not randomly assigned. The triple-difference design identifies the causal effects of committee chair positions under relatively weak assumptions, and the results reported in model 3 show how this effect is correlated with, but not necessarily causally affected by, the bill-blocking institution. However, in columns 4-8, I show that the basic finding is robust when I control for some of the most obvious potential confounders. In particular, I interact the treatment variable with a dummy indicating whether committees can reject hearing bills, the presence of term limits, levels of legislative professionalization as measured by the Squire Index (Squire 2007), population size (standardized by the standard deviation), and an indicator for state laws permitting unrestricted PAC donations to candidates. Across all models, the coefficient on the interaction term is positive and statistically significant. The fact that industries place greater value on committee chairs who reside in chambers where they may block legislation could suggest that regulated industries target those chairs in an attempt to influence whether or not a bill is reported to and, ultimately, taken to a vote on the chamber floor. Consistent with the importance of negative agenda power highlighted by theories of the procedural *Cartel party*, the strong findings could indicate that negative agenda control is deemed valuable by firms and interest groups that have a lot to lose from changes in state-level policy.

If an interest group seeks to influence the fate of a bill, it is crucial that the bill is referred to the right committees. In the next section, I explore this aspect of legislative agenda setting.

[7] Note that I do not include Block Reporting Bills$_{it}$ as a separate variable because factors that are constant within chamber-years are washed out by the legislator-year-fixed effects.

Table 4.4 – Effect of Attaining Committee Chair Position on Donations from Industries with Vested Interests. Industries with a vested interest in a committee are most sensitive to the chair and vice chair. The sensitivity increases in chambers in which committees can prevent bills from reaching the chamber floor.

	log(Contributions$_{ijt}$)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Chair$_{ijt}$	0.27**	0.28**	0.07†	0.06	0.06	0.11*	0.18**	0.12
	(0.02)	(0.02)	(0.04)	(0.05)	(0.05)	(0.05)	(0.06)	(0.08)
Vice$_{ijt}$		0.09**						
		(0.02)						
Chair$_{ijt}$×								
Block Reporting Bills$_{it}$			0.25**	0.23**	0.23**	0.24**	0.25**	0.25**
			(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)
Chair$_{ijt}$×								
Block Hearing Bills$_{it}$				0.03	0.03	0.06	0.03	-0.01
				(0.05)	(0.05)	(0.06)	(0.06)	(0.07)
Chair$_{ijt}$×								
Term Limits$_{it}$					0.01	0.06	0.04	0.02
					(0.05)	(0.06)	(0.06)	(0.06)
Chair$_{ijt}$×								
Squire Index$_{it}$						-0.44*	-0.71**	-0.42
						(0.21)	(0.27)	(0.30)
Chair$_{ijt}$×								
Population$_{it}$							0.04†	0.03
							(0.02)	(0.03)
Chair$_{ijt}$×								
Unlimited PAC Donations$_{it}$								0.07
								(0.05)
Observations	314010	314010	314010	312910	312910	312910	312910	257880
Legislators	9171	9171	9171	9135	9135	9135	9135	7927
Legislator-Sector FE	✓	✓	✓	✓	✓	✓	✓	✓
Legislator-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Sector-Year FE	✓	✓	✓	✓	✓	✓	✓	✓

NOTE: All models are estimated using OLS. Standard errors are clustered on legislators and are reported in parentheses. Significance levels: † 0.10, ∗ 0.05, ∗∗ 0.01.

4.8 Party Leaders More Valuable When Controlling Bill Referrals

To examine whether control over the bill referral process is an institutional asset valued by donating industries, I use OLS to estimate variations of the following baseline model

$$log(1 + contributions_{it}) = \alpha_i + \delta_t + \beta_1 bill\ referrals_{it} + \mathbf{x}_{it}\theta + \varepsilon_{it}\,, \tag{4.6}$$

where $bill\ referrals_{it}$ is a dummy taking on the value of 1 if legislator i controls the process of referring bills to committees in session t; all other variables are the same as defined in Equation 4.3.

I present the results from this analysis in Table 4.5. In the first column, I report the estimated coefficients from the simple baseline model outlined in Equation 4.6. The positive and statistically significant coefficient on the bill-referral dummy suggests that the access to legislators with authority over the process of referring bills to committees is greatly valued by donating industries. In terms of magnitude, the estimated coefficient is considerable.

One might be concerned about multiple treatments. In addition to referring bills to committees, party leaders perform many other tasks that donating industries may deem as valuable. I deal with this concern in models 2-4. In column 2, I present the results from an augmented model in which I include dummies for committee and party leaders. I include these variables to parse out factors that are common across all types of party and committee leaders. The coefficient of interest is still positive and strongly statistically significant.

Next, I add an interaction between majority-party status and the leadership indicator to the estimated model. The basic results remain the same: The legislator who acquire institutional control over the process of referring bills to committees experience a substantial boost in donations relative to legislators who secure other majority-party leadership positions.

Finally, in column 4, I exploit that the responsibility for referring bills to committees varies across and within states in order to wash out effects that are constant across different types of leader positions. I include dummies for the following types of leader positions: Speaker of the House, President of the Senate, President Pro Tempore, Chair-

man of Rules and Chairman of Referrals. The results still support the hypothesis that firms and interest groups place great value on access to legislators who control the bill referral process.

Table 4.5 – Industries Value of Institutional Control of Bill Referrals. Legislators who attain institutional control over the bill referral process experience a substantial boost in contributions from donating industries.

	log(1+Contributions$_{it}$)			
	(1)	(2)	(3)	(4)
Control of the Bill Referral Process	0.87**	0.74**	0.80**	0.46*
	(0.16)	(0.16)	(0.16)	(0.20)
Leader		0.27**	0.23**	0.22**
		(0.04)	(0.05)	(0.05)
Chair		0.24**	0.25**	0.25**
		(0.03)	(0.03)	(0.03)
Leader × Majority			0.29**	0.31**
			(0.09)	(0.09)
Majority Member	0.07*	0.05	0.05	0.05
	(0.03)	(0.03)	(0.03)	(0.03)
Observations	31089	31089	31089	31089
Legislators	9097	9097	9097	9097
Legislator Fixed Effects	✓	✓	✓	✓
State-year Fixed Effects	✓	✓	✓	✓
Seniority Fixed Effects	✓	✓	✓	✓
Vector of Leader Positions				✓

NOTE: All models are estimated using OLS. Standard errors are clustered on legislators and are reported in parentheses.

The finding that power over the bill referral process is a valuable institutional asset is consistent with the theoretical argument outlined by Cox and McCubbins (2005) in the theory of the procedural *Cartel Party*. If a legislator introduces a bill which is supported by a majority of legislators on the chamber floor, but opposed by party leaders, the leader who controls bill referrals may obstruct the bill by sending it to one or several committees controlled by legislators who also oppose the bill (Young 1996). It appears that campaign donors are willing to pay a premium for access to leaders who can negatively control the legislative agenda.

In the next section, I explore whether the value of committee- and party-leader

positions has changed over time.

4.9 Party-Leader Positions Are Becoming More Valuable

To examine how the value of committee and party leaders has evolved over time, I exploit that I observe leaders in multiple states in a given year. This enables me to interact the committee- and party-leader indicators with dummies for each of the years in the studied period and estimate the saturated model outlined in equation 4.7 using OLS

$$log(1+contributions_{ijt}) = \alpha_i + \delta_t + \sum_{t=1990}^{2010} [\beta_{1,t} chair_{it} \times \delta_t + \beta_{2,t} leader_{it} \times \delta_t] + \mathbf{x}_{it}\theta + \varepsilon_{it}\,, \tag{4.7}$$

The key coefficients of interests are $\beta_{1,t}$ and $\beta_{2,t}$. These coefficients capture the average campaign-finance value of committee- and party-leader positions, respectively, in a given year, t.

To begin, I estimate Equation 4.7 separately for five of the key sectors discussed above and report these results in Figure 4.4. In the panels on the left and right, I plot the estimates of $\beta_{1,t}$ and $\beta_{2,t}$, respectively, as a functions of t. The solid line indicates the point estimates, whereas the dashed lines represent the 95% confidence intervals.

Each of the panels reveal the same basic pattern: The value of attaining a committee leadership position has remained relatively constant over time, whereas party leaders seems to have become more valuable. On average, industries boost contributions to legislators attaining to a committee chair position by approximately 0.15-0.2 log points, and this effect remains relatively constant over time.

In contrast, the estimated value of party leader positions appears to have increased over the studied period. The graphs indicate that the campaign-finance value of party-leader positions grew steadily from around the mid 1990s and onwards. In the early 1990s, donating industries were equally sensitive to committee and party leaders, but by the end of the 2000s donors increase their contributions much more dramatically to leaders than to committee chairs.

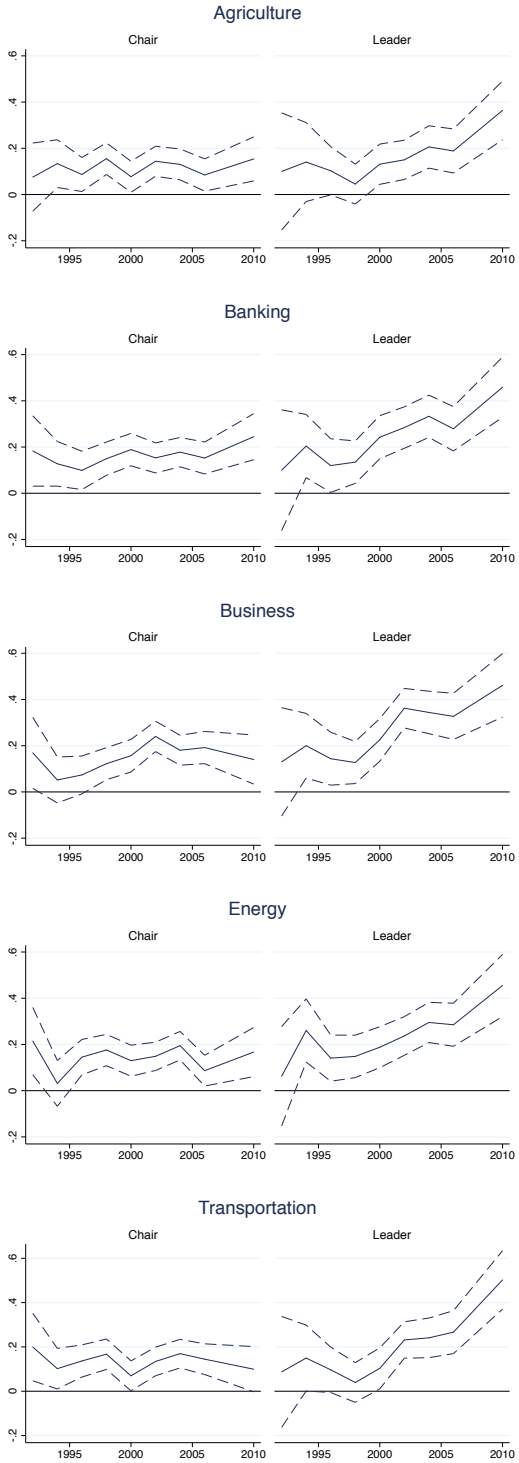
Next, I explore more formally whether the trends identified in Figure 4.4 reflect a systematic pattern across all industries. In particular, I test whether the campaign-

Table 4.6 – The Growing Value of Party-Leader Positions. On average, the campaign-finance value of party-leader positions has grown over time, whereas the value of committee chairs has remained relatively stable over the studied period.

	Full Sample		Constant Sample		Pre-session Donations	
	(1)	(2)	(3)	(4)	(5)	(6)
Leader× t	0.019*	0.021*	0.024*	0.023*	0.075**	0.073**
	(0.009)	(0.009)	(0.011)	(0.011)	(0.016)	(0.016)
Chair× t	0.005	0.006	-0.003	-0.003	0.017	0.014
	(0.006)	(0.006)	(0.008)	(0.008)	(0.011)	(0.011)
Leader	0.100	0.056	0.038	0.044	-0.754**	-0.703**
	(0.113)	(0.112)	(0.145)	(0.144)	(0.215)	(0.215)
Chair	0.207**	0.176*	0.311**	0.303**	-0.156	-0.092
	(0.074)	(0.074)	(0.092)	(0.097)	(0.143)	(0.146)
Majority Member		0.052		0.048		0.036
		(0.034)		(0.033)		(0.062)
Observations	31397	31089	7477	7471	26362	26159
Legislators	9170	9097	2008	2007	8044	7995
Legislator Fixed Effects	✓	✓	✓	✓	✓	✓
State-Year Fixed Effects	✓	✓	✓	✓	✓	✓
Seniority Fixed Effects		✓	✓	✓	✓	

NOTE: All models are estimated using OLS. Standard errors are clustered on legislators and are reported in parentheses.

Figure 4.4 – The Growing Value of Party-Leadership Positions. The value of committee-chair positions have remained relative stable over time, while the value of party leadership positions has increased since the turn of the century.



NOTE: In each panel, the y-axes show the estimated campaign-finance value of committee- and party-leader positions, respectively, as functions of the year indicated on the x-axes. The dotted lines indicated the 95% confidence intervals.

finance value of committee and party leaders grow following a linear trend over the

studied period by estimating Equation 4.8[8]

$$\begin{aligned} log(1 + contributions_{ijt}) &= \alpha_i + \delta_t + \beta_1 chair_{it} + \beta_2 leader_{it} + \beta_3 chair_{it} \times t \\ &\quad + \beta_4 leader_{it} \times t + \mathbf{x}_{it}\theta + \varepsilon_{it}\,, \end{aligned} \tag{4.8}$$

The results, presented in Table 4.6, are consistent with the trends identified in the graphical analysis. Across all models, the estimated coefficient on the interaction between t and the party-leader indicator is positive and statistically significant: On average, the value of party-leader positions linearly increases by approximately 0.02 log points each year. The coefficient on the interaction between the chair dummy and the time variable, on the other hand, is substantially relatively small and statistically indistinguishable from zero.

Both the graphical and statistical evidence indicate that the campaign-finance value of attaining a party leadership position has increased over the studied period, but the finding may be interpreted in several ways. Before turning to the more substantive interpretation, however, one might worry that the trend is simply driven by changes in the sample. As discussed above, the campaign finance data is not available for some states during the early 1990s, and if donors in these missing states happen to value leaders more than they do in other states, this would produce a positive trend in the estimated effect. However, as suggested by the panels in Figure 4.4, the positive trend is most pronounced from the mid 1990s and onwards; the period during which data is available for all states. To further substantiate that the identified trend is not a by-product of changes in the sample, I estimate the effect on the subsample for which I have data for all years and present the results in columns 3 and 4 in Table 4.6. The estimates from these models reveal the same trending pattern, suggesting that the increasing value of party leaders is not driven by sample changes.

Why is the value of party leaders growing over time? One interpretation is that the return on donations to party leaders has increased over the last twenty years. If firms and interest groups donate in an attempt to influence policy outcomes, the positive

[8]To simplify the interpretation of the coefficients, I set the time variable, t, equal to zero in 1990: $t = year - 1990$. Note that t does not enter as a separate variable in the regressions because the main effect is accounted for by the time-fixed effects.

trend suggest that the expected return on investments in party leaders has grown over time. This could either indicate that party leaders have become more powerful, or that they have become more susceptible and easy to sway by campaign contributions.

Another interpretation, however, would emphasize the changing role of party leaders. Scholars have claimed that over time it has become more common to appoint party leaders on the basis of fundraising skills (Heberlig, Hetherington, and Larson 2006; Kanthak 2007). While theoretically plausible, the empirical evidence is not consistent with this explanation. First of all, the analyses are based on contributions to individual legislators' campaigns, whereas all donations raised on behalf of the party and other organizations, such as leadership PACs, are excluded.

To further investigate whether the effect is driven by a growing emphasis on the active fundraising role of party leaders, I examine whether the effect is present in months during which legislators are not, in general, actively working on their re-election campaigns. Immediately following a general election, but before the beginning of the legislative term (in November, after the election date, and December of election years), very few legislators are actively engaged in raising campaign finance. If the positive trend in the value of party leaders is primarily explained by the growing importance of active fundraising, we would expect the effect to be zero for contributions donated during this period. In columns 5 and 6 in Table 4.6, I report the estimates from this exercise. The estimated effect on the interaction between time and leadership is positive and strongly statistically significant. In fact, the estimate is even stronger than the baseline estimates. This is inconsistent with the hypothesis that the effect is exclusively caused by a change in the role of party leaders.

The growing value of party leaders may be related to the increasing polarization of parties in the state and federal legislatures (Shor and McCarty 2011). If rank-and-file legislators are more willing to delegate power to their party leaders in times when parties are polarized, as predicted by the theory of *Conditional Party Governance*, the increasing value of party-leader positions uncovered in this section may be one of the consequence of the polarization of American politics.

4.10 Conclusion

On the basis of a comprehensive new dataset on committee and party leaders in the state legislatures, this paper uses a difference-in-difference design to document how the price that firms and interest groups are willing to pay for access to committee and party leaders varies across industries and institutional settings.

The results indicate that some industries are much more sensitive to agenda setters than others, and sensitivity to committee- and party-leader positions appears to be highly correlated, suggesting that donations to committee and party leaders maybe perceived as investments in complementary assets. The findings reveal that industries are extremely sensitive to the committee chairs by whom they are primarily regulated. Consistent with theories of legislative organization that emphasize the role of negative agenda control (Cox and McCubbins 2005), industries are more sensitive to committee chairs when institutions allow committees to obstruct new legislation from reaching the chamber floor, and party leaders are deemed more valuable when they control the process of referring bills to committees. Finally, I show that while the value of committee chairs has remained relatively stable throughout the studied period, the value of party leaders has increased substantially over the last 20 years.

That some groups enjoy privileged access to the political system has long been noted in American politics (Schattschneider 1975), but the findings in this paper may suggest that the bias is more severe and more closely connected to fundamental legislative institutions than previously assumed. It could be normatively troubling if, indeed, powerful agenda setters exchange access, or even political influence, for campaign contributions, all the more so since campaign contributions are likely only the visible tip of an iceberg of hidden lobbying activities employed by special-interest groups (e.g. Wright 1990). If committee chairs, in exchange for donations, use their negative agenda control to delay, obstruct or even prevent certain bills from reaching the chamber floor, it could mean that campaign finance induces a status-quo bias into the political process. This bias might be notably problematic in areas such as the Finance, Energy and Agriculture sectors where the substantial benefits derived from blocked legislation would be concentrated within a relatively small group of well-organized producers while groups that would benefit from the new legislation would face severe collective action problems. Future research should examine whether the privileges enjoyed by committee chairs skew

the representation of interests in the legislative process and bias public policy.

More generally, the results have implications for our interpretation of the literature on money in American politics. As noted in the introduction, many previous studies have tried to show that campaign contributions affect roll-call votes, but have failed to do so (Ansolabehere, de Figueiredo, and Snyder 2003; Wright 1990). For obvious reasons, roll-calls are only recorded for bills that reach the floor, and if, as the results in this paper may suggest, committee chairs prevent certain bills from reaching the floor in exchange for contributions, the existing literature has systematically underestimated the influence of campaign donations on public policy in American politics.

Chapter 5

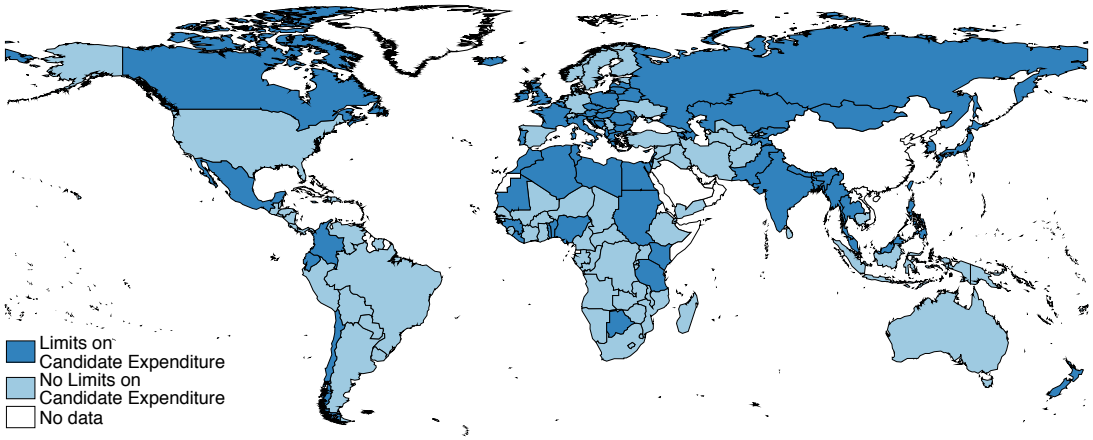
What Are the Electoral Consequences of Campaign Spending Limits?

In half of the democratic countries in the world, candidates face legal constraints on how much money they can spend on their electoral campaigns and yet, we know little about the consequences of imposing limits on campaign expenditures. I study these consequences in the context of U.K. House of Commons elections. On the basis of archival material, I collected a new dataset, covering 99.7% of all candidates running for a seat over the past 130 years, that records how much money each candidate spent, the legal maximum they faced, and exactly how they allocated their money across seven different spending categories. I identify causal effects on various measures of electoral behavior using instrumental variables exploiting within-constituency variation in spending caps caused by reforms of the spending limit formula, as well as shocks to formula inputs. The results indicate that allowing higher levels of campaign spending reduces the supply of candidates as well as electoral competition; promotes the development of more professionalized political campaigns; and benefits incumbents and candidates representing center-right parties.

5.1 Introduction

Imposing legal limits on candidate campaign expenditures is one the most common ways, globally, to regulate money in politics. As illustrated by the map in Figure 5.1, nearly two-thirds of European and Asian countries, as well as every third country in Africa, Oceania and the Americas, impose limits on candidate campaign expenditures in national elections (Ohman 2012: p. 37). In many countries including those, such as the United States, that do not currently limit campaign expenditures, legal restrictions have been at the center of passionate debates between prominent politicians, interest groups, lobbyists, supreme court judges, and political pundits for decades. Scholars from various fields of the social sciences, among them several Nobel Prize laureates, have participated in public debates on campaign spending regulation (Becker 2005; Posner 2005; Stiglitz 2012), and campaign spending limits have been a prominent bone of contention in lengthy arguments between distinguished political scientists in top political science journals for many years (Green and Krasno 1988; Krasno and Green 1993; Jacobson 1978, 1980, 1984, 1985, 1992).

Figure 5.1 – Limits on Candidate Campaign Expenditure. Approximately half of the democratic countries of the world impose limits on the money candidates are permitted to spend on their electoral campaigns.



NOTE: The map is constructed based on data from Ohman (2012).

Scholars have long theorized about the impact of campaign spending restrictions on electoral competition and on the welfare of voters. Theoretical arguments in opposition to limits on campaign spending are typically hinged around informational benefits (Austen-Smith 1987) and asymmetric spending efficiencies (Meirowitz 2008; Pastine

and Pastine 2012), whereas arguments in favor are often based on ideas denouncing socially wasteful spending (Mueller and Stratmann 1994), policy biases (Prat 2002*a*,*b*) and asymmetric fundraising efficiencies (Pastine and Pastine 2012).

In light of the apparent importance placed on the institution of campaign expenditure caps, and in contrast to the broad theoretical interest in the topic, our empirical knowledge about the electoral consequences of spending limits is surprisingly weak. Only a single study explicitly examines the electoral consequences of campaign spending limits, and this study only relies on a few recent elections in Canada (Milligan and Rekkas 2008). The striking void in the empirical literature can be attributed to several factors. First, the bulk of the research on campaign finance is based on U.S. data, but as campaign spending restrictions are deemed to be unconstitutional in the U.S., we cannot study the consequences of spending limits in this context. Second, data on campaign finance outside of the U.S. is relatively scarce, being available for only a few recent elections for most countries. Finally, even if one were equipped with the relevant data, it is not obvious how such a study should be implemented. Comparisons of campaign finance institutions across different countries might be useful for establishing descriptive facts, but may not inform us about causal consequences, and although single-country studies exploiting subnational and temporal variation might prove to be informative about causal effects, they may raise fundamental questions about their external validity.

In this paper, I study the effect of statutory limits on candidate campaign expenditure in the context of U.K. House of Commons elections. Besides the value of studying the House of Commons in its own right, there are a number of good reasons to focus on U.K. elections. First, the U.K. led the democratic world in the introduction of limits on candidate spending and, to this day, is widely considered to be an archetypal, textbook example of the spending-cap institution. Many countries, in particular countries with institutional features adopted from the Westminster system of government and countries associated with the British empire, copied or were directly influenced by central aspects of the U.K. system of campaign finance regulation,[1] hence this regulatory regime is sometimes referred to as the "the Westminster model of political finance"(Falguera, Jones, and Ohman 2014: p. 256). Although the implementation design of spending limits differs around the world, many of the designs' fundamental institutional fea-

[1]This is also indicated in Figure 5.1 by the substantial overlap between countries with candidate campaign spending limits and former British colonies.

tures can be traced back to the *Corrupt and Illegal Practices Prevention Act* which was passed in the U.K. in 1883. In other words, when studying spending limits in House of Commons elections, we intrinsically study the institutional blueprints upon which most other countries with campaign spending restrictions have based their approach. Second, the House of Commons provides a unique opportunity to study the effect of campaign spending limits over a period of more than a hundred years. Apart from the historical value of these observations and the trivial appeal of many data points, studying a period of this extent allows me to exploit within-constituency changes in spending limits induced by reforms of the limit formula and by shocks to formula inputs. To put it differently, the temporal variation allows me to study how the same constituency responds to different spending limits. Third, particular features of the U.K. institution give rise to a variation in cap levels across constituencies. Obviously, spending limits are not randomly assigned, but set according to a well-defined, mathematical formula based on the type of constituency and the number of electors. Knowing the exact conditions under which spending limits are assigned, I can very clearly specify and critically evaluate the assumptions under which causal effects are identified.

The electoral consequences of spending limits is relatively unknown academic territory, but scholars have long been interested in the role of money in politics in Britain. Pinto-Duschinsky (1981) and Ewing (1987) describe the historical development in political finance in Great Britain, and scholars have used campaign spending as a measure of constituency-level campaign intensity (Johnston 2014; Pattie and Johnston 2003; Johnston and Pattie 2007; Pattie and Johnston 2009; Johnston et al. 2011; Johnston, Pattie, and Johnston 1989; Johnston and Pattie 1995; Pattie, Johnston, and Fieldhouse 1995; Fieldhouse and Cutts 2009). Although these studies are valuable, they tend to focus on recent general elections[2] or on a limited number of constituencies in earlier general elections. Of course, this raises questions about the generalizability of their findings. Furthermore, the institution of campaign finance limits is not the explicit focus of these studies, nor do any of them move beyond examining aggregate spending by scrutinizing the detailed composition of campaign expenditures.

To substantiate the study of U.K. limits on campaign expenditures, I offer the longest-spanning dataset on campaign finance ever collected: Based on archival material

[2]In particular, but not exclusively, general elections after the Electoral Commission started to publish campaign spending data in 2001.

from the House of Commons, I compiled a new dataset that covers approximately 99.7% of all candidates running in general elections from 1885 to 2010, producing, in total, more than 61,000 candidate-years.[3] For each candidate, I record how much money they spent on their campaigns and exactly how they allocated their resources across expenditures for advertisement and printing, public meetings, managers, staff, personal expenses, rooms, and miscellaneous matters.

Using this unique dataset and applying an instrumental variables approach exploiting within-consituency changes in spending limits induced by quasi-exogenous shocks, I analyze the electoral consequences of spending limits. The results suggest that higher spending limits reduce the pool of candidates, restrict electoral competition, promote professionalized political campaigns, and benefit incumbents and center-right candidates.

The paper proceeds as follows: First, I briefly describe the historical and institutional background. After that, I introduce the new dataset and describe the major trends in U.K. campaign finance over the past 130 years. I then describe the empirical approach and discuss the conditions under which I can plausibly identify causal effects. Following that, I present the main results. Finally, I conclude with a short discussion.

5.2 Historical and Institutional Background

Corruption is expensive. Up until the late 19th century, general election campaigns in Britain were, according to modern standards, quite shady and costly affairs (Eggers and Spirling 2014*b*; Pinto-Duschinsky 1981). In many constituencies, candidates were involved in extensive dealings of bribery, voter intimidation, and various other forms of electoral fraud.

Against the backdrop of a historically scandalous general election in 1880, William Gladstone's Second Ministry introduced the *Corrupt and Illegal Practices Prevention Act* in 1883. The bill was passed with support from both major parties. The Act criminalized various forms of bribery; imposed limits on candidates' campaign expenditure; required candidates to fully disclose and document how they spent their money, with the back-up of official receipts; and introduced significant fines and punishments for

[3]The data will be made publicly available upon publication.

rule violations. Candidates, along with their campaign managers[4], who failed to file the required information to the Returning Officer within a certain number of days after the election, as well as candidates who filed erroneous information, could be subject to significant fines, banned from running for office in future, or even imprisoned.

The Act also stipulated that the legal expenditure maximum would vary across constituencies depending on the type of constituency and the number of electors therein. The intent of the variation in the caps was to acknowledge the fact that it was generally more costly to campaign in constituencies with many electors, particularly so if the electors were thinly distributed across large, rural geographical areas (Pinto-Duschinsky 1981).

The classification between county and borough constituencies was used as a simple, coarse proxy for population density and urbanization.[5] The historically important classification went back to the territorial organization of England in the 13th century, and it remained more or less intact until the first Reform Act of 1832 (Rossiter, Johnston, and Pattie 1999). Boroughs and counties were represented differently in Parliament, and franchise differed systematically across constituencies, however, many of these fundamental differences gradually became less important following a series of franchise and boundary reforms that were implemented throughout the 19th century.[6] This process of reform culminated in the *Representation of the People Act 1884*, which was introduced with a declared purpose of equalizing the differences between county and borough constituencies (Blewett 1965). As a consequence of this Act, the previous distinction between counties and boroughs, which had mattered tremendously with respect to franchise, no longer served its initial purpose. As Rossiter, Johnston, and Pattie (1999: p. 44) points out:

> The final major redistribution of the nineteenth century produced approximately equal representation, both across regions and between the Boroughs and Counties.

Although boroughs and counties were near equivalent in terms of franchise, counties were, on average, more rural and thinly populated than were boroughs. In response

[4]Note that campaign managers are often referred to as "election agents" in the U.K. context.

[5]University constituencies constituted a third, but less important, constituency type. For historical reasons, the major universities had their own parliamentary constituencies until 1949. All university constituencies are excluded from the analyses in this paper.

[6]These reforms have been carefully studied by scholars e.g. Berlinski, Dewan et al. (2011); Berlinski, Dewan, and Van Coppenolle (2014); Bronner (2014).

to these differences, it was deemed that candidates running in constituencies classified as counties should be allowed to spend more money relative to candidates in borough constituencies of the same magnitude.

The specific spending limit to be faced by the candidates in a given constituency, i, in a given general election, t, was determined by a formula based on the interaction and linear combination of the number of electors and the type of constituency.[7] A generalized version of the formula is outlined in Equation 5.1:

$$limit_{it} = \delta_t + \lambda_t electors_{it} + \psi_t county_{it} + \phi_t electors_{it} \cdot county_{it}\,, \qquad (5.1)$$

where $limit_{it}$ is the legal maximum on expenditures, as measured in British pounds, faced by any candidate running for the seat in constituency i at time t; δ_t is a baseline lump sum amount allocated to all constituencies in year t; λ_t represents the allowed spending rate per elector in year t; ψ_t is an additional lump sum amount allocated only to county constituencies; finally, ϕ_t reflects the additional spending allowed per elector, again only in county constituencies. The fundamental spending limit formula as initially described in the *Corrupt and Illegal Practices Prevention Act*, remained essentially the same throughout the studied period, but the four policy parameters (δ_t, λ_t, ψ_t, ϕ_t) were modified on 13 occasions. How they were modified is outlined in Table 5.1.

In House of Commons elections, seats are allocated using simple plurality rule: The candidate who attracts the most votes is declared the winner. During the period between 1885 and 1949, only a few constituencies still elected two representatives, and after 1950, all double-member constituencies were abolished (Butler 1963). The formula for calculating the spending limit was applied slightly differently for the few double-member constituencies; for the purposes of this paper, I focus exclusively on single-member

[7] In some periods, the number of electors was rounded before it was plugged into the formula. During the period of 1885-1910, it was rounded down to the nearest 1,000. During the period of 1969-1978, the number of electors was rounded to the nearest 8 electors in boroughs and 6 electors in counties. The exact formulae are outlined in the Appendix. The practice of rounding the number of electors could be another potential source of exogenous variation that, in principle, one could use to identify the causal effect of spending limits, but unfortunately, this practice does not induce sufficient variation in the treatment to produce strong, reliable first-stage results.

Table 5.1 – Coefficients in Spending Limit Formula over Time

Period	δ_t $(constant_t)$	λ_t $(electors_{it})$	ψ_t $(county_{it})$	ϕ_t $(electors_{it} \cdot county_{it})$	Change in Difference: County vs. Borough
1885 - 1917	350	0.03	300	0.03	
1918 - 1928	0	0.0208	0	0.0083	–/+
1929 - 1948	0	0.0208	0	0.0042	–
1949 - 1968	450	0.0063	0	0.0021	–
1969 - 1973	750	0.0063	0	0.0021	0
1974 - 1977	1075	0.0075	0	0.0025	+
1978 - 1981	1750	0.015	0	0.005	+
1982 - 1986	2700	0.023	0	0.008	+
1987 - 1991	3370	0.029	0	0.009	+
1992 - 1996	4330	0.037	0	0.012	+
1997 - 2000	4965	0.042	0	0.014	+
2001 - 2004	5483	0.046	0	0.016	+
2005 - 2010	7150	0.05	0	0.02	+

NOTE: δ_t, λ_t, ψ_t and ϕ_t are the coefficients on the inputs outlined in Equation 5.1. The formulae do not apply to (Northern) Ireland.

constituencies.[8]

In the next section, I introduce the new dataset on candidate campaign spending in House of Commons elections from 1885 to 2010.

5.3 New Data: House of Commons Campaign Spending 1885–2010

As described above, the *Corrupt and Illegal Practices Prevention Act 1883* required candidates to disclose detailed information on campaign spending immediately after each general election. To enhance transparency, and as a precaution against the temptation to submit fabricated information, the campaign expenditure returns filed by candidates and their campaign managers were compiled by the Home Office in the few months following the election and were made available for all members of the House of Commons to scrutinize. The dataset introduced in this paper is based on these filings.

Using archival material from the House of Commons, I constructed a dataset in which each observation is identified by a specific candidate in a given general election over the past 130 years. In total, this provides me with information on more than 61,000 individual candidate-years. I match each return documented in the archival material to a unique candidate identifier that is further linked to information on electoral outcomes

[8]For a detailed discussion of multi-member districts in Britain, see Eggers and Fouirnaies (2014)

Table 5.2 – Number of Missing Spending Returns

Period	# Missing Candidate Reports	Total # Candidates	Pct. Missing
1885-1917	19	9818	0.19
1919-1944	9	8682	0.10
1945-1969	9	11341	0.08
1970-1989	80	13703	0.58
1990-2010	91	17667	0.52
Total	208	61003	0.34

NOTE: Due to the loss of related filings, the election of 1918 is excluded from the calculations.

and that has been used in a series of papers by Eggers and Spirling (2014*a*,*b*,*c*). This will allow future researchers to connect the campaign finance data with information on candidates and constituencies, including data on candidate characteristics, their political careers, and parliamentary speeches given by Ministers and M.P.s.

All campaign spending returns related to the general election of 1918 appear to have been lost, but otherwise the dataset contains near complete information on all candidates running for office.[9] As reported in Table 5.2, close to 99.7% of all candidates complied with the regulations and reported their spending in a timely manner. Most of the 208 candidates who did not report their spending were either non-viable candidates running as independents or candidates representing minor parties.

As mentioned above, the regulatory system required candidates to disaggregate their expenditures by various categories and to provide documentation for each expenditure in the form of a receipt. Therefore, for each candidate in the dataset, I am able to analyze information on the following seven types of expenditures:

1. *Advertising and Printing.* This variable sums up the money that a candidate spent on advertisement, printing and publishing campaign material, issuing and distributing addresses and notices, stationary, postage, and telecommunications.[10]

[9]The House of Commons Library is not aware of how the 1918 filings were lost. However, based on comparisons with other documents from 1918, they believe that the files were submitted by the candidates, but never compiled by the Home Office since the 1918 election was held only a month after the end of WWI, and presumably compiling the expenditure returns was a relatively low priority task for the British government at the time. As a consequence, I do not think that the missing files induce any notable bias in the estimates.

[10]Candidates also faced legal restrictions on permitted types of advertisement. In particular, radio and television advertisements were not permitted.

2. *Meetings.* This variable sums up the money that a candidate spent on organizing public meetings and debates. This also includes payments to external speakers.

3. *Staff.* Payments to clerks, messengers, canvassers, and other paid staffers who worked on the campaign are included in this variable.

4. *Managers.* This variable denotes the sum of payments made to campaign managers, election agents, sub-agents, polling agents, and other people responsible for managing key aspects of the campaign.[11]

5. *Personal Expenses.* This variable sums up costs personally incurred by the candidate for the purpose of the election. For example, expenses associated with travel and accommodation are included in this number.

6. *Rooms.* The rental cost of rooms or other places of lodging for the purpose of the campaign, for example, the leasing of a committee room or campaign headquarters, is included in this variable.

7. *Miscellaneous Matters.* This variable sums up all expenses that do not fit into one of the categories outlined above.

Taken together, these seven categories provide an unprecedented and uniquely fine-grained account of how House of Commons candidates organized their electoral campaigns. However, to what extent can we trust the integrity of the data? There are three key concerns one needs to keep in mind when interpreting the results.

Firstly, were candidates incentivized to disclose true and accurate information? The extensive disclosure requirements and the threat of high fines for reporting erroneous information would suggest that massive discrepancies between actual campaign activities and reported spending are unlikely. When interpreting the results, however, one must recognize that certain types of expenditures are notoriously difficult to audit. While printing costs and costs associated with organizing meetings and renting rooms are fairly easy to verify against receipts, it is more difficult, if not to say impossible, to accurately account for labor. For example, it is a challenge to verify the actual number of hours a campaign staffer worked for a given given salary. In essence, candidates

[11] For a detailed account of the roles of the campaign managers in British General Elections, see Fisher, Denver, and Hands (2006).

were strongly incentivized to disclose accurate information on "hard" expenditures that were easily verifiable, but may have had weaker motivations to openly document "soft" expenditures.

Secondly, does the data accurately reflect the amounts spent by a candidate during the period of the campaign? The reported numbers reflect candidates' expenditures during the period from the day the election is called to the day of the general election. If a party engages in campaign activities, such as distributing printed materials in a particular constituency, say, six months prior to the general election, the costs of these activities do not count against the spending limit faced by the representing candidate.[12] Therefore, candidates may strategically rely on early campaigning in order to effectively relax the constraints on overall spending. This problem is, in all likelihood, most prevalent in periods with stable governments, when candidates can more easily predict when the Prime Minister will call for general election. It seems plausible that the data, for the most part, correctly accounts for money spent during the actual campaign, but it may not accurately pick up the full amount of pre-dissolution expenditures.[13]

Thirdly, is it a problem that the reported spending pertains solely to the individual candidate and does not take the spending of the national party into account? The spending limits initially described in the *Corrupt and Illegal Practices Prevention Act 1883* only apply to the individual candidates, not their parties. The major national parties are, for obvious reasons, keenly interested in winning seats in key swing constituencies and, as a result, they may intensify their campaign activities in these constituencies. Due to the nature of the first-past-the-post electoral system, campaigning for the party in a particular constituency is de facto equivalent to campaigning for the individual candidate, and this blurs the line between costs incurred by individual candidates and their represented parties. To the extent that general elections have become more nationalized and centralized affairs over the course of the 20th century, this concern is ostensibly most relevant in recent decades.

As a consequence of the three caveats discussed above, the reported spending may

[12] Expenditures before the election date is announced are permitted if they are designed to promote the local party rather than the individual candidate. For further details, see Pinto-Duschinsky (1981: chapter 9)

[13] After 2001, the regulation actually distinguishes between a "short" and a "long" campaign to ensure that candidates do not have the means to hide expenditures by strategically timing their campaign activities prior to the dissolution of the parliament.

not fully account for the true costs of campaigning, and one has to keep this in mind when interpreting the results presented below. However, any reporting issues are presumably somewhat constant from one year to the next within each constituency or are common across all constituencies within a given year. Whereas reporting issues like these may bias estimates in simple cross-sectional studies, they are less likely to do so in a design leveraging within-constituency variation. I will describe why this is the case in further detail below.

For the purposes of the analyses in this paper, I take into consideration all observations pertaining to single-member constituencies, but exclude multi-member and university constituencies. I reshape the data in such way that each observation is uniquely identified by a constituency in a given year. This leaves me with a panel dataset of approximately 20,000 constituency-year observations. Based on this data, I describe the main developments in candidate campaign expenditure in the next section.

5.4 Major Trends in Campaign Spending 1885-2010

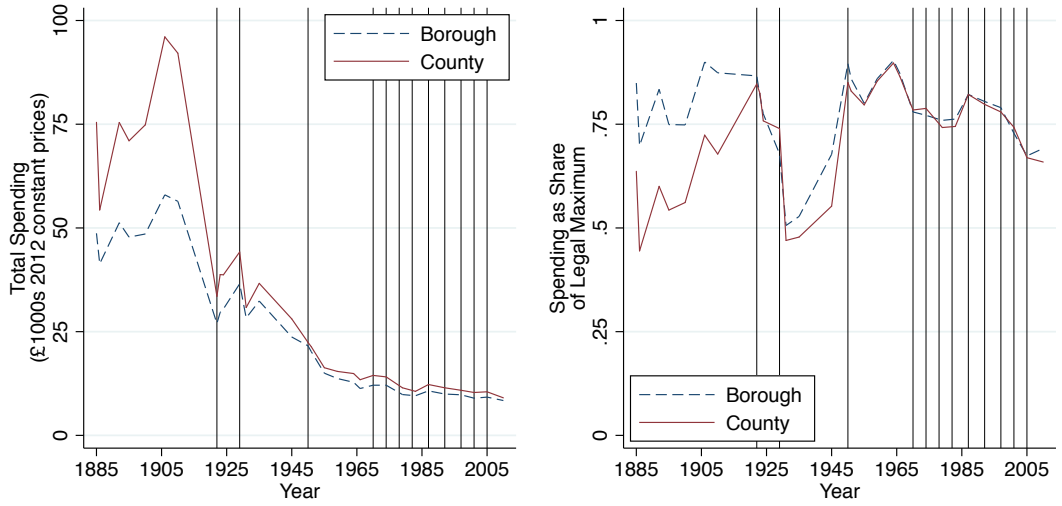
Figure 5.2 illustrates four key trends in campaign spending from 1885 to 2010. On the left in panel (a), I plot average candidate spending (in constant 2012 British pounds) across election years. I focus on the two front running parties and disaggregate spending by constituency type. The solid and dashed lines represent average candidate spending in county and borough constituencies, respectively, whereas the vertical lines indicate the first election following a revision of the spending limit formula. When describing the pattern, one must be mindful of the considerable uncertainty associated with the Consumer Price Index in the early years, in particular in those years around the First and Second World Wars.

The graph shows that candidate spending levels in both types of constituencies were relatively high in the late 19th and early 20th centuries, then dropped dramatically after World War I and continued to decrease until they more or less stabilized in the 1960s. Throughout the studied period, we see that candidates in county constituencies spent more money, on average, than did candidates in boroughs, consistent with the difference in spending limits between the two types of constituencies. Furthermore, it is clear that spending trends in counties and boroughs ran fairly parallel to each other between the reforms of the spending limits. Consider, for example, the revision in 1918. In the period

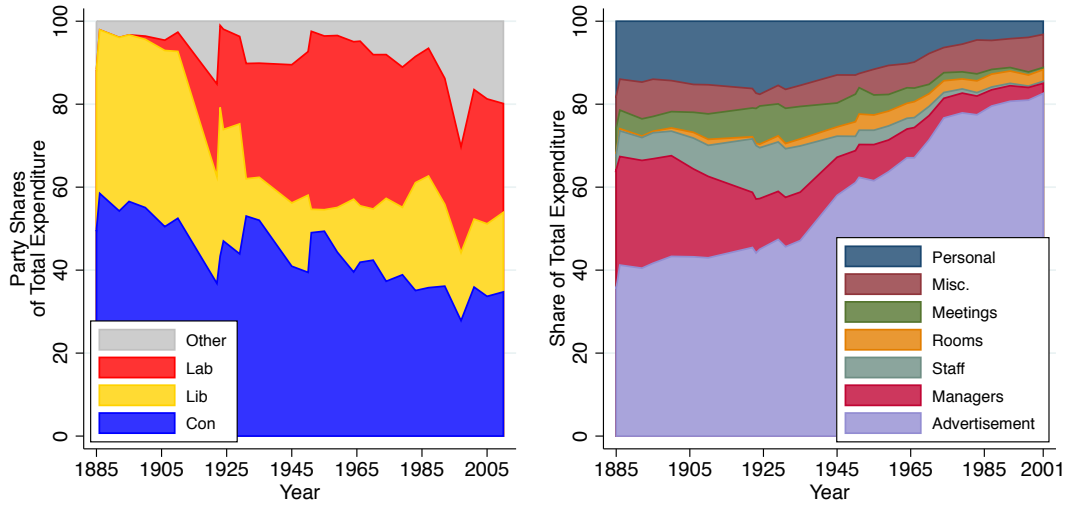
leading up to the elections of 1910 (the last election before the revision in 1918), and in the period following the revision (starting in 1922), the spending tendencies follow the same general trend.

Figure 5.2 – Major Campaign Finance Trends 1885-2010.

(a) Spending by Constituency Type



(b) Spending Shares by Parties and Types of Outlay



On the right in panel (a), I show average spending as a share of legal maximum and, again, I report the findings separately for counties and boroughs. The graph indicates that candidates, on average, spent somewhere between 60% and 80% of the legal maximum in most elections. Up until 1950, candidates in boroughs spent closer to the legal maximum than candidates in counties. From the 1950s and onwards, counties and boroughs closely followed each other. Once more, it is worth noticing that boroughs

and counties appear to follow the same overall trends. I will discuss this common trend across constituency types in further detail in the next section.

On the left in panel (b), I plot the share of total expenditures accounted for by each of the major political parties. The partisan development in candidate spending reflects more general political trends. The graph shows that Conservative candidates accounted for approximately 40-50% of the spending. The Labour party emerged around the turn of the century and grew stronger in terms of candidate spending over the next few decades until, in most elections, Labour candidates accounted for approximately 30-40% of total spending. Spending by candidates from the Liberal party decreased as the Labour party grew stronger, but regained ground by the end of the 20th century. Independents and candidates representing parties other than the three traditional ones have become more common over the recent years, and this is also reflected in the spending shares.

In the graph on the right in panel (b), I explore the question of how candidates allocate their campaign expenditures by disaggregating outlays according to the seven main types of expenditure and by plotting the relative weight of each category against the election year. The first thing to notice in this graph is that money spent on advertising and printing appears to be the most important component of total spending throughout the studied period, and that this category has become noticeably more important over the years: In the late 19th century this category accounted for approximately 40% of total spending, whereas by the end of the 20th century, it accounted for up to 80% of all expenditures. Conversely, the shares of total expenditure allocated to managers, staff and public meetings, respectively, decreased over the studied period. Outlays for personal expenses, miscellaneous matters, and rooms remained fairly stable throughout the period, accounting for approximately 20% of all expenditure.

To supplement the descriptive statistics presented in Figure 5.2, I report summary statistics on key variables used in the analyses in Table 5.3. In this Table, I disaggregate the statistics by four different sub-periods as well as by constituency type. In the next section, I outline the empirical strategy that I apply to the data described above.

Table 5.3 – Summary Statistics

	1885-1918		1919-1950		1951-1980		1981-2010		Total	
	Borough	County	Borough	County	Borough	County	Borough	County	Borough	County
Legal Maximum (£1000)	61.55	120.86	41.99	53.32	16.08	18.49	12.62	14.38	30.93	55.01
	(13.02)	(32.12)	(16.63)	(21.60)	(3.29)	(3.50)	(0.71)	(1.06)	(21.77)	(48.78)
Lab. Spending	2.08	1.87	19.31	18.34	12.75	13.39	9.48	7.96	11.46	9.93
	(10.51)	(14.03)	(10.97)	(12.78)	(4.27)	(4.98)	(3.10)	(4.10)	(9.73)	(11.95)
Lib. (Dem.) Spending	39.24	56.58	14.27	19.15	3.35	5.84	5.37	6.47	13.79	23.66
	(26.88)	(53.78)	(16.21)	(20.42)	(4.32)	(6.02)	(6.16)	(5.33)	(20.24)	(37.58)
Con. Spending	51.10	73.79	29.62	35.22	13.21	15.80	8.77	11.09	23.96	35.86
	(23.28)	(54.01)	(16.21)	(19.60)	(4.90)	(5.07)	(3.74)	(3.96)	(20.81)	(39.99)
Candidates	1.97	1.76	2.57	2.44	2.94	2.89	5.07	4.79	3.13	2.90
	(0.45)	(0.49)	(0.66)	(0.67)	(0.96)	(0.78)	(1.65)	(1.37)	(1.51)	(1.42)
Effective Candidates	1.36	1.28	1.46	1.45	1.49	1.52	1.62	1.64	1.48	1.46
	(0.14)	(0.18)	(0.15)	(0.18)	(0.12)	(0.14)	(0.12)	(0.12)	(0.16)	(0.21)
More than Two Candidates	0.08	0.03	0.52	0.47	0.62	0.67	1.00	1.00	0.57	0.52
	(0.26)	(0.16)	(0.50)	(0.50)	(0.49)	(0.47)	(0.00)	(0.00)	(0.50)	(0.50)
Unopposed Candidate	0.11	0.27	0.02	0.06	0.00	0.00	0.00	0.00	0.03	0.09
	(0.32)	(0.44)	(0.15)	(0.25)	(0.00)	(0.04)	(0.00)	(0.00)	(0.17)	(0.29)
Polling Districts	9.34	19.70	19.71	37.82	32.58	65.03	41.04	65.86	25.41	44.70
	(6.76)	(10.22)	(12.94)	(18.70)	(15.71)	(27.61)	(14.81)	(29.56)	(17.25)	(29.38)
Electors (1000)	9.22	10.68	45.23	45.42	57.70	61.87	66.18	68.87	46.66	44.98
	(4.22)	(4.17)	(13.01)	(15.07)	(11.24)	(15.08)	(7.93)	(9.59)	(22.62)	(26.07)
Observations	1875	3002	2259	2359	3019	2617	2055	2415	9208	10393

NOTE: The unit of observation is a constituency in a general election. The table reports means and standard deviations. All statistics relating to spending are reported in £1000s (constant 2014 prices).

5.5 Empirical Strategy

The ideal way to test how spending limits affect electoral outcomes would be to randomly assign those limits across parliamentary constituencies. In a hypothetical experiment like this, one could identify the average causal effect of spending limits by estimating Equation 5.2 using OLS:

$$y_{it} = \beta_1 limit_{it} + \varepsilon_{it} \tag{5.2}$$

Obviously, this is not feasible. In the absence of a viable experiment, the next best option is to exploit quasi-random shocks that induce variation in spending limits. The institutional setup in the House of Commons is ideal for this type of study because the spending limits are deterministically assigned according to a known mathematical formula with objective, observable inputs. Since all constituencies, without exception, are subject to the same general formula, the standard self-selection problems that haunt most campaign finance studies are not a cause for concern in the U.K. setting.

The main challenge, however, is that, in general, the formula inputs are not randomly

distributed across constituencies. In Equation 5.3, I substitute Equation 5.1 into 5.2 in order to highlight the endogeneity problem. It is clear from the equation that any determinant of the outcome that happens to be correlated with the elements in the square brackets would bias the estimated effect. For example, a geographically large rural county constituency differs in many systematic ways from a small urban borough constituency, and one may surmise some of the differences matter for electoral outcomes.

$$y_{it} = \beta_1[\delta_t + \lambda_t electors_{it} + \psi_t county_{it} + \phi_t electors_{it} \cdot county_{it}] + \varepsilon_{it} \quad (5.3)$$

Equation 5.3 indicates that there are two potential sources of quasi-exogenous variation: Shocks to the policy parameters $(\delta_t, \lambda_t, \psi_t, \phi_t)$ and shocks to the formula inputs $(electors_{it}, county_{it})$. I outline below how I intend to exploit both types of variation in an instrumental variables analysis.

The basic idea is to instrument for spending limits using within-constituency variation induced by constituency type reclassifications and reforms of policy parameters while differencing out common trends across constituencies. In other words, the design is an instrumental variables (IV) approach where the effects of the shocks are identified using a simple difference-in-difference design. This design identifies a local average treatment effect (LATE) under the key assumptions that a given shock:

1. has a significant effect on spending limits (relevance);
2. is as-if randomly assigned, conditional on the included covariates (exogeneity);
3. exclusively affects the outcome through the spending limits channel (exclusivity);
4. pushes spending limits in all treated constituencies in the same direction (monotonicity).

If these conditions are satisfied, the IV approach captures a weighted average of treatment effects where the weightings reflect how sensitive spending limits in constituencies are to a given shock. Since the difference in spending limits between county and borough constituencies linearly increases with the number of electors ($\phi_t > 0, \ \forall \ t$), the LATE estimates are weighted towards constituencies with a greater population of electors. When multiple instruments are combined, the obtained LATE estimate is, moreover, a

weighted combination of the individual instruments where the weightings are proportional to the instruments' first-stage effects on spending limits (Angrist, Graddy, and Imbens 2000). In the two sections below, I outline in detail how I intend to use administrative reclassifications of constituency types as well as reforms of policy parameters to identify the causal effects of spending limits on electoral outcomes.

5.5.1 Shocks to Formula Inputs: Constituency Reclassifications

As suggested by the spending limit formula in Equation 5.1 and by the coefficients in Table 5.1, candidates running in county constituencies were granted higher limits relative to candidates in comparable borough constituencies. There was clearly good reason for this: County constituencies tend to be geographically larger, thus it is generally more costly to campaign in these regions. As discussed above, prior to 1884 the differences between county and borough constituencies were distinct and significant, but over time many of differences were washed out by franchise and boundary reforms.

An important administrative difference, however, remains. The responsibility for managing the electoral register and organizing elections lies within each respective type of local government. In other words, county councils are responsible for the bureaucratic activities associated with elections in county constituencies, and borough councils are responsible for those in borough constituencies. To ensure the responsibility for the practical aspects of elections (organizing polling stations, setting up polling booths, preparing ballots, etc.) is clear, and to establish a consistent, simple connection between elected officials at different levels of government, each parliamentary constituency is mapped to a unique, relevant local authority.[14] Following any boundary changes affecting either levels of government, new mappings between parliamentary constituencies and local authorities must be established. For the majority of constituencies this is a trivial matter since "local government boundaries provide the template within which constituencies are defined" (Rossiter, Johnston, and Pattie 1999: p.17).

In some cases, however, boundary reforms result in parliamentary constituencies with borders that cut across those of local government and, sometimes, cut across county and borough divisions. In certain instances, perforce, the constituency type has to be reclassified from county to borough constituency, or vice versa. The reclassifica-

[14]For a more detailed discussion of the mapping between local authorities and parliamentary constituencies in England, see, for example, Fouirnaies and Mutlu-Eren (2015)

tion of constituency type is purely an administrative matter and, as such, is delegated to the Boundary Commission. The Home Secretary drafts a proposal consistent with the recommendations of the Boundary Commission, then parliament typically implements the proposal without much debate or modification since the reclassifications affect relatively few constituencies and are generally considered to be negligible. From 1885 to 2010, approximately 6% of the unique parliamentary constituencies were at some point reclassified by the Boundary Commission. To be clear, when I refer to a reclassified constituency, it denotes a constituency for which the boundaries remained fixed while only the administrative status changed. These reclassifications matter relatively little overall, but they provide a unique quasi-exogenous shock to the spending limit in the affected constituencies: A reclassification from borough to county induces a relaxation in the spending limit, as opposed to a tightening of the limit further to a reclassification from county to borough.

I exploit this shock in an instrumental variables analysis. The setups in the first-stage and reduced-form models are similar to a standard panel difference-in-difference design in which one observes within-constituency changes in the outcome as the treatment kicks in, while differencing out common effects across all constituencies. The first-stage and reduced-form models are given by Equations 5.4 and 5.5, respectively:

$$limit_{it} = \beta_{1,1} county_{it} + \alpha_{i,1} + \delta_{t,1} + X'_{it}\theta_{1,1} + \varepsilon_{it,1} \tag{5.4}$$

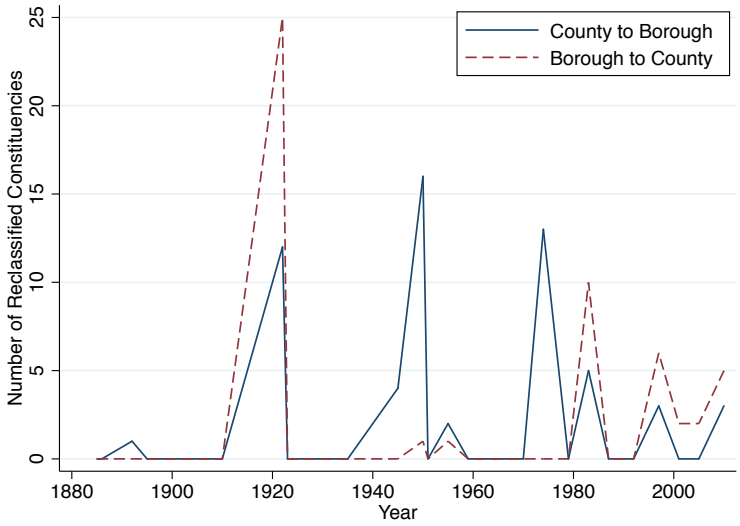
$$y_{it} = \beta_{1,2} county_{it} + \alpha_{i,2} + \delta_{t,2} + X'_{it}\theta_{1,2} + \varepsilon_{it,2} \tag{5.5}$$

where $limit_{it}$ indicates the spending limit and y_{it} the observed outcome, respectively, in constituency i at time t; $county_{it}$ is a dummy indicating whether constituency i at time t was classified as a county constituency; $\alpha_{i,1}$ and $\alpha_{i,2}$ represent constituency-fixed effects capturing all time-invariant characteristics of a constituency; $\delta_{t,1}$ and $\delta_{t,2}$ denote time-fixed effects that capture common trends across constituencies; X_{it} is a vector of additional control variables; finally, $\varepsilon_{it,1}$ and $\varepsilon_{it,2}$ are the error terms. $\beta_{1,1}$ and $\beta_{1,2}$ capture the effects of the county constituency classification on spending limits and outcomes, respectively. These coefficients and the ratio between them are of key interest.

Constituency type reclassifications constitute a sound natural experiment because

there is justification for the four identification assumptions described above being met in these particular settings. First, from the parameters outlined in Table 5.1, we know that the relevance criterion is satisfied: The county classification affects the spending limit throughout the studied period (since $\phi_t \neq 0 \; \forall \; t$). The exact magnitude of the first-stage model depends, of course, on the number of electors in the reclassified constituencies, and on the years in which the reclassifications occurred. Figure 5.3 shows how reclassifications were distributed in time across the studied period.

Figure 5.3 – Reclassification of Constituency Types. Constituencies have been reclassified periodically throughout the studied period.



Second, it seems plausible that the difference-in-difference setup identifies the causal effect in the first-stage and reduced-form models such that the exogeneity assumption is met. The causal effect is identified under a parallel trends assumption: In the absence of a constituency reclassification, from borough to county constituency, (or county to borough), the reclassified constituencies would have followed the same trend as the non-reclassified constituencies of the same type. The fact that reclassifications are exogenously imposed by the Boundary Commission suggests that individual M.P.s do not have the means to single-handedly manipulate the classification of their constituency. In other words, self-selection problems are not a major cause for concern.

Furthermore, constituency reclassifications are typically implemented in response to unintended, administrative consequences of local government reforms, *not*, as is important to note, because the constituencies were trending in certain ways that demanded

reclassification. Documentation of parliamentary debates on constituency reclassifications substantiate this claim. Many of the M.P.s who who participated in the debates appeared to be more puzzled about the arbitrariness of the changes proposed by the Boundary Commission than they were concerned about their consequences. For example, in a debate on the 17th of March, 1948, leading up to the reclassifications implemented in accordance with the *Representation of the People Act* of 1948, M.P. John Parker from Dagenham asks the Home Secretary why

> "...there seems to be no particular principle on which the Boundary Commission has acted in classifying constituencies as either county or borough".[15]

Home Secretary James Ede acknowledges that the reclassifications are purely an administrative matter with the only notable side effect being that the spending limits in a few constituencies would change:

> I admit that it appears to be very difficult to ascertain why some constituencies have been put into the county group and some into the borough group, but the answer is that this is what the Boundary Commissioners recommended, and we felt that in this matter it was desirable to follow the arrangement which had been made by the Boundary Commissioners. I think the only difference which really exists, or which matters very much, is the fact that candidates can spend less money in a borough constituency than they can in a county division of comparable size.[16]

The view that constituency classifications are not based on trends in constituency characteristics, but on somewhat arbitrary decisions related to local government reorganization is echoed in many further parliamentary debates on the issue. For example, in debates leading up to the changes implemented in 1983, M.P. Gary Waller from Brighouse and Spenborough points out how the local government reform in 1982 produced arbitrary, unintended consequences affecting constituency classifications:

> Before local government reorganisation, there was an obvious distinction between county and borough constituencies. That distinction no longer applies. The commissioners found that very often they were making decisions on fairly marginal grounds [...]. This matter is important only because the formula for election expenses differs between county and

[15] http://hansard.millbanksystems.com/commons/1948/mar/17/clause-1-constituencies-and-electors-1#S5CV0448P0_19480317_HOC_558

[16] http://hansard.millbanksystems.com/commons/1948/mar/17/clause-1-constituencies-and-electors-1#S5CV0448P0_19480317_HOC_558

borough constituencies. The distinction is really an artificial one.[17]

These quotes from the parliamentary debates almost give the impression that the reclassified constituencies were indistinquishable from unaffected constituencies, but of course this is not completely true. Table 5.4 reports how key variables differ across reclassified and unaffected constituencies in elections prior to reclassifications. Although most variables are, in fact, statistically indistinguishable between reclassified and unaffected constituencies, there are notable dissimilarities, the most apparent being that the reclassified constituencies seem to be smaller as measured by the number of polling districts. In the difference-in-difference setup, however, all comparisons are made *within* a given constituency and *after* differencing out general trends affecting all constituencies equally. The causal effect is identified under the relatively weak and justifiable assumption that the reclassified constituencies were not trending in any particular way.

[17] http://hansard.millbanksystems.com/commons/1983/mar/01/parliamentary-boundary-commissions#S6CV0038P0_19830301_HOC_280

Table 5.4 – Comparison of Reclassified and Unaffected Constituencies

	Reclassified	Unaffected	Difference
Polling Districts	27.17	37.30	-10.13
	(20.06)	(27.00)	[0.00]
Polling Stations	59.35	61.01	-1.66
	(39.53)	(35.67)	[0.64]
Electors (1000s)	51.20	50.09	1.11
	(33.85)	(24.65)	[0.64]
Conservative Incumbent	0.41	0.34	0.07
	(0.49)	(0.47)	[0.11]
Labour Incumbent	0.25	0.27	-0.02
	(0.44)	(0.45)	[0.65]
Liberal (Dem.) Incumbent	0.12	0.10	0.02
	(0.32)	(0.30)	[0.55]
Open Seat	0.22	0.24	-0.02
	(0.41)	(0.43)	[0.56]
Total District Expenditure	63.28	57.94	5.34
	(54.19)	(60.10)	[0.35]
Effective Candidates	1.45	1.47	-0.03
	(0.16)	(0.18)	[0.10]
Total	110	7975	

NOTE: Calculations are based on the elections before reclassifications were implemented. I keep all observations, for both reclassified and unaffected constituencies, pertaining to these years and report mean values. Standard deviations are reported in (parentheses). P-values from t-tests of equal means are shown in [square brackets].

Although we cannot directly test for exclusivity, the third identification assumption described above, qualitative and anecdotal evidence does suggest that it might be satisfied in this particular context. Perhaps the most obvious possible violation of the

exclusion restriction comes in form of publicity. If people in affected constituencies publicly debated the reclassifications, or if journalists interviewed the affected candidates about their opinions of the consequences, this could potentially have electoral implications. It is impossible to completely rule out violations like these. To justify that they are unlikely, however, I performed a search for the words "county" and "borough" in major national newspapers for the years in which reclassifications were implemented. None of the articles explicitly discussed reclassifications.[18] Stories about reclassifications do not appear to be newsworthy.

Another violation of the exclusion restriction could arise from changes in the behavior of the central party in response to a reclassification. Suppose, for example, that the party reacted to a reduced spending limit by planning to campaign more actively in the constituency, perhaps by organizing visits from ministers or other high-ranking party members, in an effort to offset the added spending restrictions placed on the constituency's candidate. This scenario would appear to be plausible, however, according to the transcribed interviews that David Butler conducted with campaign managers from the major parties who were responsible for coordinating the link between the national and constituency-level campaign activities from the 1950s to the 1990s, none of the campaign managers mentioned reclassifications of constituencies. This could suggest that the issue was not a major cause for concern and, while this does not rule out a violation of the exclusion restriction, it makes it arguably less plausible.

Finally, from the spending limit parameters, we know that the monotonicity assumption is satisfied. Regardless of when the reclassification occured during the studied period, a shift from borough to county status always led to a higher spending limit, and a shift from county to borough always engendered a lower limit (since $\phi_t > 0$ and $\psi \geq 0$, $\forall\, t$). To put it differently, although the magnitude of the effect differs across constituencies and over years, the direction of the reclassification shock is always the same.

5.5.2 Shocks to Formula Parameters: Spending Limit Reforms

The fundamental spending limit formula essentially remained as initially described in the *Corrupt and Illegal Practices Prevention Act 1883*, but the policy parameters (δ_t, λ_t,

[18] The search was performed on newspapers scanned and made available via the website `http://www.ukpressonline.co.uk/ukpressonline/database/search/advSearch.jsp`

ψ_t, ϕ_t) were modified 13 times over the studied period, as indicated in Table 5.1. The spending limit reform proposals were drafted by the Speaker's Conference (a committee on which all elected parties were represented) before the Second World War and by the politically independent Boundary Commission after the War. The implemented changes applied to all constituencies without exception. These reforms constitute an interesting natural experiment because they induced substantial changes in spending limits from one year to the next without affecting any of the underlying formula inputs.

Why were these reforms introduced? The official reason, as stated by the Home Secretary in the parliamentary debates for example, was that spending limits had to be adjusted in response to changes in other factors influencing the electoral environment. A number of these factors affected all constituencies (e.g. inflation[19] and extension of franchise[20]), while other factors affected only some constituencies (e.g. boundary modifications). However, even though the revised formula apply to all constituencies, not all constituencies were affected in the same way. The reforms increased spending limits in some constituencies, other constituencies remained unaffected, and yet others faced reduced spending limits. Importantly for this paper, the reforms of the policy parameters affected borough and county constituencies differently.

To simplify the discussion, let us suppose that the policy parameters were reformed only once during the studied period (at time T). The basic idea is compare how the reform, on average, affected county constituencies relative to boroughs. In a difference-in-difference spirit, I compare within-constituency changes in counties induced by the reform while differencing out within-constituency changes in boroughs.

The baseline first-stage and reduced-form models are expressed in Equations 5.6 and 5.7, respectively:

$$limit_{it} = \pi_{1,1} county_{it} \cdot 1[t \geq T]_t + \alpha_{i,1} + \delta_{t,1} + X'_{it}\theta_{1,1} + \varepsilon_{it,1}\,, \tag{5.6}$$

$$y_{it} = \pi_{1,2} county_{it} \cdot 1[t \geq T]_t + \alpha_{i,2} + \delta_{t,2} + X'_{it}\theta_{1,2} + \varepsilon_{it,2}\,, \tag{5.7}$$

[19] http://hansard.millbanksystems.com/commons/1974/feb/07/representation-of-the-people#S5CV0868P0_19740207_HOC_240

[20] http://hansard.millbanksystems.com/commons/1928/may/07/clause-5-maximum-scale-of-election#S5CV0217P0_19280507_HOC_250

where $limit_{it}$ and y_{it} indicate the spending limit and observed outcome, respectively, in constituency i at time t; $1[t \geq T]_t$ represents a dummy equal to 1 in the years after time T; $county_{it}$ indicates a county dummy; $\alpha_{i,1}$ and $\alpha_{i,2}$ denote constituency-fixed effects capturing all time-invariant characteristics of a constituency; $\delta_{t,1}$ and $\delta_{t,2}$ represent time-fixed effects that wash out common trends across constituencies; X_{it} is a vector of additional control variables; finally, $\varepsilon_{it,1}$ and $\varepsilon_{it,2}$ are the error terms. $\pi_{1,1}$ and $\pi_{1,2}$ capture the effects of the reform on spending limits and outcomes in county constituencies relative to boroughs.

Does it seem plausible that the four identification assumptions outlined above are satisfied in this context? First, regarding relevance, we know from Table 5.1 that all reforms, with the exception of the reform in 1970, affected boroughs and counties differently. The reforms up until 1970 reduced the difference between counties and boroughs, whereas the reforms after 1970 gradually increased the gap. The exact magnitude of the differences naturally depends on how the electors were distributed across county and borough constituencies, and I explore this in further detail in the discussion of the first-stage results below.

Turning to the exogeneity assumption, are the conditions under which the causal effects are identified in Equations 5.6 and 5.7 reasonable? $\pi_{1,1}$ and $\pi_{1,2}$ capture the average effect of the reform on the spending limit and the outcome, respectively, under the key common-trends assumption that county and borough constituencies would have followed the same trends in the absence of the reform.

A couple of important concerns must be taken into consideration. First, one might worry that the reform was introduced in response to some kind of trending pattern. Imagine, for example, that counties experienced a substantial growth in the number of electors, relative to boroughs, and that the Boundary Commission implemented a reduction in the legal expenditure per elector in counties to prevent the legal maximum from skyrocketing in those affected constituencies. As one would expect the size of a population to affect political competition, the estimates of $\pi_{1,1}$ and $\pi_{1,2}$ would be biased without properly accounting for this factor. Indeed, the most commonly used argument in parliamentary debates for modifying the spending limit parameters is changes in the number of electors. I account for population trends like this by controlling for number of electors and population density.[21] Presumably, the effect of population size on electoral

[21] As each parliamentary constituency is divided into a number of polling districts, I measure density

competition has changed over time so I allow the coefficients on $electors_{it}$ and $density_{it}$ to vary flexibly across years.

Another concern is the problem of multiple treatments. In addition to the effect of the spending limit reform, the coefficient on interaction term, $county_{it} \cdot 1[t \geq T]_t$, picks up all other relevant policies assigned at time T that affect boroughs and counties differently. In other words, if another reform that affected boroughs and counties differently was also implemented in year T, the estimated effect would be biased towards the effect of the additional reform. One might be particularly worried about the franchise extension reforms in the first half of the 20th century. For example, the *Representation of the People Act 1918* enfranchised all males over the age of 21 and females over the age of 30 in the same year in which the first spending limit reform was implemented. If the age and gender distributions were fairly constant across county and borough constituencies, and descriptive statistics suggest that they were (Rossiter, Johnston, and Pattie 1999: p.62), this is not a major problem, especially when one controls for population density and number of electors. However, it may still be arguable that the franchise extensions carried unobservable consequences affecting boroughs and counties differently. To counter this criticism, I show that all results are robust to the exclusion of multiple-reforms years as instruments.

Third, in consideration of the exclusion restriction, is it reasonable to assume that the reforms of spending limits exclusively affected the outcomes through the new spending limits? The two related concerns discussed in the previous section are also relevant here. If the media reported on the new spending limits, or if the national party organizations changed their behavior in response to the same, the exclusion restriction might be violated. To evaluate these possibilities, I performed a search for articles containing the words "election expenses" in the years during which a reform was implemented, and read through transcribed interviews with campaign managers in those same relevant years. I did not discover any mention of campaign managers commenting on the new spending limits, and although I did find a few newspaper articles that did make note of the reforms, none of these put more weight on counties than on boroughs.

Finally, with respect to monotonicity, did the reforms in any given year affect all constituencies in a given year in the same way? More specifically, did any given re-

using the average number of electors in each polling district.

form exacerbate or amend the difference between counties and boroughs across *all* constituencies? Or did the reforms effectively push spending limits in different directions depending on the number of electors in a given constituency? From the last column in Table 5.1, it is clear that all reforms of the policy parameters, with the exception of the 1918 reform, nudged the difference between spending limits in counties and boroughs in the same direction. The magnitude of the effect, again, depends on the number of electors in each constituency, but the direction is always the same.

5.5.3 Econometric Specification

I combine the two sources of quasi-exogenous variation in a 2SLS estimation. As the instruments tap into different variations and rely on different common-trend assumptions, I present all results separately for each of the two groups of instruments, those being the county dummy and its interaction with post-reform indicators. Equations 5.8 and 5.9 outline the baseline first-stage and reduced-form models, respectively. The variables $county_{it}$ and $county_{it} \cdot 1[t \geq \omega]_t$ are the excluded instruments.

$$\begin{aligned} limit_{it} &= \beta_{1,1} county_{it} + \sum_{\omega \in \Omega} (\pi_{\omega,1} county_{it} \cdot 1[t \geq \omega]_t) + \\ &\quad \alpha_{i,1} + \delta_{t,1} + \theta_{t,1} electors_{it} + \gamma_{t,1} density_{it} + \varepsilon_{it,1}\,, \end{aligned} \tag{5.8}$$

$$\begin{aligned} y_{it} &= \beta_{1,2} county_{it} + \sum_{\omega \in \Omega} (\pi_{\omega,2} county_{it} \cdot 1[t \geq \omega]_t) \\ &\quad + \alpha_{i,2} + \delta_{t,2} + \theta_{t,2} electors_{it} + \gamma_{t,2} density_{it} + \varepsilon_{it,2}\,, \end{aligned} \tag{5.9}$$

where $\Omega = \{1918, 1929, 1948, 1970, 1974, 1978, 1982, 1987, 1992, 1997, 2001, 2005\}$, i.e., the set of years in which reforms were introduced; $county_{it}$ denotes a dummy variable taking on the value 1 if constituency i is classified as a county at time t; $1(t \geq \omega)_t$ represents a dummy variable taking on the value 1 if the observation refers to any year after the reform in year ω; $\alpha_{i,1}$ and $\alpha_{i,2}$ indicate constituency-fixed effects; $\delta_{t,1}$ and $\delta_{t,2}$ are year-fixed effects; $electors_{it}$ denotes the number of electors, or, in some specifications, the natural log thereof in constituency i at time t; $density_{it}$ stands for the average number of electors per polling district in a given year or, again, the log thereof; finally, $\varepsilon_{it,1}$ and $\varepsilon_{it,2}$ represent the error terms.

In all the tables and figures below, I report robust standard errors that are clustered at the level of the constituency.

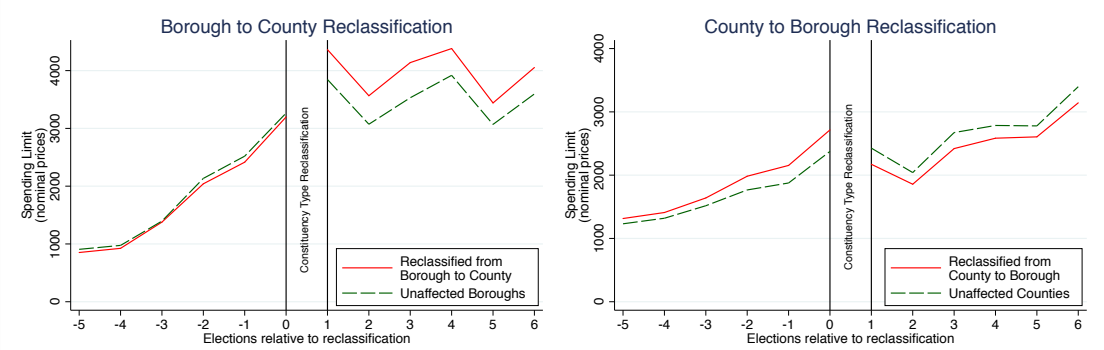
In the next section, I present the results of an OLS estimation of Equation 5.8 and evaluate the implications of these first-stage results.

5.6 First-Stage Results: Strong Effects on Spending Limits

In this section, I discuss the first-stage results. First, I look at the graphical evidence, then I turn to the statistical results. To clearly highlight changes induced by reforms and reclassifications, the graphs illustrate changes in nominal spending limits (in Figures C.1 and C.2 in the Appendix, I report the corresponding graphs in constant prices and show that the basic findings are the same).

Figure 5.4 graphically illustrates how constituency reclassifications, on average, affect spending limits. The x- and y-axes show the elections relative to the reclassification and the spending limit, respectively. The panel on the left illustrates that constituencies reclassified from borough to county status experienced a boost in the spending limit, but shows also that the affected and unaffected boroughs followed the same general trend before and after reclassification. The panel on the right focuses on the reclassifications from county to borough status, and we see that the reclassified constituencies experienced a decrease in the spending limit, while the unaffected counties appear to have remained more or less unaffected in terms of spending limit, as well. In this graph we can also detect a minor positive trending pattern. Note, however, that the positive pattern does not appear to be a general trend, but rather a specific shock kicking in between years -3 to -2. To the extent that it may be a general trend, however, it could imply an upward bias in the average effect unless one accurately accounts for the specific number of electors per constituency in the statistical analyses.

Figure 5.4 – Effect of Constituency Reclassification on Spending Limits. Constituency reclassifications from borough to county constituencies cause an increase in the spending limit, whereas reclassifications from county to borough lead to a drop in spending limits.

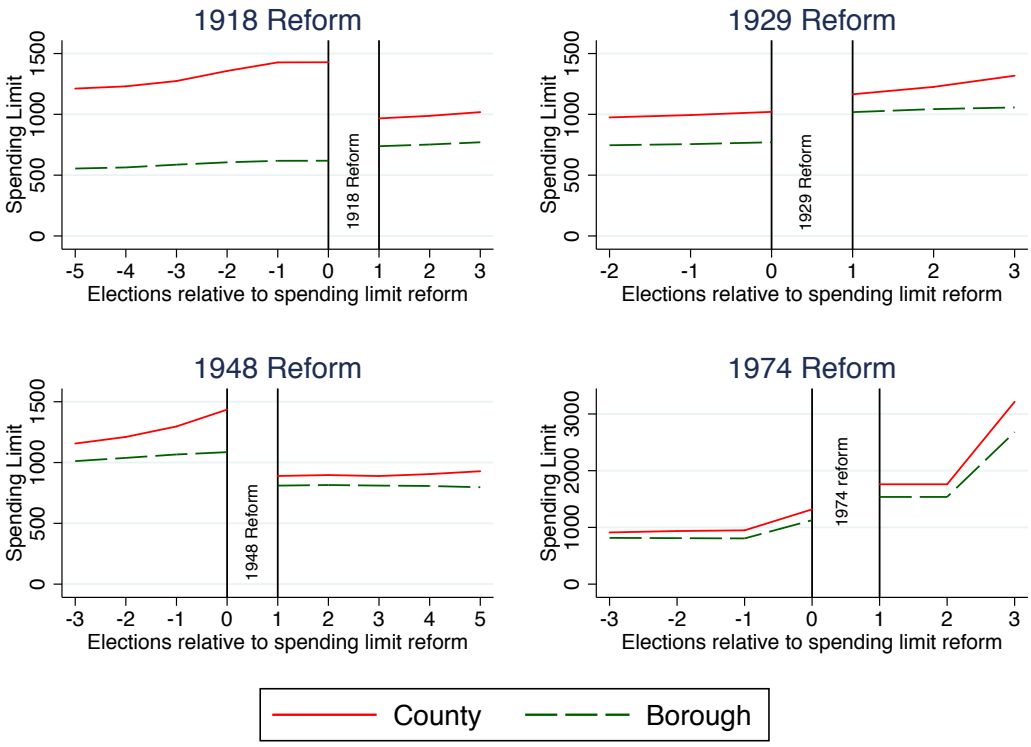


NOTE: The x-axis represents years relative to the reclassifications. The y-axis represents the spending limits. In the plot on the left, the treatment-group line reports the average limit faced by constituencies classified as boroughs in the pre-treatment period and counties in the post-treatment period. The control group is all unaffected boroughs. Conversely, in the panel on the right, the treatment-group line reports the average limit faced by constituencies classified as counties in the pre-treatment period and boroughs in the post-treatment period. The control group in this panel consists of all unaffected counties.

Turning attention to the effect of reforms of policy parameters, the four panels in Figure 5.5 illustrate how the reforms in 1918, 1929, 1948 and 1974 impacted spending limit patterns in counties as well as boroughs. The key thing to notice is how the difference between county and borough changes as the reforms were implemented. Consider, for example, the 1918 reform. The graph shows that boroughs and counties appear to have followed the same trends during the the pre- and post-reform periods, but as the reform kicked in, the average borough constituency experienced an increase in the spending limit, while the average county suffered a noticeable reduction, bringing the two types of constituencies closer together in terms of expenditure gaps. Along the same lines, and consistent with the discussion in the previous section, the reforms in 1929 and 1948 also appear to reduce the difference in spending limit between counties and boroughs, whereas the 1974 reform increased the gap between the two constituency types slightly. Although the two groups appear, for the most part, to run parallel to one another, there are discernible trending patterns to be noted. In particular, one may note a positive trend for the county constituencies prior to the reforms in 1918 and

1948, highlighting the importance of accounting for population trends in the statistical analyses.

Figure 5.5 – Reforms of Spending Limit Formula. Reforms of the spending limit formula affect county and borough constituencies differently.



NOTE: The x-axis represents years relative to the spending limit reforms. The y-axis represents the spending limits. The solid and dashed lines show the average spending limit in county and borough constituencies, respectively.

In Table 5.5, I present the statistical first-stage results. I report the coefficients on all potential instruments: the $county_{it}$ variable, which is identified using reclassification variation, along with the interactions between county and various post-reform dummies that are identified using reforms of the policy parameters. $log(spending\ limit_{it})$ in the first three columns and $spending\ limit_{it}$ in the last three columns are the outcomes, representing the endogenous variables in the 2SLS estimation. In some specifications, I linearly control for the number of electors and for population density, allowing the coefficients to vary across years. In Table C.1 in the Appendix, I show that the results are not sensitive to controlling for logged rather than linear covariates.

The coefficient on the $county_{it}$ variable is positive, highly statistically significant,

Table 5.5 – First-Stage Results. Constituency reclassifications and changes in policy parameters induce substantial, strong, and statistically significant shocks to spending limits.

	$log(Spending\ Limit_{it})$			$Spending\ Limit_{it}$ (£1000)		
$County_{it}$	0.884***	0.690***	0.689***	82.166***	59.717***	59.934***
	(0.025)	(0.006)	(0.006)	(2.267)	(0.988)	(1.035)
$County_{it} \cdot 1[t \geq 1918]_t$	-0.476***	-0.332***	-0.331***	-59.142***	-47.398***	-47.816***
	(0.019)	(0.005)	(0.004)	(1.301)	(0.629)	(0.663)
$County_{it} \cdot 1[t \geq 1929]_t$	-0.039**	-0.151***	-0.148***	2.382*	-2.809***	-2.889***
	(0.013)	(0.002)	(0.003)	(0.938)	(0.088)	(0.127)
$County_{it} \cdot 1[t \geq 1949]_t$	-0.092***	-0.051***	-0.054***	-10.088***	-7.971***	-7.824***
	(0.017)	(0.003)	(0.003)	(1.120)	(0.170)	(0.206)
$County_{it} \cdot 1[t \geq 1970]_t$	0.043***	-0.026***	-0.026***	0.513***	-0.587***	-0.595***
	(0.003)	(0.001)	(0.001)	(0.083)	(0.057)	(0.062)
$County_{it} \cdot 1[t \geq 1974]_t$	-0.035***	-0.012***	-0.011***	-0.781**	-0.102	-0.194*
	(0.006)	(0.001)	(0.001)	(0.254)	(0.073)	(0.079)
$County_{it} \cdot 1[t \geq 1978]_t$	0.047***	0.016***	0.016***	0.353***	0.032*	0.040
	(0.002)	(0.000)	(0.001)	(0.029)	(0.016)	(0.051)
$County_{it} \cdot 1[t \geq 1982]_t$	-0.058***	0.003*	0.002	-1.898***	-0.183†	-0.192†
	(0.008)	(0.001)	(0.001)	(0.376)	(0.095)	(0.109)
$County_{it} \cdot 1[t \geq 1987]_t$	0.004***	-0.010***	-0.010***	0.111***	-0.037**	-0.016
	(0.001)	(0.000)	(0.000)	(0.026)	(0.014)	(0.020)
$County_{it} \cdot 1[t \geq 1992]_t$	0.017***	0.003***	0.003***	0.127***	0.002	-0.006
	(0.001)	(0.000)	(0.000)	(0.036)	(0.014)	(0.015)
$County_{it} \cdot 1[t \geq 1997]_t$	-0.018***	0.007***	0.003***	-0.393*	0.122**	-0.002
	(0.004)	(0.001)	(0.001)	(0.181)	(0.044)	(0.049)
Observations	18478	18478	16315	18478	18478	16315
Constituencies	1636	1636	1466	1636	1636	1466
Joint instrument sign.	0.000	0.000	0.000	0.000	0.000	0.000
Constituency FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
$\theta_t electors_{it}$		✓	✓		✓	✓
$\gamma_t density_{it}$			✓			✓

NOTE: All models are estimated using OLS. Robust standard errors are clustered on constituencies and are reported in (parentheses). Significance levels: † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

and substantial in magnitude which is not surprising in light of the previous section's discussion. On average, the county constituency classification induces an increase in in the spending limit in the range of 0.69 to 0.88 log points as compared to constituencies classified as boroughs. This is equivalent to an approximate 70 to 90% increase. In levels, the average increase in the spending limit is in the range of £60,000 to £80,000. The difference between the estimates in columns 1 and 2 (and equivalently, in columns 4 and 5) suggests a minor trending pattern in the reclassified constituencies. Once I account for the number of electors, controlling for density does not appear to matter much.

Turning next to the coefficients on the interaction terms, it is immediately clear that most of the reforms, on average, had a different impact on county versus borough constituencies. Up until the 1970s, the reforms substantially reduced the difference in

spending limits between counties and boroughs, whereas later reforms increased the difference, albeit very moderately. Reforms instituted in recent years only marginally influenced the disparity between spending limits across counties and boroughs.

Given that most coefficients are independently statistically significant, it makes sense that the instruments are jointly statistically significantly different from zero as well. In summation, both the graphical and statistical analyses suggest that constituency reclassifications, as well as reforms of policy parameters, had a substantial impact on spending limits, thus satisfying the relevance criterion. Therefore, I carry on to discuss the main results in the next section, focusing on the impact of spending limits on the supply of candidates and electoral competition.

5.7 High Spending Limits Reduce Candidate Supply and Electoral Competition

In this section, I examine how campaign spending limits affect candidate supply and electoral competition. Presumably, the cost of running for office influences the supply of candidates, in that if elections are overly costly, certain candidates may be deterred from entering the race, thereby affecting the size and composition of the candidate pool from which voters are able to select public officials. I measure candidate supply using the following variables: *Unopposed Candidate*$_{it}$ is a dummy indicating an instance where only a single party fielded a candidate, thus winning the general election uncontested; *More Than Two Candidates*$_{it}$ is a dummy indicating a situation where at least three candidates ran for the seat.

Spending limits may also influence the propensity of constituents to concentrate their votes on one or two front running candidates. The effective number of candidates, *Effective Candidates*$_{it}$,[22] is indicative of the number of candidates, as well as the competition among them. *Top Two Vtsh*$_{it}$ captures the percentage of votes cast in favor of the top two candidates, a standard indicator of strategic voting behavior(e.g. Hall and Snyder Jr 2015). *Win Margin*$_{it}$, which captures the intensity of the campaign between the top two candidates, is yet another measure of electoral competition.

Table 5.6 presents the main results from the 2SLS analyses: Columns 1-4 report the

[22]The effective number of candidates is defined as $1/\sum_{c \in C} v_{cit}^2$, where v_{cit} is the vote share of candidate c, and C is the total number of candidates running in constituency i at time t

estimates based only on shocks to policy parameters; columns 5-7 report the estimates based only on reclassification shocks; in columns 8-11, I report the estimates obtained by combining the two preceding types of shocks. In Tables C.2 and C.3 in the Appendix, I show that the results are robust to various econometric specifications and different functional-form assumptions.

Table 5.6 – Effect of Spending Limits on Candidate Supply and Electoral Competition. On average, an increase in spending limits causes fewer candidates to run for office, fosters strategic voting, and reduces electoral competition.

	Policy Change IV				Reclassification IV			Combined IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Outcome: *Unopposed Candidate*$_{it}$										
log(*Spending Limit*$_{it}$)	0.16***	0.18***	0.19***	0.16*	0.08**	0.13***	0.12**	0.10***	0.15***	0.14***	0.12***
	(0.03)	(0.04)	(0.04)	(0.08)	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
	Outcome: *More Than Two Candidates*$_{it}$										
log(*Spending Limit*$_{it}$)	-0.16***	-0.22***	-0.20**	-0.30†	-0.05	-0.13*	-0.11†	-0.07*	-0.19***	-0.15**	-0.16**
	(0.05)	(0.05)	(0.07)	(0.16)	(0.03)	(0.05)	(0.06)	(0.03)	(0.05)	(0.06)	(0.05)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
	Outcome: *Effective Candidates*$_{it}$										
log(*Spending Limit*$_{it}$)	-0.12***	-0.16***	-0.14***	-0.26***	-0.05***	-0.09***	-0.08**	-0.06***	-0.13***	-0.11***	-0.11***
	(0.02)	(0.02)	(0.03)	(0.06)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
	Outcome: *Top Two Vtsh*$_{it}$										
log(*Spending Limit*$_{it}$)	4.07***	6.56***	4.88***	14.83***	0.70	2.31†	1.41	1.46†	5.13***	3.30**	4.27***
	(1.00)	(1.16)	(1.38)	(3.91)	(0.73)	(1.19)	(1.32)	(0.75)	(1.10)	(1.28)	(1.22)
Observations	17647	17647	15761	17647	17647	17647	15761	17647	17647	15761	17647
	Outcome: *Win Margin*$_{it}$										
log(*Spending Limit*$_{it}$)	3.96†	4.80†	6.70*	10.74†	3.99**	6.77*	6.58*	3.93**	5.58*	6.79*	6.54*
	(2.15)	(2.55)	(2.78)	(6.05)	(1.44)	(2.73)	(2.99)	(1.45)	(2.38)	(2.67)	(2.59)
Observations	17647	17647	15761	17647	17647	17647	15761	17647	17647	15761	17647
Constituency FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$\theta_t log(electors_{it})$		✓	✓	✓		✓	✓		✓	✓	✓
$\gamma_t log(density_{it})$			✓				✓			✓	
Excluded Instruments:											
$county_{it}$					✓	✓	✓	✓	✓	✓	✓
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega$	✓	✓	✓					✓	✓	✓	
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega > 1918$				✓							✓

NOTE: All models are estimated using 2SLS. Standard errors are clustered on constituencies and are reported in (parentheses). Significance levels: † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

In the first panel (Outcome: Unopposed Candidate), I examine how spending limits affect the probability of an uncontested election. Column 1 presents the results from the baseline model based on changes in policy parameters and without any covariate adjustment apart from constituency- and year-fixed effects. Column 5 presents the equivalent results based on the reclassification instrument. The baseline results are positive and statistically significant, suggesting that higher spending limits lead to more uncontested elections. The results are fairly stable when I control for the number of electors (in columns 2, 6, and 9) and for elector density (in columns 4, 7, and 10), as

well as when I treat the 1918 policy change mentioned earlier as a covariate rather than as an excluded instrument.

Across all models, the estimated coefficients are positive, statistically significant, and substantial in magnitude. A single log-point increase in the spending limit, or approximately the equivalent of a one-percent increment, causes an increase in the probability of an uncontested election of approximately 0.1. Estimates based on reclassification variation are slightly smaller in magnitude compared to those based on policy changes, but overall, the results are relatively stable across specifications and instruments.

In the second panel (Outcome: More Than Two Candidates), I show how spending limits affect the probability of a race being contested by more than two candidates. All these estimates are negative and statistically significant. Most estimates centered around the -0.15 mark, suggesting that a one percent increase in the spending limit reduces the probability of an election being contested by more than two candidates by approximately 15 percentage points. Again, the findings appear to be relatively stable across econometric specifications.

The results in the third panel (Outcome: Effective Candidates) are consistent with the findings reported in the two first. These results are also negative across all specifications, once more, indicating that higher spending limits reduce the effective number of candidates who run for office. On average, a one percent increase in the spending limit reduces the effective number of candidates competing in the general election by approximately 0.1.

Why might candidates be reluctant to run for office when spending limits are high, as the findings suggest? One might reasonably surmise that candidates, at least to some extent, strategically forecast how the spending limit will affect their costs and potential benefits of running for office, taking into account their financial position as compared to that of their fellow candidate(s). An explanation that is observationally consistent with the findings is that high spending limits impose too high costs on candidates: Some candidates may not have the financial resources or the connections needed to raise the additional funds, or it may be too costly – in terms of time and energy – for them to raise the money to make it worthwhile. Another possibility is that high spending limits diminish the expected benefits of running for office for some candidates by reducing their perceived probability of winning: Even if candidates have the necessary financial

resources, they may expect to lose the election because, compared to their opponent(s), they have a low marginal spending efficiency. The ultimate consequence of the above scenarios may be that some candidates strategically decide not to run at all.

In the fourth panel (Outcome: Top Two Vtsh), I examine the impact of spending limits on strategic voting as measured by the vote share of the two frontrunners. All estimates are positive and many are statistically significant. Most of the estimates are located in the interval between 1 and 5, suggesting that a one percent increase in the spending limit causes a 1 to 5 percentage-point increase in the percentage of votes cast for the top two candidates. To put it differently, electors appear to behave more strategically, and are less likely to waste votes on non-viable candidates when candidates have the wherewithal to spend more money. One way to interpret this finding revolves around information: Voters tend to vote more strategically in high information environments (Hall and Snyder Jr 2015), and, to the extent that campaign spending leads to better informed voters, high spending limits may induce a concentration of votes on the front-running candidates.

Finally, in the fifth panel (Outcome: Win Margin), I explore how spending limits affect the vote share winning margin. All estimates are positive, statistically significant, and substantial in magnitude. In other words, the winning margin noticeably widens in relation to the higher amount of money candidates are permitted to spend.

What do we make of these findings? On the one hand, the results are consistent with a scenario in which some candidates enjoy excessive financial advantages, and in which those advantages reduce electoral competition to their benefit. On the other hand, the findings are observationally consistent with an equilibrium in which higher level of campaign spending help voters to weed out weaker candidates, and in which low-quality candidates may strategically foresee their defeat and, in response, refrain from running for office. To shed further light on the channels through which campaign spending limits impact electoral competition, I explore how the limits affect campaign strategies in the next section.

5.8 High Spending Limits Promote Professionalized Campaigns

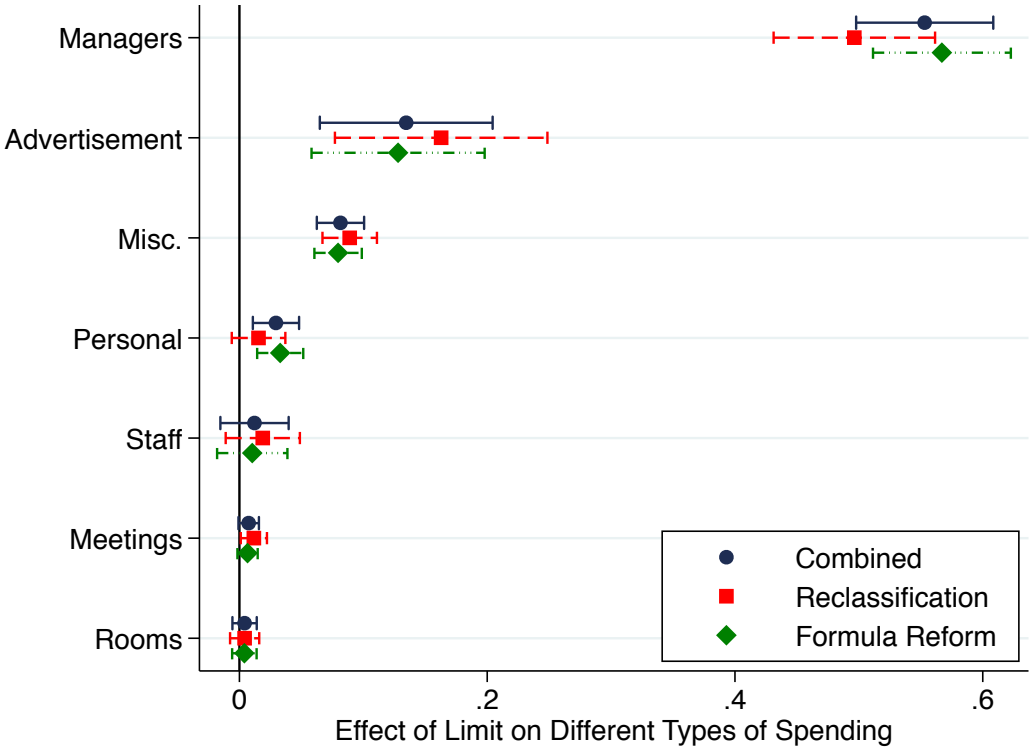
How do candidates change their campaign strategies when spending limits are raised? To answer this question, I aggregate spending across candidates within a specific constituency in a given election by each of the seven different spending categories outlined earlier:

$$y_{it}^{j} = \sum_{c \in C} y_{cit}^{j}\,, \tag{5.10}$$

where y_{cit}^{j} is the spending on category j by candidate c in constituency i at time t, and C is the set of candidates running in the constituency. I then use the seven expenditure category variables as outcomes and re-estimate the system of equations (Equations 5.8 and 5.9) for each of them. The endogenous spending limit variable and the outcomes are all measured in levels (total British pounds). In Figure 5.6, I report the results from a simple baseline model that linearly controls for the number of electors. In Tables C.4, C.5, and C.6 in the Appendix, I present the full set of results and show that they are not sensitive to the choice of instrument, econometric specification, or to functional-form assumptions.

The results suggest that some expenditure categories are much more reactive to changes in the legal spending maximum than others. In particular, the salaries of campaign managers appear to be very sensitive to the legal spending limit: When the limit increases by one pound, the average spending on campaign managers increases by approximately 40 to 50 pence. This is a substantial leap when one takes into account that the average spending on campaign managers never exceeds 30% of total spending in a given year and accounts for less than 15% of total spending in most years as indicated by Figure 5.2. This finding can be interpreted in a number of ways. One interpretation is that candidates hire better qualified and hence higher-paid campaign managers, or increase an existing campaign manager's hours, when the limits are high. Naturally, candidates want to run professional and effective campaigns. Hiring experienced, highly competent managers may help them to achieve this goal. Another interpretation is that campaign managers, who by law have the authority to approve all expenses, allocate more money to themselves when spending limits permit them to do so, not because this

Figure 5.6 – Effect of Spending Limits on Different Types of Outlays. When spending limits are high, candidates professionalize their campaigns by allocating more money to campaign managers and advertisement, and less to expenditure categories associated with face-to-face campaigning, such as public meetings.



NOTE: The graph reports point estimates and 95% confidence intervals. All estimates were obtained using 2SLS.

is necessarily the most efficient way of spending the money, but because they control the campaign budget and simply have the power to allocate the money as they see fit. Yet another interpretation is that candidates underreport salaries when the spending limit is low, but truthfully report them when the limit is high as this category of expenditure is difficult to audit.

The next most sensitive spending category is advertisement and printing. For each one-pound increase in the spending limit, candidates allocate, on average, 20 to 30 pence on advertisement and printed materials. Unlike door-to-door campaigning, this type of mass communication facilitates the diffusion of political information on a larger scale and to a wide range of voters at a comparably low cost. Presumably, the marginal return on this category of investment is relatively high – all else being equal, it is easier for voters to remember the name of a candidate whom they see advertised in the local

newspaper, and whose photo appears on distributed pamphlets and roadside billboards.

Turning to the lower end of the spectrum of estimated effects, we see that candidates do not appear to increase their spending on public meetings, personal expenses, and renting of rooms in response to heightened spending limits, or, at most, they only do so marginally. A common feature of these expense categories is that they are primarily related to face-to-face campaign strategies in which candidates travel around their constituency to meet with voters in person, by renting rooms for organized public meetings, for example. Likewise personal expenses, which mainly consists of outlays associated with travelling and accommodation, are crucial if candidates are to visit voters in all parts of their constituency.[23]

Taken together, these findings could indicate that higher spending limits give rise to more professional electoral campaigns, characterized by the engagement of salaried political advisors and managers, a greater reliance on mass communications and advertisement, and relatively few resources devoted to personal interaction between candidates and their constituents.

5.9 High Spending Limits Benefit Center-Right Candidates

Spending limits may not affect all candidates equally. Some candidates have easier access to financial resources, making it easier for them to respond to changes in the spending limit, whereas other candidates find it more difficult to acquire the necessary campaign funds. In this section, I explore how the legal restrictions on spending affect expenditures across parties. More specifically, I estimate the impact of an increase in the spending limit on total spending by each party. In Figure 5.7, I report the results from a simple baseline model that linearly controls for the number of electors, and in Tables C.7, C.8, and C.9 in the Appendix, I present the full set of results and show that they are not sensitive to the choice of instrument, econometric specification, or to functional-form assumptions.

The results reveal a clear and stable pattern across parties. On average, candidates from the Conservative and Liberal (Dem.) parties increase their spending by approx-

[23] In particularly, this was important in the earlier part of the studied period where travelling from one end of a constituency to the other was not a trivial matter in some areas of the country.

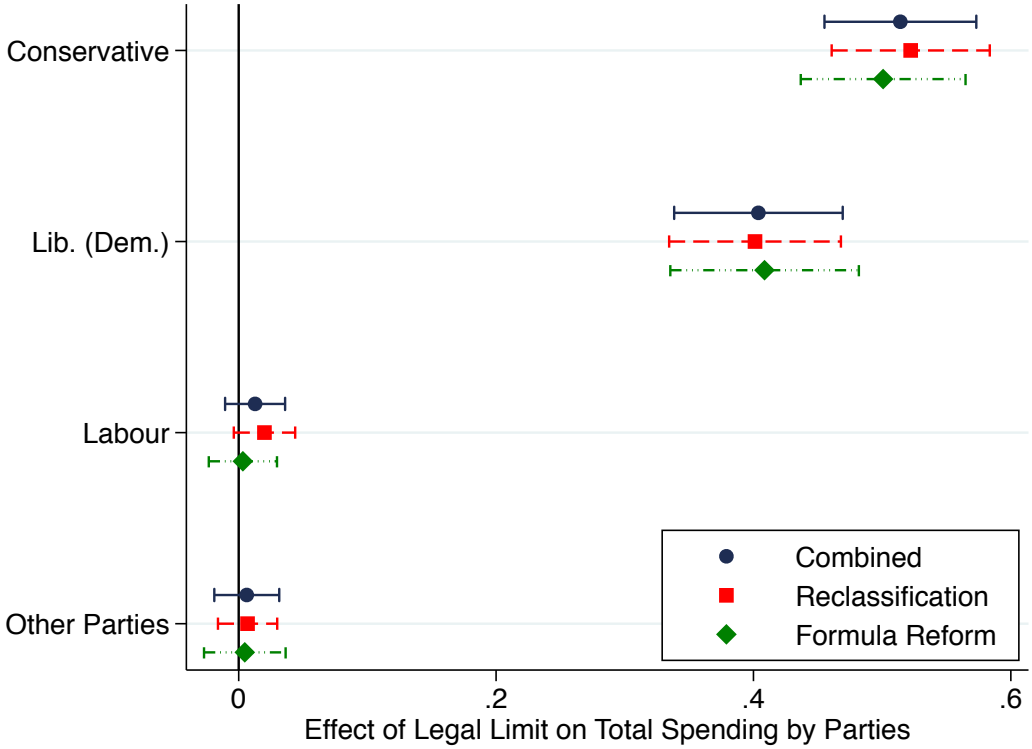
imately 50 and 40 pence, respectively, for each pound increase in the spending limit; candidates representing the Labour party as well as minor parties do not, at least on average, increase their campaign spending at all when the limit is raised.

Although it is probably not surprising that Conservative candidates react more quickly to hikes in spending limits than do other parties' candidates, the responsiveness of the Liberal (Dem.) party and the unresponsiveness of Labour and other parties are somewhat striking. When interpreting the results, one has to keep in mind that the estimated effect is a LATE and, as discussed above, weighted towards constituencies with a greater number of electors. These constituencies have high spending limits to begin with, therefore the lack of sensitivity among Labour candidates with respect to spending limits perhaps reflects that the initial, lower restrictions were not binding for these candidates to begin with. To put it differently, the initial spending limits, in general, are so high in constituencies affected by the instruments that Labour candidates do not spend even close to the limit, thus any fluctuation in the limit, either up or down, does not affect their behavior. However, it seems reasonable that the ranking of the effects across parties would be fairly similar across constituencies.

How can we account for the pattern across parties? At least up until the 1950s, and one might suppose still, to some extent, candidates in all three major parties personally contributed significant amounts to their political campaigns(for a detailed discussion see Pinto-Duschinsky 1981: ch. 5-7) However, the personal wealth of candidates varies systematically across parties, or at least it did historically. Conservative candidates were, in general, more affluent than were Liberal or, in particular, Labour candidates. The campaign spending inelasticity could be driven by constraints on personal finances faced by relatively poor Labour candidates.

Another explanation for the spending pattern focuses on the differences in external funding sources. Throughout the studied period, Labour candidates relied heavily on financial support from unions, and political campaigns were by necessity closely and centrally coordinated with these. On the other hand, the Conservative party, and to a lesser extent the Liberals, being much more decentralized in their organizational structure, predominately solicited campaign contributions from local businesses and wealthy individuals. Given the above dynamics between politicians and citizenry, it seems reasonable to assume that unions placed great import on securing a Labour majority while

Figure 5.7 – Effect of Legal Limits on Spending by Parties. Spending by Conservative and Liberal (Dem.) candidates is sensitive to changes in the legal maximum, whereas spending by candidates representing Labour and minor parties is not.



NOTE: The graph reports point estimates and 95% confidence intervals. All estimates were obtained using 2SLS.

they cared less about the actual identity of the Labour candidates themselves. Conversely, it was presumably important to the many small, local businesses supporting a Conservative candidate's campaign that their specific constituency elected the Conservative candidate. This distinction between centralised and decentralized sources of funding for Labour and Conservative candidates, respectively, is somewhat stylized, but could potentially account for the differences in the spending elasticity. In response to a rise in the spending limit, it may be easier for a Conservative candidate to solicit campaign contributions from local businesses than it would be for a Labour candidate to convince the union leaders to funnel more money towards their constituency.

5.10 High Spending Limits Benefit Incumbents

In the context of U.S. elections, studies have documented that incumbents benefit from substantial financial advantages relative to challengers (Fouirnaies and Hall 2014). Those fundraising advantages may become exaggerated when candidates are allowed to spend more money on their electoral campaigns. In this section, I explore how expenditure limits affect spending differentials between incumbents and challengers.

For each spending category, j, the spending differential is defined as the difference between expenditures by the candidate representing the party who won the seat in the previous election and all other candidates:

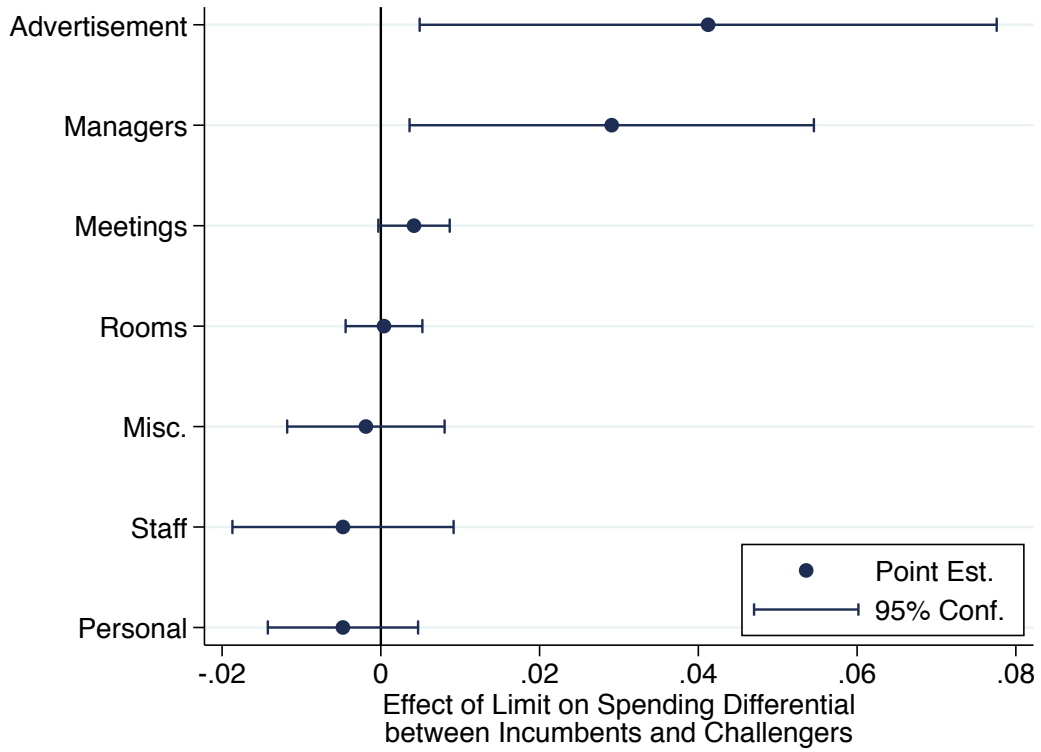
$$y_{it}^{j} = y_{cit}^{j}|_{c \in W_{it}} - \sum_{c \notin W_{it}} y_{cit}^{j}\,, \tag{5.11}$$

where y_{it}^{j} represents the spending differential on category j in constituency i at time t; y_{cit}^{j} is the spending on expenditure category j by candidate c in constituency i in election t; W_{it} is the set containing the candidate representing the winning party at time $t-1$. To avoid selection problems stemming from the strategic retirement of incumbents, the incumbency definition is based on the incumbency status of the party and not of the individual candidate.

Using the spending differentials between incumbents and challengers as outcome variables, I re-estimate the system of equations outlined above (Equations 5.8 and 5.9). In Figure 5.8, I present the main results from a simple baseline model with linear control variables. In Tables C.10, C.11 and C.12 in the Appendix, I present the full set of results and show that the findings are robust to different econometric specifications and alternative functional-form assumptions.

The results support the idea that higher spending limits financially benefit incumbent candidates: When the spending limit is raised, incumbent candidates outspend challengers. On average, a single pound increase in the spending limit precipitates an approximately 5-pence increases in the spending differentials pertaining to advertisement and campaign managers. More to the point, when the spending limit increases, the incumbent candidate spends more money on advertisement and on hiring professional political advisors than do *all* challenging candidates taken together. However,

Figure 5.8 – Effect of Legal Limit on Spending Differential between Incumbents and Challengers. Higher legal limits increase the spending differential between incumbents and challengers on expenditures related to advertisement and campaign managers.



NOTE: The graph reports point estimates and 95% confidence intervals. All estimates were obtained using 2SLS.

changes in spending limits do not appear to affect the spending differential related to other types of outlays in any systematic way.

These findings could indicate that high spending limits enable incumbents to run advanced, professional campaigns that reach broad segments of voters, whereas challengers with proportionally limited access to financial resources are held back from running more than basic and unsophisticated campaigns that crucially depend on volunteers.

5.11 Conclusion

Half of the democratic countries in the world impose limits on the amount of money candidates are legally permitted to spend on their political campaigns. Still, we know little about the electoral consequences of this widespread campaign finance institution.

This paper examines the consequences of campaign spending limits in the context

of U.K. House of Commons elections. I offer a new dataset containing disaggregated information on the campaign expenditures of candidates running for office from 1885 to 2010. On the basis of this data and employing an empirical strategy that exploits within-constituency changes in spending limits induced by shocks to formula coefficients and formula inputs, I assess how spending limits influence electoral practices and behaviors. The results suggest that allowing candidates to spend more money diminishes the pool of candidates running for office, reduces electoral competition, encourages professionalized political campaigns, and tends to benefit incumbents and candidates representing center-right parties.

To what extent can these findings be generalized to other countries? As noted in the introduction, many countries, globally, are strongly influenced or have wholly adopted central aspects of the regulatory regime governing campaign finance in the U.K. House of Commons. This could suggest that the conclusions reached in this paper may be extended to a broader spectrum of countries. The fact that Milligan and Rekkas (2008) drew similar inferences (i.e. higher limits lead to less close elections and fewer candidates) based on Canadian elections supports the idea that these conclusions may apply to other countries whose electoral processes have been shaped by the Westminster system of political finance.

The conclusions from this paper could indicate that campaign finance institutions may have, at a general level, farther-reaching, longer-term consequences for the development of political systems and may fundamentally shape political cultures more than previously assumed. The findings could shed new light on our understanding of important questions in comparative politics that have long puzzled political scientists: Why is it the case that Duverger's law, the claim that plurality rule produces two-party systems, "appears to work perfectly in the U.S." (McDonald 2009; Grofman, Blais, and Bowler 2009: p. 135), while it does a poor job of explaining the number of parties the U.K. (Gaines 2009), Canada (Johnston and Cutler 2009), and India (Nikolenyi 2009)? Why does the incumbency advantage appear to be so much stronger in U.S. elections than it does in the U.K. and Canada (Cain, Ferejohn, and Fiorina 1984; Eggers and Spirling 2015; Gaines 1998)? Why is there no Labor party in the U.S., when the labor movement was able to secure political representation in the U.K., Canada and in other countries with Westminster systems of government (Archer 2010; Sombart 1976)? Why

are U.S. political campaigns more professionalized than are campaigns in the U.K. and many other countries (Blumler and Gurevitch 2001; Plasser 2000)?

The answers to salient, comparative questions like these are obviously complex and manifold. The results presented in this paper could indicate that a certain extent of the explanations may be deeply rooted in the distinct characteristics of campaign finance institutions around the world.

Chapter 6

Conclusion

This thesis examines, empirically, how political power affects the distribution of campaign contributions, and how campaign spending affects the distribution of political power through electoral competition. More specifically, it addresses the following questions:

(i) What is the financial value of incumbency status, and who generates it?

(ii) Who values legislative agenda setters, and why do they do so?

(iii) What are the electoral consequences of statutory limits on campaign expenditure?

These questions are each examined in three separate papers, but taken together, the answers make up a coherent argument: Campaign donors contribute money to the insiders of powerful political elites in exchange for access to the policy-making process, and that the power of these elites is sustained, at least in part, due to these contributions.

In the first paper, I use a regression discontinuity design to document that incumbents in U.S. state and federal elections enjoy sizeable financial advantages relative to their challengers. On average, incumbency status fosters a 20 to 25 percentage-point boost in the incumbent party's share of total contributions. Examining the heterogeneity in the treatment effect, I discuss three explanations for the financial incumbency advantage commonly referred to in the related literature. Namely, that incumbents are financially advantaged because they a) exploit office holder benefits, such as the franking privilege and district offices; b) develop fundraising and networking skills while in office; or c) are targeted by access-seeking industries more pointedly than are challengers. The data contains nothing to support the first two hypotheses, but it does produce

strong evidence in favor of the third. Donors from access-seeking industries, especially donors from industries subject to heavy regulations, systematically target donations towards incumbents. On average, these donations generate approximately two-thirds of the financial advantage enjoyed by those incumbents.

In the second paper, I use a difference-in-difference design to show that, relative to rank-and-file legislators, U.S. legislators who are institutionally endowed with agenda-setting powers are given special treatment by campaign donors. Using a new dataset exhibiting institutional power in the state legislatures, I show that party leadership, committee chairmanship, and majority-party membership are three valuable institutional assets, but that they are valued differently by various groups in society: In general, access-seeking groups with narrow and well-defined regulatory interests value these institutional assets more than do ideologically driven groups and individuals. Ideologically driven groups and individuals are most sensitive to party leadership positions, whereas access-seeking industries are highly sensitive to all agenda-setting assets. Access-seeking industries appear to target majority-party members in general, but committee chairs in particular. A chair is held in even higher esteem by regulated industries whose business activities are overseen by that chair's committee, and in particular in chambers where committees have the faculty to block bills from reaching the chamber floor.

Finally, I use an instrumental variables approach in the third paper to study the effect of statutory spending limits on electoral behavior in the context of U.K. House of Commons elections. Exploiting within-constituency changes in spending limits induced by administrative reclassifications of constituency types and by reforms of the spending limit formula, I show that unrestrained spending diminishes the pool of candidates, reduces electoral competition, fosters professionalized campaigns, and benefits incumbents and parties with connections to wealthy individuals and businesses.

6.1 Money in Politics: Problems and Solutions

Chapters 3 and 4 in this thesis outline what might be considered to be, in modern representative democracies, intrinsic problems of representation and accountability induced by campaign finance, while Chapter 5 discusses a potential solution to those problems.

Can we conclude from the findings that the financial advantages enjoyed by powerful politicians are cause for concern? There are, potentially, two separate but connected

problems. The first problem is a question of misrepresentation of interests and concerns whether or not campaign finance creates a bias in the policy-making process by pulling policy away from the preferences of constituents. As suggested by the findings in Chapters 3 and 4, access-seeking interest groups appear to behave very strategically when they allocate campaign contributions, targeting substantial amounts towards incumbents in general, and towards agenda setters in particular. Moreover, this behavior is most pronounced when donors have significant economic interests at stake. In the long run, the sizeable contributions from these donors would not be consistent with the notion of profit-maximizing firms unless donors could expect to receive something in return for their contributions. This could suggest that powerful legislators accept campaign contributions in exchange for political favors or for giving donors privileged access to the political process. Of course, out-right vote buying is illegal in most countries: the results from the U.S. could indicate that legislators provide more sophisticated services, such as negative agenda control, earlier in the political process, making it far less likely for them to incriminate themselves. Nonetheless, the outcome of unbalanced campaign finance engender a systematic bias in the policy-making process towards the interests of access-seeking groups or to maintaining the status quo. If the interests of access-seeking groups are misaligned with the interests of the broader population, this may be a cause for concern.

The second problem is a question of accountability. It concerns whether or not financial advantages electorally insulate privileged candidates to such a degree that voters find it difficult to hold them responsible for their actions. It is important to stress that electoral advantages enjoyed by elected politicians are not, in and of themselves, necessarily normatively troubling. In equilibrium, the electoral advantages may simply be the result of esteemed conduct, conduct that is justly rewarded with a higher number of votes. However, to the extent that an electoral advantage is generated by campaign contributions, a legislator may become insulated from re-election concerns in ways that weaken the ability of voters to punish poor performance. It is difficult to ascertain the degree to which campaign finance induces an accountability problem without knowing exactly how money translate into votes, but it seems plausible that more money injected into politics would only serve to exacerbate the accountability issue.

In some countries, and most notably in the U.S., statutory limits on campaign

spending are deemed to be unconstitutional, at least in the simple form discussed in Chapter 5, hence regulating money in politics using spending limits is not feasible in these countries. Leaving aside questions of legality or constitutionality, though, could campaign spending limits be the solution to the problems of policy bias and electoral insulation induced by campaign donations?

The answer to this question is more complicated. On the one hand, imposing statutory limits on campaign spending has clear advantages. Firstly, imposing limits on spending implies limiting the extent to which candidates may feel indebted towards donors. If candidates are prohibited from running expensive electoral campaigns, the scope for selling political favors diminishes, which in turn may lessen the problem of misrepresentation.

Secondly, spending limits may level the financial field upon which electoral campaign battles are fought. The campaign finance system financially advantages incumbent legislators, in particular legislative agenda setters, and if campaign spending, to some extent, produces more votes, this benefits these candidates electorally. Imposing a legal maximum on campaign expenditure would moderate the advantages.

Thirdly, imposing restrictions on campaign spending appears to foster electoral competition. Electoral competition may encourage incumbents to be more responsive to the demands of their constituents, and it may reduce the potential bias favoring monied interest groups in the policy-making process. Due to the initial electoral advantages enjoyed by incumbents, access-seeking industries allocate more resources to incumbents than to their challengers because investments in those candidates yield a higher expected return than investments in challenging candidates. Electoral competition may not only reduce the incumbency advantage, making it less attractive for access-seeking industries to target incumbents, but may also reduce the incumbent legislators' feelings of obligation to these interest groups, alleviating the bias in the policy-making process.

On the other hand, there are notable downsides to imposing spending limits. Allowing candidates to spend unlimited reserves on campaign activities may lead to superior electoral campaigns and, as a result, produce more politically informed voters. As documented in Chapter 5, one of the consequences of higher spending is that candidates, with the aim of reaching more voters, run more professional campaigns. To the extent that these campaigns enable voters to make more informed decisions and to select better

candidates, high levels of spending may lead to socially desirable outcomes, improving the welfare of voters and the broader citizenry.

Asymmetries in campaign strategies could mean that spending limits impose constrains only on certain types of candidates. More specifically, restrictive limits may not only reduce the intrinsic disadvantages faced by challengers and parties with poor financial networks, but may even give them unfair advantages over other candidates. Typically, challengers and candidates representing left-wing parties, who have relatively easy access to networks of party activists, grassroots endeavors, unions, etc., often structure their campaigns around such labor intensive activities as door-to-door campaigns. Conversely, incumbents and center-right party candidates, who tend to be more closely connected with wealthy businesses and individuals, more often run capital intensive campaigns, involving expensive advertising and a wide distribution of printed campaign materials. While it is difficult to quantify the value of the former type of campaign activity, the value of the latter is fairly evident and relatively easy to document. As a consequence, even though all candidates would de jure face the same spending limits, only those candidates who depend on capital intensive campaign strategies would de facto be restricted.

6.2 Three Important Academic Contributions

The empirical findings in this thesis add nuances to our current understanding of the interplay between campaign finance and political power, but three aspects of the project stand out as particularly significant and highly important academic contributions that will push the research on money in politics forward.

The first, as well as the most concrete and tangible contributions born out of this Ph.D. project is the compilation of two new datasets. Many hypotheses on leadership and committee power contained in the theoretical literature on legislative organization will not be able to be tested with U.S. House data in the foreseeable future. However, the more than 30,000 observations in the new dataset on party and committee leaders in state legislatures permits the empirical testing which has, thus far, been lacking. Upon publication of the paper in Chapter 4, I will make this new dataset publicly available.

As discussed in Chapter 5, the limited availability of relevant data is presumably one of the main reasons why the literature on campaign finance revolves primarily

around U.S. elections. The new dataset on U.K. campaign spending, with over 61,000 candidate-year observations spanning a period of more than 130 years, provides a number of data points that exceeds the current number of observations from U.S. federal legislative elections. Besides the obvious value as a source for political and economic historians, this extensive new dataset will enable future researchers to test whether or not conclusions drawn from U.S. data may be generalized to Westminster systems of government. In addition to the low-hanging fruit of replicating U.S. studies based on U.K data, perhaps more importantly, the detailed and disaggregated information on different types of expenditures will allow scholars to develop and substantiate new theories around the spending side of campaign finance, a field of study which is currently underdeveloped relative to other aspects of political research. Once Chapter 5 has been published, I will make this dataset publicly available, as well.

The second and more substantive contribution resulting from the development of this thesis is the empirical evidence documenting how the campaign finance system financially benefits insiders of political elites. While scholars have long been aware that incumbents, particularly party leaders and committee chairs, raise more money than challengers, research up until this point has not convincingly disentangled selection issues from institutional effects. Leaning on much weaker assumptions than those employed in previous studies, this thesis documents the *causal* effects of incumbency status and agenda-setting positions on a candidate's ability to attract campaign contributions. In other words, the evidence isolates the institutional effects. Furthermore, while scholars have theorized about the motivations driving political donations, I use disaggregated campaign finance data, along with information on the identity of donors and variations in the institutional powers of legislators, to give a detailed empirical account of why some groups are more sensitive to political power than others.

Finally, the evidence supporting the consequences of statuatory limits on campaign spending constitute a third academically significant contribution. Many scholars of campaign finance have theorized about these consequences, but this thesis is the first major empirical study that identifies the impact of campaign spending caps on electoral behavior under relatively weak and justifiable identification assumptions. Not only do the key findings in this thesis challenge core arguments laid out in the literature on money in politics, the results of it may also help us to improve the institutional

design of policies that were originally created to uphold the fundamental principles of representative democracy.

Schattschneider (1960) famously noted that the choir of interests groups sings with a distinctively upper-class accent. While scholars have long been aware that moneyed interests may induce bias into the political process, the exact nature of the bias has been obscure. The evidence provided in this thesis suggests that the root of this bias, the fundamental reason why the voices of some interest groups ring out more clearly than others, may be deeply entrenched in the interplay between political power and campaign finance in modern representative democracies.

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Appendices

Appendix A

Appendix: Financial Incumbency Advantage

The appendix consists of two sections. The first section presents results and calculations that are mentioned but not included in the paper. The second section presents a variety of robustness checks.

A.1 Additional Results and Calculations

In the paper we mention that the impact of incumbency on total contributions is substantial. To estimate the average effect of incumbency while taking into account that the effect could differ across Republican and Democratic incumbents, we reshape the data such that we have one observation for each party in every election, and estimate equations of the form:[1]

$$Party\ Level_{pi,t+1} = \beta_0 + \beta_1 Party\_Winner_{pit} + f(Z_{pit}) + \epsilon_{pi,t+1} \tag{A.1}$$

where $Party\ Level_{pi,t+1}$ is the total contributions that go to party p in district i in election t. The variable $Party\_Winner_{pit}$ is an indicator variable that takes the value one when district i reelects the candidate from party p in election t. By setting up the regression this way, we obtain a weighted average of the incumbency effects for Democratic and Republican contributions. However, results are substantively identical when choosing instead to focus only on one party or the other. The function $f(Z_{pit})$ represents the function of the running variable, the vote share winning margin of party p in district i in election t. We use a 5 percentage-point bandwidth around the discontinuity.

Table A.1 shows how incumbency affects the *level* of campaign contributions. The results indicate that incumbency has a substantial impact on the level of donations. On average, incumbency approximately causes a \$275,000 and \$29,000 jump in campaign contributions in the U.S. House and state legislatures, respectively. Further, the estimates indicate that donations from access-oriented interest groups account for a huge part of the overall financial incumbency advantage. At the state level, approximately 71% of the overall financial incumbency advantage appears to be driven by the donations from interest groups, and at the federal level approximately 60% of the average effect seems to come from access-oriented interest groups.

Table A.2 presents a simple back-of-the-envelope calculation of the impact of the financial incumbency advantage on vote shares. The crude idea is simply to multiply the dollar estimates obtained in this paper with the estimates of the effect of incumbents'

[1] The level estimates are not sensitive to the specific estimation approach outline below. The estimates based on Democratic and Republican incumbents are essentially the same.

Table A.1 – RDD Results: Effect of Incumbency on Total Contributions and Investor Contributions

	(1) Total	(2) Investor	(3) Total	(4) Investor
Party Win (time t)	275.63*	165.70*	28.26*	20.07*
	(55.72)	(15.64)	(8.31)	(2.93)
Constant	512.83*	124.65*	70.40*	20.98*
	(37.99)	(9.18)	(5.16)	(1.29)
Observations	1136	1136	8040	8040
Level	Federal	Federal	Federal	Federal

NOTE: Standard errors in parentheses. The reported standard errors are the maximum of robust and robust standard errors clustered by elections. All models are estimated using OLS with a linear specification of the forcing variable estimated separately on each side of the discontinuity. The outcome variables in models 1 and 3 are the party's total contributions at $t+1$ (1000 dollars measured in constant 1990 dollars), and the outcome variable in models 2 and 4 are total donations to the party by investor contributors at $t+1$ (1000 dollars measured in constant 1990 dollars). All models are estimated based on a 5 pct. bandwidth. * $p < 0.05$.

campaign spending on vote shares from the literature.

Table A.2 – Calculation of the Impact of the Financial Incumbency Advantage on Vote Shares

Author	Approach	Level	Time Period	Point Estimate	Total Impact Vote Shares	Investor Impact Vote Shares
Jacobson (1985)	OLS	U.S. House	1972–1982	0.20 %	0.55 %	0.33 %
Green and Krasno (1988)	OLS	U.S. House	1978	0.10 %	0.28 %	0.17 %
Green and Krasno (1988)	TSLS	U.S. House	1978	2.20 %	6.07 %	3.67 %
Thomas (1989)	OLS	U.S. House	1978–1980	1.41 %	3.89 %	2.34 %
Abramowitz (1991)	OLS	U.S. House	1984–1986	0.42 %	1.16 %	0.70 %
Levitt (1994)	Repeat chal.	U.S. House	1972–1990	0.10 %	0.28 %	0.17 %
Gius (2010)	OLS	U.S. House	2006	0.46 %	1.27 %	0.76 %
Magee (2012)	Repeat. chal.	U.S. House	1980 – 2006	0.13%	0.37 %	0.20 %
Magee (2012)	TSLS	U.S. House	1980 – 2006	0.23%	0.63 %	0.38 %
Gierzynski and Breaux (1991)	OLS	State Legisl.	1986	5.00 %	1.41 %	1.00 %
Stratmann (2006)	TSLS	State Legisl.	1996–2000	2.67 %	0.76 %	0.54 %

NOTE: The point estimates indicate the estimated (linear) increase in the percentage points of votes through a $ 100.000 increase (1990 Dollars). For ease of comparison across years, all point estimates from the literature are a adjusted to 1990 Dollars. The total impact is calculated by multiplying the point estimates from the literature with 2.75 or 0.29 for U.S. House and State legislatures, respectively (the effect of incumbency on total campaign contributions (1990 Dollars). Likewise the investor impact is calculated by multiplying the point estimates with 1.65 and 0.20 for U.S. House and State legislatures, respectively (the effect of incumbency on campaign contributions from investors (1990 Dollars)) Calculations are available from the authors upon request.

A.2 Robustness Checks

The consistency of the RDD estimates obtained in this paper rests on the crucial assumption that all other factors are continuous around the cutoff. Recently, Caughey and Sekhon (2011) have questioned whether assignment to treatment is random in close U.S. House elections. To support the claim that our RDD estimates are not biased by sorting at the cutoff, we conduct a number of robustness checks.

First, we reestimate Equation 3.3 but include additional covariates. In Table A.3, we include state, year and state-year fixed effects, respectively. In Table A.4, we include state and year fixed effects and the Democratic Share of contributions in election t and a dummy variable indicating whether an observation pertains to an upper or lower House. Both tables indicate that the results are robust to including additional control variables.

Next, in Table A.5 we show that the results are robust to the exclusion of observations near the discontinuity (the so-called "donut" RDD, see for example Barreca, Lindo, and Waddell (2011)). This indicates that sorting around the cutoff is not biasing our results.

Table A.6 replicates the results from the home state vs. out-of-state comparisons from the paper but uses fixed effects and the donut specification, respectively. It appears that the home state vs. out-of-state analysis is robust to the different specifications.

While the sorting problem is well-studied in the U.S. House, it does not appear in the state legislatures. Table A.7 presents the results of estimating our main equation on the *lagged* Democratic percentage of all contributions. If "better" candidates are systematically able to sort across the discontinuity, we should find large imbalances in lagged money share – indeed, such imbalances, if they are to bias our estimates, should be the same order of magnitude as our estimated effects. As the table shows, we find very small differences between winners and losers, and we cannot reject the null hypothesis that they receive the same share of all contributions.

Figure A.1 presents the same lagged estimate across all bandwidths from 0-10 percentage points. Again, we can see that the difference between winners and losers in pre-treatment contributions is negligible.

Table A.3 – Robustness Check: State, Year, and State-Year Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)
Democrat Win (time t)	22.71*	24.85*	17.28*	20.10*	20.11*	20.07*
	(3.56)	(3.55)	(6.69)	(1.59)	(1.59)	(1.63)
Constant	37.15*	36.14*	41.41*	39.25*	39.25*	39.15*
	(2.31)	(2.25)	(4.02)	(1.12)	(1.12)	(1.15)
Observations	568	568	568	4020	4020	4020
Level	State	State	State	State	State	State
Fixed Effects	State	Year	State-Year	State	Year	State-Year

NOTE: Robust standard errors in parentheses. All models are estimated using OLS. The outcome variable is the Democratic party's share of total contributions in election t+1 (percentage points). All models are estimated based on a bandwidth of 5. * $p < 0.05$.

Table A.4 – Robustness Check: Including Additional Covariates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democrat Win (time t)	20.62*	17.95*	24.94*	23.66*	22.37*	21.67*	20.04*	19.93*
	(4.46)	(5.38)	(3.50)	(3.90)	(2.06)	(2.04)	(1.60)	(1.58)
Constant	37.88*	33.27*	35.71*	38.78*	37.68*	23.52*	39.20*	23.77*
	(2.97)	(5.42)	(2.24)	(12.80)	(1.48)	(5.12)	(1.12)	(4.10)
Observations	329	291	568	501	2421	2421	4020	4020
Level	State	State	State	State	State	State	State	State
Bandwidth Pct.	3	3	5	5	3	3	5	5
Controls		✓		✓		✓		✓

NOTE: Robust standard errors in parentheses. All models are estimated using OLS. The outcome variable is the Democratic party's share of total contributions in election t+1 (percentage points). The following control variables are included: The Democratic Party's share of contributions in election t, year dummies, state dummies and a chamber dummy (for the state legislatures). * $p < 0.05$.

Table A.5 – Robustness Check: Donut Test

	(1)	(2)	(3)	(4)	(5)	(6)
Democrat Win (time t)	25.14*	24.97*	28.28*	22.90*	22.09*	19.39*
	(9.92)	(5.41)	(3.92)	(6.23)	(2.42)	(1.77)
Constant	32.19*	36.44*	34.51*	35.72*	36.88*	39.09*
	(7.00)	(3.76)	(2.55)	(4.49)	(1.71)	(1.22)
Observations	76	297	536	617	2223	3822
Level	State	State	State	State	State	State
Bandwidth Pct.	1	3	5	1	3	5

NOTE: Robust standard errors in parentheses. All models are estimated using OLS. The outcome variable is the Democratic party's share of total contributions in election t+1 (percentage points). In all models, observations in the interval [-0.25 ,0.25] of the vote share winning margin are dropped. ** $p < 0.05$.

Table A.6 – Robustness Check for Home-state and Out-of-state analysis in U.S. House: Fixed Effects and Donut test

	(1)	(2)	(3)	(4)	(5)	(6)
Democrat Win (time t)	32.61*	34.15*	32.42*	0.76	27.83*	41.08*
	(5.15)	(5.04)	(6.65)	(20.56)	(7.91)	(5.59)
Home State Contribution × Democrat Win (time t)	6.04	6.82	5.33	10.74	8.22	6.39
	(4.13)	(4.18)	(3.73)	(11.83)	(5.66)	(4.31)
Home State Contribution	-9.28*	-9.37*	-8.56*	-10.05	-8.45*	-9.39*
	(2.75)	(2.93)	(2.44)	(6.94)	(3.76)	(3.00)
Constant	33.49*	32.34*	36.18*	26.36*	34.98*	27.76*
	(3.21)	(3.22)	(4.45)	(8.01)	(4.32)	(3.38)
Observations	713	713	713	108	366	676
Bandwidth Pct.	5	5	5	1	3	5
Fixed Effects	State	Year	State-Year			
Donut				✓	✓	✓

NOTE: Robust standard errors in parentheses. All models are estimated using OLS. The outcome variable is the Democratic party's share of total contributions in election t+1 (percentage points) * $p < 0.05$.

Table A.7 – State Legislatures: Testing for Sorting

	Dependent Variable: Lagged Dem share of contributions.		
	(1)	(2)	(3)
Democrat Win (time t)	3.569	2.147	2.064
	(2.815)	(1.582)	(1.218)
Constant	49.40*	49.53*	49.49*
	(1.984)	(1.123)	(0.862)
Level	State	State	State
Bandwidth Pct.	1	3	5
N	983	2909	4857

NOTE: Robust standard errors in parentheses. All models are estimated using OLS. * $p < 0.05$.

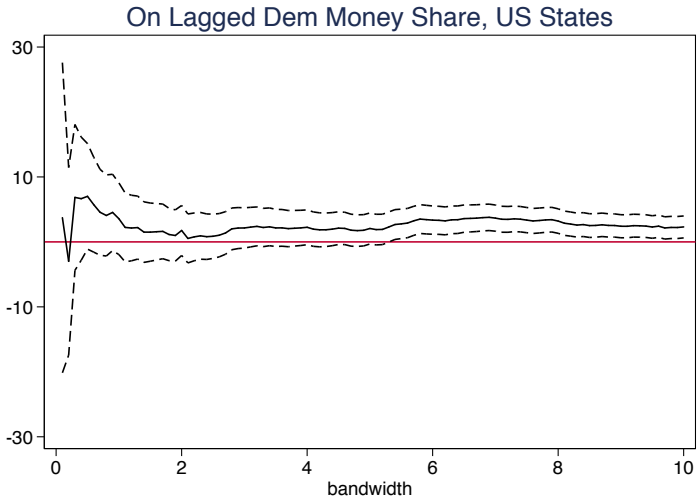


Figure A.1 – Testing for sorting using the lagged outcome variable. No evidence is found.

Finally, it is important to note that the key findings in the paper, such as the contrast in effect size across donor types, depend upon comparing *across* RDD estimates. Any fixed bias from sorting in the RDD is therefore washed out. We can also test for sorting across donor groups. Table A.8 runs these tests with our usual specifications, using the lagged percentage of money from each of the 14 types of donors identified in state legislatures. Remarkably, we reject the null hypothesis in only 2 out of the 42 tests (4.76% of the time), almost exactly the 5% rate that would be generated under the overall null of no sorting. This is not the result of using underpowered specifications, since these are the exact specifications we use to uncover the large effects documented in the paper.

Table A.8 – Testing for Sorting Across Donor Groups: State Legislatures

	Bandwidth		
Donor Group	1%	3%	5%
Agriculture	4.27	6.60	4.39
	(5.97)	(3.40)	(2.61)
Communications & Electronics	5.87	0.51	1.47
	(6.09)	(3.46)	(2.64)
Construction	8.88	3.97	1.02
	(5.15)	(2.93)	(2.25)
Defense	16.76	2.68	4.37
	(24.23)	(13.94)	(10.87)
Energy & Natural Resources	-4.75	0.86	2.59
	(5.71)	(3.25)	(2.48)
Finance, Insurance & Real Estate	-2.34	0.41	0.44
	(4.82)	(2.70)	(2.06)
General Business	1.71	2.93	3.58
	(4.77)	(2.71)	(2.09)
Government Agencies/Education/Other	10.80	2.61	1.98
	(5.01)	(2.84)	(2.18)
Health	-0.06	1.73	3.03
	(5.20)	(2.92)	(2.23)
Ideology/Single Issue	6.97	0.07	1.05
	(6.37)	(3.62)	(2.77)
Labor	1.60	2.14	2.19
	(3.16)	(1.78)	(1.40)
Lawyers & Lobbyists	3.44	3.54	4.14
	(4.31)	(2.49)	(1.95)
Party	5.65	3.34	3.12
	(4.10)	(2.33)	(1.82)
Transportation	-1.83	3.98	3.59
	(5.92)	(3.34)	(2.58)

Each cell is RDD estimate on lagged percentage of contributions from given donor group, using local linear regression estimated separately on each side of the discontinuity with the given bandwidth size.
Only 2 out of the 42 (4.7%) tests reject the null of no sorting; almost precisely the rate of false positives predicted under the null (5%). No evidence of sorting is found.

Separate from the sorting concern, we also need to verify that our results are not driven by the choice of bandwidth and specification. Figure A.2 presents estimated results for β_1 from Equation 3.3 across the full range of bandwidths and four polynomial

specifications of the running variable. Estimates are stable across these choices and never become small or negative.

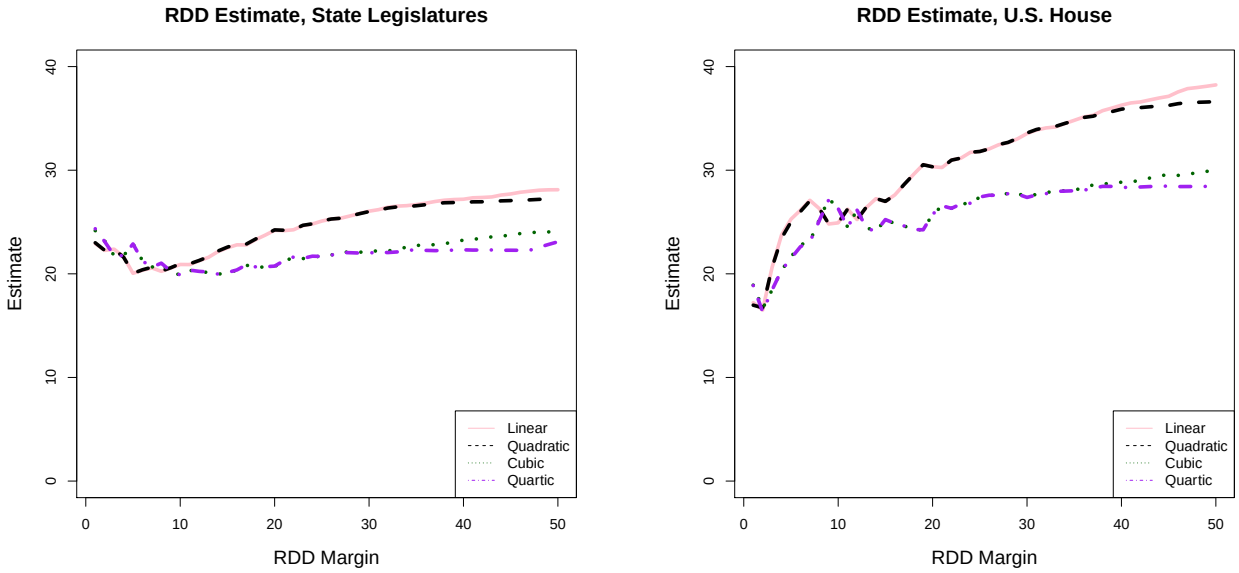


Figure A.2 – Stability of the RDD estimates across bandwidths and specification.

Appendix B

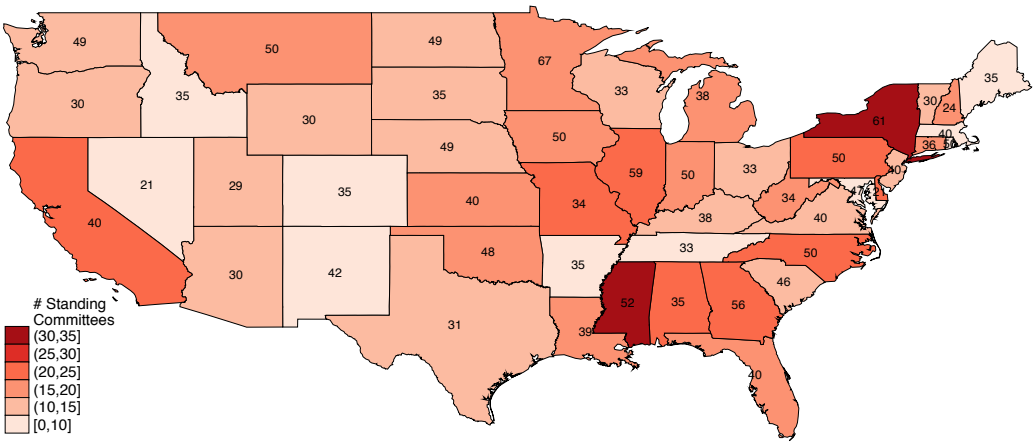
Appendix: Who Values Agenda Setters in American Legislatures?

B.1 Additional Summary Statistics

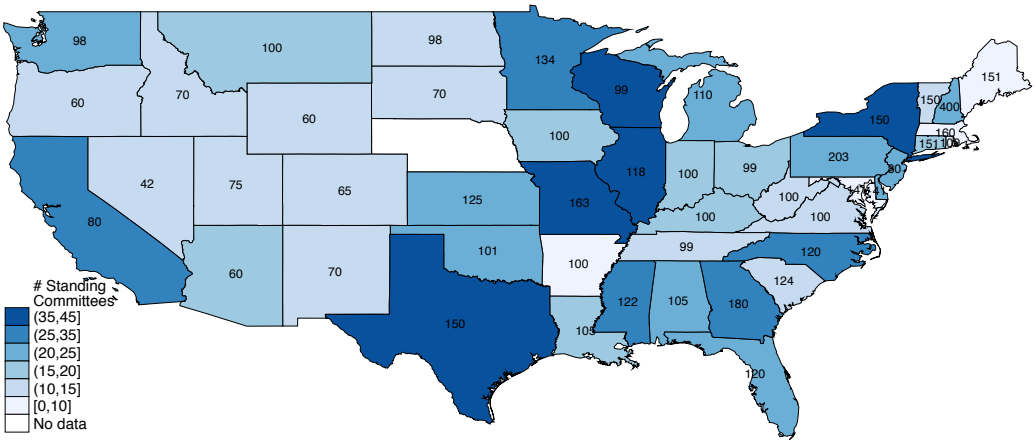
B.1.1 Number of Standing Committees across Legislative Chambers

Figure B.1 – Number of Standing Committees across States.

(a) State Senates



(b) State Houses



NOTE: The maps in panels (a) and (b) pertain to the state senates and houses, respectively. Darker colors indicate a higher number of standing legislative committees (the color codes refer to the median number of committees in a given chamber during the period 1990-2010). The reported number in black indicate the median number of legislators in the chamber during the period 1990-2010.

Table B.1 – # observations by state and chamber.

	House		Senate			House		Senate	
State	Period	N	Period	N	State	Period	N	Period	N
AK	1990-2010	400	1990-2010	117	AL	1998-2010	419	1998-2010	140
AR	2000-2010	500	2000-2010	104	AZ	1996-2010	420	1996-2010	210
CA	1998-2010	480	1998-2010	120	CO	1996-2010	455	1996-2010	122
CT	1998-2010	906	1996-2010	252	DE	2000-2010	205	2000-2010	64
FL	1998-2010	720	1998-2010	140	GA	1996-2010	1260	1992-2010	504
HI	1998-2010	306	1998-2010	85	IA	1998-2010	599	1998-2010	160
ID	1990-2010	714	1990-2010	357	IL	1996-2010	826	1996-2010	236
IN	1994-2010	800	1994-2010	200	KS	1996-2010	875	1996-2004	120
KY	1994-2010	800	1994-2010	152	LA	1999-2007	251	1999-2007	97
MA	1998-2010	960	1998-2010	240	MD	1998-2010	564	1998-2010	188
ME	1996-2010	1057	1996-2010	245	MI	1996-2010	770	1998-2010	152
MN	1996-2010	938	1996-2010	335	MO	1996-2010	1141	1996-2010	119
MS	1999-2007	366	1999-2007	156	MT	1990-2010	1000	1990-2010	252
NC	1996-2010	840	1996-2010	350	ND	1998-2010	336	1998-2010	144
NE	.- .	.	2000-2010	122	NH	1998-2010	2000	1996-2010	168
NJ	1997-2009	560	1997-2007	160	NM	1992-2010	629	1992-2004	168
NV	1990-2010	420	1990-2010	106	NY	1998-2010	900	1998-2010	370
OH	1996-2010	693	1996-2010	116	OK	2000-2010	505	2000-2010	120
OR	1990-2010	600	1990-2010	150	PA	1998-2010	1218	1998-2010	150
RI	1994-2010	700	1994-2010	348	SC	1996-2010	867	1996-2004	138
SD	2000-2010	350	2000-2010	175	TN	1996-2010	693	1996-2010	116
TX	1998-2010	900	1998-2010	109	UT	1990-2010	600	1990-2010	116
VA	1999-2009	600	1999-2007	120	VT	1998-2010	900	1996-2010	210
WA	1990-2010	980	1990-2010	244	WI	1998-2010	594	1998-2010	100
WV	1998-2010	600	1998-2010	102	WY	1990-2010	604	1990-2010	165

B.1.2 Agenda Setters across State Legislatures

B.2 Robustness Checks

B.2.1 District-fixed Effects

B.2.2 Alternative outcome: Legislators' Percent of total donations

Over the studied period, some states have imposed limits on campaign contributions from firms, and this may impose upper bounds on the estimated effects when the outcome is measured in levels (or the logarithm thereof). To ensure that the results are not sensitive to this choice, I run a model with an alternative outcome variable, *Pct. of Total Industry Contributions*$_{it}$, which is calculated as legislator i's percent of total industry donations in her chamber during session t. These results are presented in Table B.4. The estimates indicate that the basic findings are robust to the alternative definitions of the outcome variable.

B.4

Table B.2 – Share of Legislators with Leadership and Committee Chair Positions by state and chamber.

	House		Senate			House		Senate	
State	Leader	Chair	Leader	Chair	State	Leader	Chair	Leader	Chair
AK	0.10	0.42	0.18	0.57	AL	0.03	0.25	0.08	0.66
AR	0.07	0.11	0.13	0.33	AZ	0.10	0.30	0.20	0.42
CA	0.12	0.37	0.12	0.71	CO	0.11	0.17	0.22	0.40
CT	0.11	0.16	0.40	0.58	DE	0.10	0.44	0.24	0.64
FL	0.06	0.28	0.23	0.59	GA	0.04	0.18	0.11	0.44
HI	0.30	0.35	0.40	0.62	IA	0.12	0.19	0.31	0.49
ID	0.06	0.19	0.13	0.29	IL	0.12	0.31	0.25	0.36
IN	0.11	0.21	0.21	0.39	KS	0.05	0.19	0.19	0.43
KY	0.05	0.18	0.19	0.43	LA	0.02	0.20	0.07	0.59
MA	0.04	0.09	0.18	0.29	MD	0.08	0.09	0.22	0.23
ME	0.03	0.06	0.13	0.23	MI	0.09	0.25	0.36	0.40
MN	0.07	0.18	0.14	0.27	MO	0.04	0.26	0.18	0.54
MS	0.02	0.32	0.02	0.65	MT	0.07	0.14	0.16	0.40
NC	0.06	0.40	0.12	0.53	ND	0.05	0.14	0.14	0.32
NE	.	.	0.03	0.47	NH	0.02	0.05	0.39	0.62
NJ	0.15	0.25	0.31	0.43	NM	0.07	0.22	0.12	0.26
NV	0.22	0.26	0.36	0.47	NY	0.09	0.24	0.20	0.51
OH	0.09	0.21	0.27	0.45	OK	0.18	0.24	0.23	0.50
OR	0.16	0.27	0.33	0.49	PA	0.02	0.21	0.11	0.78
RI	0.10	0.09	0.21	0.19	SC	0.03	0.10	0.03	0.32
SD	0.15	0.18	0.22	0.35	TN	0.10	0.14	0.13	0.43
TX	0.01	0.24	0.05	0.47	UT	0.08	0.19	0.28	0.39
VA	0.03	0.20	0.12	0.31	VT	0.03	0.10	0.14	0.39
WA	0.13	0.23	0.27	0.44	WI	0.05	0.37	0.21	0.58
WV	0.07	0.24	0.19	0.56	WY	0.09	0.19	0.21	0.45

Table B.3 – Robustness to the Use of District-Fixed Effects. The results are not sensitive to using districts as the unit of analysis instead of legislators. This suggest that retirement decisions do not induce any notable bias in the estimates reported in the paper.

	log(1+Industry Contributions$_{it}$)				
	(1)	(2)	(3)	(4)	(5)
Leader	0.31**	0.35**	0.35**	0.35**	0.35**
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Chair	0.14**	0.18**	0.19**	0.17**	0.19**
	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)
Chair of Top Committees				0.09	
				(0.07)	
Chair of Other Committees					
Vice			0.06†	0.06†	0.06†
			(0.03)	(0.03)	(0.03)
Majority Member		0.11**	0.10**	0.10**	0.10**
		(0.03)	(0.03)	(0.03)	(0.03)
Observations	29538	28740	28740	28740	28740
Legislators	11189	10979	10979	10979	10979
District-fixed Effects	✓	✓	✓	✓	✓
State-Year Fixed Effects	✓	✓	✓	✓	✓
Seniority Fixed Effects	✓	✓	✓	✓	✓

NOTE: All models are estimated using OLS. Standard errors are clustered on legislators and are reported in parentheses.

Table B.4 – Alternative Outcome Variable. The basic results are the same when the outcome variable is measured as percent of total contributions instead of logged contributions.

	Pct. of Industry Contributions$_{it}$			
	(1)	(2)	(3)	(4)
Leader	0.77**	0.82**	0.83**	0.83**
	(0.11)	(0.11)	(0.11)	(0.11)
Chair	0.38**	0.42**	0.45**	0.35**
	(0.06)	(0.06)	(0.06)	(0.07)
Vice			0.20**	0.20**
			(0.05)	(0.05)
Chair of Top Committees				0.57**
				(0.13)
Majority Member		0.19**	0.18**	0.18**
		(0.05)	(0.05)	(0.05)
Observations	31397	31089	31089	31089
Legislators	9170	9097	9097	9097
Legislator Fixed Effects	✓	✓	✓	✓
State-Year Fixed Effects	✓	✓	✓	✓
Seniority Fixed Effects		✓	✓	✓

NOTE: All models are estimated using OLS. Standard errors are clustered on legislators and are reported in parentheses.

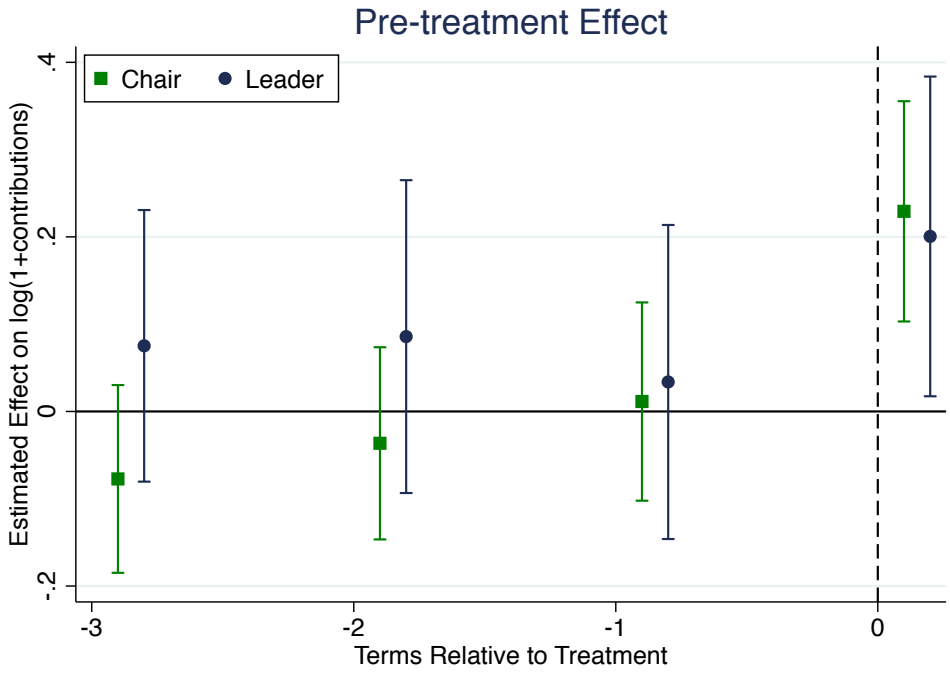
B.2.3 Effect on Pre-treatment Outcomes

To test whether the committee and party leadership treatments assigned at time t affect the allocation of donations in the pre-treatment sessions, I estimate the following models using OLS:

$$log(1+contributions_{it}) = \alpha_i + \delta_t + \sum_{\tau=0}^{3}[\beta_{1,\tau} chair_{i,t+\tau} + \beta_{2,\tau} leader_{i,t+\tau}] + \mathbf{x}_{it}\theta + \varepsilon_{it}, \quad \text{(B.1)}$$

The results are reported in Figure B.2. The estimates indicate that committee and party leader positions assigned at time t do not have a statistically significant impact on donations to the legislator in pre-treatment sessions.

Figure B.2 – Effect on Pre-treatment Donations. The effect on pre-treatment donations is statistically indistinguishable from zero.



Appendix C

Appendix: What Are the Electoral Consequences of Campaign Spending Limits?

C.1 Additional Results

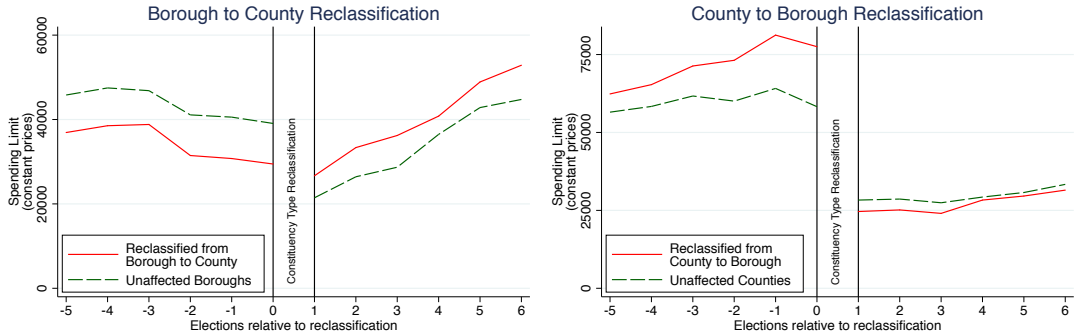
C.1.1 Additional Results: First-Stage Models

Table C.1 – First-Stage Results with Logged Covariates

	$log(Spending\ Limit_{it})$			$Spending\ Limit_{it}$ (£1000)		
$County_{it}$	0.884***	0.640***	0.688***	82.166***	58.913***	61.382***
	(0.025)	(0.004)	(0.006)	(2.267)	(1.174)	(1.445)
$County_{it} \cdot 1[t \geq 1918]_t$	-0.476***	-0.316***	-0.326***	-59.142***	-47.853***	-50.952***
	(0.019)	(0.002)	(0.005)	(1.301)	(0.724)	(1.027)
$County_{it} \cdot 1[t \geq 1929]_t$	-0.039**	-0.153***	-0.149***	2.382*	-2.263***	-1.908***
	(0.013)	(0.000)	(0.003)	(0.938)	(0.240)	(0.357)
$County_{it} \cdot 1[t \geq 1949]_t$	-0.092***	-0.045***	-0.057***	-10.088***	-7.939***	-7.997***
	(0.017)	(0.001)	(0.004)	(1.120)	(0.297)	(0.402)
$County_{it} \cdot 1[t \geq 1970]_t$	0.043***	-0.024***	-0.026***	0.513***	-0.373***	-0.442***
	(0.003)	(0.001)	(0.001)	(0.083)	(0.103)	(0.106)
$County_{it} \cdot 1[t \geq 1974]_t$	-0.035***	-0.011***	-0.013***	-0.781**	-0.087	-0.439*
	(0.006)	(0.001)	(0.001)	(0.254)	(0.113)	(0.193)
$County_{it} \cdot 1[t \geq 1978]_t$	0.047***	0.017***	0.016***	0.353***	0.075**	0.101
	(0.002)	(0.001)	(0.000)	(0.029)	(0.028)	(0.077)
$County_{it} \cdot 1[t \geq 1982]_t$	-0.058***	0.002*	0.001	-1.898***	-0.282	-0.280
	(0.008)	(0.001)	(0.001)	(0.376)	(0.173)	(0.190)
$County_{it} \cdot 1[t \geq 1987]_t$	0.004***	-0.009***	-0.010***	0.111***	-0.041	0.020
	(0.001)	(0.000)	(0.000)	(0.026)	(0.028)	(0.035)
$County_{it} \cdot 1[t \geq 1992]_t$	0.017***	0.003***	0.002***	0.127***	-0.002	-0.014
	(0.001)	(0.000)	(0.000)	(0.036)	(0.028)	(0.028)
$County_{it} \cdot 1[t \geq 1997]_t$	-0.018***	0.008***	0.003***	-0.393*	0.188*	0.024
	(0.004)	(0.001)	(0.001)	(0.181)	(0.088)	(0.086)
Observations	18478	18478	16315	18478	18478	16315
Constituencies	1636	1636	1466	1636	1636	1466
Joint instrument sign.	0.000	0.000	0.000	0.000	0.000	0.000
Constituency FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
$\theta_t log(electors_{it})$		✓	✓		✓	✓
$\gamma_t log(density_{it})$			✓			✓

NOTE: All models were estimated using OLS. Robust standard errors are clustered on constituencies and are reported in (parentheses). Significance levels: † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure C.1 – Constant Prices: Effect of Constituency Reclassification on Spending Limits. Constituency reclassifications from borough to county cause an increase in the spending limit, whereas reclassifications from county to borough lead to a drop in spending limits.



NOTE: The x-axis represents years relative to the reclassifications. The y-axis represents the spending limits. In the plot on the left, the treatment-group line reports the average limit faced by constituencies classified as boroughs in the pre-treatment period and counties in the post-treatment period. The control group is all unaffected boroughs. Conversely, in the panel on the right, the treatment-group line reports the average limit faced by constituencies classified as counties in the pre-treatment period and boroughs in the post-treatment period. The control group in this panel consists of all unaffected counties.

Figure C.2 – Constant Prices: Reforms of Spending Limit Formula. Reforms of the spending limit formula affect county and borough constituencies differently.

NOTE: The x-axis represents years relative to the spending limit reforms. The y-axis represents spending limits. The solid and dashed lines show the average spending limit in county and borough constituencies, respectively.

C.1.2 Additional Results: Candidate Supply and Electoral Competition

Table C.2 – Linear-Linear models with Linear Controls

	Policy Change IV				Reclassification IV			Combined IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Outcome: *Unopposed Candidate*$_{it}$										
Spending Limit$_{it}$	0.01**	0.02***	0.02***	0.03***	0.00†	0.01***	0.01**	0.01**	0.02***	0.01***	0.01***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
	Outcome: *More Than Two Candidates*$_{it}$										
Spending Limit$_{it}$	-0.00	-0.02***	-0.02***	-0.02	0.00	-0.01*	-0.01*	-0.00	-0.02***	-0.02**	-0.01**
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
	Outcome: *Effective Candidates*$_{it}$										
Spending Limit$_{it}$	-0.00**	-0.01***	-0.01***	-0.02***	-0.00	-0.01***	-0.01***	-0.00*	-0.01***	-0.01***	-0.01***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
	Outcome: *Top Two Vtsh*$_{it}$										
Spending Limit$_{it}$	0.06	0.46***	0.42***	0.65*	-0.08	0.19†	0.18	0.01	0.41***	0.36***	0.30**
	(0.06)	(0.10)	(0.11)	(0.30)	(0.06)	(0.11)	(0.12)	(0.06)	(0.10)	(0.10)	(0.12)
Observations	17647	17647	15761	17647	17647	17647	15761	17647	17647	15761	17647
	Outcome: *Win Margin*$_{it}$										
Spending Limit$_{it}$	0.14	0.38†	0.42†	0.80	0.23*	0.65**	0.59*	0.20†	0.45*	0.48*	0.64*
	(0.13)	(0.22)	(0.22)	(0.58)	(0.11)	(0.25)	(0.26)	(0.11)	(0.21)	(0.22)	(0.25)
Observations	17647	17647	15761	17647	17647	17647	15761	17647	17647	15761	17647
Constituency FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$\theta_t electors_{it}$		✓	✓	✓		✓	✓		✓	✓	✓
$\gamma_t density_{it}$			✓				✓			✓	
Excluded Instruments:											
$county_{it}$					✓	✓	✓	✓	✓	✓	✓
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega$	✓	✓	✓					✓	✓	✓	
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega > 1918$				✓							✓

NOTE: All models were estimated using 2SLS. Standard errors are clustered on constituencies and are reported in (parentheses). Significance levels: † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C.3 – Linear-Linear models with Logged Controls

	Policy Change IV				Reclassification IV			Combined IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Outcome: *Unopposed Candidate*$_{it}$										
Spending Limit$_{it}$	0.01***	0.02***	0.02***	0.05***	0.01***	0.01***	0.01**	0.01***	0.02***	0.01***	0.01***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
	Outcome: *More Than Two Candidates*$_{it}$										
Spending Limit$_{it}$	-0.01***	-0.02***	-0.02**	-0.05†	-0.01	-0.01*	-0.01*	-0.01**	-0.02***	-0.02***	-0.02**
	(0.00)	(0.00)	(0.01)	(0.03)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
	Outcome: *Effective Candidates*$_{it}$										
Spending Limit$_{it}$	-0.01***	-0.01***	-0.01***	-0.05***	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
	Outcome: *Top Two Vtsh*$_{it}$										
Spending Limit$_{it}$	0.33***	0.52***	0.33**	2.31***	0.07	0.25†	0.24†	0.18*	0.48***	0.42***	0.39**
	(0.09)	(0.11)	(0.12)	(0.67)	(0.08)	(0.13)	(0.14)	(0.08)	(0.10)	(0.11)	(0.13)
Observations	17647	17647	15761	17647	17647	17647	15761	17647	17647	15761	17647
	Outcome: *Win Margin*$_{it}$										
Spending Limit$_{it}$	0.30	0.39†	0.52*	1.95	0.43**	0.73*	0.64*	0.36*	0.45†	0.47*	0.71*
	(0.19)	(0.23)	(0.25)	(1.22)	(0.15)	(0.29)	(0.31)	(0.15)	(0.23)	(0.24)	(0.30)
Observations	17647	17647	15761	17647	17647	17647	15761	17647	17647	15761	17647
Constituency FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$\theta_t log(electors_{it})$		✓	✓	✓		✓	✓		✓	✓	✓
$\gamma_t log(density_{it})$			✓				✓			✓	
Excluded Instruments:											
$county_{it}$					✓	✓	✓	✓	✓	✓	✓
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega$	✓	✓	✓					✓	✓	✓	
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega > 1918$				✓							✓

Note: All models were estimated using 2SLS. Standard errors are clustered on constituencies and are reported in (parentheses). Significance levels: † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

C.1.3 Additional Results: Effect on Different Types of Spending

Table C.4 – Linear-Linear models with Linear Controls

	Policy Change IV				Reclassification IV			Combined IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Outcome: *Manager Differential*$_{it}$										
Spending Limit$_{it}$	0.32***	0.52***	0.49***	0.10*	0.26***	0.43***	0.41***	0.30***	0.50***	0.48***	0.44***
	(0.02)	(0.03)	(0.03)	(0.05)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
	Outcome: *Printing Differential*$_{it}$										
Spending Limit$_{it}$	0.37***	0.15***	0.16***	0.13	0.42***	0.21***	0.21***	0.39***	0.17***	0.18***	0.20***
	(0.04)	(0.03)	(0.04)	(0.12)	(0.04)	(0.04)	(0.05)	(0.04)	(0.03)	(0.04)	(0.04)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
	Outcome: *Personal Differential*$_{it}$										
Spending Limit$_{it}$	0.01**	0.03***	0.03**	0.01	0.00	0.01	0.01	0.01	0.03**	0.02*	0.01
	(0.00)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
	Outcome: *Room Differential*$_{it}$										
Spending Limit$_{it}$	0.04***	0.01	0.01	0.02	0.04***	0.01†	0.01	0.04***	0.01	0.01†	0.01†
	(0.01)	(0.00)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
	Outcome: *Meetings Differential*$_{it}$										
Spending Limit$_{it}$	0.02***	0.01**	0.01**	0.05***	0.03***	0.02***	0.02***	0.02***	0.01**	0.02***	0.02***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
	Outcome: *Staff Differential*$_{it}$										
Spending Limit$_{it}$	0.14***	0.02†	0.03*	0.15	0.16***	0.04**	0.05**	0.14***	0.03*	0.04*	0.04**
	(0.03)	(0.01)	(0.02)	(0.10)	(0.03)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.01)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
	Outcome: *Misc. Differential*$_{it}$										
Spending Limit$_{it}$	0.07***	0.07***	0.07***	0.02	0.08***	0.08***	0.08***	0.08***	0.08***	0.07***	0.08***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
Constituency FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$\theta_t electors_{it}$		✓	✓	✓		✓	✓		✓	✓	✓
$\gamma_t density_{it}$			✓				✓			✓	
Excluded Instruments:											
$county_{it}$					✓	✓	✓	✓	✓	✓	✓
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega$	✓	✓	✓					✓	✓	✓	
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega > 1918$				✓							✓

NOTE: All models were estimated using 2SLS. Standard errors are clustered on constituencies and are reported in (parentheses). Significance levels: † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C.5 – Linear-Linear models with Logged Controls

	Policy Change IV				Reclassification IV			Combined IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Outcome: *Manager Differential*$_{it}$										
Spending Limit$_{it}$	0.50***	0.57***	0.53***	0.56***	0.38***	0.50***	0.51***	0.42***	0.55***	0.56***	0.50***
	(0.03)	(0.03)	(0.03)	(0.05)	(0.02)	(0.03)	(0.04)	(0.02)	(0.03)	(0.03)	(0.03)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
	Outcome: *Printing Differential*$_{it}$										
Spending Limit$_{it}$	0.23***	0.13***	0.21***	-0.13	0.33***	0.16***	0.22***	0.29***	0.13***	0.20***	0.15***
	(0.03)	(0.04)	(0.04)	(0.11)	(0.03)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.04)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
	Outcome: *Personal Differential*$_{it}$										
Spending Limit$_{it}$	0.02**	0.03***	0.02*	-0.01	0.00	0.02	0.02	0.01	0.03**	0.03**	0.02
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
	Outcome: *Room Differential*$_{it}$										
Spending Limit$_{it}$	0.02***	0.00	0.01*	0.01	0.03***	0.00	0.01	0.02***	0.00	0.01*	0.00
	(0.00)	(0.01)	(0.01)	(0.02)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
	Outcome: *Meetings Differential*$_{it}$										
Spending Limit$_{it}$	0.01***	0.01	0.02**	0.09***	0.02***	0.01*	0.02**	0.02***	0.01†	0.01**	0.01*
	(0.00)	(0.00)	(0.00)	(0.02)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
	Outcome: *Staff Differential*$_{it}$										
Spending Limit$_{it}$	0.06***	0.01	0.05**	0.01	0.11***	0.02	0.04*	0.09***	0.01	0.04**	0.02
	(0.01)	(0.01)	(0.02)	(0.05)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
	Outcome: *Misc. Differential*$_{it}$										
Spending Limit$_{it}$	0.08***	0.08***	0.08***	0.17***	0.09***	0.09***	0.09***	0.09***	0.08***	0.09***	0.09***
	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
Constituency FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$\theta_t log(electors_{it})$		✓	✓	✓		✓	✓		✓	✓	✓
$\gamma_t log(density_{it})$			✓				✓			✓	
Excluded Instruments:											
$county_{it}$					✓	✓	✓	✓	✓	✓	✓
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega$	✓	✓	✓					✓	✓	✓	
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega > 1918$				✓							✓

Note: All models were estimated using 2SLS. Standard errors are clustered on constituencies and are reported in (parentheses). Significance levels: † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C.6 – Linear-Log models with Logged Controls

	Policy Change IV				Reclassification IV			Combined IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Outcome: *Manager Differential*$_{it}$										
$log(Spending\ Limit_{it})$	56443.04***	60554.04***	58344.33***	30941.44***	35324.14***	45714.74***	44962.88***	38461.57***	52537.17***	48122.21***	43659.29***
	(2954.06)	(2810.93)	(3106.48)	(2537.62)	(1948.91)	(2773.72)	(3293.78)	(2439.85)	(2762.23)	(3166.20)	(2491.04)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
	Outcome: *Printing Differential*$_{it}$										
$log(Spending\ Limit_{it})$	25301.02***	13520.48***	22168.49***	-2938.84	30740.70***	14998.67***	21496.77***	29780.21***	13813.54***	20408.96***	13279.45***
	(3450.20)	(3837.68)	(4590.13)	(5736.87)	(3056.43)	(4019.09)	(4879.19)	(3034.90)	(3578.25)	(4371.65)	(3713.62)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
	Outcome: *Personal Differential*$_{it}$										
$log(Spending\ Limit_{it})$	2512.97**	3443.97***	2609.36*	258.46	171.76	1421.91	732.15	531.47	2427.84**	1274.43	1330.37
	(827.44)	(987.11)	(1222.91)	(1095.00)	(687.36)	(1016.08)	(1220.60)	(699.02)	(937.37)	(1136.57)	(923.65)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
	Outcome: *Room Differential*$_{it}$										
$log(Spending\ Limit_{it})$	2014.92***	556.38	1298.98*	1015.80	2661.82***	387.70	856.26	2570.44***	536.15	987.46†	539.89
	(477.69)	(535.18)	(599.10)	(871.25)	(457.28)	(553.13)	(646.13)	(445.10)	(496.82)	(571.50)	(505.47)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
	Outcome: *Meetings Differential*$_{it}$										
$log(Spending\ Limit_{it})$	1297.31***	977.71*	1865.55***	4368.32***	1822.78***	1074.61*	1535.42**	1688.23***	1072.06*	1705.55***	1325.44**
	(382.26)	(453.73)	(531.72)	(978.61)	(317.16)	(496.56)	(579.43)	(315.36)	(436.90)	(518.05)	(467.70)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
	Outcome: *Staff Differential*$_{it}$										
$log(Spending\ Limit_{it})$	6894.07***	1598.37	5238.29**	3194.60	9994.34***	1737.17	4580.17**	9461.17***	1825.29	4702.88**	2113.69
	(1407.49)	(1547.72)	(1656.15)	(2641.06)	(1462.12)	(1408.75)	(1667.56)	(1423.84)	(1359.19)	(1506.35)	(1304.99)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
	Outcome: *Misc. Differential*$_{it}$										
$log(Spending\ Limit_{it})$	9237.29***	9206.81***	9142.04***	11526.45***	8375.59***	8199.98***	8297.08***	8473.90***	8800.69***	8622.37***	8537.92***
	(833.84)	(998.78)	(1123.21)	(1339.21)	(590.51)	(993.30)	(1110.16)	(601.92)	(929.32)	(1032.58)	(903.73)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
Constituency FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$\theta_t log(electors_{it})$		✓	✓	✓		✓	✓		✓	✓	✓
$\gamma_t log(density_{it})$			✓				✓			✓	
Excluded Instruments:											
$county_{it}$					✓	✓	✓	✓	✓	✓	✓
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega$	✓	✓	✓					✓	✓	✓	
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega > 1918$				✓							✓

NOTE: All models were estimated using 2SLS. Standard errors are clustered on constituencies and are reported in (parentheses). Significance levels: † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

C.1.4 Additional Results: Effect across Parties

Table C.7 – Linear-Linear models with Linear Controls

	Policy Change IV				Reclassification IV			Combined IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Outcome: *Conservative Spending*$_{it}$										
Spending Limit$_{it}$	0.52***	0.43***	0.42***	0.15	0.53***	0.42***	0.41***	0.52***	0.44***	0.42***	0.42***
	(0.03)	(0.04)	(0.04)	(0.13)	(0.03)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.04)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
	Outcome: *Liberal (Dem.) Spending*$_{it}$										
Spending Limit$_{it}$	0.38***	0.39***	0.38***	0.32	0.38***	0.38***	0.37***	0.38***	0.39***	0.38***	0.38***
	(0.05)	(0.04)	(0.05)	(0.21)	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
	Outcome: *Labour Spending*$_{it}$										
Spending Limit$_{it}$	0.06***	0.01	0.02	-0.01	0.06***	0.02	0.03	0.05***	0.01	0.02	0.02
	(0.01)	(0.02)	(0.02)	(0.06)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
	Outcome: *Other Party Spending*$_{it}$										
Spending Limit$_{it}$	0.02	-0.02	-0.02	0.02	0.02	-0.03†	-0.03	0.02	-0.02	-0.02	-0.03
	(0.01)	(0.02)	(0.02)	(0.06)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Observations	18478	18478	16584	18478	18478	18478	16584	18478	18478	16584	18478
Constituency FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$\theta_t electors_{it}$		✓	✓	✓		✓	✓		✓	✓	✓
$\gamma_t density_{it}$			✓				✓			✓	
Excluded Instruments:											
$county_{it}$					✓	✓	✓	✓	✓	✓	✓
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega$	✓	✓	✓					✓	✓	✓	
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega > 1918$				✓							✓

NOTE: All models were estimated using 2SLS. Standard errors are clustered on constituencies and are reported in (parentheses). Significance levels: † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C.8 – Linear-Linear models with Logged Controls

	Policy Change IV				Reclassification IV			Combined IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Outcome: *Conservative Spending$_{it}$*										
Spending Limit$_{it}$	0.50***	0.45***	0.48***	0.30**	0.52***	0.44***	0.48***	0.51***	0.45***	0.50***	0.44***
	(0.03)	(0.04)	(0.04)	(0.11)	(0.03)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.04)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
	Outcome: *Liberal (Dem.) Spending$_{it}$*										
Spending Limit$_{it}$	0.41***	0.39***	0.41***	0.30*	0.40***	0.39***	0.44***	0.40***	0.39***	0.46***	0.38***
	(0.04)	(0.04)	(0.05)	(0.12)	(0.03)	(0.05)	(0.05)	(0.03)	(0.04)	(0.04)	(0.05)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
	Outcome: *Labour Spending$_{it}$*										
Spending Limit$_{it}$	0.00	-0.00	0.02	-0.04	0.02†	0.01	0.01	0.01	0.00	0.01	0.00
	(0.01)	(0.02)	(0.02)	(0.09)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
	Outcome: *Other Party Spending$_{it}$*										
Spending Limit$_{it}$	0.00	-0.02	-0.01	0.12†	0.01	-0.04*	-0.04†	0.01	-0.02	-0.02	-0.04†
	(0.02)	(0.02)	(0.03)	(0.06)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
Constituency FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$\theta_t log(electors_{it})$		✓	✓	✓		✓	✓		✓	✓	✓
$\gamma_t log(density_{it})$			✓				✓			✓	
Excluded Instruments:											
$county_{it}$					✓	✓	✓	✓	✓	✓	✓
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega$	✓	✓	✓					✓	✓	✓	
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega > 1918$				✓							✓

NOTE: All models were estimated using 2SLS. Standard errors are clustered on constituencies and are reported in (parentheses). Significance levels: † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C.9 – Linear-Log models with Logged Controls

	Policy Change IV				Reclassification IV			Combined IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Outcome: *Conservative Spending*$_{it}$										
log(Spending Limit$_{it}$*)*	56554.86***	48779.87***	52017.35***	21428.57***	48557.16***	40364.53***	43728.05***	49680.77***	44177.93***	44693.30***	38575.02***
	(3740.48)	(4029.46)	(4439.05)	(5893.52)	(3007.46)	(3968.75)	(4590.34)	(2998.91)	(3708.38)	(4172.58)	(3643.55)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
	Outcome: *Liberal (Dem.) Spending*$_{it}$										
log(Spending Limit$_{it}$*)*	46059.01***	41565.55***	44229.13***	14690.29*	37307.65***	35683.40***	37852.87***	38517.59***	37860.96***	38385.91***	33112.83***
	(4329.94)	(4655.69)	(5252.63)	(6812.87)	(3294.35)	(4500.01)	(5223.80)	(3319.37)	(4254.45)	(4812.80)	(4130.19)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
	Outcome: *Labour Spending*$_{it}$										
log(Spending Limit$_{it}$*)*	225.98	829.58	3190.80	4124.52	1862.29†	735.87	2472.26	1428.13	886.77	2822.15	1101.05
	(1523.56)	(1711.91)	(2394.95)	(4583.18)	(1128.13)	(1767.18)	(2308.99)	(1139.58)	(1609.25)	(2183.64)	(1686.82)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
	Outcome: *Other Party Spending*$_{it}$										
log(Spending Limit$_{it}$*)*	798.33	-1536.12	-400.50	6240.24†	640.83	-4094.29*	-3047.50	786.40	-2410.94	-1572.86	-2840.92
	(1825.11)	(2025.01)	(2736.62)	(3200.62)	(1090.89)	(1911.73)	(2417.57)	(1166.75)	(1817.40)	(2341.67)	(1753.79)
Observations	18478	18478	16315	18478	18478	18478	16315	18478	18478	16315	18478
Constituency FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$\theta_t log(electors_{it})$		✓	✓	✓		✓	✓		✓	✓	✓
$\gamma_t log(density_{it})$			✓				✓			✓	
Excluded Instruments:											
$county_{it}$					✓	✓	✓	✓	✓	✓	✓
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega$	✓	✓	✓					✓	✓	✓	
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega > 1918$				✓							✓

NOTE: All models were estimated using 2SLS. Standard errors are clustered on constituencies and are reported in (parentheses). Significance levels: † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

C.1.5 Additional Results: Incumbents-Challengers Differential

Table C.10 – Linear-Linear models with Linear Controls

	Policy Change IV				Reclassification IV			Combined IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Outcome: *Manager Differential*$_{it}$										
Spending Limit$_{it}$	-0.00	0.03*	0.03*	-0.05	-0.01	0.03†	0.03†	-0.00	0.03*	0.03*	0.03†
	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	16783	16783	15100	16783	16783	16783	15100	16783	16783	15100	16783
	Outcome: *Printing Differential*$_{it}$										
Spending Limit$_{it}$	-0.02	0.04*	0.04†	-0.02	-0.03†	0.04†	0.04	-0.02	0.04*	0.04*	0.05†
	(0.01)	(0.02)	(0.02)	(0.06)	(0.02)	(0.02)	(0.03)	(0.01)	(0.02)	(0.02)	(0.02)
Observations	16783	16783	15100	16783	16783	16783	15100	16783	16783	15100	16783
	Outcome: *Personal Differential*$_{it}$										
Spending Limit$_{it}$	-0.00	-0.00	-0.00	0.01	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)
Observations	16783	16783	15100	16783	16783	16783	15100	16783	16783	15100	16783
	Outcome: *Room Differential*$_{it}$										
Spending Limit$_{it}$	0.00	0.00	0.00	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	16783	16783	15100	16783	16783	16783	15100	16783	16783	15100	16783
	Outcome: *Meetings Differential*$_{it}$										
Spending Limit$_{it}$	0.00	0.00*	0.01*	0.01†	0.00	0.00	0.00	0.00	0.00*	0.00†	0.00
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	16783	16783	15100	16783	16783	16783	15100	16783	16783	15100	16783
	Outcome: *Staff Differential*$_{it}$										
Spending Limit$_{it}$	0.00	-0.01	-0.01	0.03	0.01	0.00	0.00	0.01	-0.00	-0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	16783	16783	15100	16783	16783	16783	15100	16783	16783	15100	16783
	Outcome: *Misc. Differential*$_{it}$										
Spending Limit$_{it}$	-0.01†	-0.00	0.00	-0.02	-0.01*	-0.00	0.00	-0.01*	-0.00	0.00	-0.00
	(0.00)	(0.01)	(0.01)	(0.02)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Observations	16783	16783	15100	16783	16783	16783	15100	16783	16783	15100	16783
Constituency FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$\theta_t electors_{it}$		✓	✓	✓		✓	✓		✓	✓	✓
$\gamma_t density_{it}$			✓				✓			✓	
Excluded Instruments:											
$county_{it}$					✓	✓	✓	✓	✓	✓	✓
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega$	✓	✓	✓					✓	✓	✓	
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega > 1918$				✓							✓

NOTE: All models were estimated using 2SLS. Standard errors are clustered on constituencies and are reported in (parentheses). Significance levels: † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C.11 – Linear-Linear models with Logged Controls

	Policy Change IV				Reclassification IV			Combined IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Outcome: *Manager Differential*$_{it}$										
Spending Limit$_{it}$	0.02*	0.04*	0.04**	0.02	0.01	0.04*	0.03†	0.02†	0.04**	0.03*	0.04*
	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
Observations	16783	16783	14834	16783	16783	16783	14834	16783	16783	14834	16783
	Outcome: *Printing Differential*$_{it}$										
Spending Limit$_{it}$	0.02	0.05**	0.03	0.11	0.01	0.06*	0.05	0.01	0.05**	0.04†	0.06*
	(0.02)	(0.02)	(0.02)	(0.07)	(0.02)	(0.03)	(0.03)	(0.01)	(0.02)	(0.02)	(0.03)
Observations	16783	16783	14834	16783	16783	16783	14834	16783	16783	14834	16783
	Outcome: *Personal Differential*$_{it}$										
Spending Limit$_{it}$	-0.00	-0.01	-0.00	-0.02	-0.00	-0.01	-0.00	-0.00	-0.01	-0.00	-0.01
	(0.00)	(0.00)	(0.01)	(0.02)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Observations	16783	16783	14834	16783	16783	16783	14834	16783	16783	14834	16783
	Outcome: *Room Differential*$_{it}$										
Spending Limit$_{it}$	0.00	0.00	0.00	0.02	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	16783	16783	14834	16783	16783	16783	14834	16783	16783	14834	16783
	Outcome: *Meetings Differential*$_{it}$										
Spending Limit$_{it}$	0.00†	0.00*	0.00	0.01	0.00	0.01†	0.00	0.00	0.00*	0.00†	0.01†
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	16783	16783	14834	16783	16783	16783	14834	16783	16783	14834	16783
	Outcome: *Staff Differential*$_{it}$										
Spending Limit$_{it}$	-0.00	-0.01	-0.01	0.00	0.00	-0.00	-0.00	0.00	-0.01	-0.01	-0.00
	(0.01)	(0.01)	(0.01)	(0.04)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	16783	16783	14834	16783	16783	16783	14834	16783	16783	14834	16783
	Outcome: *Misc. Differential*$_{it}$										
Spending Limit$_{it}$	-0.00	-0.00	0.00	-0.04*	-0.01	0.00	0.00	-0.00	-0.00	0.00	-0.00
	(0.00)	(0.01)	(0.01)	(0.02)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Observations	16783	16783	14834	16783	16783	16783	14834	16783	16783	14834	16783
Constituency FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$\theta_t log(electors_{it})$		✓	✓	✓		✓	✓		✓	✓	✓
$\gamma_t log(density_{it})$			✓				✓			✓	
Excluded Instruments:											
$county_{it}$					✓	✓	✓	✓	✓	✓	✓
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega$	✓	✓	✓					✓	✓	✓	
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega > 1918$				✓							✓

NOTE: All models were estimated using 2SLS. Standard errors are clustered on constituencies and are reported in (parentheses). Significance levels: † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C.12 – Linear-Log models with Logged Controls

	Policy Change IV				Reclassification IV			Combined IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Outcome: *Manager Differential*$_{it}$										
$log(Spending\ Limit_{it})$	2667.64†	3336.75*	3810.57*	-2788.29	1302.25	3667.16*	3668.39*	1446.17	3173.60*	3270.78*	2727.85†
	(1364.12)	(1549.25)	(1628.59)	(1752.88)	(1045.00)	(1586.05)	(1847.39)	(1053.75)	(1439.14)	(1635.99)	(1440.27)
Observations	16783	16783	14834	16783	16783	16783	14834	16783	16783	14834	16783
	Outcome: *Printing Differential*$_{it}$										
$log(Spending\ Limit_{it})$	2560.13	4977.92*	3689.46	2006.29	828.42	5618.83*	4452.88	1041.36	5066.61*	4062.29	5008.84*
	(1766.23)	(2004.63)	(2636.44)	(4220.86)	(1542.29)	(2486.96)	(3158.66)	(1504.60)	(2006.82)	(2745.58)	(2284.31)
Observations	16783	16783	14834	16783	16783	16783	14834	16783	16783	14834	16783
	Outcome: *Personal Differential*$_{it}$										
$log(Spending\ Limit_{it})$	-542.06	-571.53	-142.37	-829.33	-453.13	-574.17	-181.52	-443.67	-545.59	-155.39	-533.37
	(443.08)	(514.07)	(728.23)	(933.53)	(428.02)	(620.33)	(799.04)	(411.46)	(518.05)	(718.30)	(567.82)
Observations	16783	16783	14834	16783	16783	16783	14834	16783	16783	14834	16783
	Outcome: *Room Differential*$_{it}$										
$log(Spending\ Limit_{it})$	12.65	-34.16	316.58	-53.17	195.61	280.78	465.79	140.97	60.46	365.35	148.90
	(232.43)	(262.68)	(320.60)	(658.57)	(189.98)	(331.00)	(388.94)	(186.84)	(264.05)	(334.24)	(302.02)
Observations	16783	16783	14834	16783	16783	16783	14834	16783	16783	14834	16783
	Outcome: *Meetings Differential*$_{it}$										
$log(Spending\ Limit_{it})$	373.62†	380.38	388.52	-515.87	187.75	478.70†	372.82	204.31	360.91	326.82	299.83
	(206.80)	(248.73)	(321.83)	(648.46)	(168.29)	(285.45)	(336.93)	(167.08)	(246.28)	(308.86)	(266.91)
Observations	16783	16783	14834	16783	16783	16783	14834	16783	16783	14834	16783
	Outcome: *Staff Differential*$_{it}$										
$log(Spending\ Limit_{it})$	-537.38	-849.77	-966.27	-417.61	356.33	-4.50	-235.97	203.34	-484.09	-464.85	-118.23
	(669.95)	(766.27)	(1019.71)	(2216.11)	(550.77)	(939.60)	(1128.27)	(539.57)	(760.21)	(988.91)	(868.20)
Observations	16783	16783	14834	16783	16783	16783	14834	16783	16783	14834	16783
	Outcome: *Misc. Differential*$_{it}$										
$log(Spending\ Limit_{it})$	-486.57	-224.62	-28.99	-2390.39*	-473.76	122.40	238.46	-482.90	-124.62	27.59	-133.34
	(459.18)	(548.94)	(690.48)	(1089.96)	(342.04)	(617.25)	(738.13)	(340.42)	(529.21)	(658.77)	(562.18)
Observations	16783	16783	14834	16783	16783	16783	14834	16783	16783	14834	16783
Constituency FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$\theta_t log(electors_{it})$		✓	✓	✓		✓	✓		✓	✓	✓
$\gamma_t log(density_{it})$			✓				✓			✓	
Excluded Instruments:											
$county_{it}$					✓	✓	✓	✓	✓	✓	✓
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega$	✓	✓	✓					✓	✓	✓	
$county_{it} \cdot 1[t \geq \omega]_t, \forall \omega > 1918$				✓							✓

NOTE: All models were estimated using 2SLS. Standard errors are clustered on constituencies and are reported in (parentheses). Significance levels: † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

C.2 Spending Limit Formulae over Time

$$\begin{aligned} limit_i^{1885} &= 350 + 300 \cdot County_i + 30 \cdot \lfloor \frac{pop_i}{1000} - 2 \rfloor \cdot (1 + County_i) \\ &\approx 350 + 300 \cdot County_i + 0.03 pop_i + \cdot 0.03 County_i \cdot pop_i \end{aligned} \tag{C.1}$$

$$\begin{aligned} limit_i^{1918} &= \frac{5}{240} \cdot pop_i + \frac{2}{240} \cdot County_i \cdot pop_i \\ &\approx 0.021 pop_i + 0.008 \cdot County_i \cdot pop_i \end{aligned} \tag{C.2}$$

$$\begin{aligned} limit_i^{1929} &= \frac{5}{240} \cdot pop_i + \frac{1}{240} \cdot County_i \cdot pop_i \\ &\approx 0.021 pop_i + 0.004 \cdot County_i \cdot pop_i \end{aligned} \tag{C.3}$$

$$\begin{aligned} limit_i^{1949} &= 450 + \frac{1}{160} \cdot pop_i + \frac{1}{480} \cdot County_i \cdot pop_i \\ &\approx 450 + 0.006 pop_i + 0.002 \cdot County_i \cdot pop_i \end{aligned} \tag{C.4}$$

$$\begin{aligned} limit_i^{1969} &= 750 + \frac{1}{20} \cdot \lfloor \frac{3 \cdot pop_i}{24} \rfloor + \frac{1}{20} \cdot \lfloor \frac{pop_i}{24} \rfloor \cdot County_i \\ &\approx 750 + 0.006 pop_i + 0.002 \cdot County_i \cdot pop_i \end{aligned} \tag{C.5}$$

$$\begin{aligned} limit_i^{1974} &= 1075 + \frac{3}{50} \cdot \lfloor \frac{3 \cdot pop_i}{24} \rfloor + \frac{3}{50} \cdot \lfloor \frac{pop_i}{24} \rfloor \cdot County_i \\ &\approx 1075 + 0.0075 pop_i + 0.0025 \cdot County_i \cdot pop_i \end{aligned} \tag{C.6}$$

$$limit_i^{1978} = 1750 + 0.015 \cdot pop_i + 0.005 \cdot pop_i \cdot County_i \tag{C.7}$$

$$limit_i^{1982} = 2700 + 0.023 \cdot pop_i + 0.008 \cdot pop_i \cdot County_i \tag{C.8}$$

$$limit_i^{1987} = 3370 + 0.029 \cdot pop_i + 0.009 \cdot pop_i \cdot County_i \tag{C.9}$$

$$limit_i^{1992} = 4330 + 0.037 \cdot pop_i + 0.012 \cdot pop_i \cdot County_i \tag{C.10}$$

$$limit_i^{1997} = 4965 + 0.042 \cdot pop_i + 0.014 \cdot pop_i \cdot County_i \tag{C.11}$$

$$limit_i^{2001} = 5483 + 0.046 \cdot pop_i + 0.016 \cdot pop_i \cdot County_i \tag{C.12}$$

$$limit_i^{2005} = 7150 + 0.05 \cdot pop_i + 0.02 \cdot pop_i \cdot County_i \tag{C.13}$$