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Essays in Politcal Economics

by

Eric A. Avis

A dissertation submitted in partial satisfaction of the
requirements for the degree of
Doctor of Philosophy

in

Economics

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Frederico Finan, Chair
Professor Ernesto Dal Bo
Professor Kei Kawai

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Abstract

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Professor Frederico Finan, Chair

This dissertation studies the role of political institutions in curbing rent-seeking and corruption. In each of the three chapters, I describe and analyze how economic incentives and institutions affect the behavior of elected officials and, ultimately, have an impact on policy choices and welfare. In the first chapter, *Interest Groups, Campaign Finance and Policy Influence: Evidence from the U.S. Congress*, I study how the financing of political campaigns by special interest groups distorts legislative voting in the United States. In the second chapter, *Do Government Audits Reduce Corruption? Evidence from Exposing Corrupt Politicians*, we study how randomized audits of government resources can reduce corruption by enhancing political and judicial accountability. In the third chapter, titled *Money and Politics: The Effects of Campaign Spending Limits on Political Entry and Competition*, we study the effects of the imposition of limits to campaign spending on political competition.[1]

In *Interest Groups, Campaign Finance and Policy Influence: Evidence from the U.S. Congress*, I study the effects of special interest group contributions on legislative voting. Empirical research on this question has led to mixed results (Ansolabehere et al., 2003). The main empirical challenge to establishing this relationship is that it is difficult to disentangle the effects of ideology and special interest groups in determining the behavior of legislators. In this paper, I address these concerns by exploiting a new comprehensive dataset of interest group positions on bills and by improving upon the identification strategies of previous studies. To identify the effects of interest group contributions, I create instruments which are based on the interaction of national industry shocks with historical connections between interest groups and politicians. I find a consistent, robust reduced-form relationship between interest group contributions and legislative behavior. I then develop and estimate a structural model which allows me to further unpack how ideology and interest groups interact in determining the votes of legislators. I find that more ideological (Republican) legislators tend to be less influenceable by contributions, highlighting the possibility that ideology can act as a commitment to voters against special interests. I also use my estimates to study the effects of a counterfactual policy change which would ban contributions from special

[1] The second chapter reproduces work from Avis et al. (2018a) and the third chapter reproduces material found in Avis et al. (2018b).

interest groups. I find that several bills would have failed to pass under the counterfactual policy regime, including significant bills allocating several hundred billions of dollars.

Corruption and rent-seeking are major impediments to economic development worldwide (Rose-Ackerman, 1999). In the second chapter, I study how institutions play a central role in curbing corruption in the context of a developing country. In *Do Government Audits Reduce Corruption? Evidence from Exposing Corrupt Politicians*, written with Claudio Ferraz and Fred Finan, we study the extent to which government audits of public resources can reduce corruption by improving the selection of politicians and by enhancing accountability. We exploit a Brazilian anti-corruption program, which randomly audits municipalities for their use of federal funds. To differentiate whether the effects of the audits operate through a political selection or discipline channel, we develop and estimate a career concerns model of political accountability (Holmstrom, 1999; Persson and Tabellini, 2002). In the model, audits affect the beliefs of mayors about the costs of engaging in corruption and provide information about mayors to voters before elections. We exploit the randomized nature of audits together with the different incentives facing first-term versus term-limited (second-term) mayors to separately identify the effects of political selection, electoral discipline and legal discipline. We find that most of the reduction in corruption is due to a legal disciplining effect, as audits primarily increase the perceived non-electoral costs of engaging in corruption.

Beyond the use of randomized audits of public resources, policymakers have also focused efforts on reforming campaign finance laws to curb rent-seeking and improve economic outcomes. In *Money and Politics: The Effects of Campaign Spending Limits on Political Entry and Competition*, written jointly with Claudio Ferraz, Fred Finan and Carlos Varjao, we study the effects of campaign spending limits on political entry and competition. Although campaign spending limits are one of the most common used policy tools worldwide in curbing the role of money in politics, there is little causal evidence on their effects (with Milligan and Rekkas (2008) and Fouirnaies (2018) being the only exceptions). In this chapter, we exploit a reform in Brazil which imposed spending limits on mayoral elections. These limits were implemented with a sharp, unexpected discontinuity which was the result of the use of different formulas to calculate inflation rates. Using a regression-discontinuity design, we find that stricter spending limits increase political competition. Stricter limits create a larger pool of candidates running for office, which is on average less wealthy. Moreover, stricter limits reduce the likelihood that incumbent mayors are reelected and leads to the election of less wealthy mayors that rely less on self-financing for their campaigns. However, in municipalities with stricter spending limits, we find that mayors are less successful in obtaining federal block grants. These findings were found to be consistent with the predictions of a contest model with endogenous entry of candidates.

To my parents.

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Chapter 1

Interest Groups, Campaign Finance and Policy Influence: Evidence from the U.S. Congress

1.1 Introduction

> *Across the spectrum, money changed votes. Money certainly drove policy at the White House during the Clinton administration, and I'm sure it has in every other administration too.* [Former Representative Joe Scarborough (R-FL)]
>
> *The evidence is pretty overwhelming that the money does not play much of a role in what goes on in terms of legislative voting patterns and legislative behavior.* [Bradley Smith, former Federal Elections Commission chairman][1]

The influence of money on policymaking is a major concern across American society, among journalists and policymakers to the electorate at large. With the total cost of the 2016 House and Senate campaigns surpassing $4 billion, there is a growing belief that the very pillars of representative democracy are under threat if politicians primarily serve the interests of special interest groups over those of their constituents. In fact, in a 2015 representative poll of the U.S. population, 84% believed that money had too much influence in political campaigns and 85% believed that politicians, once elected to public office, promote policies that directly help their donors.[2]

[1]The first quote is from Schram (1995) and the second quote from *The Sound of Ideas*, WCPN, March 29, 2011. Both are retrieved from Lessig (2011).

[2]These statistics are from a 2015 *New York Times/CBS News* poll of the adult U.S. population. Specifically, the first question was "Thinking about the role of money in American political campaigns today, do you think money has too much influence, too little influence, or is it about right?" and the second question was "How often do you think candidates who win public office promote policies that directly help the people and groups who donated money to their campaigns", to which 55% responded most of the time, 30% sometimes and 9% rarely. See https://www.nytimes.com/interactive/2015/06/01/us/politics/document-poll-may-28-31.html.

Although it is commonly believed that politicians respond to the economic incentives created by special interest groups, it is difficult to empirically establish this relationship. Indeed, an alternative possibility is that politicians primarily vote according to their ideological preferences and that special interest groups donate to like-minded politicians without seeking to influence their behavior (Ansolabehere et al., 2003). Disentangling these two views is difficult due to limitations in the available data on the policy preferences of special interest groups. Furthermore, even with high-quality data, it is difficult to interpret what theoretical mechanisms might be driving the observed relationships between special interest group preferences, contributions and legislative voting.

In this paper, I analyze the effects of special interest group contributions on legislative voting, overcoming the empirical challenges in the following ways. First, I combine a new detailed dataset of special interest group positions on several thousand bills to comprehensive data on contributions. This not only greatly expands the scope of this study relative to previous work, the panel structure of the data allows for improvements in the identification of the effects of money on votes. Second, I develop and estimate a structural model of legislative voting in an environment where interest groups compete to buy influence. Estimating the model allows me to disentangle how ideology and special interest groups shape the voting behavior of politicians. In addition, the model estimates provide novel evidence on two important follow-up questions, namely on the potential effects of campaign finance reform aimed at limiting contributions, and on the magnitude of the rate of return of interest group contributions.

I collect a dataset of interest group positions to the substantive bills considered by the House of Representatives from the 110th to 114th Congresses. This dataset builds on the extensive efforts of the nonpartisan organization Maplight, which collects for each bill the positions of various interest groups. These positions are obtained from a variety of public record sources, including news databases, congressional hearing testimonies, and trade associations' websites. With over 10,000 bills for which at least one interest group is coded as having a public position, this is by far the most extensive dataset used yet to uncover the effects of contributions on votes. In fact, across a review of 40 papers on this question, the typical study analyzed just 5 votes and only one study analyzed more than 35 votes (see Table 1.1). Moreover, the data allow for improvements in the empirical strategy. First, the large number of votes allows me to exploit the panel structure of the data, whereas previous work typically relies on cross-sectional variation. Second, not only does the support or opposition of interest groups for legislation vary at the bill-level, the level of disaggregation for interest group positions is remarkably fine. Contributing organizations are classified into 427 sub-industry categories, which are themselves aggregated into 98 industries.[3] Either level of aggregation allows for rich variation in the mix of organizations supporting or opposing each bill.[4]

[3]These categories are provided by the Center of Responsive Politics, a nonpartisan organization that has coded campaign contributions meticulously into each category. For example, the industry "Crop Production & Basic Processing" contains sub-industry categories such as "cotton", "sugar cane and sugar beets", and "vegetables, fruits and tree nuts."

[4]As a point of comparison, the analysis in Ansolabehere et al. (2003) aggregates roll call votes into a single index for each congressional session (for each congressman), and aggregates campaign contributions into just two categories (business and labor).

I begin the analysis by examining the reduced-form relationship between money and votes. The key threat to causal identification is that the preferences of politicians and those of their contributors are likely to be correlated. To rule out this concern, I follow two different approaches that lead to nearly identical point estimates. First, I control for an exhaustive set of covariates, which include a wide variety of fixed effects to control for the influence of parties and ideology. Across all specifications, I find a significant, substantial relationship between contributions and votes: increasing contributions in support of a bill by 10 percent increases the probability that the politician votes in support of the bill by 2 percentage points.[5] Second, I use an instrumental variable strategy in the spirit of the shift-share instrument (Bartik, 1991; Altonji and Card, 1991) which exploits variation in past industry contribution shares interacted with national shocks in industry contributions. Reassuringly, I find similar results using this strategy. Furthermore, I consider whether the effect of money on votes is heterogeneous in the way that would be expected if contributions bought influence. I do so by testing whether the effect is lower for politicians who have announced their retirement and therefore have less to gain by supporting their donors.[6] Consistent with this hypothesis, I find that the effect of contributions is attenuated for retiring congressmen.

I use these findings to motivate and develop a structural model of legislative voting in an environment with interest group influence. In the model, each politician weighs her ideological preferences against her campaign contributions when choosing how to vote. As in the spatial theory of voting (Downs, 1957), each politician has an ideal or bliss point within a common ideological space and evaluates policies based on their distance to the ideal point. The key structural assumption is that legislators consider contributions and ideology additively separably given a legislator-specific parameter which measures responsiveness to contributions. Interest groups supply contributions to legislators to increase the probability of achieving the required threshold to alter each bill's outcome (e.g. a majority or two-thirds of legislator votes). Although a rich theoretical literature studies related models of vote buying, this model has been intractable empirically because a large amount of vote-level data is required to estimate the legislator-specific structural parameters. This study overcomes this hurdle and is the first to structurally estimate a spatial model of roll call voting which includes special interest group influence.

I estimate the model for the 110th to the 114th Congresses of the U.S House of Representatives. Given the complexity of the likelihood function, I use a Bayesian Markov Chain Monte Carlo approach to obtain the posterior distribution of the model's structural parameters. Consistent with the reduced-form evidence, I find that interest group contributions significantly affect legislative voting. Since I jointly estimate each legislator's responsiveness to contributions together with their ideal points in the ideological space, I can analyze the relationship between these two structural parameters. Previous research has hypothesized about the potential commitment value of electing an ideologically extreme politician. For instance, Mian et al. (2010) found that for the roll call vote on the Emergency Economic Stabilization Act of 2008, moderate congressmen were more prone to influence by special interest groups than ideological congressmen. In line with this hypothe-

[5] I also test whether other stages of legislation are also affected by campaign contributions, specifically, the sponsorship and cosponsorship of bills. I find that in both cases, campaign contributions significantly increase the likelihood that a politician engages in an action supporting the bill.

[6] This identification strategy was also used by Mian et al. (2010).

sis, I find systematic evidence that moderate Republicans are more influenceable than extremist Republicans. However, I do not find a link between ideology and influenceability for Democrats. These findings highlight a potential trade-off faced by voters between electing a candidate that is more ideologically extreme against a candidate who is more moderate, but may be more prone to capture by special interest groups.

In order to quantify the effect of money on the outcome of congressional votes, I simulate equilibrium outcomes under a ban on Political Action Committee contributions.[7] I find that, in expectation, about 0.5 percent of individual roll call votes would have differed. However, what is ultimately of policy relevance is whether this alters the passage of bills. I find that, in expectation, the number of different votes outcomes ranges from 3 to 19 depending on the Congress in question, out of around 1000-1500 votes per Congress. I document which votes are most likely to have had different outcomes and I find that these tend to be for bills which passed with bipartisan support under intensive lobbying by special interest groups. These include roll call votes on the passage of bills such as the Food, Conservation and Energy Act of 2008, which authorized $288 billion in spending over five years, and on the conference report passed by the House on the Agricultural Act of 2014, worth $956 billion over ten years. According to the counterfactual simulations, neither of these bills are likely to have passed without the lobbying of special interest groups.

Given the large value of these bills to special interest groups, a puzzle in the political economy literature is why organized groups contribute so little in political campaigns (Tullock, 1972). The model estimates allow me to shed light on the Tullock puzzle from a new angle. In the estimation, I invert the equilibrium spending by the interest groups to recover the value of the bills to these groups. This allows me to compute the rate of return to interest group contributions. Using this strategy, I estimate the average rate of return to contributions for special interest groups to be 17 percent. Although this number may seem large, it is an order of magnitude smaller than that found in some other studies in this area (e.g. using lobbying data, Kang (2015) finds an average rate of return of 155%).

This paper contributes to the literature studying the influence of special interest groups on legislative behavior. Some recent studies have found evidence that campaign contributions have affected particular votes (e.g. Mian et al. (2010); Conconi et al. (2014)), while others have found the opposite. For example, in an influential review of the extant literature, Ansolabehere et al. (2003) concluded that in the majority of studies reviewed, campaign contributions had no significant effect on legislative voting behavior.[8] The majority of these studies focus on a narrow set of votes for a specific issue area. In contrast to these papers, I study a much broader set of votes, improve upon the identification strategy of previous work, and expand on it by taking a structural approach to complement the reduced-form evidence.[9]

[7]Although this policy may be unrealistic given recent Supreme Court rulings, it sees considerable support among the U.S. public. In a 2013 Gallup poll, 50% of Americans supported a proposal to establish a campaign finance system where federal campaigns are funded by the government and where all contributions from individuals and private groups are banned. Moreover, 79% would vote for a law placing a limit on the amount of money that House and Senate candidates can raise and spend on political campaigns.

[8]See also Milyo et al. (2000) and Stratmann (2005) for surveys of this literature.

[9]This paper also relates to the literature studying other determinants of legislative voting. Recent contributions

While this paper focuses on the effect of contributions on votes, there are other ways in which special interest groups influence politics. Recent contributions include Bombardini and Trebbi (2011), who emphasize that special interest groups buy influence with not only contributions but also by mobilizing votes, and Chamon and Kaplan (2013), who argue that the threat of funding the opposing candidate buys influence. Interest groups may also buy influence through other channels, such as lobbying (Blanes i Vidal et al., 2012; Bertrand et al., 2014), or charitable giving (Bertrand et al., 2018). This paper complements this literature by arguing that although often overlooked as a source of influence, campaign contributions can also have a direct effect on policymaking.[10]

Methodologically, this paper contributes to a growing literature in political economy using structural modeling to study campaigning (Stromberg, 2008; Kawai and Sunada, 2015), lobbying and legislative voting (Spenkuch et al., 2018; Canen et al., 2018). However, there is little work studying the bridge between interest groups and policymaking.[11] The most notable exception is Kang (2015), who estimates a contest model to investigate the effect of lobbying on public policy relating to the energy sector. Using data from the 110th Congress, the author finds that lobbying has a small but significant effect on the probability of a policy being enacted. Different from this study, I model how individual votes are aggregated into policy outcomes instead of viewing the government as the unit of analysis. This is possible because contributions map the ties between each legislator and interest group pair, whereas lobbying data is only observed at the legislature level. This allows me to investigate the effects of interest groups on policy by exploiting variation across legislators in the context of a model which includes the institutional feature of majority voting.

This paper also contributes to the literature on campaign finance reform. Empirical studies in this area with convincing identification strategies are scarce due to the fact that reforms are not usually enacted at random. For example, Milligan and Rekkas (2008) use an instrumental variable strategy based on the formula which determines campaign spending limits in Canada and Avis et al. (2017), exploit an unexpected discontinuity in the implementation of campaign spending limits in Brazilian municipalities. Because the U.S. lacks quasi-experimental variation at the federal level, empirical studies have focused on variation across states. For example, Barber (2016) considers the effect of campaign contribution limits using a difference-in-differences strategy and finds that higher PAC limits leads to less polarization. An advantage of the structural estimation in the present paper is that it allows for the study of similar counterfactual policies at the federal level.

Finally, the paper contributes to the literature pioneered by Poole and Rosenthal (1985) on the

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emphasize the effects of single-issue voters (Bouton et al., 2018b), electoral incentives (Conconi et al., 2014), and district-level economic factors (Facchini and Steinhardt, 2011)

[10] In this respect, this study relates to classic work in this area such as Snyder (1990) and Stratmann (1992). There is also an active literature which studies how interest groups allocate contributions, for instance to seek access to legislators in relevant committees (Romer and Snyder, 1994; Berry and Fowler, 2016; Powell and Grimmer, 2016), with procedural power (Fouirnaies and Hall, 2018) or who are centrally located within the network (Battaglini and Patacchini, 2016). Finally, some other work focuses on the effects of contributions on other outcomes, such as corporate returns. (Fowler et al., 2017).

[11] An exception is the Protection for Sale literature which empirically tests the Grossman and Helpman (1994) model. This literature was pioneered by Goldberg and Maggi (1999) and Gawande and Bandyopadhyay (2000); see Imai et al. (2009) for a review.

estimation of ideal points from spatial models of voting. These methods were first developed to scale roll-call data into ideal point estimates for legislators and have been widely used across a variety of applications.[12] The Poole and Rosenthal Nominate ideal points incorporate all motives that predict voting into a single measure, which is usually interpreted as ideology. By also including interest group contributions in the estimation of my model, I can separate ideology from the influence of special interest groups, which allows me to estimate a measure of each legislator's "true" ideology free of the bias induced by special interest groups.

The paper is organized as follows. Section 2 provides a brief overview of the institutional background and describes the data. Section 3 presents the reduced-form evidence that contributions affect policymaking. Section 4 develops the structural model. Section 5 presents the empirical strategy and section 6 the results. Section 7 concludes.

1.2 Background and Data

Campaign finance and influence in the U.S. Congress

Why do interest groups contribute to political campaigns? Although there exist anecdotal cases of explicit quid-pro-quo exchanges or bribery, scholars have argued that the ways in which interest groups buy influence are usually subtler. For example, an interest group may need to develop a relationship with a given legislator, based on reputation or trust, since any explicit contracts linking money to legislative behavior are illegal (Snyder, 1992). Only then, in this view, can contributions buy the access required to influence policy. For instance, Clawson et al. (1998) argue:

> Campaign contributions are best understood as gifts, not bribes. They are given to establish a personal connection, open an avenue of access, and create a generalized sense of obligation. Only rarely–when the normal system breaks down–does a contributor expect an immediate reciprocal action by a politician. Even then the donor would normally use circuitous language to communicate this expectation.

Indeed, a rich literature views contributions as a means to buy access. For instance, in a randomized field experiment, politicians were more likely to meet with an interest group if they were informed that it had donated to their campaign (Kalla and Broockman, 2016) . However, one must ask why would an interest group buy access, if not ultimately to shape or influence policy in its favor. This idea–that access ultimately buys influence–is consistent with studies which show that politically connections increase firm value (e.g. Goldman et al. (2008)).

[12]For example, Bonica (2013, 2014) scales contributions data to produce a set of ideal point estimates on a common ideological space for PACs and legislators. An advantage of my approach relative to the latter is that I allow donors to have much more flexible preferences over policy. Indeed, in the estimation, the bill-level positions data allows the interest groups to have high-dimensional preferences over policy whereas if I had scaled their preferences to the common ideological space with the legislators, much of this information would be lost. For example, two interest groups, say the dairy and steel industries, might both appear to be centrist if they are scaled onto the same ideological space as legislators, but in reality support and oppose distinct sets of bills.

Although campaign contributions are thought to play a central role in the influence activities of special interest groups, other options are also available. For example, considerable sums are spent directly on lobbying members of Congress. Yet focusing this study on campaign contributions presents several advantages. First, the funding of campaigns is well regulated, and consistent, high-quality, detailed data are available for each political candidate. Second, I can connect interest groups to the candidates they fund, which is not possible with other forms of data such as lobbying disclosure data. Third, focusing on contributions allows me to engage directly with large theoretical and empirical literatures which have focused on campaign finance as a means to buy influence. Finally, recent studies have found contributions to be highly correlated with other influence strategies such as lobbying (Smith, 2015) and charitable giving (Bertrand et al., 2018).[13] This suggests that one can view contributions as a proxy for the broader set of influence activities undertaken by an interest group.

There are several stages of the legislative process for which interest groups can affect policymaking. In this study, I will primarily focus on roll call voting on bills, as this is the stage that has received the most attention in the literature. Since earlier stages of legislation are also likely to be important, I will also briefly analyze effects on bill sponsorship and cosponsorship.

Data

The analysis in this paper combines four sources of data. First, and most importantly, I collect a dataset of interest group positions to the substantive bills considered by the House of Representatives in the 110th to 114th Congresses. This dataset builds from the extensive efforts of the nonpartisan organization Maplight, which gathered interest group positions on bills from a variety of public sources, including news databases, congressional hearings, and organization websites.[14] Only substantive bills are considered, and thus ceremonial bills such as the naming of a public park are excluded. For each bill in the database, each special interest group is either labeled as in support, in opposition, or with no public position.

Second, I collect data on campaign contributions to congressional candidates. Both individual and PAC contributions are carefully coded into distinct interest groups by the Center for Responsive Politics, a nonpartisan organization. In this paper, I focus on PAC contributions which I will equivalently refer to as interest group contributions. Two levels of aggregation exist: first, interest groups are coded in some 427 sub-industry categories, which are then aggregated into 98 industries. Examples of industries include actual industries such as “Automotive", “Dairy” or “Real Estate", as well as issue-areas such as “Environment", “Gun Control" or “Gun Rights".[15] The finer level of aggregation will distinguish between categories such as ‘Auto Manufacturers”, “Auto Dealers” and “Auto Repair” for the automotive industry while for other industries such as dairy no finer level of categorization exists. There is a trade-off to be made when choosing between the two levels of aggregation. Since it is plausible that if one sub-industry supports a bill, the other sub-industries

[13] In particular, lobbyists play an important role in the financing of political campaigns, both directly with their own money and indirectly by steering contributions from their clients to the politicians they target (Lessig, 2011).

[14] For more details, see http://classic.maplight.org/us-congress/guide/data/support-opposition.

[15] For more details, see https://www.opensecrets.org/resources/ftm/ch12p1.php.

within that industry also support the bill, I use the industry-level aggregation throughout this study. As a robustness check, I replicate the reduced-form analysis using the finer aggregation level and find virtually identical results.

Third, I link these data to congressional data on bills. For each bill, I collect data on the sponsor, cosponsors, committee that handled the bill, and roll call votes. The focus in the analysis will be on roll call votes that clearly support the passage of the bill, so that the position of each interest group on the vote is clear, as opposed to amendments for example, where this is not necessarily the case, and the possibility of strategic voting is more likely. Therefore, I include "on passage" and "agree to the resolution" roll call votes as well as motions to suspend the rules (which are done to accelerate the passage of less controversial bills). I acquire the roll call data from Voteview. Fourth, I collect data on sponsorship, cosponsorship and politician characteristics from the organizations GovTrack and Propublica.

Before proceeding to the reduced-form evidence of the effect of money on legislators, let us consider an example. The *Energy Independence and Security Act of 2007*, originally the *Clean Energy Act of 2007*, was first passed by the House on January 18, 2007 roll call vote. The purpose of the act was "to move the United States toward greater energy independence and security, to increase the production of clean renewable fuels, to protect consumers, to increase the efficiency of products, buildings, and vehicles, to promote research on and deploy greenhouse gas capture and storage options, and to improve the energy performance of the Federal Government, and for other purposes."[16] Six interest group sub-industry categories supported the passage of the bill, including "Environmental Policy" and "Alternative Energy Production and Services", and 13 groups opposed it, including "Petroleum Refining and Marketing", "Independent Oil & Gas Producers", and "Natural Gas Transmission and Distribution" among others. These are then aggregated into a total of 10 industries: for instance, the three interest groups just named in opposition are all part of the "Oil and Gas" industry. The question I seek to answer is whether campaign contributions provided by these industries influenced the roll call vote.

1.3 Reduced-form Evidence

Econometric specification

To quantify the effects of contributions on votes, I first create measures of supporting and opposing contributions for each politician-vote pair. For a given politician i on vote r, I construct $C_{ir}^{Support} = \sum_j C_{ijr} Support_{jr}$ and $C_{ir}^{Oppose} = \sum_j C_{ijr} Oppose_{jr}$, where $Support_{jr}$ and $Oppose_{jr}$ are dummy variables for whether, respectively, the interest group supports or opposes the bill and C_{ijr} are total contributions to the politician in term $t(r)$, the election cycle prior to this roll call vote.[17]

[16] See https://www.congress.gov/bill/110th-congress/house-bill/6.

[17] I aggregate contributions over the electoral cycle because it is unlikely that contributions are used to immediately affect a vote as a quid-pro-quo exchange (Clawson et al., 1998). I also consider an alternative definition for C_{ijr}: the total contributions to the politician in the term divided by the total number of bills in this term for which the SIG took a position on. The results are similar for both definitions.

In the first approach, I estimate variants of the following reduced-form relationship between a legislator's roll call vote and the campaign contributions he received with respect to this vote:

$$\mathrm{y}_{ir} = \alpha + \beta_1 C_{ir}^{Support} + \beta_2 C_{ir}^{Oppose} + \sum_k x_{ir}^k \eta_r^k + \sum_l w_{ir}^l \mu_i^l + \varepsilon_{ir} \tag{1.1}$$

where y_{ir} is a yes vote, C_{ir} are the measures of support and opposition in campaign contributions, η_r are vote fixed effects, μ_i are legislator fixed effects, x_{ir}^k are legislator characteristics indexed by k, w_{ir}^l are vote characteristics indexed by l.[18] In this section, I transform the two contribution measures by taking the inverse hyperbolic sine in order to interpret the estimates as elasticities.

The most important potential confounds are that ideology and party predict both the distribution of contributions that a legislator receives in addition to her voting behavior. To control for these, I include in x_{ir} the legislator's party affiliation and a polynomial of the legislator's baseline ideology scores.[19] Hence, I flexibly allow for both ideology and party to play a differential role for each vote. The party fixed effects imply that the effects of contributions on votes are identified only by variation within each vote across legislators of the same party. Furthermore, the potential confound of ideology is also controlled for provided that the polynomial in legislator scores correctly specifies the relationship between voting and ideology.

I also interact legislator fixed effects with a vector of vote characteristics w_{ir}, so that each of these characteristics may have heterogeneous effects on voting which vary by legislator. In particular, I include term fixed effects, the party affiliation of the bill's sponsor, and whether the legislator's party supported the vote.[20] The term fixed effects will control for factors such as committee membership which vary from one congressional session to the next. Furthermore, controlling for the bill sponsor's party allows each legislator to have a different propensity to vote for bills sponsored by his own versus the opposing party. Lastly, controlling for which parties supported the bill allows each legislator to exhibit different levels of party loyalty.

Although I include a rich set of controls in the regression model, a threat to causal identification is the possibility that conditional on the observables, contributions C_{ir} are correlated with unobservable characteristics ε_{ir} which affect how politicians vote. For example, this could occur if an interest group, anticipating that a legislator's preferences will shift in its favor in the following term, increases its contribution to him. To address this type of concern, I propose the following instrument, which is analogous to the "shift-share" instrument (Bartik, 1991; Altonji and Card, 1991):

$$\tilde{C}_{ir}^{Position} = \sum_j \left(\frac{C_{ijt^0}}{C_{jt^0}} C_{jt(r)} position_{jr} \right) \tag{1.2}$$

[18] I drop abstentions from the analysis. The results are robust to whether or not I include abstentions.

[19] More precisely, I measure ideology by including a second degree polynomial in the first and second dimensions of each legislator's past Nominate scores. Although I am referring to these controls as "ideology", the measurement of these variables do not restrict them to ideological concerns, and in practice they will also capture other factors that influence legislative voting such as constituent interests.

[20] I measure this last variable with dummies indicating whether the majority of the bill's cosponsors are Democrats or Republicans.

where $C_{jt(r)} \equiv \sum_{i'} C_{i'jt(r)}$ are the total national-level contributions from SIG j in election year t, C_{ijt^0}/C_{jt^0} is the share of contributions from SIG j going to politician i in the reference year t^0, $position_{jr}$ are the SIG positions for each vote r.[21] The instrument captures both district-specific and politician-specific factors which make it more likely for the politician to receive more campaign contributions in years where the interest group decides to invest more into politics at the national scale. For example, consider a shock to oil prices which causes the oil industry to increase contributions nationally in a given time period. I exploit the variation in local contributions that is predicted by the initial distribution of oil industry contributions across districts. This initial distribution could be the result of politician-specific factors such as personal ties, and/or district-specific factors such as the historical presence of the industry due to the location of natural resources. The exclusion restriction is satisfied if these historical factors which contributed to a politician or district receiving a higher share of the industry's contributions at time t^0 only affect the district's current politician's vote through an increase in campaign contributions today.

Another approach to identification is to exploit exits from congress (Mian et al., 2010). Every congressional session, several representatives announce their retirement from public office (including running for other offices). If legislators value contributions because they have re-election concerns, a retiring representative will have less of an incentive to favor the interest groups that funded her. Therefore, I also estimate equation (1.1) including interactions between contributions and a dummy for retiring congressmen. If contributions influence voting, then β will be attenuated towards zero for retiring representatives.

Descriptive statistics

Table 1.2 reports summary statistics for the sample used in the regression analysis. Each observation consists of a legislator-vote pair. On average, a politician receives approximately \$36,000 in contributions in support and \$12,000 in opposition of a vote. Although these numbers may appear small, it is important to note that they include many bills for which few organizations took positions. Hence, there are cases where a politician has received no support for the given bill, and on the other spectrum cases where the politician has received well over \$100,000.

Figure 1.1 displays the distribution of the number of special interest groups in support and opposition of each bill, while Figure 1.2 presents the distribution of the support and opposition in contributions across vote-politician pairs (excluding zero contributions observations). We see that there are in general more special interest groups in support than in opposition of bills, and likewise that there are more supporting contributions than opposing contributions.

Figure 1.3 plots the relationship between interest group contributions and the ideology of politicians, as captured by the Poole and Rosenthal Nominate score. This figure shows that PACs typically allocate more funds to moderate politicians than to the ideologically extreme.

[21] I fix SIG contribution shares to an intial period rather than update the shares in each period (e.g. as in Beaudry et al. (2014)). This choice is motivated by the possibility that if in addition to contributions and ε_{it} being correlated, ε_{it} is serially correlated, then future shares will be endogenous. In any case, the results are similar if I instead use lagged shares in the instruments (results are available upon request).

Results

Figure 1.4 displays the relationship between roll call votes and campaign contributions graphically, controlling only for roll call vote fixed effects. We see that the relationship between money and votes is remarkably strong when other confounds are not adjusted for. Indeed, on average, going from a net amount of contributions in support of a bill of \$0 to $\$100,000$ increases the likelihood of voting yes by 30 percentage points.

Table 1.3 reports the results from the estimation of the linear probability model (1.1). In the first specification (column 1), I control for vote fixed effects interacted with the representative's party as well as politician fixed effects interacted with the vote characteristics. In this and every future specification, I cluster standard errors by politician and by vote. I find that a 10 percent increase in supporting contributions is associated with a 1.6 percentage point increase in the probability of voting for a bill, whereas a 10 percent increase in opposing contributions is associated with a 1.1 percentage point decrease in the probability of voting for the bill.

To assess the robustness of this result, I next consider the model where we also control for the politician's ideology by interacting vote fixed effects with second-order polynomials in the politician's ideology scores. Since there are 1001 votes in the sample, this adds a considerable number of covariates to the estimation. However, the results are very similar: the point estimates for a 10 percent increase in contributions become respectively 1.4 and 0.7 percentage points.

I next consider how retirement affects the relationship between contributions and votes. In columns 3 and 4, I replicate the first two columns with the addition of interactions with a retirement dummy. The point estimates indicate that retiring politicians are less likely to be influenced by money in their votes. This is especially the case for opposing contributions, where I can reject the null hypothesis of contributions having no effect for non-retiring members ($p < 0.01$), but cannot reject the null for retiring members.

The remaining specifications use the shift-share instruments I derived in the previous section. To construct these instruments, it is necessary to have the industry shares across politicians at an earlier date, which I choose to be the 107th Congress (the earliest Congress for which I have contributions data categorized by industry). This requirement reduces the sample to politicians who ran in the 107th session. Using this "balanced" sample, I replicate the findings using both OLS and IV regressions. The IV estimates suggest similar effects for contributions on votes in line with the OLS estimates.

Contested versus lopsided votes Although I have found evidence for a significant and robust relationship between contributions and votes, a concern lies in the fact that many roll call votes are lopsided. In fact, it could be the case that the results derived so far stem from contributions affecting lopsided votes, where the final outcome of the bill is not in question. In Figure 1.5, I plot the ex-post distribution of the share of yea votes. The distribution is bimodal: many votes are close to unanimous, whereas many others are close to the 50 percent threshold. I next test whether the effects of contributions on votes depends on whether a vote is contested or lopsided. As in Snyder and Groseclose (2000), I define a lopsided (contested) vote to be one in which at least (most) 65 percent voted yea. Table 1.4 shows that the effects of contributions are larger for contested than for lopsided votes, indicating that interest groups influence the more relevant votes.

Individual versus PAC contributions Another way to assess the validity of the research design is to compare the effects of individual contributions to those of PAC contributions. In contrast to PACs, it is unlikely that individuals can directly affect roll call votes. Doing so would require access to politicians which is implausible for most individual donors. Instead, it is more likely that individuals seek to affect policy by attempting to alter the electoral odds of political candidates. Recent evidence by Bouton et al. (2018a) supports this view. Therefore, individual contributions are more likely to reflect constituent interests rather than directly alter roll call votes. I test this by estimating a linear probability model where I include the contributions received by each politician by individuals and by PACs supporting and opposing each bill. The results in Table 1.5 are consistent with the hypothesis. While PAC contributions maintain a significant and substantial effect on voting, I do not find an effect for individual contributions.

Diversity in contribution sources Do legislators respond more to interest groups when the source of contributions is more diverse? Recent evidence in political science suggests that interest group coalitions are more likely to be effective in gaining influence when they represent a more diverse set of interests (Phinney, 2017). To explore this question, I compare the fit of the model estimated so far (with the full set of controls) to an identical model where I instead define $C_{ir}^{Support} = \sum_j \log(C_{ijr} Support_{jr})$ and $C_{ir}^{Oppose} = \sum_j \log(C_{ijr} Oppose_{jr})$. Using a Vuong (1989) closeness test, I find that the latter model is preferred ($p = 0.01$), which suggests that legislators are more likely to respond to a sum of contributions when it is derived from a greater variety of sources.[22]

Effects on different stages of the legislative process Do campaign contributions affect who introduces a bill, or who becomes a cosponsor in support of a bill? Table 1.6 reports the results for bill cosponsorship and Table 1.7 for bill sponsorship. Note that because many bills are never voted on, the sample is considerably larger when analyzing the sponsorship and cosponsorship of bills. Campaign contributions in support of a bill consistently affect cosponsorship: a ten percent increase in contributions is associated with a 3 percentage point increase in the probability of being the bill's cosponsor. Similarly, I find that contributions have a statistically significant effect on bill sponsorship. I show these relationships graphically in Figures 1.6 and 1.7.

1.4 Model

In this section, I describe the model of legislative voting and interest group influence. There are two types of players in the game: incumbent legislators $i = 1, ..., I$ and special interest groups

[22]Another way to rationalize this result is to suppose that interest groups offer other benefits to politicians when they fund them, such as votes (Bombardini and Trebbi, 2011). In this case, a greater amount of small contributions would have a larger effect because they imply a larger number of votes exchanged.

$j = 1, ..., J$. The game proceeds in two stages. First, interest groups simultaneously choose the amount of contributions to give to each legislator. Then, for every roll call vote, each legislator casts her vote taking her own ideological preferences into account as well as the contributions she has received. For each roll call, if a sufficient number of legislators vote in approval, the vote succeeds and otherwise it fails. Importantly, interest groups payoffs are tied to the success or failure of the roll calls.

Legislators

I extend the spatial model of voting (e.g. Clinton et al. (2004)).[23] In this framework, each legislator's ideological preferences can be summarized by her ideal policy bliss point in a finite dimensional space. Denoting the sequence of roll call votes by $r = 1, ..., R$, each vote is assumed to have a "yea" point ξ_r and a "nay" point ψ_r in the same space. The legislator i prefers to vote yea if her ideal point p_i is closer to ξ_r than ψ_r, and prefers to vote nay otherwise.[24]

Note that the model is silent with respect to the origin of these ideological preferences: they may be the product of personal, party and/or constituent interests. Moreover, the legislator cares about her own position-taking in this framework, rather than about the overall outcome of any vote. For example, this can arise if the legislator, seeking re-election, cares about how her constituents perceive her voting record.[25]

I extend this framework by modeling the economic influence of special interest groups. For each vote, the legislator takes into account the amount of contributions she has received from interest groups that support and oppose the bill. Let the binary variable y_{ir} denote whether the legislator decides to cast a supporting or opposing vote. The legislator's utilities from casting a yea or nay vote for r are:

$$u_i(y_{ir}) = \begin{cases} -\|p_i - \xi_r\|^2 + \gamma_i \omega\left(C_{i1r}^{Support}, ..., C_{iJr}^{Support}\right) + \eta_{ir}^{+} & \text{if } y_{ir} = 1 \\ -\|p_i - \psi_r\|^2 + \gamma_i \omega\left(C_{i1r}^{Oppose}, ..., C_{iJr}^{Oppose}\right) + \eta_{ir}^{-} & \text{if } y_{ir} = 0 \end{cases}$$

where the function $\omega(\cdot)$ aggregates contributions received from interest groups, γ_i denotes the legislator's responsiveness to contributions, and η_{ir}^{+} and η_{ir}^{-} are random preference shocks. The term $C_{ijr}^{Support} = C_{ij} Support_{jr}$ denotes contributions received in support for a given bill from a given interest group, and analogously $C_{ijr}^{Oppose} = C_{ij} Oppose_{jr}$ denotes contributions in opposition. In order to allow for diminishing returns and for the sources of contributions to matter, I assume

[23] Embedding the standard spatial model of voting within my model allows the results of this paper to engage directly with the large body of literature studying roll call voting, as it ensures that if I find that interest groups influence voting, this does not stem from differences in the modeling approaches.

[24] I ignore abstentions for parsimony, although the model can easily be extended to accommodate a third choice option for the legislator.

[25] The assumption that legislators do not have instrumental utility over vote outcomes is an important restriction that I make to keep the model tractable. Note that this assumption is also used in most empirical analyses of roll-call voting in the literature (Poole and Rosenthal, 1997). It is also a common feature in theoretical models of vote-buying (e.g. Dekel et al. (2009)) although other work relaxes this assumption (e.g. Dal Bo (2007)).

that $\omega(\cdot)$ is strictly increasing and concave in each of its arguments. This implies that given a sum of contributions, a legislator will be more prone to influence if they stem from a greater variety of sources. This assumption is consistent with the evidence found in the reduced-form analysis (section 1.3) as well as in the political science literature (Phinney, 2017).[26]

I let γ_i be legislator-specific to allow for the possibility that each legislator places a different weight on contributions relative to ideology in her decision-making. This heterogeneity could stem from district-level differences, for instance, if legislators in electorally competitive districts are more (or less) responsive to contributions. A differential impact of contributions could also stem from differences across legislators: for example, the political selection literature suggests that some legislators may be more prone to being influenced by special interests than others (Besley, 2006).

In Figure 1.8, I illustrate an example of a roll call vote. I assume that $\xi_r = -0.3$, $\psi_r = 0.3$, and that politician ideal points are measured in a one-dimension continuum from -1 to 1. Panel A shows the value and the probability of voting for the bill as a function the politician's ideology and whether or not she has received two \$20,000 contributions in support of the bill. The underlying assumption here is that γ_i is homogeneous across legislators, such that the effect of contributions on the probability of a yes vote is close to uniform across the ideological spectrum. In Panel B, I show an example where γ_i is larger for the more ideologically moderate politicians; unsurprisingly, we see larger effects of contributions on the probability of a yes vote for the centrist legislators in this case.

The legislator's total utility can be written as $U_i = \sum_r u_i(y_{ir}) + h(C_{i1},...,C_{iJ})$, where $h(\cdot)$ denotes the legislator's taste for contributions. In the model, this function will not play a role in the determination of the equilibrium strategies.

The voting stage

I next describe the voting stage of the game. I assume that the difference $\eta_{ir}^{+} - \eta_{ir}^{-}$ is i.i.d. with the distribution function F. This implies that each vote is an independent decision for the legislator. Prior to the random utility draws, the legislator's probability of voting in favor of r is:

$$\begin{aligned} P(y_{ir} = 1) &= P(U_i(y_{ir} = 1) \geq U_i(y_{ir} = 0)) \\ &= F\left(\beta_r p_i - \alpha_r + \gamma_i \Delta\omega_{ir}\right) \end{aligned} \tag{1.3}$$

where I define the vote-specific parameters $\alpha_r = \xi_r \xi_r' - \psi_r \psi_r'$ and $\beta_r = 2(\xi_r - \psi_r)$, and the net importance of contributions $\Delta\omega_{ir} = \omega(C_{i1r}^{Support},...,C_{iJr}^{Support}) - \omega(C_{i1r}^{Oppose},...,C_{iJr}^{Oppose})$. Henceforth, I will work with the parameters α_r and β_r rather than ξ_r and ψ_r in order to simplify the exposition. The newly defined parameters have intuitive interpretations. The parameter α_r denotes the *valence*

[26] I do not make explicit why a legislator's value from voting in a given direction is increasing in the support received by interest groups. It could be that contributions provide information to the legislator, that they provide access to lobbyists, or that the relationship between votes and contributions is derived from an underlying bargaining game. Instead of arguing for and imposing a specific channel of influence, I will instead allow contributions to operate in a flexible manner as captured by the politician-specific factor γ_i.

or popularity of the vote, whereas the parameter β_r denotes the *ideological divisiveness* of the vote, i.e. the extent to which the vote divides legislator by their ideologies.[27] Finally, $\Delta\omega_{ir}$ denotes the net support in contributions directed in favor of a bill to a particular legislator, where I omit the dependence on $C_{i1r}, ..., C_{iJr}$ for expositional clarity.

We will say that a vote *succeeds* if it has reached the sufficient threshold of individual votes for its success. In the House of Representatives, this will be a simple majority for a roll call vote on the passage of a bill and a two-thirds majority for motions to suspend the rules or to override presidential vetoes. Let d_r denote the vote requirement for success and let the binary variable s_r denote whether the vote succeeds or not. Then, the probability of a successful vote is:

$$P(s_r = 1) = P\left(\sum_i y_{ir} > d_r\right) \tag{1.4}$$

Since each y_{ir} is a draw from a Bernoulli distribution with a distinct probability, it follows that $\sum_i y_{ir}$ has a Poisson binomial distribution. Since it is computationally infeasible to calculate the probability in equation (1.4), I instead use the asymptotic distribution as an approximation. By the Central Limit Theorem of Lyapunov, it follows that the asymptotic distribution of $\sum_i y_{ir}$ is normal.[28] Therefore, I will assume that the probability of vote success is given by

$$P(s_r = 1) = 1 - \Phi\left(\frac{d_r - \mu_r}{\sigma_r}\right) \tag{1.5}$$

where $\Phi(\cdot)$ denotes the standard normal distribution function, $\mu_r = \sum_i P(y_{ir} = 1)$ is the expected number of supporting votes and $\sigma_r^2 = \sum_i P(y_{ir} = 1)(1 - P(y_{ir} = 1))$.

Special interest groups

Special interest groups have two motives to fund politicians. First, for each roll call vote, each group has a preference for whether or not the vote succeeds and hence allocate contributions to alter the probability of the vote's success. Second, I allow interest groups to have a separate taste for funding each politician, outside of the vote-buying motive. For example, an interest group may prefer to fund certain politicians for "consumption" purposes (Ansolabehere et al., 2003), or to increase their efforts in Congress on their behalf. In this sense, legislators who are members of important committees, are more experienced, or belong to the party that holds majority power will have more to offer.[29]

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[27] The parameters α and β are often referred to as difficulty and discrimination parameters in the item response theory literature.

[28] A similar argument is found in Lindbeck and Weibull (1987). A sufficient condition for the theorem to hold is that $\sum_{i=1}^{I} P(y_{ir} = 1)(1 - (P(y_{ir} = 1))) \to \infty$ as $I \to \infty$.

[29] An implicit assumption in the above argument is that interest groups buy influence only through the use of campaign contributions. In practice, other tools can be used to buy influence, such as lobbying, turning out votes or charitable giving. I assume that these choices are made independently of the contributions analyzed in this model.

Let us consider in more detail the two motives. Special interest groups seek to influence the votes of politicians in order to increase the probability that a favorable bill succeeds or to increase the probability that an unfavorable bill fails. Let b_{jr} denote the benefit to interest group j should vote r succeed. I normalize the benefit of a bill failing to 0. Therefore, b_{jr} denotes the net benefit to the interest group relative to the status quo and may take positive or negative values. In addition, the interest group receives a benefit $\rho_{ij}g(C_{ij})$ from funding each politician. The interest group's utility function is:

$$V_j = \sum_r P(s_r = 1) b_{jr} + \sum_i \rho_{ij} g\left(C_{ij}\right) - \sum_i C_{ij} \tag{1.6}$$

where C_{ij} denotes the contribution from interest group j to legislator i. I assume that $g(\cdot)$ is strictly increasing and concave.

Note that this formulation allows interest groups to have more flexible preferences over bill outcomes than if we projected their preferences onto an ideological space like the legislators. This is important because empirically, many industries contribute to both parties and hence are ideologically centrist, yet support different types of legislation. For example, the electric utilities and steel production industries may both be ideologically moderate, but they tend to support and oppose distinct sets of bills.

Equilibrium

The solution concept is the subgame perfect Nash equilibrium. To define it we require some more notation. Let $\mathbf{y}_i = (y_i)_{r=1}^R$ denote the vector of a legislator's votes and $\mathbf{y}_{-i}$ denote all other legislators' votes. Similarly, let $\mathbf{C}_j$ denote the vector of an interest group's contributions and $\mathbf{C}_{-j}$ denote the contribution vectors of all other interest groups. An equilibrium consists of legislator vote vectors $(\mathbf{y}_1, ..., \mathbf{y}_I)$ and special interest group contribution vectors $(\mathbf{C}_1, ..., \mathbf{C}_J)$ such that, (i) $\mathbf{y}_i$ maximizes U_i given $\mathbf{y}_{-i}$, $\mathbf{C}_1, ..., \mathbf{C}_J$ for $i = 1, ..., I$, (ii) in light of the anticipated outcome of the second-stage, $\mathbf{C}_j$ maximizes V_j given $\mathbf{C}_{-j}$ for $j = 1, ..., J$ and (iii) $C_{ij} \geq 0$ for all i, j.

In the second stage of the game, I have already established the unique equilibrium strategies of the legislators. That is, conditional on the matrix of contributions $\mathbf{C}$, $y_{ir} = 1$ if $u_i(y_{ir} = 1|\mathbf{C}) \geq u_i(y_{ir} = 0|\mathbf{C})$ and $y_{ir} = 0$ otherwise.

In solving the first stage of the game, the interest groups take as given the best responses of legislators. Rather than fully characterize the equilibrium, I will consider the first order conditions of interest group j's maximization problem. Necessary conditions for interior equilibrium contributions are:

$$\sum_r b_{jr} \frac{\partial P(s_r = 1)}{\partial C_{ij}} + \rho_{ij} g'(C_{ij}) = 1 \quad \forall i \tag{1.7}$$

This condition states the interest group will allocate contributions across politicians until the marginal benefit of giving to each politician equals the marginal cost of 1. The marginal benefit from donating is given by the sum of two terms. The first term is equal to the sum of the interest group's valuation of each vote's success multiplied by the extent to which its contribution to the legislator affects the probability of altering the vote's outcome. The second term is the marginal benefit from funding the legislator for reasons other than voting.

I derive the following equation for the partial derivative of the probability of vote success with respect to contributions:

$$\frac{\partial P(s_r = 1)}{\partial C_{ij}} = \frac{1}{\sigma_r}\phi\left(\nu_r\right) f\left(u_{ir}\right)\gamma_i \frac{\partial \Delta\omega_{ir}}{\partial C_{ij}}\left[1 + \frac{\nu_r}{\sigma_r}\left(\frac{1}{2} - F\left(u_{ir}\right)\right)\right] \quad \forall i \tag{1.8}$$

where

$$u_{ir} = \beta_r p_i - \alpha_r + \gamma_i \Delta\omega_{ir}$$
$$\nu_r = (d_r - \mu_r)/\sigma_r,$$

$\phi(\cdot)$ denotes the standard normal density and f is the density of the distribution function F. This equation shows that the marginal effect of contributions on the success of a vote is given by the sum of two terms: respectively, the effect through the expected number of votes μ_r and the variance σ_r^2.

Consider the first effect. It is the product of three factors. The first factor $\phi(\nu_r)/\sigma_r$ measures the marginal effect of an additional vote on the probability of success. This term is increasing in the density $\phi(\nu_r)$, which is increasing in the expected closeness of the vote. The interest group takes every other group's contributions into account when computing this expectation. The second factor $f(u_{ir})$ measures the expected marginality of the legislator the interest group is seeking to influence, again taking the contributions of other groups into account. The closer the legislator is to indifference between a yea or nay vote, the more likely a contribution will affect his vote.[30] The third factor $\gamma_i \frac{\partial \Delta\omega_{ir}}{C_{ij}}$ measures the overall influenceability or responsiveness of the legislator to campaign contributions relative to ideology. This factor includes γ_i, which can vary flexibly across legislators.

Consider now the second term in equation (1.8), which measures how contributions affect the probability of a vote success through the variance of the overall vote. This term is the product of four factors, where the first three are the same as above. To understand the intuition behind the fourth factor, $\frac{\nu_r}{\sigma_r}\left(\frac{1}{2} - F\left(u_{ir}\right)\right)$, consider an interest group that supports a vote for the passage of a bill. In this case, if the vote is expected to fail, the interest group has an incentive to contribute more to those legislators who are more likely to vote against the bill, whereas if the vote is expected to pass, the group has incentive to contribute more to those legislators who are more likely to vote for the bill. This is because the probability of winning is locally convex in the former case and concave in the latter. There are similar results in work studying vote-buying in legislatures (Snyder, 1991) and the incentives created by the Electoral College (Stromberg, 2008).

1.5 Empirical Strategy

I take the following two-step approach to estimate the model. First, I use the equilibrium vote equation to estimate the vote parameters α_r, β_r, legislator ideal points p_i and legislator responsive-

[30]This result is analogous to previous findings in the vote-buying literature. For example, Snyder (1991) finds that interest groups should focus their resources on legislators that slightly oppose a policy rather than on those who strongly support or oppose it. See also Groseclose and Snyder (1996); Dekel et al. (2008, 2009).

ness to contributions γ_i. In order to mitigate concerns of the potential endogeneity of contributions, I will use an instrumental variable strategy. Second, I will use the first-step parameter estimates together with the equilibrium conditions for contributions to estimate the valuation of vote outcomes by special interest groups.

Specification and identification

In equilibrium, we can describe a legislator's vote choice with the latent variable framework:

$$y_{ir} = \begin{cases} 1 & \text{if } y_{ir}^* > 0 \\ 0 & \text{if } y_{ir}^* \leq 0 \end{cases} \tag{1.9}$$

where

$$y_{ir}^* = \beta_r p_i - \alpha_r + \gamma_i \Delta\omega_{ir} + \eta_{ir} \tag{1.10}$$

is the unobserved latent variable (see equation (1.3)). I make three assumptions in the first step.

First, consider the influenceability parameter γ_i. From equation (1.10), we see that γ_i is identified by the covariance between $\Delta\omega_{ir}$ and y_{ir}. The key challenge to identification is the possibility that the contributions received by a politician are correlated with his unobserved preference shock for a bill, i.e. if $Cov(\Delta\omega_{ir}, \eta_{ir}) \neq 0$. In order to overcome this hurdle, I specify the following equation:

$$\Delta\omega_{ir} = z_{ir}\Pi + \xi_{ir} \tag{1.11}$$

where the unobserved ξ_{ir} may be correlated with η_{ir} and z_{ir} are instruments that satisfy the exclusion restriction $Cov(z_{ir}, \eta_{ir}) = 0$. Since y_{ir} is a binary dependent variable, I specify the joint distribution of (η_{ir}, ξ_{ir}) to be normally distributed with mean zero and variance-covariance matrix:

$$Var(\eta_{ir}, \xi_{ir}) = \begin{bmatrix} 1 & \tau \\ \tau & \sigma_\xi^2 \end{bmatrix}$$

Here I normalize the variance of η_{ir} to 1 as is commonly done for probit models.

Second, I assume that the ideological space is unidimensional. In this case, the vote parameters $\{\alpha_r, \beta_r\}$ and the ideal points p_i are only identified up to a scale and location normalization. A solution to this problem is to pin down the ideal points of two legislators. I will follow this convention by setting tight priors of $p = 1$ for Rep. Trent Franks (R), a right-wing legislator, and $p = -1$ for Rep. Barbara Lee (D), a left-wing legislator. This has two consequences to our interpretation of the results: first, this implies that more positive values of p indicate a more conservative rather than liberal ideology, and second, this pins down the scale of the ideal points, such that a representative with an ideal point of 0.5 is located closer to Franks than to Lee.

Third, I specify the functional form $\omega(C_{i1}, ..., C_{iJ}) = \sum_j \log C_{ij}$. This allows for diminishing returns to contributions and for legislators to be more prone to influence when contributions stem from a variety of sources. Both of these properties are consistent with the results of the reduced-form analysis.[31]

I next consider the second step of the estimation. Previously, I derived necessary conditions for interior equilibrium contributions of special interest groups (see equations (1.7)-(1.8) in section 1.4). Accordingly, a special interest group contributes more to those legislators which are the most ideally positioned in Congress to advance its agenda. Assuming log functional forms for $\omega(\cdot)$ and $g(\cdot)$, the contributions allocated from an interest group j to a legislator i satisfy the condition:

$$C_{ij} = \max\left\{0, \sum_r \frac{\partial P(s_r = 1)}{\partial \Delta\omega_{ir}} position_{jr} v + \zeta_i + w_{ij}\delta + \varepsilon_{ij}\right\} \tag{1.12}$$

I assume that $b_{jr} = q_{jr}v$, where q_{jr} is a categorical variable which takes values of 1 if the interest group supports the bill, -1 if it opposes it and 0 otherwise. This implies that the valuation of the outcome of a vote v is homogeneous across interest groups which take a position on a bill. I also parametrize $\rho_{ij} = \zeta_i + w_{ij}\delta + \varepsilon_{ij}$, which I describe in more detail shortly.

Equation (1.12) shows that the vote valuation v is identified by the covariance between interest group contributions to legislators and what I shall name the "legislative voting power" of a politician for a given group. In words, legislative voting power is equal to the sum of the marginal benefits of contributions through the probability of flipping the outcome of each vote that an interest group values. I compute this term using the first-stage estimates together with the data on contributions.[32]

The parameter ρ_{ij} captures the value of giving contributions to a legislator for all reasons other than vote buying. w_{ij} are observable characteristics for legislator-interest group pairs, δ is a vector of coefficients to be estimated, ζ_i are legislator-specific intercepts and ε_{ij} is a normal i.i.d. error term with variance σ_ε^2. Thus, ζ_i will flexibly capture legislator-specific factors, such as her years of experience, powerful position(s) in Congress, or any unobservable characteristic that affects the return of giving to this legislator. Furthermore, I include in w_{ij} dummy variables that indicate, from the perspective of group j, whether or not politician i is in a relevant committee, ideologically aligned, or ideologically opposed. These are meant to capture the fact that interest groups have a greater incentive to fund politicians with relevant committee assignments, and may also be more or less likely to contribute to a politician with similar or opposed ideological preferences.[33]

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[31] I take $\log(C+1)$ to deal with the case where C is zero. I also made use of the alternative functional form $\text{asinh}(C)$ and found very similar results.

[32] I assume that the observed set of contributions are a pure-strategy equilibrium of the contributions game played by the interest groups. Since the optimization problem is not globally concave, I verify that for each positive solution to the first-order condition, the interest group is better off making this contribution to the politician than no contribution at all.

[33] I measure the committee variable using the procedure to match interest groups to committees in Fouirnaies and Hall (2018). To measure whether interest groups and legislators are ideologically aligned, I consider whether the interest group funds both parties or a single party. If an interest group solely funds one party, then it is ideologically aligned with members of that party, and ideologically opposed to members of the opposing party.

Estimation

I estimate the model for each of the 110th to 114th Congresses of the U.S. House of Representatives. Let $\Psi = \{y, \Delta\omega, z, w, q\}$ denote the data, where y is a matrix of roll call votes, $\Delta\omega$ is a matrix of interest group contributions, z is the matrix of instruments, and w are the controls in the contributions equation. Unlike in the reduced-form where I restrict the sample of roll call votes, here I will estimate the model using all roll call data, including votes for which every legislator's contributions are coded as zeros. Therefore the ideal points will be estimated using the full sample of roll call data. I will use the same shift-share instruments as I used in the reduced-form analysis (see section 1.3).

In the first step, I estimate the parameters $\theta = \{\alpha_r, \beta_r, p_i, \gamma_i, \sigma_\xi, \tau, \Pi\}$ given data Ψ. Given the equations (1.9), (1.10) and (1.11), I derive the first-step log-likelihood function:

$$\log \mathscr{L}(\theta|\Psi) = \sum_i \sum_r \left[y_{ir} \log \Phi(x_{ir}) + (1 - y_{ir}) \log(1 - \Phi(x_{ir})) + \log \phi \left(\frac{\Delta\omega_{ir} - z_{ir}\Pi}{\sigma_\xi} \right) - \log \sigma_\xi \right] \tag{1.13}$$

where

$$x_{ir} = \frac{y^*_{ir} + \tau(\Delta\omega_{ir} - z_{ir}\Pi)/\sigma_\xi}{(1 - \tau^2)^{\frac{1}{2}}} \tag{1.14}$$

and $\phi(\cdot)$ and $\Phi(\cdot)$ denote respectively the standard normal density and distribution functions. In the second-step I estimate the remaining parameters $\{v, \xi_i, \delta, \sigma_\varepsilon\}$ using the censored regression model (1.12).

To estimate the model, I adopt a Bayesian Markov Chain Monte Carlo (MCMC) approach for two reasons. First, due to the large number of parameters and the complexity of the likelihood function, it is difficult to numerically maximize the likelihood. Not only do maximization algorithms often fail to converge, even if they do succeed they are not guaranteed to have attained the global maximum (Train, 2009). The Bayesian MCMC approach on the other hand does not require the maximization of any function. Second, since the Bayesian approach is standard practice in the estimation of spatial models of voting, it allows me to rule out the possibility that the results stem from methodological differences with previous work. In any case, it has been shown that the Bayesian approach yields similar if not identical results to alternative frequentist approaches in similar contexts (see Clinton and Jackman (2009) for a discussion on this issue).

A concern with the Bayesian approach is that the results may be affected by the choice of priors. To address this potential issue, I set uninformative priors. Similar to previous studies on roll-call voting, I set the priors of α and β to be distributed $N(0, 25I)$, where I denotes the identity matrix. I set the priors of the remaining parameters to be distributed $N(0, 10I)$.[34] I will show in section 1.6 that the results are unlikely to be driven by the priors.

The estimates I present are based on 10 Markov chains, initialized randomly with a diffused set of initial parameters, each with 20,000 draws with the first 10,000 from each chain discarded

[34]Since σ_ξ and σ_ε must be positive, I use a truncated normal distribution for these parameters.

as burn-in.[35] To assess convergence, I compute the Potential Scale Reduction Factor (Gelman and Rubin, 1992) and check that the values are close to 1+. I also visually inspect trace plots for the Markov chains and do not find anomalies.

1.6 Results

In this section, I present the results of the structural estimation described in Section 1.5. To streamline the discussion, I focus on the 112th House of Representatives unless stated otherwise.

Model fit

I begin by assessing the model's fit in the following three steps. First, I inspect the posterior of legislator ideal points. Figure 1.9 plots the marginal posterior of the ideal points pooling every representative in the sample, together with its associated prior. Consistent with previous research, we see that the posterior follows a bimodal distribution, with Democrats on the left, Republicans on the right, and little to no overlap between the members of the two parties.

Second, I compare these findings to those previously obtained in the literature. To this end, I compare the posterior mean of each legislator's ideal point with the commonly used Nominate ideal point estimates (Poole and Rosenthal, 1985). The latter are estimated using a spatial model of voting which does not include interest group influence. I plot the posterior means I obtain against the Nominate estimates in Figure 1.10. I find that the two sets of estimates are very similar, with a Pearson correlation coefficient of 0.98. This correlation is strong for both Democrats (0.87) and Republicans (0.88).

Third, I assess to what extent the model accurately predicts the outcomes of roll call votes. For each vote, I classify it as a predicted yea vote if the estimated probability of a yea vote is at least 0.5, and classify it as a predicted nay vote otherwise. I find that the model correctly classifies 92.4 percent of all roll call votes, indicating that the spatial model I estimate fits the data well.

Effects of interest group contributions on votes

Baseline estimates I next investigate this paper's main research question: are legislative votes influenced by special interest group contributions? To answer this question, I begin by analyzing the estimates of a baseline model where it is assumed that each legislator is equally responsive to contributions, i.e. $\gamma_i = \gamma$. Specifically, I will test the null hypothesis that $\gamma \leq 0$ to the alternative hypothesis that $\gamma > 0$.

Figure 1.11 plots the marginal posterior of γ together with its associated prior. Since the density of the posterior is much more concentrated than that of the prior, we can be confident that the results

[35] Specifically, I use Hamiltonian Monte Carlo (HMC) sampling, a form of MCMC sampling, as implemented by the Stan programming language (Carpenter et al., 2017). In short, in order to generate efficient transitions spanning the posterior, this method uses the gradient of the log probability function that is being sampled. It uses an approximate Hamiltonian dynamics simulation based on numerical integration. A Metropolist reject step is then applied.

are being driven by the data rather than by the prior. Formally testing the null against the alternative hypothesis, I reject the null with over 99 percent confidence. Therefore, my results are consistent with the hypothesis that legislative voting is influenced by special interest groups.

To interpret the effect size, consider a roll call vote with median ideological divisiveness in this sample (i.e. $|\beta_r| = 4.47$). I ask the following question: how many contributions would be required so that the median (ideological) Democrat would have the same probability of voting for the bill as the median Republican? I compute this amount to be 56 separate \$5000 contributions or 77 separate \$500 contributions. The large magnitudes reflect the relative importance of ideology compared to special interest group contributions. However, there are also votes which are far less divisive. For example, for a vote at the 10th percentile of ideological divisiveness ($|\beta_r| = 1.04$), the required contribution amounts to flip the Democrat would shrink by 77%. I will return to the question of the economic significance of these results when I discuss the policy counterfactuals.

Has influenceability changed over time? With the growing polarization of Congress, it may be the case that partisanship and ideology have increased in importance relative to moneyed interests (McCarty et al., 2016). On the other hand, it could also be the case that influenceability has risen in recent years, for example if politicians have become more willing to exchange favors for contributions given the rising costs of campaigns. Plotting the mean posteriors of influenceability for each congressional term from the 110th (2007-2008) to the 114th (2015-2016) in Figure 1.12, I do not find a discernible pattern in the average influenceability of Congress over time.

Full model with heterogeneity I next present the results for the full model in which each legislator may differ in his or her responsiveness to contributions γ_i.[36] I report summary statistics in Table 1.8. Two notable patterns emerge. First, the median estimates of legislator influenceability are remarkably consistent across congressional terms and across parties. Second, there is considerable variation in influenceability within both parties, and within any congressional term.

I next examine in more detail the heterogeneity in influenceability across legislators by exploring which factors are correlated with it. A first possibility it that ideology can operate as a commitment against the influence of special interest groups: for example, Mian et al. (2010) found that for the vote on the Emergency Economic Stabilization Act, interest group contributions did not influence the most ideologically conservative representatives, but they did affect the votes of the moderates. In the context of my model, this could be the case if ideologically extreme legislators primarily receive contributions for consumption rather than vote-buying. Therefore, they may still receive contributions despite being less responsive to them in their voting behavior (i.e. a higher value of ρ_{ij} may make up for a lower γ_i). In order to explore these potential relationships in more detail, I will focus on explaining the variation in influenceability in the cross-section of legislators of the 112th House of Representatives.

Figure 1.13 plots the ideal point estimates against influenceability, where each circle represents a bin of 10 Democrats and each diamond a bin of 10 Republicans. We see that unlike ideology, there is a lot of overlap in influenceability across both parties. Moreover, there does not emerge

[36]In the estimation of this version of the model, I impose the additional structural restriction that $\gamma_i > 0$ for each legislator by restricting the domain of the prior to positive values.

an obvious monotonic pattern between ideology and influenceability: we cannot say that more conservative legislators are generally more or less responsive to contributions by inspecting this figure. However, for Republicans it appears that the more ideologically extreme legislators are less influenceable than the moderates.

I examine these links more formally by regressing influenceability on politician and district characteristics. Table 1.9 reports the results. In the first column, we see that ideological extremeness is negatively correlated with influenceability. I disaggregate this relationship across parties in column 2, which shows that ideological extremeness only mitigates influenceability for Republicans. The effect sizes are quite large: increasing the ideological extremeness of a Republican by 1 standard deviation reduces his influenceability by 0.4 standard deviations.

I next explore other potential correlates of influenceability. In column 3, I add politician characteristics which relate to his or her power in Congress: whether the representative is a member of the majority party leadership, is a committee chair, or is a member of a powerful committee.[37] Since these members of Congress are more powerful, they are the target of more intensive lobbying by special interest groups. It is not clear however, whether this should spill over to influencing votes, where each member's vote is equally important. I find that these characteristics generally predict higher levels of influenceability. This suggests there are interactions between different influence strategies employed by special interest groups, from the lobbying of congressmen during the earlier stages of the legislative process to the influence of votes. Investigating the strategic interactions between these influence activities could be an interesting avenue for future research.

In column 4 I add other personal characteristics (years of congressional experience, age, gender) and whether the legislator represents a competitive district.[38] Interestingly, experience is negatively correlated with influenceability. Two potential explanations are that voters select less influenceable candidates over time or that politicians with more legislative experience represent safer districts. However, the latter is less likely to be a potential explanation as I do not find that the competitiveness of a district predicts influenceability.[39] Finally, I do not find that the age or gender of a politician are significant predictors of influenceability.

Special interest group valuation of bills

Second-step estimates The equilibrium conditions (1.12) for contributions provide an additional statistical test of the influence hypothesis. In this case, the null hypothesis is that interest groups do not allocate contributions across legislators in a way that is consistent with the model's predictions. Specifically, I test the null hypothesis that $v \leq 0$ against the alternative hypothesis that $v > 0$.

I first test the hypothesis graphically. Consider the contributions made from each interest group to each legislator. For this sample, Figure 1.15 displays the relationship between contributions

[37]I code the following as powerful committees: Appropriations, Rules, and Ways and Means.

[38]I define a competitive district with a dummy for districts where the Democratic presidential vote share is within 10 percentage points of 50. Varying this definition does not affect the results.

[39]This null result is not necessarily surprising. While legislators who represent competitive districts may value contributions more, they are also more likely to value the preferences of their constituents. Therefore, it is theoretically ambiguous whether they should prefer to trade off more votes to appeal to interest groups.

and different components of legislative voting power. We see that interest groups respond to the incentives given by the majority-voting rule. The more contributions from an interest group to a legislator is likely to flip the overall outcome of votes the interest group cares about, the more it contributes to this politician. Note however that this figure does not adjust for the fact that certain legislators may receive more contributions for other reasons than voting. To address this, I estimate v in the second-step including the controls ρ_{ij}.

Figure 1.14 plots the posterior distribution of the valuation v. This parameter measures the average value placed by an interest group on the outcome of a vote for which it takes a position on. The posterior mean for the value of a vote is \$5.24M (standard deviation = 0.24). Since the posterior of v places close to no mass on the interval $v \leq 0$, I reject the null hypothesis. The data are consistent with the model's predictions for how interest groups should optimally allocate contributions.

Policy counterfactuals

How economically significant are the effects of contributions on votes? To further address this question, I compare the outcomes of the votes as predicted by the model to those that would arise in an environment without interest group influence. Equivalently, I compare how outcomes would change under a policy where political contributions from special interest groups are banned.

More precisely, let $\pi(\theta)$ denote the joint posterior of the parameters. I compare voting outcomes under the observed profile of contributions $\mathbf{C}$ to those under a ban to contributions which sets $\mathbf{C} = \mathbf{0}$. Specifically, I compute the following:

$$\sum_i \sum_r \left| \int P(y_{ir} = 1|\theta, \mathbf{C})\pi(\theta)d\theta - \int P(y_{ir} = 1|\theta, \mathbf{0})\pi(\theta)d\theta \right| \tag{1.15}$$

This object will indicate the expected number of roll call votes which would have different outcomes in a counterfactual environment with no interest group contributions.

An advantage of the structural approach is that I fully specify the structure of the polity together with the uncertainty in the environment. Therefore, I can also examine the effect of the policy on the overall outcome of votes in addition to the specific votes of legislators. Formally, I estimate the expected number of different vote success outcomes:

$$\sum_r \left| \int P(s_r = 1|\theta, \mathbf{C})\pi(\theta)d\theta - \int P(s_r = 1|\theta, \mathbf{0})\pi(\theta)d\theta \right| \tag{1.16}$$

Using the above methodology, I find that the expected number of different votes per legislator to range from 3.2 to 7.0 (out of 1000-1400) votes per congressional term (see Table 1.10). This translates to 1.4 to 2.8 votes per roll call differing on average (out of about 440 representatives). The small magnitude of these figures can be explained by the fact for most votes in each Congress, no interest group has taken a position and therefore banning contributions would have no effect. If we restrict the sample of roll call votes to those where at least one interest group has taken a position, I find that on average 9.7 to 23.8 votes per roll call differ under the PAC contributions ban.

These numbers translate to an expected difference in overall vote success outcomes ranging from 2.9 to 18.8, depending on the Congress. It is interesting to note that these figures show more variance across time than those for individual legislator votes, highlighting the importance of each term's agenda. For example, the smallest figure (2.9) stems from the last Congress of the Obama presidency, which was particularly divisive.

We can also examine the extent to which these figures are smaller due to fact that for a typical bill, there are interest groups supporting each side and hence are partially "canceling" each other's influence. To this end, I also compute outcomes under two alternative counterfactual scenarios, where respectively, only opposing or only supporting contributions remain (see Table 1.10, Panels B and C). With these results, I can then estimate the extent to which the expected difference in outcomes is smaller due to there being contributions on both sides of many votes. I find that on average, 26 to 37 percent of the effects on individual votes are canceled out, compared to 25 to 55 percent of the effects on vote outcomes.

I further explore which bills are likely to have had different outcomes under the hypothetical ban to PAC contributions. In Table 1.11, I briefly describe for each Congress two votes for which there is the greatest probability of an alternative outcome. For each one of these votes, I compare the actual result of the roll call vote to the expected outcome predicted by the model and finally to the expected outcome predicted under the counterfactual ban. We see that although these are extreme cases, the model fits the data well. We also find that the counterfactual outcomes are starkly different.

Let us consider the passage vote in the 113th Congress on the *Cyber Intelligence Sharing and Protection Act* as an example. The stated purpose of the bill was "to provide for the sharing of certain cyber threat intelligence and cyber threat information between the intelligence community and cybersecurity entities, and for other purposes". This bill saw both support and opposition by many special interest groups. For instance, several large telecommunications and information technology companies backed the bill, such as Microsoft, Facebook and AT&T, whereas various civil rights as well as conservative groups opposed it.[40] On net however, the contributions donated by the supporting groups were an order of magnitude larger than those by opposing groups. The outcome of the vote saw both bipartisan support and opposition, as 196 out of 225 Republicans and 92 out of 190 Democrats voted yea. Therefore, this was a vote where not only was there considerable support by special interest groups, the ideological dimension is lacking in its explanation of the variation in support across legislators. Under the hypothetical contributions ban, I find that the expected number of yea votes is reduced to just 108, indicating that the bill would have likely failed to pass.

[40] For example, see https://www.washingtonpost.com/business/technology/cispa-whos-for-it-whos-against-it-and-how-it-could-affect-you/2012/04/27/gIQA5ur0lT_story.html.

Average rate of return to contributions

These results allow me to address the Tullock puzzle in a novel way (Tullock, 1972). Gordon Tullock posed the puzzle: if the value of policy is worth so much, and campaign contributions influence policy, why is there so little money in politics? Three decades later, Ansolabehere et al. (2003) noted that although the sum of campaign contributions had reached \$3 billion in the 2000 elections, this amount still paled in comparison to the potential benefits accrued from federal government spending. Among several examples presented in the paper, a particularly striking one concerned the dairy industry. Although dairy producers gave \$1.3 million in contributions in 2000, they received price supports close to \$1 billion dollars in the 2002 Farm Bill. Taken at face value, this would imply a rate of return to contributions of close to \$770 per dollar, or 77,000%. However, such a calculation ignores the initial predispositions of each legislator with respect to the bill. To more accurately assess the returns to contributions, a better approach would be to consider the difference in the likelihood of the bill passing with or without the contributions by dairy producers. Moreover, one should also consider in the calculation the equilibrium responses of other interest groups should the dairy producers withdraw their contributions.

The structural estimates allow me to follow this improved approach. An interest group j's expected return to contributions is:

$$\frac{E\left[V_j|\mathbf{C}_j,\mathbf{C}_{-j}\right]-E\left[V_j|\mathbf{0},\tilde{\mathbf{C}}_{-j}\right]}{\sum_i C_{ij}}$$

where $\tilde{\mathbf{C}}_{-j}$ denotes equilibrium contributions from all other interest groups when $\mathbf{C}_j = 0$.

I compute this object using the posterior $\pi(\theta)$. I estimate the average expected return to contributions to be 17%.[41] This estimate is several orders of magnitude lower than that produced by the naive calculation presented above. In this light, this paper proposes another answer to the question: Why is there is so little money in politics? On the one hand, in line with previous studies, I find that interest groups place a substantial value in the outcomes of bills. On the other hand, legislation requires the passing bills, and although I find evidence that contributions affect individual votes, the probability that an interest group's contributions are the decisive factor that changes the outcome of any bill is small.

1.7 Discussion

This paper investigated the effects of interest group contributions on legislative voting. Using new data on interest group positions, I first found evidence of a robust reduced-form relationship between campaign contributions from special interest groups and legislative voting. I then developed and estimated a structural model of voting in an environment where politicians are influenced by the campaign contributions of interest groups. Estimating this model provided further supporting

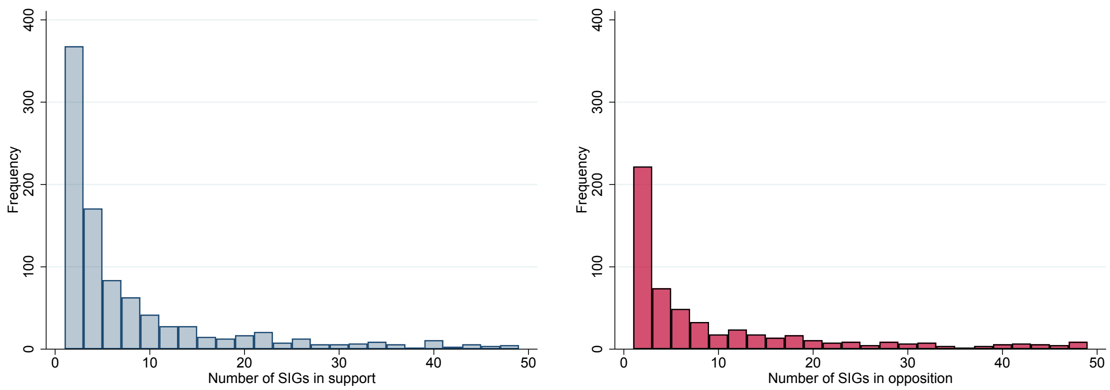
[41] My estimate is significantly lower than the 155% estimated in Kang (2015). Interestingly, although Bombardini and Trebbi (2011) use a very different methodology, they find a similar (median) rate of return of 13% when both money and votes are accounted for.

evidence of the hypothesis that interest groups influence votes, and that contributions are allocated in a way that is consistent with the model of vote buying. Moreover, the structural estimates revealed a pattern between legislator ideological ideal points and responsiveness to contributions: moderate Republicans were more influenceable than ideologically extreme Republicans, whereas no such relationship was found among Democrats.

The structural estimates were then leveraged to address two additional questions. First, I investigated the potential outcomes of a ban to PAC contributions. Here, I found that although most votes would have gone unchanged, there are several highly salient bills which would have likely failed without special interest groups. Second, I computed the average rate of return to contributions to be 17%. This result addresses the Tullock puzzle in a novel way, indicating that perhaps political contributions are not so large because although the value of policy is high, it is unlikely that a contribution will be the decisive factor that determines a bill's outcome.

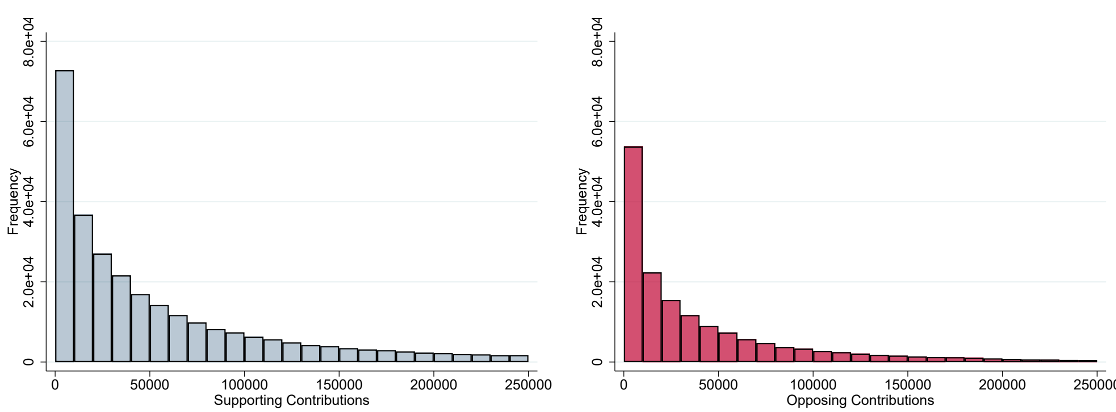
1.8 Figures and Tables

Figure 1.1: Special interest group positions on bills



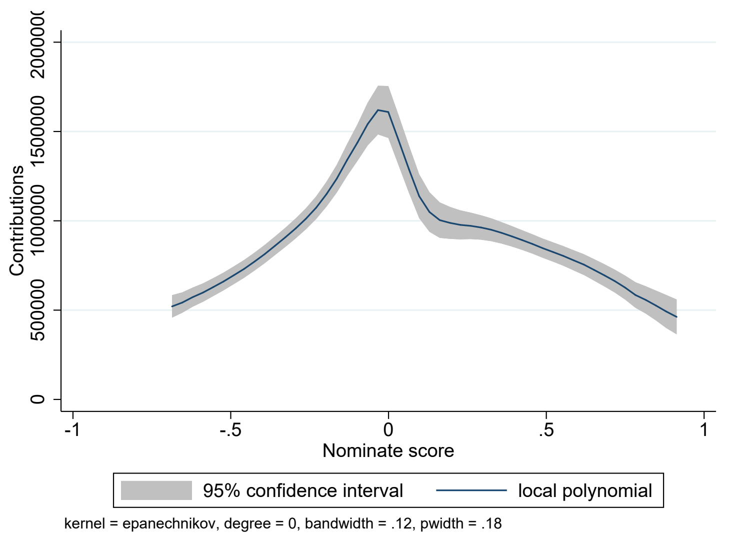
Notes: The unit of observation is a bill. This figure displays the distribution of the total number of supporting and opposing special interest groups for each bill.

Figure 1.2: Special interest group contributions by vote-legislator



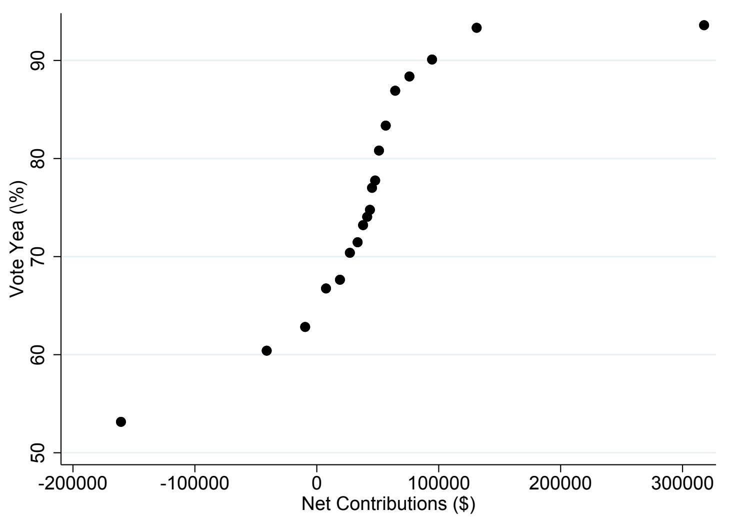
Notes: The unit of observation is a legislator-vote pair. This figure displays the distribution of the total amount of supporting contributions and opposing contributions for each pair.

Figure 1.3: SIG contributions and politician ideology



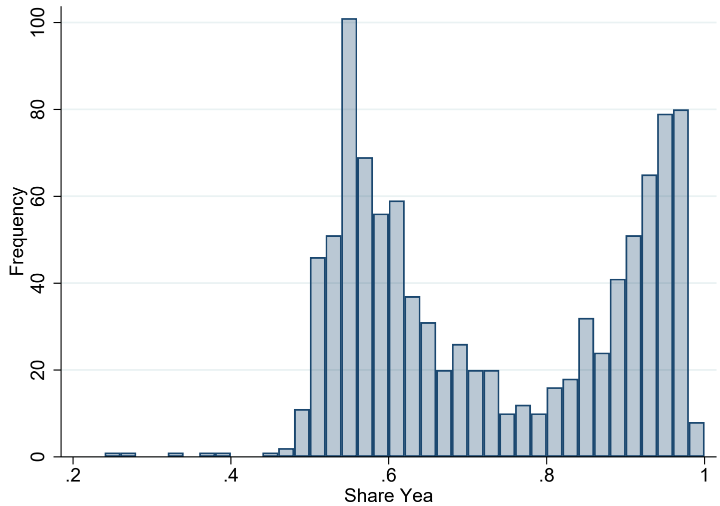
Notes: This figure plots a local polynomial regression of aggregate interest group contributions on politician ideology, as measured by the first dimension of the Nominate score.

Figure 1.4: Roll call voting and campaign contributions



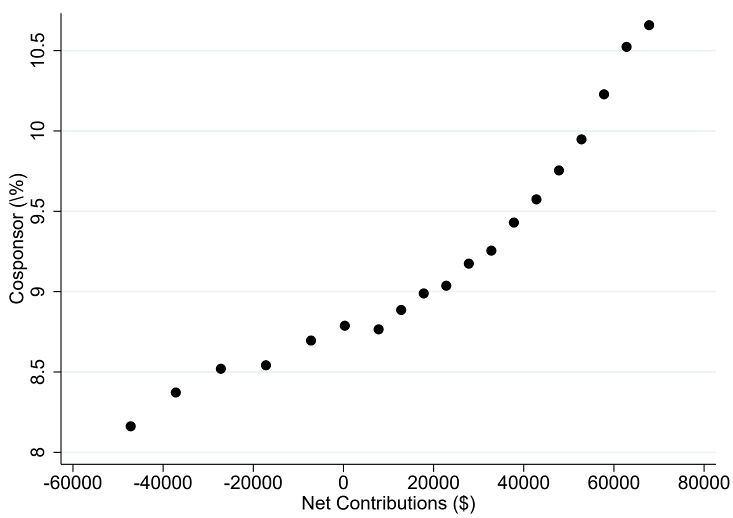
Notes: The dependent variable is a "Yea" roll call vote for the passage of a bill and the independent variable is the net contributions the politician received in support of the bill. Both variables are residuals from regressions on vote fixed effects. The data are then aggregated into 20 equal sized bins.

Figure 1.5: Distribution of roll call vote results



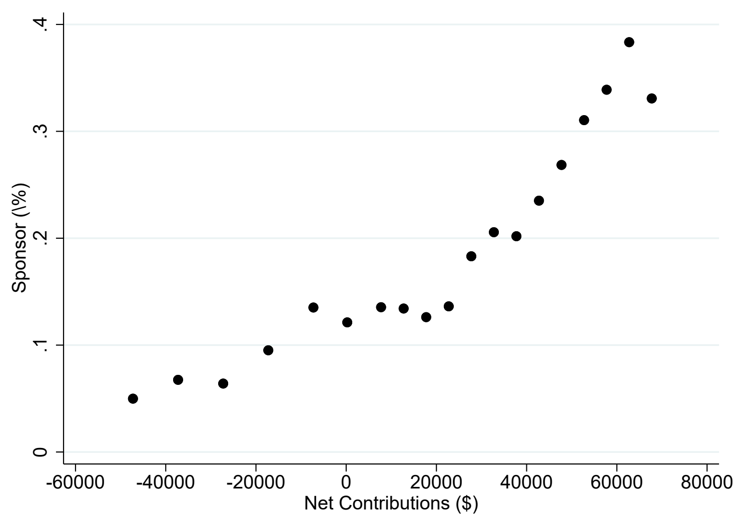
Notes: For each roll call vote, the share of "yea" votes are computed. This figure displays the distribution of this variable across the roll call votes in the sample.

Figure 1.6: Bill cosponsorship and campaign contributions



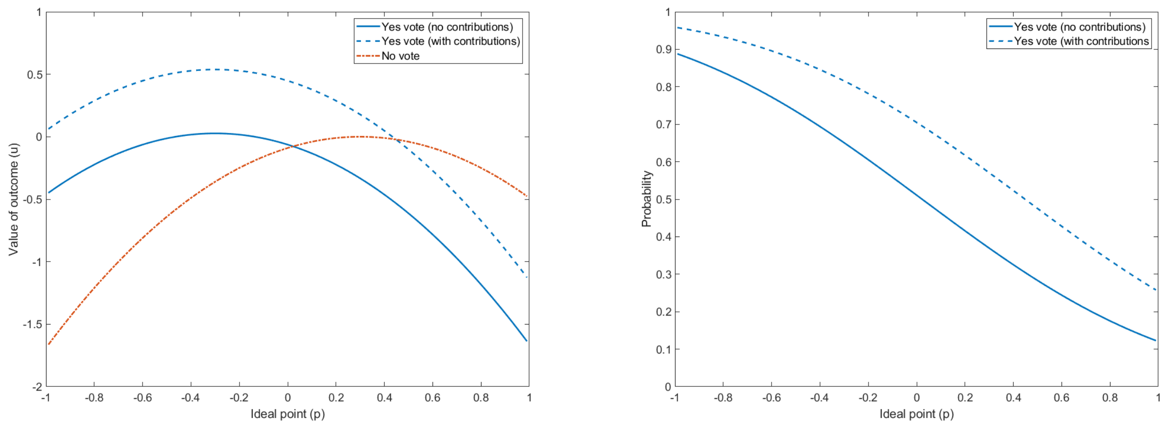
Notes: The dependent variable is the cosponsorship for a bill and the independent variable is the net contributions the politician received in support of the bill. Both variables are residuals from regressions on politician fixed effects $\times$ vote characteristics and vote $\times$ party fixed effects.

Figure 1.7: Bill sponsorship and campaign contributions

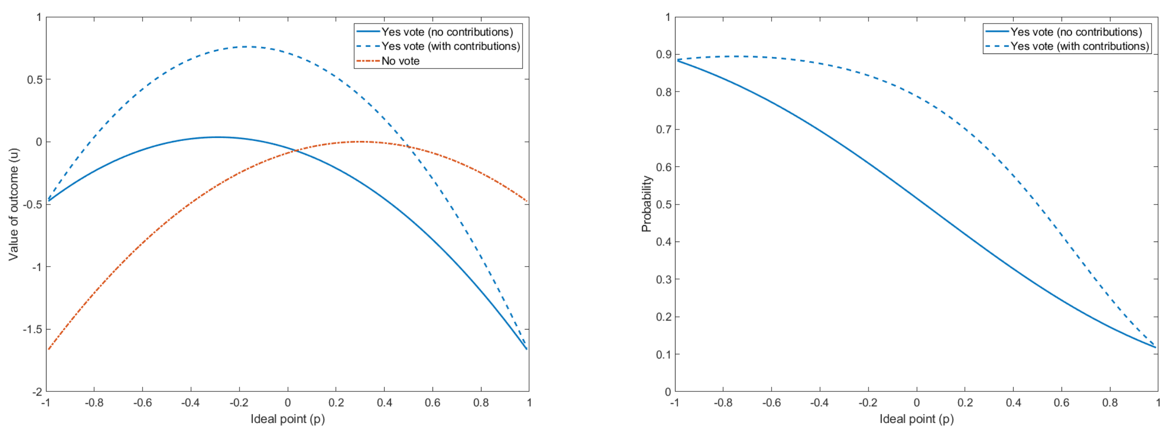


Notes: The dependent variable is the sponsorship for a bill and the independent variable is the net contributions the politician received in support of the bill. Both variables are residuals from regressions on politician and vote fixed effects.

Figure 1.8: Example of the spatial model of voting



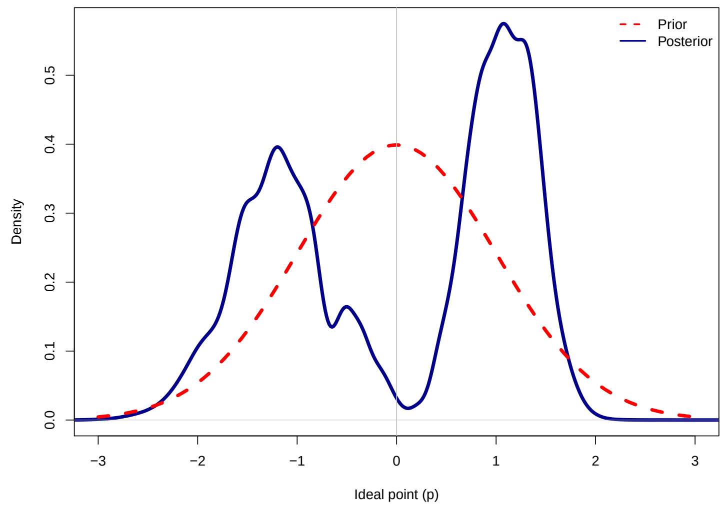
(a) Homogeneous influenceability (γ_i)



(b) Heterogeneous influenceabiilty (γ_i)

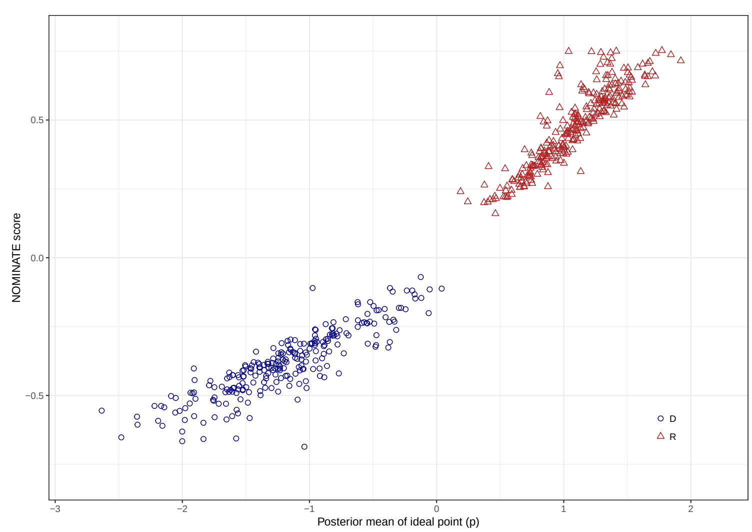
Notes: This figure demonstrates an example of a one-dimension spatial model. In this example, a continuum of politicians are indexed from -1 to 1 along the horizontal axis according to their ideal points. I consider a bill where a yes vote is located at -0.3 and a no vote is located at 0.3. In Panel A, I consider the case where $\gamma_i = 0.03$ is homogeneous across legislators. On the left I compare the value of a yes vote without contributions to a yes vote with two \$20,000 contributions and a no vote without contributions. On the right, I plot the probability of a yes vote with and without contributions assuming a standard normal preference shock. In Panel B, I repeat this example but under the assumption of a heterogeneous γ_i with the same mean, such that the ideologically moderate are more influenceable than the extreme.

Figure 1.9: Posterior of ideal points p_i



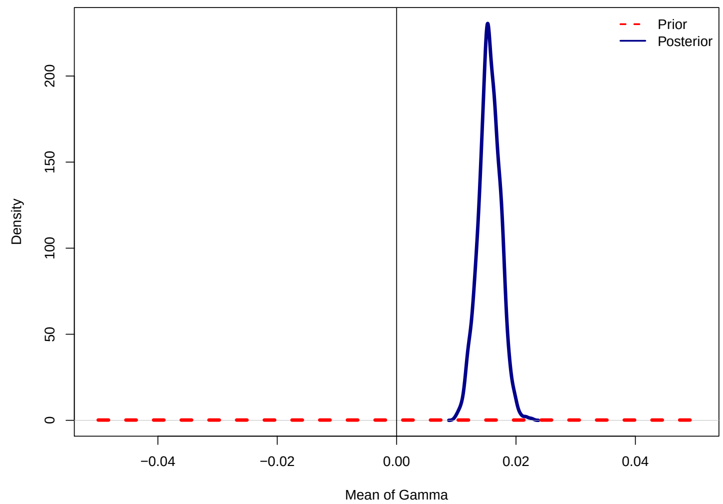
Notes: This figure plots the marginal posterior of ideal points p_i, pooling over all representatives. The dashed line is the density of the associated prior. The sample consists of the 112th Congress.

Figure 1.10: Comparison of mean posterior of ideal points and Nominate scores



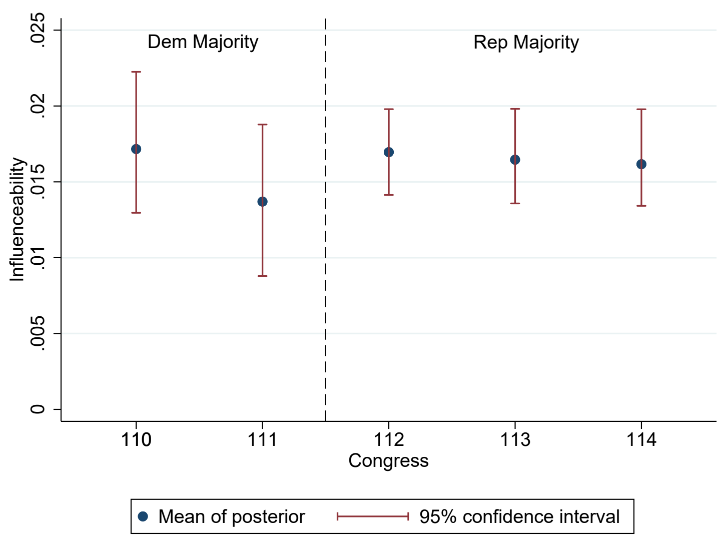
Notes: This figure plots the posterior means of the ideal points of representatives against their first-dimension Nominate scores. The sample consists of the 112th Congress. Blue circles indicate Democrats and red triangles denote Republicans. Pearson correlation coefficient = 0.98.

Figure 1.11: Posterior of influenceability (γ)



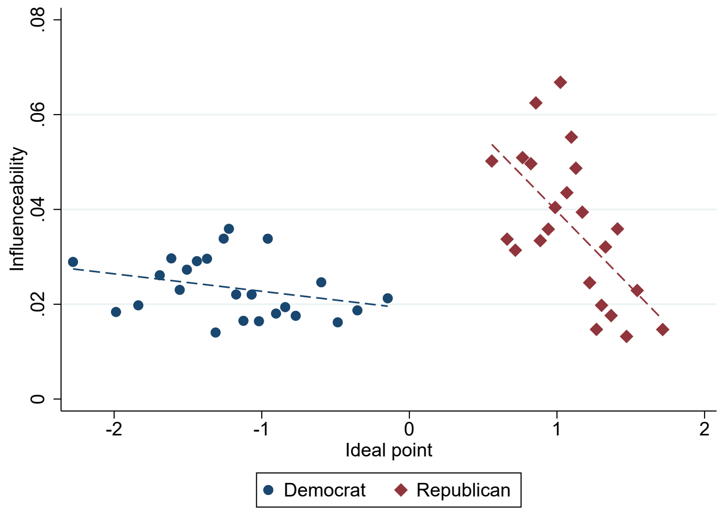
Notes: The solid line depicts the marginal posterior density of influenceability (γ) for the baseline model where $\gamma_i = \gamma$ for all politicians i. The dashed line is the density of the associated prior. The sample consists of the 112th Congress.

Figure 1.12: Influenceability (γ) over time



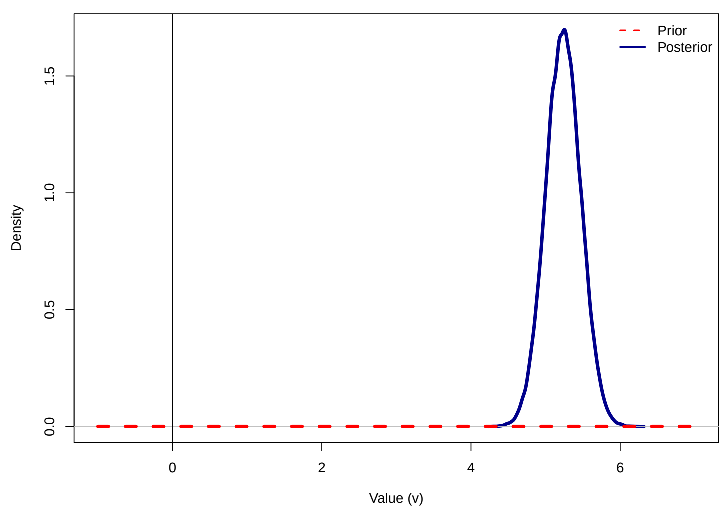
Notes: Each point denotes the mean posterior of influenceability (γ) together with its 95% credible interval for each of the 110th to 114th Congresses of the House of Representatives (2007-2015) The first four years were under a Democratic majority whereas the last six were under a Republican majority.

Figure 1.13: Ideology and influenceability



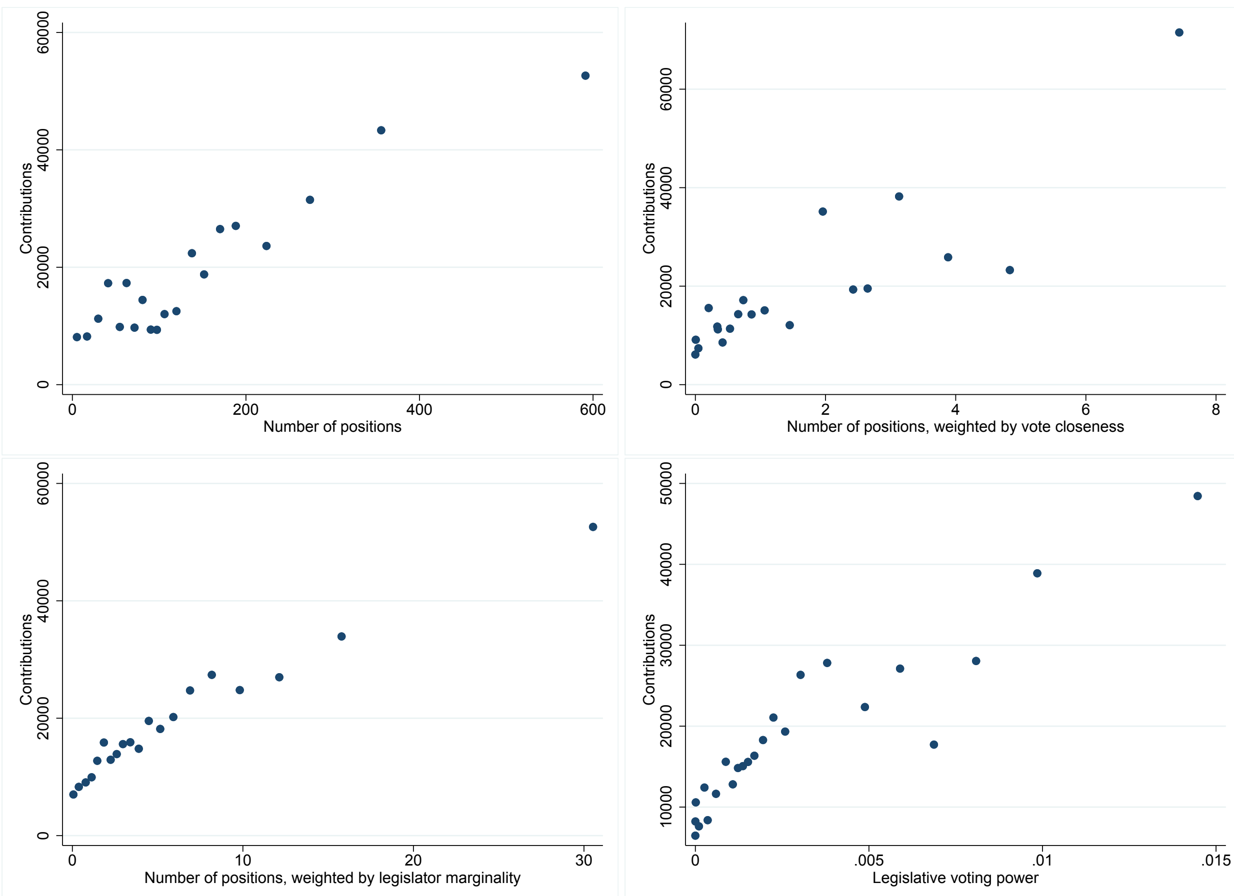
Notes: This figure plots the mean posterior of ideal points against the mean posterior of influenceability for the full model where γ_i varies across representatives. Each bin contains approximately 10 legislators. Circles indicate Democrats and diamonds Republicans. The dashed line indicates the best linear fit for each party. The sample consists of the 112th Congress of the House of Representatives.

Figure 1.14: Posterior of vote value v



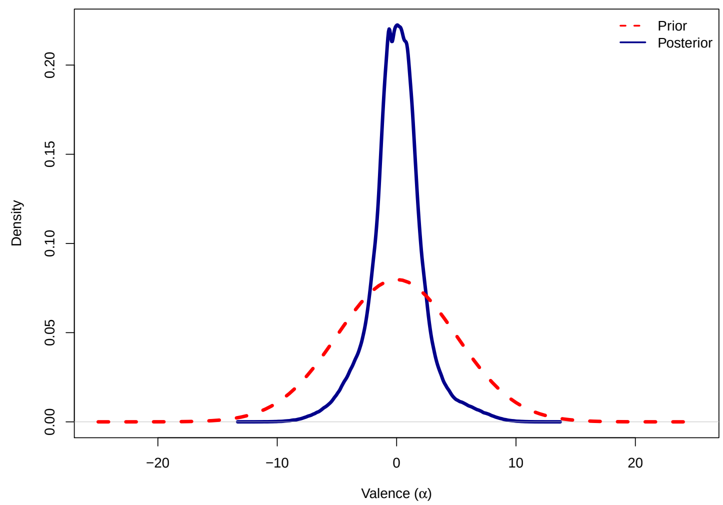
Notes: This figure plots the marginal posterior of vote valuation to interest groups (v), pooling over all votes. The dashed line is the density of the associated prior. The unit is $M. The sample consists of the 112th Congress.

Figure 1.15: Determinants of interest group contributions to legislators



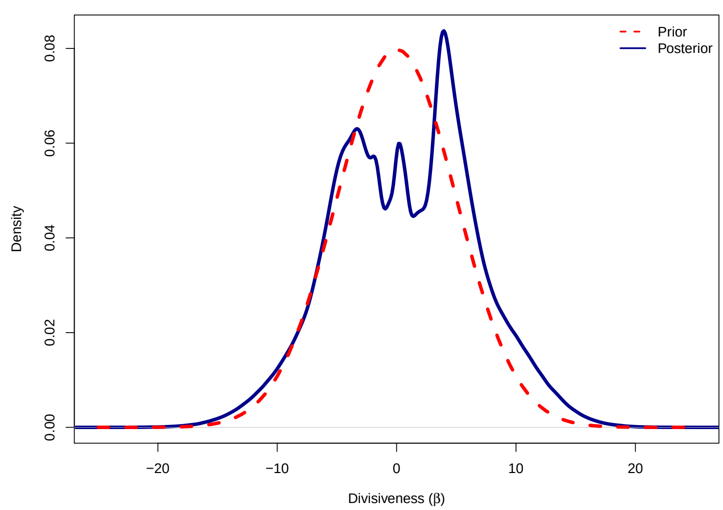
Notes: This figure plots contributions donated from an interest group to a politician against (i) the number of positions on bills taken by this group, (ii) the sum of positions, each weighted by the expected vote closeness, (iii) the sum of positions, each weighted by the legislator's expected marginality, and (iv) the sum of positions, each weighted by the product of the expected vote closeness and the legislator's expected marginality (i.e. weighted by legislative voting power). See the text for a more detailed description. The sample consists of the 112th Congress.

Figure 1.16: Posterior of valence α_r



Notes: This figure plots the marginal posterior of valence (or difficulty) α_r, pooling over all votes. The sample consists of the 112th Congress.

Figure 1.17: Posterior of divisiveness β_r



Notes: This figure plots the marginal posterior of divisiveness (or discrimination) β_r, pooling over all votes. The sample consists of the 112th Congress.

Table 1.1: Summary of roll call voting studies

Study	*Issues Covered*	*# Votes*
Silberman, Durden (1976)	minimum wage	2
Chappell (1981)	cargo preference	1
Kau, Rubin (1981)	various economic	8
Kau, Kennan, Rubin (1982)	various economic	8
Chappell (1982)	variety	7
Welch (1982)	dairy subsidy	1
Evans (1986)	tax, Chrysler	8
Kau, Rubin (1984)	variety	10
Peltzman (1984)	variety	333
Feldstein, Melnick (1984)	health care	1
Coughlin (1985)	domestic content	2
Johnson (1985)	bank, real estate	9
Wright (1985)	variety	5
Wayman (1985)	arms control	19
Frendreis, Waterman (1985)	trucking	4
Schroedel (1986)	banking	3
Wilhite, Theilmann (1987)	labor	2
Tosini, Tower (1987)	trade (textiles)	1
Jones, Keiser (1987)	labor	1
Saltzman (1987)	labor	1
MacArthur, Marks (1988)	domestic content	1
Grenzke (1989)	variety	30
Vesenka (1989)	agriculture	14
Neustadl (1990)	labor, business	2
Wright (1990)	tax, agriculture	2
Langbein, Lotwis (1990)	gun control	6
Durden et al. (1991)	strip mining	3
Mayer (1991)	aircraft carriers	1
Stratmann (1991)	agriculture	10
Rothenberg (1992)	MX missile	8
Langbein (1993)	gun control	6
Marks (1993)	trade	5
Nollen, Quinn (1994)	trade	6
Stratmann (1995)	agriculture	10
Bronars, Lott (1997)	variety	35
Stratmann (2002)	banking	2
Witko (2006)	variety	20
Mian, Sufi, Trebbi (2010)	banking	2
Facchini, Steinhardt (2011)	immigration	NA
Dorsch (2013)	banking	1
Conconi, Facchini, Zanardi (2014)	trade	15

Notes: This table replicates and extends Table 1 in Ansolabehere et al. (2003). Each row indicates a study on roll call voting, the issues covered therein, and the number of votes analyzed.

Table 1.2: Descriptive statistics: roll-call vote sample

	Mean	Standard Deviation	Min	Max
Panel A: Main dependent and independent variables				
Yea vote	76.18	42.59	0	100
SIG contributions in support ($)	36,268	78,368	0	2,276,479
SIG contributions in opposition ($)	11,983	35,289	0	1,032,825
Individual contributions in support ($)	30,396	71,626	0	3,468,760
Individual contributions in opposition ($)	9570	36,270	0	2,233,187
Retiring	0.05	0.22	0	1
Close roll call vote	0.45	0.49	0	1
Panel C: Vote characteristics				
Sponsor is Democrat	0.40	0.49	0	1
Majority Democrat cosponsors	0.38	0.48	0	1
Majority Republican coponsors	0.41	0.49	0	1
110th Congress	0.25	0.43	0	1
111th Congress	0.14	0.34	0	1
112th Congress	0.14	0.35	0	1
113th Congress	0.18	0.38	0	1
114th Congress	0.27	0.44	0	1
Panel D: Legislator characteristics				
Democrat	0.48	0.49	0	1
Ideology (first-dimension nominate)	1.05	0.44	0.315	1.913
Ideology (second-dimension nominate)	1.31	0.94	0.09	3.65
Majority party leadership	0.026	0.16	0	1
Committee chair	0.05	0.21	0	1
Member of powerful committee	0.26	0.44	0	1
Congressional experience	11.90	9.03	1	60
Age	57.79	10.22	30	91
Female	0.17	0.37	0	1
Competitive district	0.36	0.48	0	1
N	407,173			

Notes: This table reports summary statistics for the main reduced-form estimation sample. The sample consists of a panel of legislators across votes from the 110th to 114th Congresses of the U.S. House of Representatives. The sample is restricted to votes where at least one special interest group has taken a position. Contributions are computed using the formula described in the text. The binary dependent variable for a yes vote takes values of 0 or 100. See Section 1.3 for more details on the variables.

Table 1.3: The effect of campaign contributions on roll call voting

	OLS						IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Contributions in Support	0.163***	0.144***	0.163***	0.146***	0.261***	0.226***	0.320***	0.273***
	(0.029)	(0.024)	(0.030)	(0.024)	(0.046)	(0.035)	(0.062)	(0.049)
Contributions in Opposition	-0.110***	-0.0684**	-0.118***	-0.0757***	-0.0828*	-0.0145	-0.222***	-0.105*
	(0.033)	(0.027)	(0.033)	(0.027)	(0.048)	(0.040)	(0.074)	(0.061)
Retiring × Cont. Support			0.00161	-0.0147				
			(0.039)	(0.032)				
Retiring × Cont. Opposition			0.154**	0.131**				
			(0.072)	(0.061)				
Sample	Full	Full	Full	Full	Balanced	Balanced	Balanced	Balanced
Legislator FE x Vote Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Vote FE x Legislator Party	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Vote FE x Legislator Ideology	No	Yes	No	No	No	Yes	No	Yes
R-Squared	0.747	0.781	0.747	0.781	0.766	0.791		
N	407,173	407,173	407,173	407,173	173,309	173,309	173,309	173,309

Notes: This table reports the effects of campaign contributions on roll call votes. Campaign contributions in support and opposition are aggregated based on the positions of special interest groups for each bill, and then the inverse hyperbolic sine is applied on the totals. Retiring is a dummy for the politician announcing his retirement from politics in the term of the vote. The Balanced sample is restricted to politicians serving from the 107th congressional session onwards. Standard errors are two-way clustered by vote and by legislator. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.4: The effect of campaign contributions for close versus lopsided votes

	(1)	(2)	(3)	(4)
Contributions in Support	0.163***	0.144***	0.258***	0.225***
	(0.029)	(0.024)	(0.054)	(0.043)
Contributions in Opposition	-0.110***	-0.0684**	-0.193***	-0.113***
	(0.033)	(0.027)	(0.047)	(0.039)
Lopsided × Contributions in Support			-0.165**	-0.141***
			(0.069)	(0.053)
Lopsided × Contributions in Opposition			0.188***	0.0931*
			(0.066)	(0.055)
Sample	Full	Full	Full	Full
Legislator FE x Vote Characteristics	Yes	Yes	Yes	Yes
Vote FE x Legislator Party	Yes	Yes	Yes	Yes
Vote FE x Legislator Ideology	No	Yes	No	Yes
R-Squared	0.747	0.781	0.747	0.781
N	407,173	407,173	407,173	407,173

Notes: This table reports the effects of campaign contributions on roll call votes. Campaign contributions in support and opposition are aggregated based on the positions of special interest groups for each bill, and then the inverse hyperbolic sine is applied on the totals. Lopsided is a dummy for whether the vote was lopsided or close (using the definition of Snyder and Groseclose (2000)). Standard errors are two-way clustered by vote and by legislator. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.5: The effect on votes for individual versus SIG contributions

	(1)	(2)
SIG Contributions in Support	0.178***	0.153***
	(0.030)	(0.023)
SIG Contributions in Opposition	-0.104***	-0.0648**
	(0.031)	(0.026)
Individual Contributions in Support	0.00782	0.0146
	(0.022)	(0.018)
Individual Contributions in Opposition	-0.0457	-0.0508
	(0.036)	(0.031)
Individual = PAC F-test p-value	.0001	.00004
Legislator FE x Vote Characteristics	Yes	Yes
Vote FE x Legislator Party	Yes	Yes
Vote FE x Legislator Ideology	No	Yes
R-Squared	0.748	0.781
N	407,173	407,173

Notes: This table reports the effects of campaign contributions on roll call votes. Campaign contributions in support and opposition are aggregated separately for individuals and special interest groups (i.e. PACs), and then the inverse hyperbolic sine is applied on the totals. Standard errors are two-way clustered by vote and by legislator. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.6: The effect of campaign contributions on bill cosponsorship

	OLS						IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Contributions in Support	0.267***	0.235***	0.269***	0.236***	0.318***	0.275***	0.447***	0.377***
	(0.015)	(0.013)	(0.016)	(0.013)	(0.023)	(0.020)	(0.033)	(0.029)
Contributions in Opposition	-0.133***	-0.0886***	-0.135***	-0.0912***	-0.154***	-0.108***	-0.239***	-0.177***
	(0.022)	(0.019)	(0.022)	(0.019)	(0.033)	(0.029)	(0.044)	(0.037)
Retiring × Cont. Support			-0.0504**	-0.0399*				
			(0.025)	(0.023)				
Retiring × Cont. Opposition			0.0144	0.0134				
			(0.029)	(0.027)				
Sample	Full	Full	Full	Full	Balanced	Balanced	Balanced	Balanced
Legislator FE x Vote Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Vote FE x Legislator Party	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Vote FE x Legislator Ideology	No	Yes	No	No	No	Yes	No	Yes
R-Squared	0.344	0.433	0.344	0.433	0.355	0.446	0.355	0.446
N	1890420	1890420	1890420	1890420	777509	777509	777509	777509

Notes: This table reports the effects of campaign contributions on bill cosponsorship. Campaign contributions in support and opposition are aggregated based on the positions of special interest groups for each bill, and then the inverse hyperbolic sine is applied on the totals. Retiring is a dummy for the politician announcing his retirement from politics in the term of the vote. The Balanced sample is restricted to politicians serving from the 107th Congress onwards. Standard errors are two-way clustered by vote and by legislator. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.7: The effect of campaign contributions on bill sponsorship

	OLS			IV
	(1)	(2)	(3)	(4)
Contributions in Support	0.0307***	0.0314***	0.0360***	0.0420***
	(0.002)	(0.002)	(0.003)	(0.003)
Contributions in Opposition	-0.00907***	-0.00907***	-0.00460	-0.0200***
	(0.002)	(0.002)	(0.005)	(0.006)
Retiring × Cont. Support		-0.0126***		
		(0.004)		
Retiring × Cont. Opposition		-0.00202		
		(0.005)		
Sample	Full	Full	Balanced	Balanced
Vote FE	Yes	Yes	Yes	Yes
Politician FE	Yes	Yes	Yes	Yes
R-Squared	0.00214	0.00216	0.00542	0.00537
N	1972385	1972385	808479	808479

Notes: This table reports the effects of campaign contributions on bill sponsorship. Campaign contributions in support and opposition are aggregated based on the positions of special interest groups for each bill, and then the inverse hyperbolic sine is applied on the totals. Retiring is a dummy for the politician announcing his retirement from politics in the term of the vote. The Balanced sample is restricted to politicians serving from the 107th Congress onwards. Standard errors are two-way clustered by vote and by legislator. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.8: Influenceability and ideal point estimates

	Congress				
Variable	110	111	112	113	114
Panel A: Overall					
Median influenceability (γ_i)	0.07	0.07	0.05	0.06	0.05
Standard deviation of γ_i	0.06	0.06	0.06	0.05	0.07
Median ideal point (p_i)	-0.60	-0.55	0.56	0.65	0.73
Standard deviation of p_i	1.15	0.99	1.13	1.27	1.36
Panel B: Democrats					
Median influenceability (γ_i)	0.11	0.08	0.05	0.04	0.05
Standard deviation of γ_i	0.06	0.06	0.04	0.04	0.06
Median ideal point (p_i)	-1.37	-1.05	-1.12	-1.28	-1.43
Standard deviation of p_i	0.47	0.40	0.47	0.52	0.48
Panel C: Republicans					
Median influenceability (γ_i)	0.06	0.06	0.06	0.08	0.06
Standard deviation of γ_i	0.05	0.04	0.07	0.06	0.08
Median ideal point (p_i)	0.78	0.82	1.01	1.10	1.17
Standard deviation of p_i	0.36	0.39	0.32	0.32	0.37
Number of votes (R)	1386	987	1434	1021	1125
Number of legislators (I)	448	446	445	444	440

Notes: This table reports statistics from the Bayesian MCMC procedure described in the text. Median influenceability is the median of the mean posteriors of γ_i across all legislators in the given Congress. The standard deviation of influenceability is the standard deviation of the mean posteriors of γ_i across legislators in the Congress. The ideal point statistics were analogously calculated.

Table 1.9: Correlates of influenceability

	Influenceability			
	(1)	(2)	(3)	(4)
Ideological extremeness	-0.204***			
	(0.047)			
Democrat		-0.114	-0.0432	0.0785
		(0.089)	(0.096)	(0.110)
Ideological extremeness × Republican		-0.424***	-0.425***	-0.456***
		(0.102)	(0.104)	(0.105)
Ideological extremeness × Democrat		-0.0736	-0.0763	-0.0483
		(0.048)	(0.049)	(0.052)
Majority party leadership			0.692**	0.701**
			(0.270)	(0.277)
Committee chair			0.482**	0.646***
			(0.195)	(0.205)
Member of powerful committee			0.0418	0.137
			(0.107)	(0.114)
Experience				-0.0189***
				(0.006)
Age				0.000305
				(0.008)
Female				-0.0157
				(0.106)
Competitive district				-0.000889
				(0.107)
R-Squared	0.0447	0.0766	0.0954	0.121
N	413	413	413	413

Notes: This table reports the coefficients from regressions of the mean posterior of influenceability on politician and district characteristics. Ideological extremeness is the absolute value of the legislator's ideal point. All ideology and influenceability variables are standardized. Powerful committees are Appropriations, Rules, and Ways and Means. Experience is measured as the years of tenure in Congress. Competitive districts are those where the Democratic presidential vote share is within 10 points of 50. The sample consists of the 112th House of Representatives. Heteroskedasticity robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.10: Counterfactuals - Overall effects

Variable	Congress 110	111	112	113	114
Panel A: Counterfactual – No PAC contributions					
Expected number of vote changes per legislator	6.07	3.19	7.00	6.44	4.09
	(0.19)	(0.12)	(0.19)	(0.17)	(0.11)
Expected number of vote changes per roll call	1.96	1.44	2.17	2.80	1.60
	(0.06)	(0.05)	(0.06)	(0.07)	(0.04)
Expected number of vote success outcome changes	10.09	7.66	18.85	13.07	2.87
	(0.74)	(1.17)	(1.12)	(1.35)	(0.70)
Panel B: Only opposing contributions					
Expected number of vote changes per legislator	5.83	3.47	7.55	7.24	4.64
	(0.19)	(0.14)	(0.20)	(0.19)	(0.13)
Expected number of vote changes per roll call	1.89	1.57	2.34	3.15	1.81
	(0.06)	(0.06)	(0.06)	(0.08)	(0.05)
Expected number of vote success outcome changes	13.23	10.92	22.42	18.52	5.16
	(0.82)	(1.26)	(1.25)	(1.53)	(0.95)
Panel C: Only supporting contributions					
Expected number of vote changes per legislator	2.40	1.57	2.50	1.87	1.41
	(0.09)	(0.06)	(0.09)	(0.06)	(0.05)
Expected number of vote changes per roll call	0.77	0.71	0.77	0.81	0.55
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
Expected number of vote success outcome changes	2.34	2.57	2.74	2.43	1.29
	(0.48)	(0.61)	(0.68)	(0.63)	(0.45)
Panel D: Canceling effect (%)					
Vote changes	26.28	37.21	30.80	29.55	33.23
	(0.40)	(0.53)	(0.47)	(0.46)	(0.52)
Vote outcome changes	35.17	45.52	26.26	37.02	52.40
	(4.02)	(5.33)	(2.92)	(4.47)	(7.71)
Number of roll call votes (R)	1386	987	1434	1021	1125
Number of legislators (I)	448	446	445	444	440

Notes: This table reports the results of the counterfactual analysis. For each outcome, the mean is reported together with the standard deviation in parentheses. In panel A, predicted votes are compared to the counterfactual where there are no PAC contributions. In panels B and C, predicted votes are compared to the counterfactuals where, respectively, only opposing or supporting contributions remain. In panel D, I compute the % change in the effects that are "canceled out" due to there being contributions on opposing sides for the same bills.

Table 1.11: Counterfactuals - Votes most likely to flip

		Vote outcome		
Congress	Bill name	Actual	Predicted	Counterfactual
114	Protecting Cyber Networks Act	307-116	314-109	145-278
114	Fairness in Class Action Litigation and Furthering Asbestos Claim Transparency Act of 2016	211-188	212-187	192-206
113	Cyber Intelligence Sharing and Protection Act	288-127	291-124	108-307
113	Agricultural Act of 2014	251-166	256-161	70-347
112	Leahy-Smith America Invents Act	304-117	314-107	45-376
112	United States-Korea Free Trade Agreement Implementation Act	278-151	281-148	119-310
111	Dodd-Frank Wall Street Reform and Consumer Protection Act	223-202	224-201	197-228
111	American Clean Energy and Security Act of 2009	219-212	223-208	136-295
110	Food, Conservation, and Energy Act of 2008	306-110	311-105	101-315
110	Food, Conservation, and Energy Act of 2008	231-191	232-190	106-316

Notes: This table reports for each congressional session, the two votes for which there is the greatest probability that the vote outcome would change under the counterfactual policy of no PAC contributions. The “Actual" column denotes the number of yea and nay votes for each roll call in the data. The “Predicted” column denotes the yea and nay votes predicted by the posterior of the parameters obtained by the Bayesian MCMC estimation. The “Counterfactual" column denotes the predicted yea and nay votes under the counterfactual where there are no PAC contributions.

Chapter 2

Money and Politics: Estimating the Effects of Campaign Spending Limits on Political Entry and Competition

2.1 Introduction

Among the many factors critical for a properly functioning democracy, few have been as widely debated as campaign financing. For some, money in politics serves as an expression of free speech and an effective instrument for informing voters and building an inclusive democracy. For others, the unrestrained use of money in politics can erode the functioning of democracy as it can lead to excessive campaigning, unequal access to power, and politicians who are beholden to special interest groups.[1]

In practice, almost every country with political pluralism has adopted some type of political finance regulation ranging from information and disclosure requirements to limits on campaign contributions and/or expenditures (Scarrow, 2007). Countries such as Canada and the UK have been limiting campaign spending by parties and individuals for many decades.[2] More recently Belgium, Chile, France, Israel, New Zealand, South Korea and many others have also adopted campaign spending caps in order to limit the role of money in elections.[3]

Despite the widespread adoption of spending limits, our understanding of how they impact the political process is limited. As we show theoretically in a contest model with endogenous entry of heterogeneous candidates, spending limits can affect both who enters politics and who gets elected.

[1]For example, see Coate (2004), Prat (2002), Prat (2006), and Scarrow (2007).

[2]Currently, political parties in Canada can spend only 73.5 cents for every voter in districts in which they are competing. In the United Kingdom, legislation regulating expenditures has been in place since the Corrupt and Illegal Practices Prevention Act 1883. In the 2005 general election, campaign expenditure at the national level were limited to approximately US$42,000 per constituency contested.

[3]Two thirds of the OECD countries have introduced campaign spending limits for parties or candidates (Speck, 2013). One of the few exceptions among rich countries is the U.S. where the Supreme Court ruled mandatory spending limits as an unconstitutional curtailment of free speech.

But because the decision to run for office depends not only on a candidate's own characteristics, but those of his opponents, the effects of spending limits on electoral outcomes can be ambiguous. Empirically, to estimate these effects presents some difficult challenges. Campaign finance reform is usually applied uniformly across elections and jurisdictions, which makes it difficult to identify an appropriate comparison group. In addition, few countries provide information on the characteristics and campaign spending of both their elected and non-elected candidates. It is important to have data on both types of candidates if, as theory suggests, spending caps affect not only the identity of who is elected, but also who chooses to run.

In this paper, we provide causal estimates of the effects of campaign spending limits on political entry, selection, and political behavior. We do so in the context of a recent campaign finance reform in Brazil. Amid a massive corruption scandal that included the diversion of public funds to political campaigns, the Congress passed a law that imposed campaign spending limits in future elections. The spending caps vary by municipality and create a discontinuity of about 25 percentage points in the amount candidates can spend in local elections. We exploit this discontinuity together with a rich dataset on all candidates elected and non-elected to explore how spending limits affect the entry decisions of candidates, their characteristics, and electoral results for mayors.

Our analysis shows that spending caps can affect all three phases of the political process: entry, selection, and post-election behavior. In municipalities with a higher spending cap, campaign contributions are 11 percent higher, with 75% of this difference coming from self-financing. We find that municipalities subject to higher spending limits are less politically competitive as measured by the total number candidates or the effective number of candidates (i.e. the total number of candidates weighted by their vote shares). Our estimates suggest that a 25 percent point increase in spending caps leads to a 9 percent decrease in the number of individuals who run for office. Higher spending limits also affect the composition of the candidate pool by attracting wealthier candidates who are better able to self-finance their campaigns.

For political selection, we find that incumbents are 11 percentage points more likely to get re-elected in places with higher spending caps. Two factors explain this effect on incumbency rates. First, as we have already mentioned, incumbents face fewer challengers in municipalities with higher spending limits. Second, we find that whereas a 25 percentage point increase in spending limits increases incumbent spending by 14 percent, it only increases the average challenger spending by 8 percent. Incumbents, who on average raise more campaign contributions, benefit disproportionately more than challengers from an increase in the spending cap. Independent of incumbency status, we also find that similar to the effects on political entry, places with higher limits elected mayors who were wealthier and relied on more self-financing.

We care about who gets elected because it can matter for what policies are selected and how well they are implemented (e.g. Chattopadhyay and Duflo (2004); Jones and Olken (2005); Meyersson (2014)). Given the political selection effects, we also estimate the impacts of spending limits on behavior while in office. Although these mayors have only recently been elected, they must apply for discretionary funds to fund public goods and services. We measure their success to obtain these federal block grants and find that higher spending limits increase the amount of block grants a municipality receives. We interpret this as a political selection effect, driven by wealthier mayors who have more social connections, power, and ability to attract funds. We do not, however, want to suggest that voters are better off in places with higher spending limits. Block grants, while necessary, are not sufficient for the provision of additional public goods under high levels of corruption (Ferraz and Finan, 2011). Only time will tell whether spending limits affect public goods provision and improve outcomes.

Given that a political corruption scandal brought about these campaign finance reforms, a natural question to ask is to what extent were these regulations enforced. While it is difficult to test this directly, it is important to note that had the candidates found a way to circumvent the caps then it is unlikely we would have found any effects on our political outcomes. Moreover, we also collected data to test whether the caps affected the use of undeclared campaign funds. We constructed two proxies from two different data sources: 1) the share of candidates' campaign accounts that were rejected or found to be irregular by independent electoral judges; 2) candidates' share of in-kind contributions – in kind contributions do not have formal receipts and are difficult to monitor. We do not find any evidence that spending caps impacted either of these proxies.

Our findings contribute to a large but mostly theoretical literature on the effects of campaign finance policy on electoral outcomes. Most of the models in this literature study the welfare effects of campaign finance reform in an environment in which candidate entry is fixed and campaign contributions provide valuable information about candidates (e.g. Austen-Smith (1987); Prat (2002); Coate (2004); Ashworth (2006)). In our paper, we draw from a different class of models that abstracts from why campaign spending affects voting to instead focus on the effects of spending limits on the entry decisions of candidates. We build on the extensive literature studying contests and all-pay auctions in the context of political lobbying and campaigning (e.g. Che and Gale (1998); Fang (2002); Pastine and Pastine (2012); Cotton (2012)). Relative to this previous literature, we incorporate candidate heterogeneity, which allow us to characterize the conditions under which spending limits affect the size and quality of the candidate pool.

The empirical literature on campaign finance policy is much less developed, especially when focused on spending limits. The majority of empirical studies have instead studied contribution limits mostly within the U.S. and rely on the fact that these limits vary by state and across time for identification (e.g. Stratmann and Aparicio-Castillo (2006); Barber (2016)). This type of identifying variation, however, can be problematic. The decision to impose contribution limits is itself endogenous and a function of many of the electoral outcomes that limits presumably impact.

To our knowledge, there are only two other empirical investigations on the effects of campaign spending limits. Milligan and Rekkas (2008) use spending caps as an instrument to estimate the effects of campaign spending on electoral outcomes in Canada's federal elections. The instrument is based on a formula that specifies spending limits as a function of the number of electorates and the size of the district. Although the focus of their paper is on estimating the elasticity of spending on votes, they also find that higher limits are associated with fewer candidates, lower voter turnout, and larger win margins. Fouirnaies (2018) also relies on a similar formula-based spending limit to estimate the effects on electoral competition in the British House of Commons elections during the period 1885-2010. Again consistent with our findings, he finds that spending limits are associated with less competition and a candidate pool with a higher proportion of upper class candidates.

Different from these studies, our research design requires much weaker assumptions to identify the causal effect of spending limits. These previous studies rely on variation that was determined according to the size and density of the electoral district. But the concern is that in both the cross-section and over time, these variables are likely to be correlated with other factors that directly impact the political process. In contrast, our regression discontinuity approach exploits an unexpected law change that created a sharp discontinuity in spending limits among otherwise similar municipalities. In addition, we also study the effects of spending limits on a broader set of outcomes including detailed candidate characteristics for both the elected and non-elected and performance measures.

Our study also relates to a large literature on the effects of campaign spending on electoral outcomes (e.g Levitt (1994); Gerber (1998); Erikson and Palfrey (2000); Da Silveira and De Mello (2011)). A central finding in this literature is that the elasticity of vote share with respect to campaign spending is larger for challengers than for incumbents. This has led several studies to conclude that the introduction of spending limits may reinforce incumbency advantage (Levitt, 1994; Jacobson, 1990). Our findings suggest that this is not necessarily the case, once we account for the entry and compositional effects of spending caps.

Finally, our work also speaks to research on the identity of politicians and whether limits to campaign spending might level out the playing field between richer and poorer candidates. There is a growing literature following the citizen-candidate models of Osborne and Slivinski (1996) and Besley and Coate (1997) suggesting that identity matters for policy implementation (e.g. Chattopadhyay and Duflo (2004); Besley et al. (2011); Corvalan et al. (2016)). In countries where inequality is high, access to political power might be easier for richer candidates and this might have direct consequences on who gets elected and which types of policies are implemented. Our work suggests that higher spending caps increase the average wealth of candidates that run for and are elected as mayor, and that this affects political behavior in office.

The rest of the paper is organized as follows. Section 2.2 describes Brazil's campaign financing laws and presents the data used in the empirical analysis. Section 2.3 presents the theoretical framework. Section 2.4 discusses our research design and in Section 2.5 we present our findings. Section 2.6 concludes.

2.2 Background and Data

In this section, we describe campaign financing in Brazil and the 2015 campaign financing law. The law limits how much candidates from different municipalities can spend. These spending limits form the basis of our identification strategy. We then discuss our data, and present some basic descriptive statistics.

Municipal Elections and Campaign Financing

Local elections in Brazil are held every four years, with the most recent election taking place in October 2016. Candidates need to be registered as a member of a political party in order to run for a political office. The elections are held to elect a municipal mayor and a local council. For municipalities with less than 200,000 registered voters, which represents 98 percent of all municipalities, mayors are elected based on simple plurality. For municipalities with 200,000 or more registered voters, candidates for mayor must be elected with at least 50 percent of the votes or a second round runoff is held. Once elected, mayors then face a two-term limit. In contrast, local legislators are elected based on an open-list proportional representation system and can be reelected indefinitely. Mayors are important political figures in Brazil. Each year, municipalities receive millions of dollars from federal and state governments to provide basic public services such as primary education, health care, and sanitation. The mayor sets the agenda for how resources are spent and allocated.

Political parties are financed yearly by private contributions and public funds (Fundo Partidário), which are distributed across parties based on the share of votes a party received in the previous election for Congress. Parties use these resources to fund individual candidates. On top of public funding, private contributions can be received by candidates after they have officially registered their candidacy (before August 15^{th}) up until election day. Citizens are allowed to contribute up to 10% of their annual income, unless contributing to their own campaign, in which case there are no limits. Prior to 2015, corporations could contribute up to 2% of gross annual revenues, and there were no restrictions on either total contributions or total campaign spending. Political Action Committees do not exist in Brazil and independent organizations cannot spend resources to campaign for candidates or parties. Campaign spending has to be made by individual candidates or political parties on their behalf.

Similar to the U.S., both street campaigns and media ads are important forms of campaigning. But different from the U.S., candidates do not need to buy time on TV or radio. In Brazil, TV and radio ads are free and air at predetermined times of the day for 35 days before the election as determined by Brazil's electoral law. Airtime is distributed according to the share of votes that the candidate for mayor's coalition has in Congress (see Da Silveira and De Mello (2011)). While airtime is free, candidates do spend significant amount of resources on producing the ads. Since 2010, candidates have also been allowed to campaign using the Internet, including social media – although prior to 2018, they could not purchase ads on social media outlets.

The 2015 Campaign Finance Reform

On March 14, 2014, Brazil's Federal Police launched an investigation into a local money laundering scheme involving gas stations. This investigation, titled "Lava Jato", has since become one of the largest corruption scandals in the world as investigators have already uncovered over R$12 billion in paid bribes and made 188 convictions for corruption and money laundering. Key members of Brazil's main parties including the PT, the PP, and the PMDB were found guilty of diverting billions of dollars through procurement contracts to fund their political campaigns.

In response to the scandal, Brazil's Supreme Court ruled to ban all corporate donations to candidates and parties. This decision led the Brazilian Congress to pass a law on September 2015 that established a cap for campaign spending in future elections.[4] The law states that candidates running for mayor can only spend the maximum of either R$100,000 (approximately $30,000) or

[4]See http://www.planalto.gov.br/ccivil_03/_ato2015-2018/2015/lei/l13165.htm.

70% of the highest amount spent by a candidate in the same municipality in the previous election. As stated, the law creates a kink in the amount that candidates can spend at around R$142,858 (70% of R$142,858 is R$100,000.6). For any value lower than R$142,858 the cap is given by R$100,000 while for higher values the cap is given by 70% of the largest value spent in the previous election.

The law also stipulated that the caps disclosed in December 2015 should be inflation adjusted (see Figure 2.1 for a timeline of the events leading up the 2016 elections). For municipalities capped at R$100,000, they increased the limit by 8.04 percent, which corresponds to the increase in the price index between October 2015 (the month the law was issued) and October 2016. But for municipalities capped at 70 percent of the maximum amount spent in the 2012 election, the cap was adjusted by 33.7 percent, which corresponds to the increase in the price index that took place between October 2012 and June 2016. As a result, the inflation-adjusted caps created a discontinuity in the campaign spending limits of about 25 percent, which is what our research design will exploit (see Figure 2.2).[5]

The spending limits apply to any: i) spending made directly by the candidate, ii) spending made by the party on behalf of the candidate, iii) transfers made by the candidate to other candidates (within or across parties) or to political parties, iv) campaign donations estimated in kind or computed as gifts. Candidates that spend more than the limit are subject to severe punishments including a fee of 100% the amount that exceeds the limit.

Campaign contributions and expenditures are tightly regulated in Brazil. Prior to the election, all candidates and parties have to open a bank account to be used exclusively for campaign purposes. All individual contributions and expenditures should be reported to the Electoral Commission within 72 hours and must identify all the entities involved. Every transaction is also monitored and made public as soon as the Electoral Court receives the information. Candidates can declare as contributions loans coming from their own resources. But these loans must come from official financial institutions certified by the Central Bank.

After the elections, candidates and parties have 30 days to submit their final accounts to electoral judges and the incentives to report truthfully are high. The electoral commission makes the accounts available online and issues the documentation that would allow any candidate, political party, or public prosecutors to check and contest the accounts of other candidates within 3 working days. After this period, the commission rules whether the accounts of the candidates and parties are considered to be: i) regular and approved, ii) approved with some problems but without irregularities, iii) rejected due to significant irregularities. If rejected, candidates could be banned from running for office in the future.

A more severe violation is the use of undeclared resources, which includes for instance vote buying. In extreme cases, the electoral commission can cancel a candidate's registration during the election or bar them from taking office. Many candidates have lost their mandates in Brazil for buying votes with undeclared resources. Parties and party leaders can also be punished for irregularities in their campaign finance accounts. Parties can lose access to public funds for up to one year, and party leaders can be prosecuted in civil and criminal courts for irregularities

[5]The information on the spending caps is publicly available and can be assessed at the Electoral Court webpage at: http://www.tse.jus.br/eleicoes/eleicoes-2016/prestacao-de-contas/divulgacao-dos-limites-legais-de-campanha.

associated with diversion of resources for personal gains or other forms of corrupt practices.

Data

The data used in this paper come from several sources. The election data come from Brazil's Electoral Commission (TSE) and are available online at the level of an individual candidate. Our data covers all candidates that ran for mayor in 2012 and 2016. In addition to their election results, for each candidate we know a basic set demographic characteristics, such as their gender, age, education level, and self-reported wealth, as well as their campaign contributions and expenditures. For campaign contributions we know the source of the contribution (individual or party) and the amount. For campaign expenditures we have a description of the type of spending in large categories and the tax code of the firm that received the transfer for the good or service (e.g. candidate A rented a car from rent-a-car company B and spent X). Based on this information, we compute at the municipal level, our main political outcomes: campaign contribution and spending (total and by categories), the number of candidates that ran for mayor, characteristics of the candidate pool, and re-election rates.

We complement these data with information from the 2010 population census, aggregated at the municipality level. The census data include basic demographic and socio-economic characteristics of the municipality, such as: population size, average income, literary rates, and share of the urban population.

Descriptive statistics for the 2016 elections appear in Table 2.1. On average, elections for mayor attract 3 candidates. Only 13 percent of candidates are female, and only 50 percent of candidates have a college degree. The average candidate in a municipality self-declares asset holdings of about R$1,000,000, but this number masks a lot of heterogeneity as the maximum amount self declared by a candidate in a municipality ranges from R$43,600 to R$24.2 million. In Brazil, incumbents do not enjoy much of an advantage. Conditional on running for reelection, incumbents were only re-elected in 48.2 percent of municipalities, and received on average 46.8 percent of the votes. In the analysis, we drop open seats (where the mayor is term-limited) so that the sample remains comparable when considering the effects of the spending limits on incumbents. To include these elections does not affect any of our results on political entry and selection.[6]

2.3 Model

Our model builds on the extensive literature studying contests and all-pay auctions in the context of political lobbying and campaigning.[7] In our framework, we extend the n-player contest model

[6]These results are available upon request.

[7]For example, see Tullock (1980), Siegel (2009), Jia et al. (2013), Kang (2015). For a review of the literature, see Corchón (2007) and Konrad (2009).

with generalized technologies of Cornes and Hartley (2005) in order to incorporate two types of campaign technologies, where one is subject to a cap and the other is not.[8]

We consider an environment in which $I \geq 2$ candidates compete in an election. Each candidate, indexed by i, chooses how much to spend across two technologies: she chooses an amount x_i to spend through formal channels, which is reported to the election commission, and an amount z_i to spend through informal channels. Informal spending, which isn't reported to the electoral commission, can include anything from effort spent campaigning on her social media accounts to the use of illicit forms of campaigning, such as vote buying. The candidate's total *input* into the electoral contest is the weighted sum $y_i \equiv a_i x_i + b_i z_i$, where a_i and b_i are measures of each technology's effectiveness in producing votes. We assume that $b_i < a_i$ for all candidates, so that spending through formal means is more effective. We will refer to a_i interchangeably as the *campaign effectiveness* or *popularity* of a candidate. After each candidate simultaneously chooses her campaign expenditures, each voter selects his preferred candidate in the election.

Voters. We assume there is a continuum of voters who vote sincerely. Each voter's payoff from electing a candidate i is increasing with diminishing returns in the candidate's input into the electoral race. Thus, voters are "impressionable" and respond to campaign spending (Baron, 1994; Grossman and Helpman, 1996). After the candidates have selected their expenditures, an electoral shock ξ_{in} is drawn independently for each voter-candidate pair. Therefore, voter n's utility if he votes for candidate i is $v_{in} = \log(y_i) + \xi_{in}$.

We normalize the voter's utility to $v_{0n} = 0$ if he chooses to abstain. We assume that ξ_{in} are drawn independently from a type I extreme-value distribution, and thus it follows that the share of voters who select candidate i is $p_i = \frac{y_i}{1+\sum_{k=1}^{I} y_k}$. A candidate's vote share is given by the share of non-abstaining voters who select that candidate, which is

$$s_i = \frac{y_i}{\sum_{k=1}^{I} y_k}. \tag{2.1}$$

Politicians. For parsimony, we will assume that candidates seek solely to maximize their expected vote shares net of the costs of campaigning.[9] Normalizing the benefits from the vote share to 1, we write the candidate's utility function as

$$u_i(x_i, z_i) = s_i(x_i, z_i) - c_i(x_i + z_i) \tag{2.2}$$

where we assume that the marginal cost to raising campaign contributions is c_i, whether those funds end up being reported or not.

A strategy for player i is an expenditure pair (x_i, z_i). While her formal spending is capped at $\bar{x}$, she can spend unlimited amounts informally. Let x_{-i} and z_{-i} denote the formal and informal spending of the other candidates. The solution concept we use is the pure-strategy Nash equilibrium: a

[8]Although the contest model has not, to our knowledge, been applied to study campaign spending caps, it has been extended to consider the effect of public campaign spending laws (Klumpp et al., 2015).

[9]Equivalently, we can assume that politicians seek to maximize the probability of being elected net of the costs of campaigning by letting equation (2.1) denote the politician's probability of winning the election.

strategy profile in which each candidate's expenditures maximize her payoff given the expenditures of her opponents. Formally, the candidate's problem is, given x_{-i} and z_{-i}, to maximize $u_i(x_i, z_i)$ subject to the constraints $0 \leq x_i \leq \bar{x}$ and $z_i \geq 0$.

To solve this problem, we first note that given any pair of spending vectors (x_{-i}, z_{-i}), candidate i's marginal utility is always higher with respect to formal spending compared to informal spending. Therefore, the candidate will only spend through informal channels when she is binding at the cap. Second, given the structure of the game, candidate i's best response (x_i, z_i) can be written as a function of the aggregate input of other candidates $\tilde{Y}_i := \sum_{k \neq i} y_k$. Since the objective function is globally concave in spending, the unique best response function to $\tilde{Y}_i$ is:

$$(x_i, z_i) = \begin{cases} (0,0) & \text{if } x_i^* \leq 0 \\ (x_i^*, 0) & \text{if } 0 < x_i^* < \bar{x} \\ (\bar{x}, 0) & \text{if } x_i^* \geq \bar{x} \text{ and } z_i^* \leq 0 \\ (\bar{x}, z_i^*) & \text{otherwise} \end{cases} \tag{2.3}$$

where $x_i^* = \frac{1}{a_i}\left[\sqrt{\frac{a_i}{c_i}\tilde{Y}_i} - \tilde{Y}_i\right]$, and $z_i^* = \frac{1}{b_i}\left[\sqrt{\frac{b_i}{c_i}\tilde{Y}_i} - \tilde{Y}_i\right] - \frac{a_i \bar{x}}{b_i}$. Equation 2.3 distinguishes between four cases. In the first, the candidate does not enter the race because the costs of doing so outweighs her benefits. In the second case, the candidate enters the race and spends exclusively through formal means some amount under the cap. In the third, she spends the exact amount of the cap through formal channels, but does not spend additional funds informally. In the fourth and final case, the candidate spends up to the cap through formal channels, and then spends on top of this through informal channels.

Proposition 1. *There is a unique pure strategy Nash equilibrium for the simultaneous-move game played by the candidates.*

Proof. First, rewrite the best response function $\left(x_i(\tilde{Y}_i), z_i(\tilde{Y}_i)\right)$ into the input $y_i(\tilde{Y}_i)$ chosen by each candidate as a best response of the aggregate inputs of other candidates (see Appendix 2.7 for additional details). Then, transform these best response functions into share functions $s_i(Y)$ which represent the share of total inputs that a candidate will spend as a best response when total spending by other candidates is $\tilde{Y}_i \equiv Y - y_i$. We derive this function to be

$$s_i(Y) = \max\left\{\min\left\{\max\left\{1 - \frac{c_i Y}{a_i}, 0\right\}, \frac{a_i \bar{x}}{Y}\right\}, 1 - \frac{c_i Y}{b_i}\right\} \tag{2.4}$$

We can then sum the individual share functions into an aggregate share function: $S(Y) = \sum_{k=1}^{I} s_k(Y)$. This function is greater than 1 for sufficiently small values of Y, equal to zero for sufficiently large values of Y, is strictly decreasing whenever positive, and is continuous. Thus, there is a unique Y^* such that $S(Y^*) = 1$, which is the aggregate input in equilibrium. This value pins down the unique equilibrium spending (x_i, z_i) of each candidate. □

Comparative Statics We next consider how the spending cap $\bar{x}$ affects equilibrium outcomes. For the remainder of this section, we assume that there is at least one candidate whose formal spending is binding at the cap (otherwise, there are trivially no effects from a marginal change in the cap). For expositional purposes, we also assume that no candidate is at a knife-edge case whenever computing derivatives (i.e. we ignore the special cases $x_i^* = 0$, $x_i^* = \bar{x}$, and $z_i^* = 0$). The proofs for this section are included in Appendix 2.7.

Lemma 1. *Total equilibrium inputs in the contest are increasing in the spending cap, i.e.* $\frac{\partial Y^*}{\partial \bar{x}} > 0$.

Proposition 2. *The effects of spending limits on campaign expenditures.*

$$\frac{\partial x_i^*}{\partial \bar{x}} = \begin{cases} \frac{1}{a_i}\frac{\partial Y^*}{\partial \bar{x}}\left(1 - \frac{2c_iY^*}{a_i}\right) & \text{if } 0 < x_i^* < \bar{x} \\ 1 & \text{otherwise} \end{cases}$$

$$\frac{\partial z_i^*}{\partial \bar{x}} = \begin{cases} \frac{1}{b_i}\left[\frac{\partial Y^*}{\partial \bar{x}}\left(1 - \frac{2c_iY^*}{b_i}\right) - a_i\right] & \text{if } z_i^* > 0 \\ 0 & \text{otherwise} \end{cases}$$

While the above lemma states that total equilibrium inputs are increasing in the cap, the proposition shows that each candidate's expenditures is not necessarily increasing in the cap. This result is an extension to previous findings in the literature studying contests and all-pay auctions in the context of political lobbying.[10] To illustrate why formal spending is not necessarily increasing in the spending cap, consider a situation where there are two high-effectiveness entrants spending at the cap and a low-effectiveness entrant spending less than the cap. Whereas the binding candidates will increase their spending with an increase in the cap, the non-binding candidate will only increase her spending if her effectiveness is sufficiently high relative to her cost of fundraising (if $a_i > 2c_iY^*$).

Let us now consider a candidate who spends informally in equilibrium. A similar condition then determines whether this candidate will increase her inputs when the spending cap increases: i.e if $b_i > 2c_iY^*$. Whether this translates to an increase in informal spending is less obvious, as the candidate will substitute informal spending for formal spending. If other candidates are sufficiently increasing their inputs as a reaction to the increase in the cap ($\partial Y^*/\partial \bar{x}$ is large), it is possible for the candidate to increase both formal and informal spending. Otherwise, she will decrease informal spending because of substitution to formal spending.

Proposition 3. *The effects of spending limits on political entry.*

[10] Che and Gale (1998) consider a two-player all-pay auction and show that bid caps may increase total expenditures. On the other hand, considering an n-player contest, Fang (2002) finds that imposing an exogenous cap never increases total expenditures. In contrast to Fang (2002), our model also allows bidders to differ in their abilities to convert expenditures into inputs in the contest function, and hence we find that bid caps may have either effect in the n-player contest.

A candidate enters the race if and only if

$$\frac{a_i}{c_i} > Y^* \tag{2.5}$$

Therefore, the number of entrants in equilibrium decreases in the spending limit.

We find that increasing the spending cap decreases the number of entrants. Intuitively, this is because in equilibrium, total inputs into the contest Y are increasing in the spending limit. Thus, with higher spending limits, elections are more competitive in the sense that a candidate must make more expenditures to achieve the same vote share. On the other hand, the candidate's fundraising cost is the same for any cap, and hence she is less likely to enter when the cap is high.

An increase in the spending limit will also affect the composition of the pool of entrants. Equation (2.5) shows that the threshold to entry depends on the ratio of the candidate's popularity a_i to the marginal cost c_i. As the spending cap increases, the entrants with the lowest ratios will exit first. Suppose that a and c are uncorrelated across candidates. Then, increasing the limit will cause the entrants with the highest fundraising costs to drop out of the race. If the cost to fundraising is lower for wealthier candidates, this would result in a wealthier entrant pool. In addition, the entrant pool will be composed of more popular candidates. In this sense, only the most electable candidates will choose to run when limits are generous.

Proposition 4. *The effects of spending limits on electoral outcomes.*

Increasing the spending limit decreases the vote share of the candidates whose equilibrium formal spending is less than the cap, and increases the vote share of the candidates whose equilibrium formal spending equals the cap.

Finally, we show that an increase in the spending limit may increase or decrease an entrant's vote share. The main finding is intuitive: the candidates who spend less than the cap will face a more competitive contest under the high cap. This result has implications regarding the effect of spending limits on incumbency advantage. If incumbent characteristics are such that they are more likely to be binding spenders than challengers, then incumbency advantage will increase in the spending limit.

2.4 Research Design

We are interested in estimating the causal effects of campaign spending limits on political entry and selection. As we discussed in Section 2.2, prior to the 2016 municipal elections the Brazilian government imposed a cap on the amount of money a candidate could spend in the election. The law created a discontinuity in the spending cap for municipalities with a candidate that spent above R$142,857 in the 2012 elections.

Visually, the effects of the law on candidate spending for the 2016 elections can be clearly seen in Figure 2.3. For municipalities that did not have a 2012 candidate who spent above R$142,857, their candidates were capped at R$108,039. For the municipalities above this threshold, the spending cap jumps up by about 25 percent and then increases linearly as determined by the rule. It is also clear from Figure 2.3 that the caps were not binding for the majority of the municipalities. As a result, one should interpret our findings as intent-to-treat estimates.

To identify the effects of spending limits, we exploit the discontinuity at R$142,857 using a standard regression discontinuity design approach. Let $S_{m,2012}$ denote the maximum amount spent by a candidate in municipality m during the 2012 elections. The treatment effect on outcome $Y_{m,2016}$ of the spending cap is given by:

$$\text{Treatment Effect} = \lim_{s \downarrow 142,857} E[Y_{m,2016}|S_{m,2012} = s] - \lim_{s \uparrow 142,857} E[Y_{m,2016}|S_{m,2012} = s]. \tag{2.6}$$

The first conditional expectation measures the expected outcome at the threshold for municipalities in which candidates' campaign spending is capped at R$133,700. The second conditional expectation function measures the expected outcome at the threshold for municipalities in which candidates' campaign spending is capped at R$108,039. Under the assumption that these two conditional expectations are continuous in s, this difference estimates the causal effect of campaign spending limits on political outcomes, at the point of discontinuity.

We estimate these conditional expectations by local linear regression using only data within a bandwidth h of the threshold. Formally, we estimate the following OLS model, for $S_{m,2012} \in (142,857-h, 142,857+h)$,

$$Y_{m,2016} = \alpha + \beta \mathbb{1}\{S_{m,2012} > 142,857\} + \delta_0 S_{m,2012} + \delta_1 S_{m,2012} \mathbb{1}\{S_{m,2012} > 142,857\} + \varepsilon_{m,2016} \tag{2.7}$$

where $\mathbb{1}\{S_{m,2012} > 142,857\}$ is an indicator equal to 1 when $S_{m,2012} > 142,857$, and $\varepsilon_{m,2016}$ represents the error term. The parameter β measures the treatment effect. For our choice of bandwidth h, we rely on the approach developed by Calonico et al. (2014). This optimal bandwidth choice is a function of the data and is thus different for each outcome, $Y_{m,2016}$. We also explore the robustness of our results to alternative bandwidth sizes.

Before presenting our results, it is important to test the validity of our research design. In Panel (a) of Figure 2.4 we plot the density of our "running variable", $S_{m,2012}$. Unsurprisingly, we do not find any evidence of manipulation or endogenous sorting around the discontinuity threshold. This is completely expected: campaign expenditures are made public immediately following each election, and no one could have anticipated the recent law change back in 2012. As a point of comparison, Panel (b) of Figure 2.4 plots the distribution of campaign spending for the 2016 election. In contrast to the previous plot, Panel (b) does exhibit substantial bunching at the spending cap of R$108,039.

Another general concern associated with regression discontinuity designs is the possibility that other determinants of our outcomes of interest are also varying discontinuously at the cutoff point. Although we cannot directly test this assumption for unobserved characteristics, we can examine whether any observable characteristics of the municipality also exhibit discontinuous jumps at the cutoff point. In Figure 2.5, we present a series of plots, exploring various municipal characteristics that are correlated with our political outcomes of interest, such as GDP per capita, illiteracy, and the share of the urban population. In each graph, we plot a bin scatter of the municipal characteristic against the maximum amount a candidate spent in the municipality during the 2012 elections (i.e. our running variable). In addition to these binned averages, we also fit a second-order polynomial on each side of the point of discontinuity and 95% confidence intervals for each bin. We do not find any evidence of other characteristics jumping at the cutoff point. All the differences are close to zero in magnitude and statistically insignificant. Importantly, these comparisons also include our main political outcomes of interest but measured for the 2012 elections (i.e. the "pre-treatment period"). These plots represent only a subset of the characteristics for which we tested. Table 2.2 presents the entire set. Out of the 19 municipality characteristics tested, only one displayed a discontinuous jump at the cutoff point (population). But whether or not we control for population does not affect any of our point estimates.[11] For our main specification, we control for population and other municipal characteristics measured in the 2010 Census.

2.5 Results

Effects of Spending Caps on Campaign Expenditures and Contributions

In this section, we estimate the causal effects of the spending caps on candidates' campaign spending and contributions. We begin with the graphical evidence. In Panel A of Figure 2.6, we plot binned averages of the amount candidates spent in the 2016 elections against our running variable (the maximum amount spent by a candidate in the 2012 elections centered at R$142,857). We also fit a second-order polynomial, separately estimated on each side of the discontinuity. The discontinuity at zero provides an estimate of the gap in candidates' campaign spending imposed by the law. The estimated discontinuity implies that a 25 percent increase in the spending cap increased maximum campaign spending by approximately 12 percent during the 2016 elections for municipalities near the discontinuity. In Panel B, we reproduce the graph presented in Panel A, but for the mean amount spent by a candidate. We see a similar increase of approximately 10 percent, which further suggests that the caps did bind for many candidates.

We refine the graphical analysis in Table 2.3. Each row corresponds to a different dependent variable, and each numbered column presents the estimated impact for a different regression specification. In column 1, we present our baseline estimates of Equation 2.7, using the bandwidth proposed by Calonico et al. (2014). In columns 2 and 3, we explore the robustness of our estimates to different bandwidth choices. In column 4, we further test the sensitivity of our results by fitting a local quadratic polynomial on each side of the discontinuity instead of a local linear polynomial.

Our results are robust to these various modeling choices. In our baseline specification, the highest-spending candidate just to the left of the discontinuity spent on average R$84,823 to become mayor, compared to R$95,036 for candidates in municipalities just to the right of the discontinuity. This represents a 12.0 percent increase in spending. The point estimates in columns 2-4 are similar: they indicate increases in maximum spending ranging from 11.6 to 13.9 percent. The estimates on average spending, although a bit noisier, are also consistent across specifications.

[11] These results are available upon request.

They imply that the higher spending cap led to increases in mean spending ranging from 8.5 to 11.7 percent. To have a sense of the magnitudes, the estimate of R$10,000 would be enough to hire 10 full time workers at the minimum wage to knock on voters' doors for 1 full month. Alternatively, R$5,000 to R$8,000 would be enough to hire a digital marketing professional for one month to manage a campaign's digital platform and social media.

The theory does not provide clear predictions on the effects of a spending cap on the minimum or total amount spent in an election. In some cases, a higher spending cap will induce the minimum-spending candidate to reduce spending further, or even exit the race. Thus increasing the spending cap does not necessarily lead to an increase in the minimum or total spending within a race. Consistent with this ambiguous prediction, we do not find significant effects on either of these two outcomes. The minimum amount spent by a candidate is similar on both sides of the point of discontinuity: we estimate a statistically insignificant increase of R$989. Similarly, we also find a statistically insignificant increase in total spending of about 4 percent at the cutoff point.

In Table 2.4, we consider the effects of spending caps on the amount and composition of the candidates' campaign contributions. On average, candidates spend 99% of their campaign contributions.[12] Reflecting our findings on spending, we find that the average amount of campaign contributions raised by candidates are R$6,179 higher for municipalities with the higher limit. Approximately 75% of this increase comes in the form of candidates financing their own campaigns, which likely stems from the law's ban on corporate donations. In 2012, candidates received on average 16 percent of their contributions from corporations, and self-financed 25 percent of their campaigns. In contrast, 2016 candidates self-financed 40 percent of their campaign expenditures. We can interpret these results in two ways. On the one hand, in the face of the corporate ban, the higher caps induced the existing candidates to contribute more to their own campaigns. On the other hand, higher caps may have attracted a wealthier pool of candidates with greater financial means to run for office. We examine this possibility in the next section.

Do Spending Caps Affect Candidate Entry?

According to our model, the number of candidates who enter the race should decrease as spending limits increase. Additionally, higher spending limits may also attract individuals who have a higher ex-ante probability of winning. We test these predictions in Table 2.6. As before, the rows indicate different dependent variables, and the numbered columns present the estimated effects of the caps for different modeling choices.

Spending caps affect the entry decisions of potential candidates. Compared to the municipalities just to the right of the threshold (i.e. the less constrained municipalities), the cap led to a 0.26 increase in the number of candidates for municipalities capped at R$108,039. On average, 2.9 candidates run for mayor, so this effect represents a 9 percent increase in the size of the candidate pool. This result is presented visually in Figure 2.7. In contrast to the plot presented in Panel C of Figure 2.5, which displayed the effects on the number of candidates who participated in the 2012 elections, we see a significant jump in the number candidates at the point of discontinuity.

[12] In Brazil, candidates are not allowed to accumulate war chests.

To test whether this increase in candidate entry actually increased political competition, we study the effect of the cap on the effective number of candidates. This measure is computed by taking the inverse of the sum of squared vote shares of each running candidate within an electoral race. If all candidates have the same vote share, then this measure is equal to the actual number of candidates. At the other extreme, if one candidate wins every vote, then the effective number of candidates is one. If a change in the spending cap only leads to the entry or exit of candidates winning few or no votes, then we would not find an effect on the effective number of candidates. On the contrary, we find that the more restrictive spending cap increases the effective number of candidates by 0.143, suggesting that the restrictive cap did increase the competitiveness of mayoral races.

In Table 2.6, we also test whether higher caps affected the types of parties that entered the contest based on their size and ideology. To measure ideology, we rely on a measure of party position along a left-to-right scale as created by Power and Zucco (2012). The index, which ranks parties from 1 (="left") to 10 (="right"), is constructed from a survey of federal legislators elected in 2006. We find no evidence that the caps impacted the average ideological score of the candidate pool, nor the tails of the distribution. The increase in political competition was also not entirely the result of smaller parties entering into the race: higher caps reduced the number of smaller parties by 6 percent, although the effects are imprecisely measured.[13]

To examine whether higher spending limits also induce greater participation from individuals with a higher ex-ante propensity to get elected, we first estimate the probability of winning the 2016 election based on the follow set of observable characteristics: gender, age, race, education level, political experience[14], party affiliation and self-reported assets. We estimate this propensity score for the sample of candidates that are outside the bandwidth of the RD regressions. The results, which are reported in Table 2.5, suggest that candidates who are male, wealthier, incumbents, or have more political experience are more likely to win. Based on these estimates, we then impute a candidate's ex-ante probability of winning the election.

We find that individuals with higher expected winning probabilities are more likely to participate in municipalities with a higher spending limit. For a 25 percent increase in the spending limits, high-propensity types are 2.0 percentage points more likely to enter, which represents a 6 percent increase.

To see where these effects are coming from, in the remaining rows, we estimate the effects on individual attributes of the candidate pool. Although the estimates tend to be fairly noisy across the various attributes, higher limits do appear to affect an important factor: they tend to attract wealthier candidates. In our baseline specification, the average level of assets among candidates is 40 percent higher in municipalities with a higher spending cap. This result is perhaps unsurprising given our finding that the majority of the extra spending under the high-cap is self-funded.

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[13]We define the "small" parties to be all political parties except for the six most successful in the 2012 municipal elections: the PMDB, PP, PSB, PDB, PSDB and PT. Together, these six parties won the majority of mayoral elections in 2012. In total, there are thus 30 small parties in the 2016 elections. Our results are robust to the choice of party classification.

[14]The number of times a candidate was elected to any political position since the 2000 election.

Spending Caps and Political Selection

While restricting campaign spending does increase political competition, it appears to do so at the cost of attracting individuals with a lower ex-ante propensity to be elected. Whether spending caps affect political selection is therefore an empirical question.

The graphical evidence presented in Figure 2.8 suggest that it does. Here, we plot binned averages of re-election rates against the maximum amount spent in the municipality by a candidate for the 2012 elections. In computing this graph, we restrict the sample to the 2,721 incumbents who were eligible for re-election. We see a positive jump in the reelection rate at the point of discontinuity.

In Table 2.7 we refine the analysis further, by considering a range of alternative specifications and by conditioning on whether or not the mayor ran for re-election. In Panel A we consider all incumbents who are not term-limited, whereas in Panel B we consider only those who run for re-election. When considering the whole sample of eligible incumbents, re-election rates increase by 11 percentage points at the point of discontinuity, which is a sizable effect given that the baseline re-election rate is only 23 percent. Among those that ran for re-election, the effects are similar: we estimate a 16 percentage point increase in the re-election rate from a baseline of 38 percent. To further analyze whether incumbents benefit from the higher cap, we test whether incumbents see their vote shares increase or decrease as a function of the cap. We find that incumbent vote shares increase by 6.6 percentage points under the higher cap.

Considering the apparent incumbency advantage granted by the higher spending cap, it is plausible that more incumbents choose to run for re-election under the high cap. We do not, however, find this to be the case. Although the standard errors are admittedly large, incumbents are not more likely to run for re-election in places with higher spending limits.

Why do incumbents benefit from the higher spending limit? According to the model, a candidate's electoral success depends on her share of inputs into the competition. Increasing the cap will benefit the incumbent if it induces fewer challengers to enter the race, or in addition if challengers cannot match the incumbent's increase in spending due to higher fundraising costs. The data are also consistent with this latter hypothesis. We find that incumbent spending increases significantly by R$10,312 under the high spending cap. On the other hand, total challenger spending hardly increases by a statistically insignificant R$1,108.

In Table 2.8, we explore whether the spending caps also affected the characteristics of the winners. Other than being an incumbent, we do not find much evidence that the caps changed the identity of the winner. The one exception is that there is some evidence that the caps led to the election of wealthier candidates (at the 90% significance level). Interestingly, the spending caps also did not impact the likelihood that a mayor from the Workers' Party (PT) won, despite the party experiencing sweeping losses in local elections throughout the country due to its involvement in the national corruption scandal.

Finally, in Table 2.9, we investigate the effects of the spending caps on the contributions of the winning candidates. We find that the winners under the high cap raised more campaign funds on average than those under the low cap. Moreover, our results suggest that the entirety of this difference is explained by the difference in the amount of funds that candidates self-finance. Indeed, we do not find evidence that winners under the high cap have raised more individual, party, or other donations. Thus, together with our evidence of the effect of caps on candidate assets, our results suggest that high spending limits benefit wealthier candidates, who spend their own funds to get elected.

Spending Caps, Campaign Technologies, and Information

As our model predicts, candidates who face a stricter cap may resort to other forms of campaigning that do not count against their spending limit, such as the use of social media or relying on "dark money".

Social Media To test whether politicians are substituting towards more social media use, we estimate the impact of spending limits on Facebook campaigning activity by mayoral candidates.[15] To find a candidate's Facebook page, we searched on Google for the "Candidate's Ballot Name + Candidate's Number + City name + Facebook" and scraped the link of the first Google search result using the Facebook API.[16] This procedure indicates that 35% of mayoral candidates had a Facebook page during the election period.[17] For each candidate, we count the number of Facebook posts and the number of reactions that followers had for each post (likes and comments). Figure 2.10 plots the daily number of Facebook posts by mayoral candidates in 2016. It shows that candidates disproportionately use Facebook during the election period, especially in the days just before the election.

Table 2.10 presents our estimates of the effect of spending limits on the probability that a candidate had a Facebook page, the number of posts, and the number of reactions. We find that a lower spending limit increases the likelihood of having a Facebook page by 6 percentage points (or 18 percent given a mean of 0.345). A lower limit also increases the average number of posts by candidates by 18 percent and the number of reactions (e.g. likes) of these posts by 35%. These results suggest mayoral candidates facing a low cap did compensate for the cap by investing more on social media campaigning.[18]

Dark money and irregularities in accounts Politicians who are constrained by the cap might also resort to the use of dark money. Although dark money is difficult to measure, we have two different proxies for the use of undeclared campaign funds. A common vehicle for dark money to appear in politics is for the politician to claim the donation (and hence the expenditure) as in-kind rather than a cash expenditure. In these cases there is no formal receipt of the contribution (i.e. no paper trail) and the candidate estimates the value of the amount spent. An example is

[15] Brazil is one of the largest users of Facebook in the world.

[16] A candidate was coded as not having a Facebook page if: i) the first search result was not a Facebook page, ii) the Facebook page was of a news web site, iii) the search for two different candidates yielded the same Facebook page.

[17] Similar results were found by manually searching a small sample of candidates' Facebook pages.

[18] We also estimated the effects separately for incumbents and challengers, see Table 2.11. Although our point estimates are slightly larger for challengers, we cannot reject the null hypothesis that the effects between challengers and incumbents are the same.

the use of a restaurant to host a fund-raising event where the candidate self-reports the cost of renting the restaurant. In this case, it is easy to under-declare the amount spent. Because we have data on which expenditures have receipts and which are self-declared values, we can test whether candidates are more likely to declare in-kind spending when a municipality has a lower spending cap.

In Table 2.12, we estimate the effects of the spending caps on the amount of contributions, distinguishing between cash contributions versus in-kind. We present the estimates for both the pool of candidates, as well as the election winners. In both cases, the effects of the caps are on cash contributions, as opposed to the in-kind contributions. Although politicians may channel dark money in other ways, we do not find any evidence that spending caps impacted the types of contributions politicians receive.

Candidates who are subject to tighter spending limits might want to manipulate their accounts and spend in ways that go against the rules imposed by the electoral commission. To test this, we assembled a dataset on the decisions of the electoral judges who evaluated the candidates' accounts.[19] In total, we collected judges' decisions on the campaign finances of 10,735 candidates, and of these 67% were approved. Among the rejected accounts, commonly cited irregularities included acts of vote buying or use of undeclared funds.

In Figure 2.9a, we test whether the spending caps affected the share of candidates who campaign finances were found to be irregular. We do not find any significant difference in the share of irregular accounts when comparing municipalities near the discontinuity. The point estimate is -0.002 (s.e. = 0.03). This results that mayors did not change their behavior and commit more (observable) irregularities when faced with a stricter spending limit.

Party Spending When parties spend on behalf of their candidates, this expenditure counts against the candidate's spending totals and is thus subject to the limits. In some cases, however, it is difficult to determine the exact amount of the party's contribution. A common example is when the party hosts an event or produces an advertisement for several of its candidates. Given that we observe party expenditures, we can investigate whether parties exploit this loophole by testing for whether party expenditures at the municipal level respond to the spending caps. But as the Figure 2.9b depicts, we do not find any evidence that parties are substituting for the lack of spending in the municipalities with the lower limit.

Voter Knowledge A key argument against imposing spending caps is that with less spending, voters may become less informed. In the previous section we showed that campaigning through social media and citizen engagement through Facebook increased when municipalities face a more stringent cap of campaign spending, which goes against the idea that caps make citizens less informed. In this section we use two alternative measures of voter knowledge to test this hypothesis. First, several studies have documented a strong association between political knowledge and both turnout and invalid votes (e.g. Lassen (2005)). We test whether turnout is lower and invalid votes

[19]These data were scraped from the website www.jusbrasil.com.br for decisions published after September 2016.

are higher when candidates face a lower spending cap. Second, we use a direct measure of information by counting the number of times candidates' names are searched on Google.

In Table 2.13 we report estimates of the effects of spending caps on turnout and the share of blank or invalid votes. Although these are imperfect proxies for voter information, we find no evidence to support the hypothesis that lower spending caps will lead to less informed voters. In both cases, our estimates are precisely estimated zeros.

To further evaluate the impact of spending limits on voters' knowledge, we estimate its impact on the number of times candidates names are searched on Google. If a higher spending limit increases electorate knowledge, it is likely that more voters will search for mayoral candidates by their names online. We used Google Adwords too construct the number of monthly searches each candidate received.[20] Google Adwords only gives ranges on the number of searches: 0-10, 10-100, 100 - 1k, 1k - 10k, 10k-100k, etc. Hence, we created an index of Google searches. Table 2.14 shows the distribution of this index across candidates in September 2016.

Figure 2.11 plots the evolution of the number of Google searches for candidates' names. The plot clearly shows that voters interest on candidates grows as the election becomes closer, peaking in September, the month just before the election. We use average index of Google searches across candidates to test whether spending caps affect searches. Table 2.15 reports the impact of the high spending cap on the average index of Google searches across candidates in a municipality. Results suggest that a higher spending limit does not lead to an increase in the number of searches for candidates' names. In fact, the point estimates suggest a decrease in the number of searches under the high cap, although these results are not significant at usual levels of confidence. If voters were to become more informed under the high cap, challengers were the ones who would probably get a larger increase in searches since they are less well known in the beginning of the race. When we break the results by incumbents' and challengers' searches, results suggest that a higher spending limit does not affect incumbents' names searches and, if anything, reduces searches for challengers' names.

An alternative interpretation of the analysis above is that more information about a candidate leads to less Google searches because voters already know about the candidate. To test whether Google searches are complements or substitutes with information, we correlate Google searches with TV and radio advertising time across candidates. Radio and TV political advertising is regulated and candidates' air time is a function of the representativeness of their party coalition in Congress.[21] Results in Table 2.16 show that candidates' ad time share is positively correlated with his Google searches after controlling for municipality fixed effects and several candidates' characteristics. This can be interpreted as evidence that as voters get more informed about candidates they search more about them on Google. In sum, we do not find evidence that the spending limits affected voters' knowledge based on our different proxies of access to information.

[20] First, we drop all candidates that in the same state have the same ballot name (978 candidates). After that, Adwords gives us the number of searches candidates ballot name had in the states where they are running.

[21] 10% of airtime is splited equally among all candidates in the municipality and 90% is split according to the seat share of their coalition in Congress.

Political Behavior

Who is in office matters for what types of policies are selected and how they are implemented. The primary responsibility of mayors in Brazil is to provide local public goods, which are mainly funded from federal transfers. While a formula determines a large part of the amount of transfers a municipality receives, mayors can also procure additional funding by applying for block grants. These block grants are formal agreements between the municipality and the federal government specifying the provision of a particular public good or service. Whether a municipality receives a block grant is a function of the mayor's effort, ability, and political networks.[22]

In 2017, 85% of municipalities received a block grant, and on average these block grants amount to about 7% of the funds they receive from the federal government. In Table 2.17, we estimate the effects of spending caps on the amount and type of block grants municipalities received in 2017. Municipalities to the left of the discontinuity receive on average 2.74 block grants at an average value of R$1.2 million. Municipalities to the right of the discontinuity receive 13.6% more grants for an additional funds of R$524,000 (or an 44.5% increase in average value). These additional funds are disproportionately in agriculture, which could reflect the fact that the municipalities near the threshold tend to be predominately rural (see Table 2.18 for how the municipalities within the optimal bandwidth compare to those outside).

Our findings suggest a political selection effect: places with higher spending caps elected more experienced and wealthier candidates, who might be more connected and better able to obtain additional funding. But places with higher caps also had higher reelection rates and thus, more mayors that are serving on a second term. This could confound this explanation if these mayors tend to receive more resources from the federal government. But as previously shown in Ferraz and Finan (2011), mayors serving on a second term, who are ineligible for reelection due to term-limits, are less likely to secure block grants.

In Table 2.19, we present the partial correlations between mayor characteristics and the amount of blocks the municipality received. Consistent with our interpretation, we find that while incumbents are associated with less additional funding, the experience, education, and wealth of the mayor are strongly correlated with obtaining additional funding.

In sum, we find strong evidence that spending caps matter for who is elected into office, which in turns affects political behavior. These results do not, however, inform us about voter welfare. Brazil's constant battles with local corruption and waste imply that these block grants while necessary are not sufficient for the provision of additional public goods.[23] We would also like to caution the reader that because of data availability we are only measuring a specific type of mayor's behavior during the first year in office.

[22]See for instance Brollo and Nannicini (2012) and Azulai (2017).

[23]See for instance Ferraz et al. (2012)

2.6 Discussion

The role of money in politics is widely debated in many democracies. This paper examines the effects of limiting how much money candidates can spend on their campaigns. We exploit a natural experiment induced by an electoral reform in Brazil that set a lower spending cap for some municipalities compared to others. Using data on the number of candidates, their characteristics, and voting outcomes, we find that setting a more stringent limit on campaign spending increases political competition, reduces the chances of richer candidates getting elected, and reduces reelection rates.

These findings suggest that in countries where high levels of spending have become an equilibrium outcome due to corruption and the influence of special interests, setting a spending limit might increase political competition and allow for new entrants into politics. In countries where political elites come disproportionately from richer families, this policy might also reduce the concentration of political power in the hands of richer individuals. These effects might have direct and indirect consequences for policy outcomes.

By reducing the cost of political campaigns, spending limits might also reduce the incentives incumbent politicians have to divert resources from public funds for their campaigns. Whether campaign spending limits reduce corruption or affect project choices by elected politicians are important topics for future research.

2.7 Appendix

Additional details for the proof to Proposition 1 The best response function $\left(x_i(\tilde{Y}_i), z_i(\tilde{Y}_i)\right)$ can be transformed to the best response function $y_i(\tilde{Y}_i)$ as follows:

$$y_i = \begin{cases} 0 & \text{if } y_i^+ \leq 0 \\ y_i^+ & \text{if } 0 \leq y_i^+ \leq \bar{y}_i \\ \bar{y}_i & \text{if } y_i^- \leq \bar{y}_i \leq y_i^+ \\ y_i^- & \text{if } \bar{y}_i \leq y_i^- \end{cases} \tag{2.8}$$

where $y_i^+ = \sqrt{\frac{a_i}{c_i}\tilde{Y}_i} - \tilde{Y}_i$, $y_i^- = \sqrt{\frac{b_i}{c_i}\tilde{Y}_i} - \tilde{Y}_i$, and $\bar{y}_i = a_i\bar{x}$.

Then, we make the transformation $s_i(Y) = \frac{y_i(\tilde{Y}_i)}{Y}$ with $Y = \tilde{Y}_i + y_i$ and we obtain equation (2.4).

Lemma 1 *Total equilibrium inputs in the contest are increasing in the spending cap, i.e.* $\frac{\partial Y^*}{\partial \bar{x}} > 0$.

Proof: By equation (2.4), we have $\frac{\partial s_k(Y)}{\partial \bar{x}} > 0$ for $Y > 0$ if k is binding and $\frac{\partial s_j(Y)}{\partial \bar{x}} = 0$ for $Y > 0$ if j is not binding. Therefore, since at least one candidate is binding, $\frac{\partial S(Y)}{\partial \bar{x}} > 0$ for $Y > 0$. Recall that equilibrium total inputs Y^* is given by $S(Y^*) = 1$. Hence it follows that $\frac{\partial Y^*}{\partial \bar{x}} > 0$.

Proposition 2 *(The effects of spending limits on campaign expenditures.)*

$$\frac{\partial x_i^*}{\partial \bar{x}} = \begin{cases} \frac{1}{a_i}\frac{\partial Y^*}{\partial \bar{x}}\left(1-\frac{2c_iY^*}{a_i}\right) & \text{if } 0 < x_i^* < \bar{x} \\ 1 & \text{otherwise} \end{cases}$$

$$\frac{\partial z_i^*}{\partial \bar{x}} = \begin{cases} \frac{1}{b_i}\left[\frac{\partial Y^*}{\partial \bar{x}}\left(1-\frac{2c_iY^*}{b_i}\right)-a_i\right] & \text{if } z_i^* > 0 \\ 0 & \text{otherwise} \end{cases}$$

Proof: Suppose that $0 < x_i^* < \bar{x}$. Then $s_i(Y) = 1 - \frac{c_iY}{a_i}$, and $x_i(Y) \equiv \frac{Ys_i(Y)}{a_i} = \frac{Y}{a_i} - \frac{c_iY^2}{a_i^2}$. Then the first result follows by differentiating $x_i(Y)$ with respect to $\bar{x}$. Suppose instead that $x_i^* > \bar{x}$. Then $x_i = \bar{x}$ and the result follows immediately.

Now suppose that $z_i^* > 0$. Then $s_i(Y) = 1 - \frac{c_iY}{b_i}$, $y_i(Y) = Y - \frac{c_iY^2}{b_i}$ and $x_i(Y) = \bar{x}$. Therefore, since $y_i \equiv a_ix_i(Y) + b_iz_i(Y)$, we have $z_i(Y) = \frac{Y}{b_i} - \frac{c_iY^2}{b_i^2} - \frac{a_i\bar{x}}{b_i}$. The result then follows by differentiating $z_i(Y)$ with respect to $\bar{x}$. Finally, suppose that $z_i^* < 0$. Then $z_i = 0$ and the result follows immediately.

Proposition 3 *(The effects of spending limits on political entry.) A candidate enters the race if and only if*

$$\frac{a_i}{c_i} > Y^*$$

Therefore, the number of entrants in equilibrium decreases in the spending limit.

Proof: From Lemma 1, we have that $\frac{\partial Y^*}{\partial \bar{x}} > 0$, that is, total inputs are increasing in the spending cap. From equation (2.4), the condition for strictly positive spending (and hence by definition, entry) is $\frac{a_i}{c_i} > Y^*$. Therefore the number of candidates for which this condition holds is decreasing in Y^*, and hence decreasing in the spending limit $\bar{x}$.

Proposition 4 *(The effects of spending limits on electoral outcomes.) Increasing the spending limit decreases the vote share of the candidates whose equilibrium formal spending is less than the cap, and increases the vote share of the candidates whose equilibrium formal spending equals the cap.*

Proof: Let $\mathscr{J}$ denote the set of candidates who are non-binding and let j index elements of this set. Then $s_j(Y) = 1 - \frac{c_jY}{a_j}$. Since $\frac{\partial Y^*}{\partial \bar{x}} > 0$ by Lemma 1, we have $\frac{\partial s_j(Y^*)}{\partial \bar{x}} < 0$ for all $j \in \mathscr{J}$. Therefore $\frac{\partial \sum_{j\in\mathscr{J}} s_j(Y^*)}{\partial \bar{x}} < 0$, i.e. the vote share of non-binding candidates is decreasing in the spending limit.

Let $\mathscr{B}$ denote the set of candidates who are binding and index the elements of this set by b. These are candidates whose formal spending is equal to the spending limit, and whose informal spending may or may not be strictly positive. We have $S(Y) = \sum_{j\in\mathscr{J}} s_j(Y) + \sum_{b\in\mathscr{B}} s_b(Y)$. Since in equilibrium we must have $S(Y^*) = 1$, we have $\frac{\partial S(Y^*)}{\partial \bar{x}} = 0$. Therefore $\frac{\partial \sum_{b\in\mathscr{B}} s_b(Y^*)}{\partial \bar{x}} > 0$, that is the vote share of binding candidates is increasing in the spending limit.[24]

[24]Note that this not necessarily imply that the vote share is increasing for *each* binding candidate.

2.8 Figures and Tables

Figure 2.1: Timeline

09/29/2015	Law 13.165 establishes preliminary limits on campaign spending for the 2016 election
07/05/2016	Beginning of intra-party campaign for mayor candidate
07/20/2016	Establish final limits on campaign spending valid for 2016 election
07/20/2016-08/05/2016	Parties chose candidates
08/15/2016	Last day for candidacy registration
08/16/2016- 09/30/2016	Campaign period
10/02/16	Municipal elections

Figure 2.2: Campaign Spending Limits in 2016

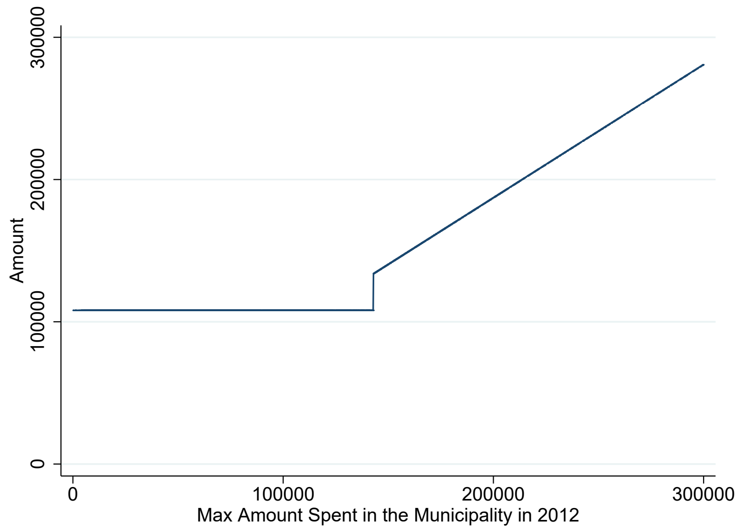
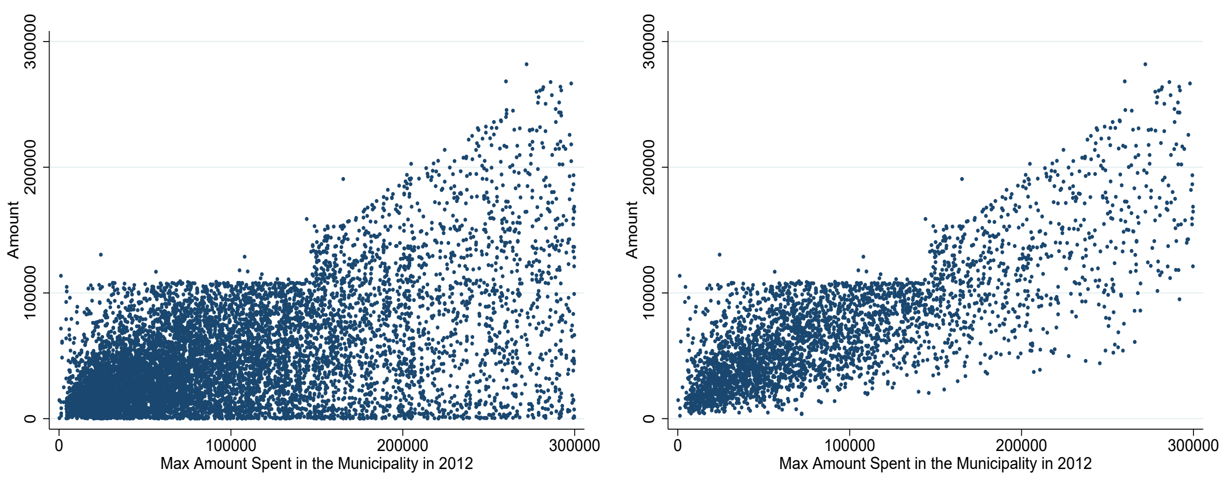


Figure 2.3: Campaign Spending in the 2016 Elections

(a) Candidate spending

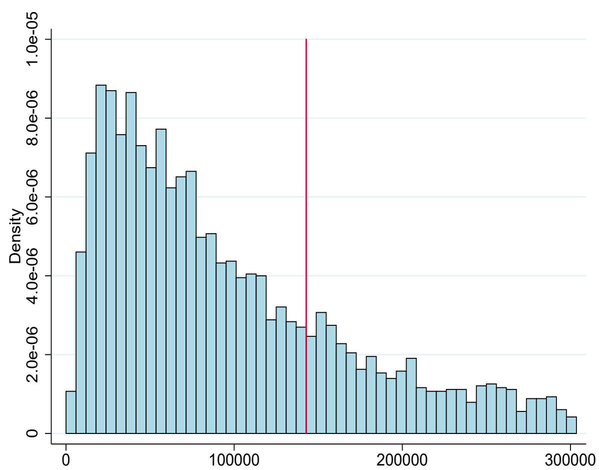
(b) Maximum spending by municipality



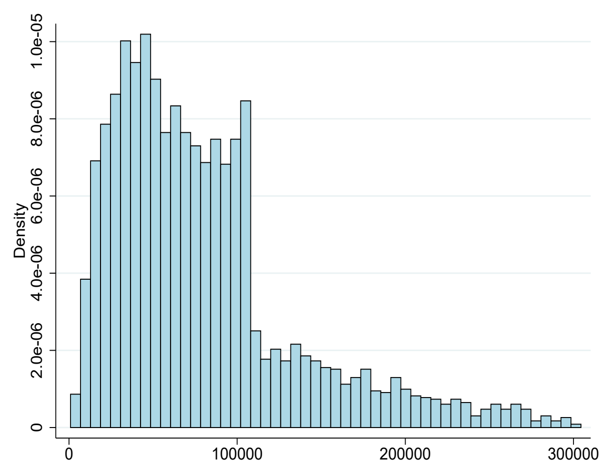
Notes: In panel (a), each point denotes the amount spent by a candidate in the 2016 elections. In panel (b), each point denotes the maximum amount spent by a candidate by municipality in the 2016 elections.

Figure 2.4: Campaign Spending in the 2012 and 2016 Elections

(a) Maximum amount spent in 2012 elections

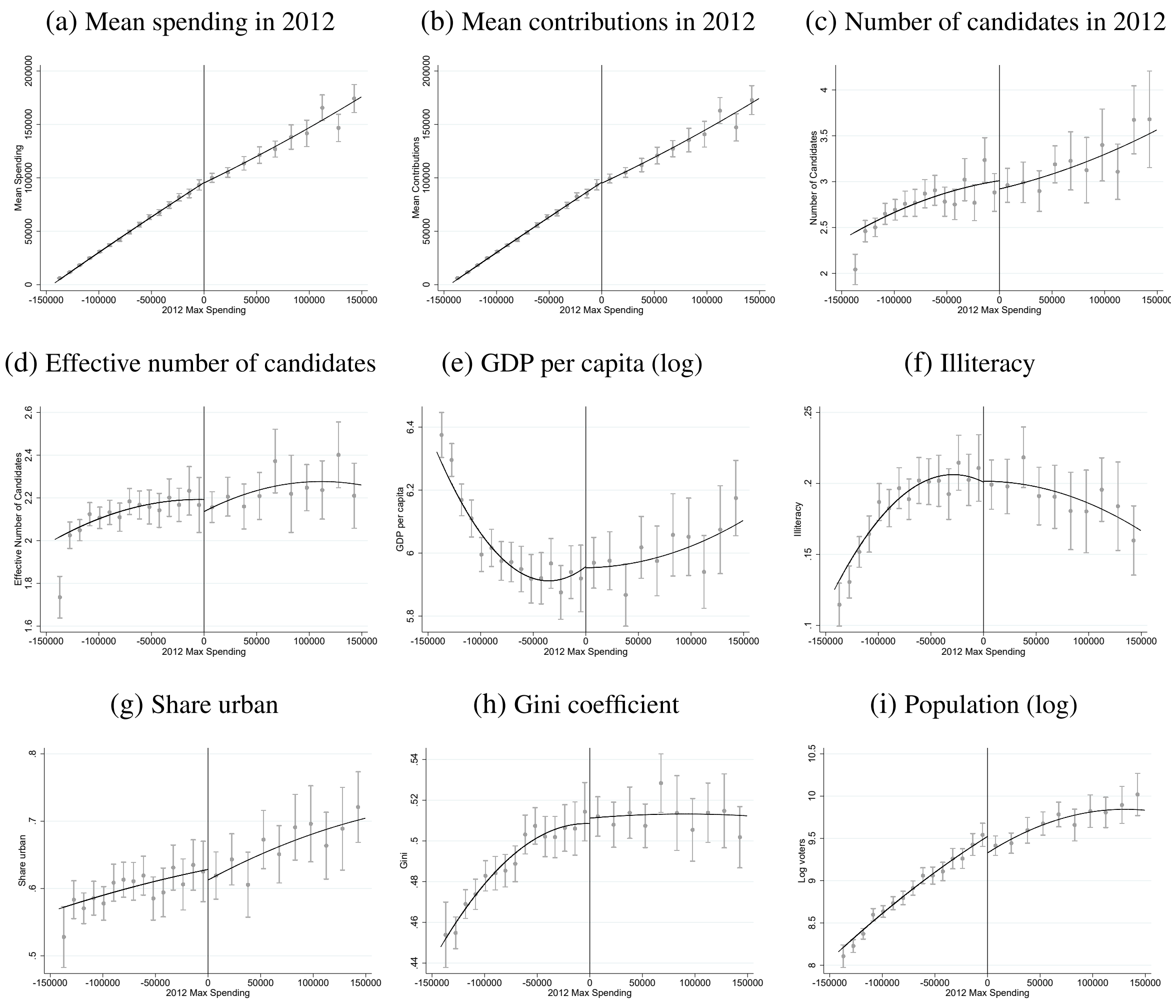


(b) Maximum amount spent in 2016 elections



Notes: Panel (a) plots the distribution of the running variable, the maximum amount spent by a candidate within a municipality in the 2012 elections. The red line denotes the discontinuity of the rule at R$142,857. Panel (b) plots the distribution of the maximum spent on campaigning by a candidate within a municipality in the 2016 elections.

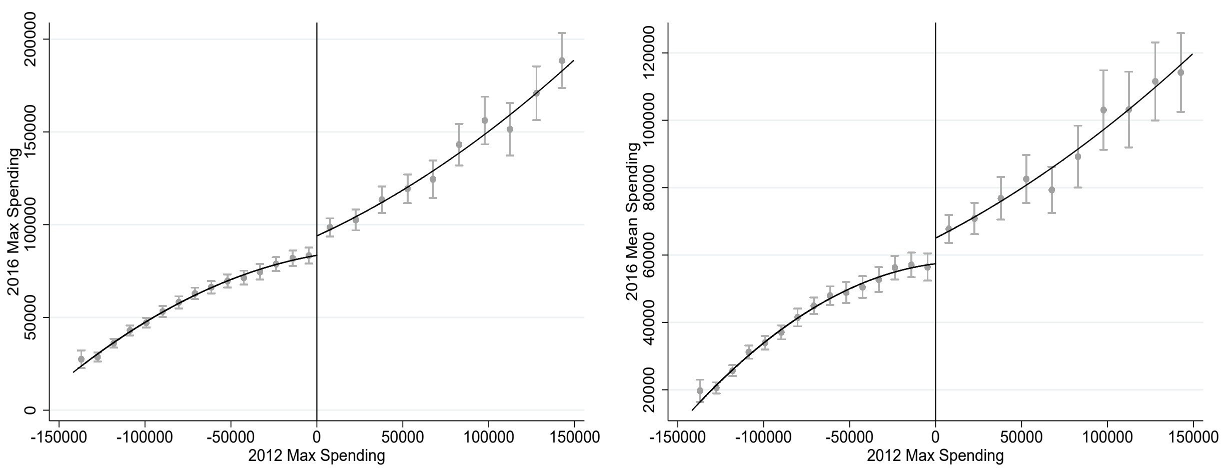
Figure 2.5: Discontinuities in Municipal-level Baseline Covariates



Notes: These figures plot the results of RD regressions of various municipal characteristics on maximum spending in 2012 (the running variable). The horizontal axis denotes the difference in maximum spending relative to the discontinuity at R$142,857. In each regression, a global second-order polynomial is estimated on each side of the discontinuity. Each point denotes the sample-average within a bin. 95% confidence intervals for each bin are computed using the methods of Calonico et al. (2014).

Figure 2.6: Effects of Spending Limits on Campaign Expenditures

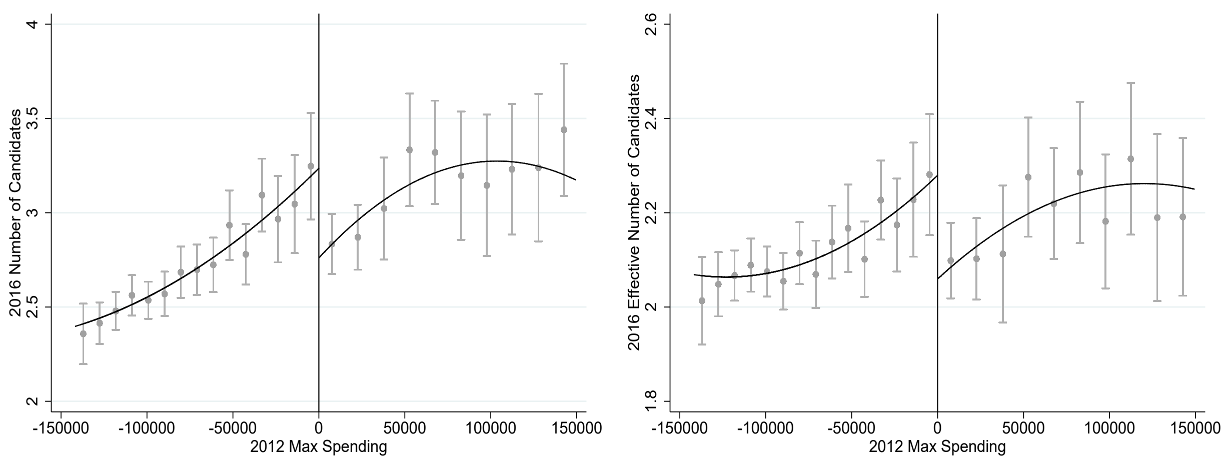
(a) Maximum candidate spending (b) Mean candidate spending



Notes: This figure plots the results of the regression discontinuity design, where the dependent variable is respectively (a) the maximum spending by candidates and (b) the mean spending by candidates. The horizontal axis denotes the difference in maximum spending relative to the discontinuity at R$142,857. In each regression, a global second-order polynomial is estimated on each side of the discontinuity. Each point denotes the sample-average within a bin and its corresponding 95% confidence interval.

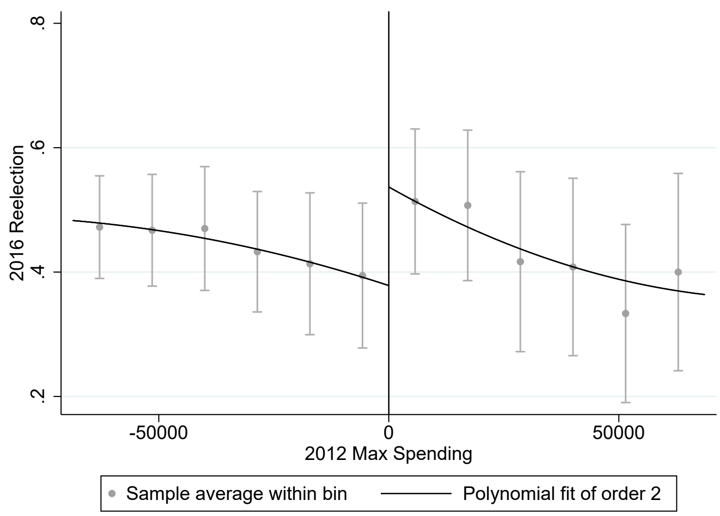
Figure 2.7: Effects of Campaign Spending Limits on Political Competition

(a) Number of candidates (b) Effective number of candidates



Notes: This figure plots the results of the regression discontinuity design, where the dependent variable is (a) the number of candidates, and (b) the effective number of candidates. The horizontal axis denotes the difference in maximum spending relative to the discontinuity at R$142,857. In each regression, a global second-order polynomial is estimated on each side of the discontinuity. Each point denotes the sample-average within a bin and its corresponding 95% confidence interval.

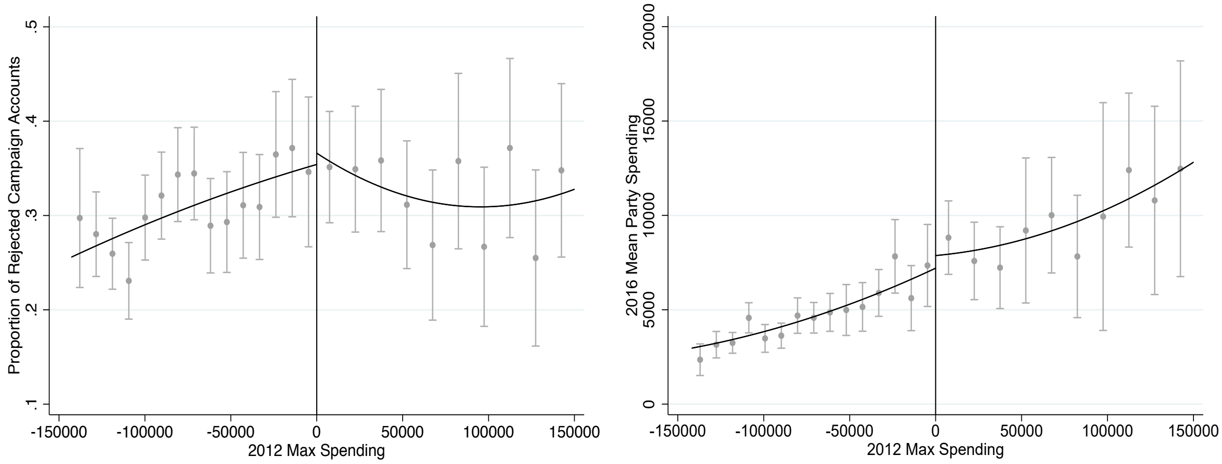
Figure 2.8: Effects of Campaign Spending Limits on Reelection



Notes: This figure plots the results of the regression discontinuity design, where the dependent variable is a dummy for whether the incumbent is reelected. The sample is restricted to incumbents who run for reelection. The horizontal axis denotes the difference in maximum spending relative to the discontinuity at R$142,857. In each regression, a global second-order polynomial is estimated on each side of the discontinuity. Each point denotes the sample-average within a bin and its corresponding 95% confidence interval.

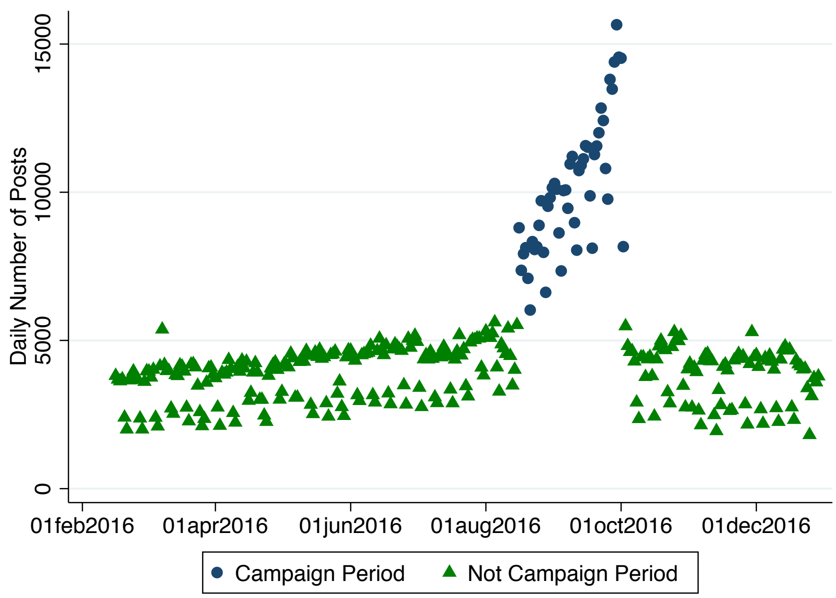
Figure 2.9: Effects of Campaign Spending Limits on Other Forms of Spending

(a) Effects of Spending Limits on Rejected Accounts (b) Effects of Spending Limits on Party Spending



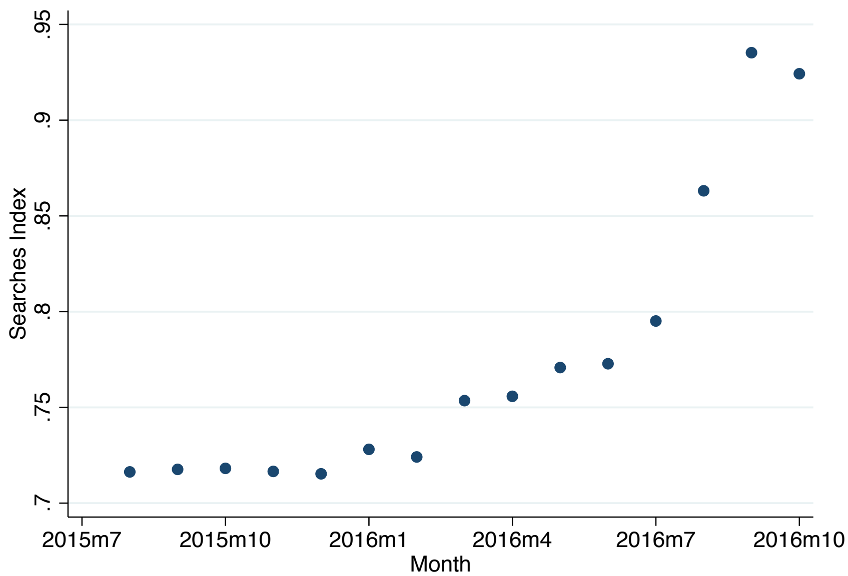
Notes: This figure plots the results of the regression discontinuity design, where the dependent variable is (a)the share of candidates who campaign finances were found to be irregular, and (b) the mean spending by parties. The horizontal axis denotes the difference in maximum spending relative to the discontinuity at R$142,857. In each regression, a global second-order polynomial is estimated on each side of the discontinuity. Each point denotes the sample-average within a bin and its corresponding 95% confidence interval.

Figure 2.10: Daily Number of Facebook Posts



Notes: Each point on the plot represents the total number of Facebook posts posted by all mayoral candidates in a given day. Blue circles are campaign period days: from august 16 (official start of campaign period) to october 2 (election day)

Figure 2.11: Google Searches Index



Notes: Each dot on the plot represents the average Google Searches Index across all mayoral candidates in a given month

Table 2.1: Summary Statistics

	Mean (1)	Standard Deviation (2)	Observations (3)
Panel A: Within-Municipality Average Candidate Characteristics			
Campaign spending (R$1000)	77.28	145.97	5562
Campaign contributions (R$1000)	74.94	122.17	5562
Own funds	29.98	54.62	5562
Individual donations	31.15	50.38	5562
Party donations	11.35	49.79	5562
All other donations	0.15	2.09	5562
Female	0.125	0.207	5562
Age	49.21	11.25	5562
High school	0.830	0.249	5562
College	0.505	0.333	5562
Political experience	0.944	0.620	5562
Assets (R$1000)	1006.80	5483.58	5562
Propensity to win	0.361	0.081	5562
Panel B: Municipality Characteristics			
Number of candidates	2.925	1.333	5562
GDP per capita	6.080	0.501	5544
Illiteracy	0.174	0.107	5544
Share urban	0.639	0.220	5544
Gini coefficient	0.494	0.066	5544
Log voters	9.225	1.078	5562
Hiring limit	137.22	192.72	5562
Open seat	0.241		5562
Panel C: Incumbent Outcomes			
Reelection	0.482		2618
Incumbent vote share	0.468	0.184	2618

Notes: This table displays means and standard deviations of various characteristics computed for the municipality and incumbent samples. In panel A, statistics on campaign spending, female gender, age, high school and college completion, assets, and propensity to win are computed for municipality-level means. In panel B, the statistics are calculated for municipality-level characteristics. The "open seat" variable is a dummy for whether the seat in 2012 is occupied by a term-limited mayor. In panel C, statistics for reelection and the incumbent vote share are computed for the sample of incumbents who rerun in 2016.

Table 2.2: Covariate Smoothness

Dependent Variable	Mean (1)	Observations (2)	Estimate (3)
Panel A: Municipal Characteristics in 2010			
GDP per capita (log)	5.911	2558	0.012
	(0.031)		(0.040)
Illiteracy	0.208	2201	-0.002
	(0.008)		(0.010)
Share Urban	0.625	2419	-0.015
	(0.014)		(0.018)
Gini Coefficient	0.511	2431	-0.000
	(0.005)		(0.006)
Population (log)	9.509	1986	-0.158**
	(0.053)		(0.068)
Panel B: Mean Candidate Characteristics in 2012			
Number of Candidates	3.052	1763	-0.007
	(0.086)		(0.108)
Effective Number of Candidates	2.203	1902	-0.030
	(0.042)		(0.048)
Small Party	0.425	2270	0.004
	(0.020)		(0.025)
Female	0.121	2553	0.022
	(0.013)		(0.016)
Age	47.962	2433	0.072
	(0.437)		(0.548)
High School Degree	0.853	2024	0.001
	(0.018)		(0.023)
College Degree	0.509	1986	0.037
	(0.024)		(0.030)
Campaign Spending	94396.87	1057	-1965.04
	(2431.97)		(2921.36)
Campaign Contributions	94737.97	1085	-2551.47
	(2364.50)		(2882.86)
Own Funds	24319.94	1297	2572.27
	(1796.36)		(2464.03)
Individual Donations	36355.43	1538	-1498.32
	(1762.72)		(2202.32)
Party Donations	10572.97	1036	-2074.31
	(1389.72)		(1688.48)
Corporate Donations	15876.27	1233	937.79
	(1518.43)		(2145.03)
Wealth (log)	11.551	2199	-0.017
	(0.154)		(0.183)

Notes: Standard errors are in parentheses. The mean in column (1) is the estimated value of the dependent variable for a municipality at the cutoff point whose spending limit is $R108,039 in 2016. The bandwidth is selected with the optimal procedure by Calonico et al. (2014) and the number of observations is reported in column (2). Each figure in column (3) reports the estimate and standard error for the treatment effect from a separate regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.3: Effects of Spending Limits on Campaign Expenditures

	Mean	Obs	(1)	(2)	(3)	(4)
Maximum Spending	84823.66	1068	10212.56***	9834.92***	10337.88***	11781.30***
	(2283.84)		(2971.87)	(3280.29)	(2758.98)	(3502.78)
Mean Spending	58471.54	1274	5705.95**	4940.07*	5540.17**	6833.53**
	(1875.60)		(2461.39)	(2763.56)	(2251.89)	(2946.03)
Minimum Spending	32829.54	1745	988.86	16.68	1452.29	-408.74
	(2090.75)		(2820.98)	(3175.00)	(2582.36)	(3777.42)
Total Spending	169000.64	1159	7335.85	11682.88	7156.26	12238.92
	(6006.29)		(7511.99)	(8425.47)	(6850.61)	(8760.41)
Bandwidth	CCT	CCT	CCT	75% CCT	125% CCT	CCT
Polynomial Order	One	One	One	One	One	Two

Notes: Each figure in columns (1)-(4) reports the estimate and standard error of a separate regression. Standard errors are in parentheses. The mean is the estimated value, based on specification (1), of the dependent variable for a municipality at the cutoff point whose spending limit is $R108,039. The dependent variables are respectively the mean, maximum, minimum, and total campaign expenditures by candidates computed at the municipality-level. The CCT bandwidth is selected with the optimal procedure by Calonico et al. (2014), and the number of observations under this bandwidth is reported in the table. State fixed effects and municipal controls (income per capita, illiteracy, share urban, population, gini coefficient) are included in all regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.4: Effects of Spending Limits on Campaign Contributions

	Mean	Obs	(1)	(2)	(3)	(4)
Overall Contributions	58270.00	1332	6179.41**	5754.40**	5641.58**	6988.09**
	(1845.51)		(2441.96)	(2732.77)	(2232.75)	(3064.40)
Own Funds	23889.92	1459	4641.85**	4487.98*	3648.77*	4262.16*
	(1459.86)		(2162.27)	(2426.65)	(1963.48)	(2581.21)
Individual Donations	25747.75	1439	200.07	280.40	489.96	266.29
	(1325.32)		(1752.67)	(1937.98)	(1621.45)	(2186.17)
Party Donations	7074.12	1429	1210.84	1201.15	963.01	1377.40
	(790.16)		(1060.98)	(1200.61)	(968.80)	(1263.92)
All Other Donations	113.91	1626	12.34	-14.16	15.50	3.30
	(45.62)		(63.50)	(64.28)	(63.73)	(69.08)
Bandwidth	CCT	CCT	CCT	75% CCT	125% CCT	CCT
Polynomial Order	One	One	One	One	One	Two

Notes: Each figure in columns (1)-(4) reports the estimate and standard error of a separate regression. Standard errors are in parentheses. The mean is the estimated value, based on specification (1), of the dependent variable for a municipality at the cutoff point whose spending limit is $R108,039. Each dependent variables is a municipality-level mean. "Overall Contributions" is equal to the sum of the four categories: own, individual, party, and other contributions. State fixed effects and municipal controls (income per capita, illiteracy, share urban, population, gini coefficient) are included in all regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.5: Probability of Winning the Election

	(1) Winner of the Election
Age	-0.0167***
	(0.00298)
Age Squared	0.0000161***
	(0.00000339)
Female	-0.235***
	(0.0878)
White	-0.254
	(0.388)
Black	-0.623
	(0.437)
Brown	-0.398
	(0.392)
High School	-0.119
	(0.0828)
College	-0.0494
	(0.0625)
Log Assets	0.0435***
	(0.00902)
Incumbent	0.612***
	(0.0732)
Political Experience	0.0560**
	(0.0262)
Party Fixed Effects	Yes
Observations	6438

Notes: Robust standard errors are in parentheses. The sample is restricted to observations that are excluded from the RD regressions. The dependent variable is equal to one if the candidate wins the election and zero otherwise. The regression also controls for party fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.6: Effects of Campaign Spending Limits on Candidate Entry

	Mean	Obs	(1)	(2)	(3)	(4)
Number of Candidates	3.187	2012	-0.256**	-0.279**	-0.247***	-0.279**
	(0.092)		(0.102)	(0.119)	(0.092)	(0.132)
Effective Number of Candidates	2.253	2198	-0.143***	-0.164***	-0.129***	-0.184***
	(0.042)		(0.049)	(0.056)	(0.044)	(0.068)
Propensity to Win	0.352	1985	0.020***	0.022***	0.018***	0.022***
	(0.005)		(0.007)	(0.008)	(0.006)	(0.008)
Small Party	0.488	2166	-0.038	-0.044	-0.037	-0.049
	(0.021)		(0.026)	(0.031)	(0.024)	(0.030)
Average Ideology Index	5.122	2237	0.059	0.003	0.068	-0.101
	(0.081)		(0.100)	(0.114)	(0.091)	(0.144)
Maximum Ideology Index	6.263	2076	-0.109	-0.197	-0.072	-0.196
	(0.112)		(0.131)	(0.152)	(0.118)	(0.166)
Minimum Ideology Index	3.981	2277	0.198	0.158	0.188	0.120
	(0.106)		(0.129)	(0.145)	(0.119)	(0.173)
Female	0.156	1863	-0.030	-0.034	-0.018	-0.043*
	(0.016)		(0.020)	(0.023)	(0.018)	(0.025)
Age	49.094	2468	-0.364	-0.202	-0.407	-0.154
	(0.453)		(0.539)	(0.608)	(0.494)	(0.756)
White	0.617	1771	-0.021	-0.019	-0.013	-0.028
	(0.022)		(0.027)	(0.031)	(0.024)	(0.033)
College Degree	0.562	2083	-0.025	-0.017	-0.025	-0.028
	(0.021)		(0.027)	(0.031)	(0.025)	(0.031)
Political Experience	0.866	2254	0.062	0.053	0.044	0.056
	(0.040)		(0.048)	(0.055)	(0.044)	(0.067)
Wealth (log)	11.477	1811	0.401*	0.483**	0.313	0.498**
	(0.175)		(0.214)	(0.247)	(0.194)	(0.253)
Bandwidth	CCT	CCT	CCT	75% CCT	125% CCT	CCT
Polynomial Order	One	One	One	One	One	Two

Notes: Each figure in columns (1)-(4) reports the estimate and standard error of a separate regression. The dependent variables are two measures of the number of candidates who run for office, followed by municipality-level averages of various candidate characteristics. The "Propensity to Win" denotes the propensity for a candidate to win an election based on his observable characteristics (see Table 2.5). State fixed effects and municipal controls (income per capita, illiteracy, share urban, population, gini coefficient) are included in all regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.7: Effects of Campaign Spending Limits on Incumbents

	Mean	Obs	(1)	(2)	(3)	(4)
Panel A: All incumbents						
Rerun	0.605	1559	0.057	0.029	0.057	0.054
	(0.040)		(0.049)	(0.057)	(0.043)	(0.061)
Reelection	0.226	1726	0.110***	0.122**	0.088**	0.110**
	(0.030)		(0.042)	(0.048)	(0.037)	(0.050)
Panel B: Incumbents who rerun in 2016						
Reelection	0.384	762	0.160**	0.182**	0.145**	0.141**
	(0.050)		(0.062)	(0.072)	(0.058)	(0.068)
Change in Vote Share	-0.133	678	0.066**	0.078**	0.055**	0.066*
	(0.024)		(0.030)	(0.032)	(0.027)	(0.034)
Incumbent Spending	73442.98	682	10311.77**	9059.49**	10360.66**	11370.41**
	(2903.77)		(4348.01)	(4514.72)	(4183.28)	(5484.85)
Total Challenger Spending	98797.86	878	1107.77	-715.63	2436.11	1137.01
	(5462.11)		(7411.32)	(8117.57)	(6645.58)	(9993.36)
Mean Challenger Spending	49012.44	976	3970.59	3699.74	3483.18	4872.82
	(2443.98)		(3440.95)	(3784.47)	(3157.33)	(4349.70)
Bandwidth	CCT	CCT	CCT	75% CCT	125% CCT	CCT
Polynomial Order	One	One	One	One	One	Two

Notes: Each figure in columns (1)-(4) reports the estimate and standard error of a separate regression. Standard errors in parentheses, clustered by state-party. The mean is the estimated value, based on specification (1), of the dependent variable for a municipality at the cutoff point whose spending limit is $R108,039. State and party fixed effects and municipal controls (income per capita, illiteracy, share urban, population, gini coefficient and 2012 incumbent spending) are included in all regressions. In Panel A, the sample consists of all incumbents who are not term-limited. In Panel B, the sample consists of incumbent mayors who choose to rerun in 2016. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.8: Effects of Campaign Spending Limits on Political Selection

	Mean	Obs	(1)	(2)	(3)	(4)
Propensity to Win	0.382	2112	0.017	0.022*	0.016*	0.020
	(0.008)		(0.011)	(0.012)	(0.010)	(0.013)
Female	0.138	2024	0.007	-0.000	0.013	-0.004
	(0.025)		(0.032)	(0.037)	(0.029)	(0.039)
Age	49.299	2359	-0.483	-0.649	-0.503	-0.804
	(0.765)		(0.964)	(1.097)	(0.883)	(1.354)
White	0.634	2204	0.002	0.003	0.003	0.001
	(0.032)		(0.040)	(0.046)	(0.036)	(0.047)
College Degree	0.547	2520	0.002	0.004	0.000	0.025
	(0.031)		(0.039)	(0.044)	(0.036)	(0.056)
Political Experience	0.898	1903	0.059	0.088	0.041	0.070
	(0.075)		(0.094)	(0.109)	(0.085)	(0.104)
Wealth (log)	11.749	2814	0.514*	0.525*	0.436*	0.573
	(0.230)		(0.266)	(0.301)	(0.242)	(0.359)
Worker's Party (PT)	0.033	2608	-0.003	-0.005	-0.000	-0.005
	(0.012)		(0.015)	(0.017)	(0.013)	(0.019)
Ideology Index	5.290	1774	-0.000	0.031	-0.030	-0.021
	(0.124)		(0.142)	(0.162)	(0.130)	(0.158)
Bandwidth	CCT	CCT	CCT	75% CCT	125% CCT	CCT
Polynomial Order	One	One	One	One	One	Two

Notes: Each figure in columns (1)-(4) reports the estimate and standard error of a separate regression. Standard errors are in parentheses. The mean is the estimated value, based on specification (1), of the dependent variable for a municipality at the cutoff point whose spending limit is $R108,039. The dependent variables are characteristics of the winning candidates. The "Propensity to Win" dependent variable denotes the propensity for a candidate to win an election based on his observable characteristics (see Table 2.5). State fixed effects and municipal controls (income per capita, illiteracy, share urban, population, gini coefficient) are included in all regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.9: Effects of Campaign Spending Limits on the Campaign Contributions of Winners

	Mean	Obs	(1)	(2)	(3)	(4)
Overall Contributions	76140.38	1093	9136.78***	9187.39**	8617.55***	10628.05**
	(2542.77)		(3479.48)	(3888.34)	(3193.54)	(4170.59)
Own Funds	29448.08	1361	10747.16***	11125.72***	9352.32***	11907.39***
	(2541.11)		(3711.19)	(4162.91)	(3358.36)	(4594.10)
Individual Donations	38930.25	1163	-3208.61	-3639.71	-2809.18	-3728.12
	(2550.80)		(3479.94)	(3847.64)	(3222.52)	(4139.73)
Party Donations	7964.54	1227	1195.99	2455.58	528.46	3290.46
	(1442.29)		(2010.14)	(2171.93)	(1879.62)	(2472.87)
All Other Donations	245.42	1846	-24.93	-78.32	11.33	-76.18
	(117.59)		(172.86)	(181.01)	(167.22)	(196.99)
Bandwidth	CCT	CCT	CCT	75% CCT	125% CCT	CCT
Polynomial Order	One	One	One	One	One	Two

Notes: Each figure in columns (1)-(4) reports the estimate and standard error of a separate regression. Standard errors are in parentheses. The mean is the estimated value, based on specification (1), of the dependent variable for a municipality at the cutoff point whose spending limit is $R108,039. The "Overall Contributions" dependent variable is equal to the sum of the four categories: own, individual, party, and other contributions. State fixed effects and municipal controls (income per capita, illiteracy, share urban, population, gini coefficient) are included in all regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.10: Effects of Spending Limits on Facebook Campaign Activity

	Mean	Obs	(1)	(2)	(3)	(4)
Has Facebook Page	0.345	1919	-0.060**	-0.084**	-0.044*	-0.088**
	(0.023)		(0.029)	(0.034)	(0.026)	(0.037)
Number of Posts (log)	1.191	1914	-0.182*	-0.231*	-0.136	-0.203
	(0.087)		(0.110)	(0.127)	(0.100)	(0.128)
Number of Reactions (log)	2.283	1816	-0.354*	-0.426*	-0.250	-0.421*
	(0.172)		(0.214)	(0.247)	(0.192)	(0.254)
Bandwidth	CCT	CCT	CCT	75% CCT	125% CCT	CCT
Polynomial Order	One	One	One	One	One	Two

Notes: Each figure in columns (1)-(4) reports the estimate and standard error of a separate regression. Standard errors are in parentheses. The mean is the estimated value, based on specification (1), of the dependent variable for a municipality at the cutoff point whose spending limit is $R108,039. The dependent variables are respectively the proportion of candidates with a Facebook Page, the log plus one of the average number of candidates' posts and the log plus one of the average number of reactions candidates' posts got computed at the municipality-level between the beginning of the campaign period and election day. The CCT bandwidth is selected with the optimal procedure by Calonico et al. (2014) , and the number of observations under this bandwidth is reported in the table. State fixed effects and municipal controls (income per capita, illiteracy, share urban, population, gini coefficient) are included in all regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.11: Effects of Spending Limits on Facebook Campaign Activity of Incumbents and Challengers

	Mean	Obs	(1)	(2)	(3)	(4)
Panel A: Incumbents						
Has Facebook Page	0.324	1457	-0.050	-0.060	-0.035	-0.054
	(0.040)		(0.048)	(0.054)	(0.044)	(0.060)
Number of Posts (log)	1.156	1545	-0.055	-0.079	-0.035	-0.020
	(0.162)		(0.187)	(0.214)	(0.171)	(0.219)
Number of Reactions (log)	2.002	1745	0.045	-0.019	0.031	0.028
	(0.264)		(0.327)	(0.367)	(0.297)	(0.460)
Panel B: Challengers						
Has Facebook Page	0.340	2148	-0.038	-0.069*	-0.026	-0.065*
	(0.027)		(0.032)	(0.037)	(0.029)	(0.041)
Number of Posts (log)	1.190	1987	-0.173	-0.215	-0.117	-0.189
	(0.103)		(0.129)	(0.149)	(0.117)	(0.151)
Number of Reactions (log)	2.332	1858	-0.399	-0.466	-0.292	-0.444
	(0.204)		(0.252)	(0.290)	(0.227)	(0.293)
Bandwidth	CCT	CCT	CCT	75% CCT	125% CCT	CCT
Polynomial Order	One	One	One	One	One	Two

Notes: Each figure in columns (1)-(4) reports the estimate and standard error of a separate regression. Standard errors are in parentheses. The mean is the estimated value, based on specification (1), of the dependent variable for a municipality at the cutoff point whose spending limit is $R108,039. The dependent variables are respectively the proportion of candidates with a Facebook Page, the log plus one of the average number of candidates' posts and the log plus one of the average number of reactions candidates' posts got computed at the municipality-level between the beginning of the campaign period and election day. Panel A presents the results using only incumbent candidates and panel B using the average across challengers in closed seat municipalities. The CCT bandwidth is selected with the optimal procedure by Calonico et al. (2014) , and the number of observations under this bandwidth is reported in the table. State fixed effects and municipal controls (income per capita, illiteracy, share urban, population, gini coefficient) are included in all regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.12: Effects of Spending Limits on In-Kind versus Cash Contributions

	Mean	Obs	(1)	(2)	(3)	(4)
Panel A: Candidates						
Estimated Donations	10827.874	2143	796.145	867.618	718.126	966.594
	(534.242)		(737.883)	(854.836)	(675.718)	(945.401)
Money Donations	47301.296	1325	5512.462**	4777.192*	4796.944*	6265.976*
	(1859.203)		(2380.400)	(2666.112)	(2172.280)	(2986.047)
Panel B: Winners						
Estimated Donations	14396.408	1583	-1050.121	-1193.842	-816.557	-758.768
	(1010.294)		(1317.140)	(1493.318)	(1174.397)	(1560.678)
Money Donations	61368.192	1053	10149.728***	9716.734**	10070.159***	11824.313***
	(2628.278)		(3614.709)	(4006.428)	(3322.418)	(4292.634)
Bandwidth	CCT	CCT	CCT	75% CCT	125% CCT	CCT
Polynomial Order	One	One	One	One	One	Two

Notes: Each figure in columns (1)-(4) reports the estimate and standard error of a separate regression. Standard errors are in parentheses. The mean is the estimated value, based on specification (1), of the dependent variable for a municipality at the cutoff point whose spending limit is $R108,039. For each panel, the dependent variables are respectively the amount of contributions given in kind (Estimated Donations) and the amount of contributions given in cash (Money Donations). The CCT bandwidth is selected with the optimal procedure by Calonico et al. (2014) , and the number of observations under this bandwidth is reported in the table. State fixed effects and municipal controls (income per capita, illiteracy, share urban, population, gini coefficient) are included in all regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.13: Effects of Campaign Spending Limits on Voter Information

	Mean	Obs	(1)	(2)	(3)	(4)
Turnout	0.839	2645	-0.005	-0.005	-0.005	-0.005
	(0.003)		(0.003)	(0.004)	(0.003)	(0.004)
Share of Blank or Invalid Votes	0.069	2193	0.012	0.013	0.010	0.014
	(0.005)		(0.007)	(0.008)	(0.007)	(0.009)
Bandwidth	CCT	CCT	CCT	75% CCT	125% CCT	CCT
Polynomial Order	One	One	One	One	One	Two

Notes: Each figure in columns (1)-(4) reports the estimate and standard error of a separate regression. Standard errors are in parentheses. The mean is the estimated value, based on specification (1), of the dependent variable for a municipality at the cutoff point whose spending limit is $R108,039. The "Turnout" dependent variable is the number of votes divided by the number of eligible voters. The "Share of Blank or Invalid Votes" dependent variable denotes the number of votes cast which are either blank or invalid divided by the number of eligible voters. State fixed effects and municipal controls (income per capita, illiteracy, share urban, population, gini coefficient) are included in all regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.14: Distribution of Candidates' Number of Searches in September 2016

Number of Searches	Index Value	Number of Candidates
0 -10	0	5,806
11 - 100	1	5,540
101 - 1,000	2	2,800
1,001 - 10,000	3	834
10,001 - 100,000	4	116
100,001 - 1,000,000	5	3
Total		15,099

Notes: This table displays the distribution of Candidates' Google searches in September 2016.

Table 2.15: Effects of Spending Limits on Google Searches

	Mean	Obs	(1)	(2)	(3)	(4)
Google Searches	0.919	2267	-0.034	-0.040	-0.020	-0.044
	(0.043)		(0.050)	(0.058)	(0.045)	(0.063)
Incumbents' Google Searches	1.017	1429	-0.012	0.028	-0.022	0.119
	(0.073)		(0.085)	(0.096)	(0.078)	(0.126)
Challengers' Google Searches	0.932	1808	-0.102*	-0.081	-0.081	-0.114
	(0.055)		(0.064)	(0.075)	(0.058)	(0.079)
Bandwidth	CCT	CCT	CCT	75% CCT	125% CCT	CCT
Polynomial Order	One	One	One	One	One	Two

Notes: Each figure in columns (1)-(4) reports the estimate and standard error of a separate regression. Standard errors are in parentheses. The mean is the estimated value, based on specification (1), of the dependent variable for a municipality at the cutoff point whose spending limit is $R108,039. The dependent variables are respectively the average September Google searches index for all mayoral candidates, for incumbents and for challengers computed at the municipality-level. The CCT bandwidth is selected with the optimal procedure by Calonico et al. (2014) , and the number of observations under this bandwidth is reported in the table. State fixed effects and municipal controls (income per capita, illiteracy, share urban, population, gini coefficient) are included in all regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.16: Correlation Between September Google Search and Candidates' Ad Time Share

VARIABLES	(1) September Google Search
Ad Share	0.17**
	(0.08)
Ln(Campaign Spending)	0.09***
	(0.01)
Incumbent	0.09***
	0.03
Female	0.033
	(0.03)
Political Experience	0.06***
	(0.02)
Age	0.00
	(0.00)
College	0.02
	(0.02)
Race Fixed Effect	y
Party Fixed Effect	y
City Fixed Effect	y
Observations	14,612

Notes: Robust standard errors are in parentheses. The dependent variable is the September Google Search Index for the mayoral candidate. Ad Share is the advertisement time share of the mayoral candidate in the municipality. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.17: Effects of Campaign Spending Limits on Block Grants

	Mean	Obs	(1)	(2)	(3)	(4)
Number of Transfers	2.742	1984	0.373*	0.526**	0.321*	0.470*
	(0.150)		(0.212)	(0.242)	(0.194)	(0.264)
Value of Transfers	1.177e+06	1207	523687.444**	579289.072**	407358.233**	597019.955**
	(155194.292)		(217126.688)	(235307.505)	(199955.535)	(260903.406)
Prop. Agriculture	0.049	1866	0.033**	0.036*	0.031**	0.036*
	(0.012)		(0.017)	(0.019)	(0.015)	(0.019)
Prop. Integration	0.114	2125	0.022	0.029	0.016	0.038
	(0.016)		(0.021)	(0.024)	(0.019)	(0.030)
Prop. Cities	0.326	1651	-0.046	-0.061	-0.028	-0.069
	(0.028)		(0.034)	(0.039)	(0.031)	(0.042)
Prop. Education and Health	0.272	1982	0.022	0.023	0.024	0.027
	(0.026)		(0.031)	(0.036)	(0.029)	(0.042)
Prop. Other	0.250	2072	-0.053*	-0.038	-0.057**	-0.040
	(0.024)		(0.028)	(0.032)	(0.025)	(0.039)
Bandwidth	CCT	CCT	CCT	75% CCT	125% CCT	CCT
Polynomial Order	One	One	One	One	One	Two

Notes: Each figure in columns (1)-(4) reports the estimate and standard error of a separate regression. Standard errors are in parentheses. The mean is the estimated value, based on specification (1), of the dependent variable for a municipality at the cutoff point whose spending limit is $R108,039. The dependent variables are the total number and value of the federal block grants and also the proportion of grants received by a municipality from different ministries. The CCT bandwidth is selected with the optimal procedure by Calonico et al. (2014) , and the number of observations under this bandwidth is reported in the table. State fixed effects and municipal controls (income per capita, illiteracy, share urban, population, gini coefficient) are included in all regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.18: Summary Statistics: Municipalities Inside vs Outside the optimal bandwidth

	Inside Optimal Bandwidth	Outside Optimal Bandwidth
GDP per capita (log)	5.984	6.205
	(0.491)	(0.477))
Illiteracy	0.193	0.1485
	(0.106)	(0.102)
Share Urban	0.639	0.6316
	(0.209)	(0.232)
Gini Coefficient	0.505	0.4784
	(0.0640)	(0.0659)
Population (log)	9.543	9.121
	(0.887)	(1.298)
Number of Candidates 2012	2.957	2.762
	(1.198)	(1.223))
Effective Number of Candidates 2012	2.165	2.071
	(0.499)	(0.492)
Small Party 2012	0.399	0.361
	(0.296)	(0.306))
Female 2012	0.135	0.121
	(0.208)	(0.204))
College Degree 2012	0.480	0.461
	(0.331)	(0.339)
Campaign Spending 2012	95347.9	115818.27
	(89060.4)	(289987.0)
Corporate Donations 2012	17690.9	30443.9
	(36081.8)	(88307.1))
Wealth (log) 2012	11.56	11.48
	(2.188)	(2.240)

Notes: This table displays means and standard deviations of various characteristics computed separately for the municipalities inside and outside the optimal bandwidth used in the RD regressions.

Table 2.19: Correlates of the Amount of Block Grants a Municipality Receives, 2017

	(1) Value of Transfers	(2) Number of Transfers	(3) Ln(Value of Transfers+1)
Log (assets+1)	265141.3**	0.200***	0.0865**
	(95220.4)	(0.0410)	(0.0320)
Incumbent	-928304.5	-0.916**	-0.609*
	(752382.6)	(0.324)	(0.253)
Female	-529446.6	-0.284	-0.615
	(1039785.1)	(0.448)	(0.349)
Political Experience	1318176.1***	0.606***	0.289**
	(307440.7)	(0.132)	(0.103)
College	3224072.0***	2.051***	1.691***
	(868230.4)	(0.374)	(0.292)
High School	877479.5	0.760	0.545
	(924150.6)	(0.398)	(0.311)
White	1188212.0	0.346	-1.231
	(3970012.0)	(1.711)	(1.334)
Black	685745.2	-0.879	-2.945
	(4680117.3)	(2.017)	(1.573)
Brown	1881289.7	0.412	-0.704
	(4016850.5)	(1.731)	(1.350)
Age	37389.0	-0.00391	-0.0123
	(33671.9)	(0.0145)	(0.0113)
Age Squared	-39.24	0.00000113	0.0000127
	(37.19)	(0.0000160)	(0.0000125)
Party Fixed Effects	Y	Y	Y
Observations	2331	2331	2331

Notes: Standard errors in parentheses. The sample is restricted to observations that are excluded from the RD regressions. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Chapter 3

Do Government Audits Reduce Corruption? Estimating the Impacts of Exposing Corrupt Politicians

3.1 Introduction

Politicians throughout the world embezzle billions of dollars each year, and in so doing induce the misallocation of resources, foster distrust in leaders, and threaten the very pillars of democracy (Rose-Ackerman, 1999). And while the adverse consequences of corruption have been long recognized, there is little consensus over how best to fight it.[1] One point of growing emphasis in the literature has been the importance of political institutions that constrain rent-seeking, and in particular the role of elections in selecting and disciplining politicians.[2] Another strand of the literature has instead focused on the effectiveness of a country's judicial and prosecutorial institutions: If severe enough, the legal consequences of rent extraction should also discipline politicians (Becker, 1968; Becker and Stigler, 1974).

Although a successful anti-corruption strategy is likely to include reforms to strengthen both sectors, the efficacy of these institutions ultimately depends on a government's ability to detect corruption in the first place. This has led several countries to adopt audit programs aimed at uncovering the misuse of public resources, which not only increase the probability of detecting wrongdoing, but also provide the requisite information to both voters, as well as prosecutors, to hold politicians accountable for malfeasance.

In this paper, we investigate the role government audits play in reducing political corruption in local government through the promotion of electoral and judicial accountability. We do so in the context of Brazil's anti-corruption program which began in 2003 and has since audited 1,949

[1] See for example Fisman et al. (2014) for estimates of wealth accumulation of politicians in India and Pande (2008) and Olken and Pande (2012) for overviews of the economics of corruption in developing countries.

[2] See Besley (2007) and Ashworth (2012) for reviews of agency models and Ferraz and Finan (2008), Ferraz and Finan (2011), Bobonis et al. (2015) for evidence on how elections can discipline politicians.

municipalities at random. Many municipalities have been audited multiple times. Consequently, for several rounds of later audits, we are able to compare the corruption levels discovered among the municipalities that are being audited for the first time (*control group*) to the corruption levels of municipalities that have also been audited in the past (*treatment group*). Because municipalities are selected at random, this comparison estimates the causal effects of a past audit on future corruption levels, in a setting in which both groups face the same ex-ante probability of being audited.

We find that corruption levels are approximately 8 percent lower among treated municipalities compared to control municipalities. According to most political agency models, whether a municipality has been audited in the past should not have long-term consequences on rent-seeking. If mayors have a two-term limit and are perfectly informed about the probability of an audit, the experience of an audit should only affect corruption in the following term through its selection effects. But mayors in Brazil are not perfectly informed: Although they can potentially compute the probability of an audit for any given lottery, they do not know the likelihood of future audits occurring. Faced with this uncertainty, it is plausible that mayors update their beliefs over the audit risk through the information they acquire from their own and others' audit experiences.[3]

Consistent with this interpretation, we find that past audits also affect the corruption levels of neighboring municipalities with local media, which are the places where the mayors are the most likely to learn about the outcome of another municipality's audit. For these municipalities, having an additional neighbor audited leads them to reduce their own corruption by 7.5 percent. We also find evidence of smaller spillover effects across partisan networks, which is again consistent with the mayors learning based on others' experiences. The average municipality in our sample receives 15 million reais in federal transfers per year. Based on our estimates of a random sample of audit reports, 30 percent of the funds audited were found to be diverted, implying that the audits reduced corruption by R$567,135 per year per municipality, once we account for the spillover effects.

We interpret the main findings in the context of a simple model of political accountability, which we subsequently estimate. Based on our model, there are several reasons why the audits may have led to a reduction in local corruption. First, the audits may have reduced corruption through a political selection effect. As documented in Ferraz and Finan (2008), in places that were audited before the election, voters were able to reward good and punish bad incumbents who were up for re-election. Second, the audits may have led to a stronger electoral disciplining effect. If an audit increases a mayor's posterior beliefs of the likelihood of an audit and he has re-election concerns, then he has less incentive to engage in corruption. Of course, an unfavorable audit can also trigger other non-electoral costs, such as legal punishment or reputation costs. So even if a mayor does not have re-election concerns, an update in the probability of being found to be corrupt can lead to what we call a legal disciplining effect. Finally, the audits may have also affected the political environment more generally by inducing a better selection of candidates (i.e. an entry effect).[4]

[3] Although we model learning over the audit probability, it could also be the case that agents learn about the costs associated with audits. In Appendix 3.9, we solve for and estimate such a model. Although the structure of the learning process differs between the two models, both produce very similar results.

[4] Outside of the context of our model, there are two other possible explanations for the reduction in the corruption. One interpretation for our findings is that the audits teach mayors to better hide corruption. The other interpretation is

Investigating these mechanisms both in the reduced-form and structurally, we find consistent evidence for a legal disciplining effect, with less support for the other channels. Our model estimates suggest the disciplining effects from legal costs can explain 72 percent of the reduction in local corruption. Consistent with this result, we also document that an audit can generate substantial legal costs. Using data on anti-corruption crackdowns and federal convictions, we find that having been audited increases the likelihood of incurring a legal action by 20 percent.

We use our structural estimates to explore four counterfactual policies aimed at further reducing corruption. First, we simulate changes in the perceived audit probability, which would occur if the program increased the frequency of audit lotteries or the number of municipalities audited in each lottery. Second, we simulate increases in the legal costs of corruption. In practice, legal costs could be increased if, for example, the judicial system imposed harsher fines or punishment for engaging in corruption. Third, we consider policies which would affect the education or occupational backgrounds of candidates running for office. Finally, given the spillover effects we document and the importance of the media in disseminating information, we simulate a policy in which all municipalities receive access to information about neighboring audits.

Based on these simulations, policies aimed at either increasing the legal costs of corruption or increasing the probability of being audited would most reduce corruption. Based on our estimated model, a 10 percent increase in legal costs would decrease corruption by approximately 9.8 percent. Similarly, a 10 percent increase in the audit probability would decrease corruption for first term mayors by 14.6 percent and second term mayors by 9.3 percent. As we discuss later, these findings are in line with other estimates reported in the literature (e.g. Bobonis et al. (2015), Olken (2007), Zamboni and Litschig (2015)). In contrast to these policies, we find modest effects associated with our entry and information treatments. The latter result stems from the fact that with more access to information, mayors and voters are more likely to update their priors about the audit risk in both directions. Some mayors will acquire additional information which leads them to overestimate the probability of audits, while others will acquire information which leads to underestimation. Unless mayors have biased priors or do not update their beliefs using Bayes' rule, the effects of this policy on rents will be comparatively minor compared to the first two policy counterfactuals we considered.

Our findings are related to three broad literatures. First, our study contributes to a large literature on the determinants of political corruption and the design of policies aimed at curbing corruption. For example, Bobonis et al. (2015) find that audit reports in Puerto Rico released just prior to the election (compared to those released after an election) induced a significant short-term reduction in municipal corruption levels that later dissipated in the subsequent rounds of audits. An important distinction between our studies is that in our context the audits are conducted at random, and thus politicians are not able to anticipate them. Di Tella and Schargrodsky (2003) examine the effects of an anti-corruption crackdown and find that the prices paid for homogeneous supplies by public hospitals in Buenos Aires fell by 15 percent after the government began to disseminate information on prices. Olken (2007) implemented a randomized experiment where prior to the start

that the federal government offered fewer transfers in response to an audit, and thus made it harder for future mayors to engage in corruption. We test for both of these channels and do not find support for these interpretations.

of a national wide infrastructure project, villages in Indonesia were randomly assigned into groups with different audit probabilities. The study found that 24 percent of reported funds were found to be "missing", but when faced with a certain audit this difference was reduced by 8 percentage points. Zamboni and Litschig (2015) investigate the effects of a randomized experiment conducted by the Controladoria-Geral da União (CGU) designed to test whether increased audit risk reduces corruption and mismanagement. In this experiment, the CGU announced in May of 2009 to 120 municipalities that in one year time, 30 of them would be randomly selected for an audit. Based on this temporary increase in audit risk of about 20 percentage points, the authors found that the treatment reduced the proportion of local procurement processes involving waste or corruption by about 20 percent. Finally, Lichand et al. (2016) also examine the effects of Brazil's audit program with a focus on corruption in health. Using a difference-in-differences strategy, the study tests whether corruption is lower in municipalities that neighbor municipalities that were audited in the past. Consistent with our spillover effects on corruption across all sectors, they find that corruption in health reduced by 5.4 percent in places that neighbor an audited municipality.

We complement these studies in various ways. First, our findings suggest that audits can be an effective policy instrument for not only promoting electoral accountability, but also enhancing judicial punishment. Second, there are several motives for reducing corruption in response to an audit. In our study, we are able to decompose the effects of these various channels, and highlight the relative importance of legal costs in disciplining the behavior of politicians. Finally, another advantage of our data is the ability to distinguish between acts of corruption versus acts of mismanagement.[5] We do not find any evidence that the audits reduced irregularities associated with mismanagement.

Our study also contributes to a body of research documenting the importance of legal institutions for economic development, and in particular corruption (Glaeser and Shleifer, 2002; La Porta et al., 2004). For example, Glaeser and Goldin (2006) argue that reductions in corruption over time in the U.S. were due to a combination of increased political competition, an active media uncovering corruption scandals, and an independent judiciary that successfully prosecuted corrupt officials. Also using variation across U.S. states, Alt and Lassen (2008) show that corruption is much lower among states in which state supreme court judges are elected rather than appointed. Finally, Litschig and Zamboni (2015) exploit variation in the presence of the judiciary across Brazil's municipalities to show that corruption is lower in municipalities with a state judiciary present. In contrast to these studies, however, we show using data on the police crackdowns and convictions of politicians that audits can be a critical instrument in promoting judicial accountability. As far as we know, this is the first paper that examines how both political and judicial accountability can affect corruption.

Finally, our study also relates to a growing empirical literature that examines the relationship between electoral accountability and politician performance. There is a growing literature showing that when voters are informed, elections can discipline corrupt politicians (e.g. Ferraz and Finan (2008), Winters and Weitz-Shapiro (2013)). Similarly, a series of papers have exploited variation

[5]Recent studies have tried to distinguish between active and passive waste. For example, Bandiera et al. (2009) use data on public procurement from Italy to show that more than 80 percent of waste can be classified as passive.

in term limits to show that incumbents respond to re-election incentives. For example, Besley and Case (1995) show that re-election incentives affect the fiscal policy of U.S. governors, while List and Sturm (2006) provide evidence that term limits even influence secondary policies, such as environmental policy. In relation to the Brazilian context, Ferraz and Finan (2011) find that mayors who are in their second term, and hence do not have opportunity for re-election, engage in much more corruption relative to mayors with re-election incentives. Similarly, de Janvry et al. (2012) find that Brazil's Bolsa Escola – a conditional cash transfer program that was targeted in a decentralized manner – performed much better in places where mayors had re-election incentives.

While these studies provide convincing evidence consistent with standard political agency models, they are unable to quantify the electoral selection effects that are also central to models of political accountability. Recently, some progress has been made in this direction by taking a more structural approach. Aruoba et al. (2015) estimate a model of political accountability to quantify the discipline and selection of U.S. gubernatorial elections. Using data from 1982-2012 of U.S. governors, they find that the possibility of re-election leads to a 13 percentage point increase in the fraction of governors who exert high effort in their first term in office, as measured by voters' job approval. Although set in a different context, they too find that selection effects are weaker than discipline effects. Sieg and Yoon (2016) estimate a dynamic game of electoral competition with adverse selection to compute the welfare impacts of term limits. According to their model, term limits can lead to two opposing welfare effects. On the one hand, term limits can be welfare-reducing by weakening the disciplining and selection effects that elections induce. But on the other hand, term limit can also reduce any potential entrenchment effects. Also using data from U.S. gubernatorial elections, they find that the former effects dominate, and term limits reduce voter welfare by 6 percent. Our paper complements these studies by not only disentangling selection from incentive effects, but also allowing for the possibility of a legal disciplining effect.

The rest of the paper is organized as follows. Section 3.2 provides background on the Brazil's anti-corruption program and presents the data used in the empirical analysis. Section 3.3 presents the theoretical framework. Section 3.4 discusses our research design and in Section 3.5 we present our reduced-form findings. In Section 3.6 we discuss the estimation of the model and present our counterfactual simulations. Section 3.7 concludes.

3.2 Background and Data

Corruption in Brazil and the Randomized Anti-Corruption Program

Brazil is one of the most decentralized countries in the world. Each year, municipalities receive millions of dollars from the federal government to provide basic public services such as primary education, health care, and sanitation. An elected mayor decides how to allocate these resources in conjunction with a locally-elected legislative body. With only minimal federal oversight accompanying these transfers, corruption at the local level has been a serious concern.

Corruption in Brazil occurs through a combination of fraud in the procurement of goods and services, diversion of funds, and over-invoicing of goods and services (Ferraz and Finan, 2011).

Common irregularities include incomplete public works (paid for but unfinished) and the use of fake receipts and phantom firms (i.e., firms that only exist on paper). Corruption tends to be more prevalent in places that receive more federal transfers, or where the local media and the judiciary are absent (Brollo et al., 2013; Zamboni and Litschig, 2015).

In response to widespread corruption and a lack in the capacity to systematically detect and punish malfeasance, the federal government created in 2003 Brazil's Controladoria Geral da União (CGU) – Office of Comptroller-General. The CGU, which is functionally autonomous and possesses the constitutional powers of a ministry, centralizes all of the Federal Government's internal control activities, and sets government directives for combating corruption. In order to establish horizontal accountability, the CGU also forms part of a complex system of federal agencies responsible for preventing, investigating, and punishing illicit acts in the political and public spheres. To this end, the Federal Court of Accounts (TCU), the Office of the Federal Public Prosecutor (MPF), and the Federal Police are responsible for inspecting, controlling, correcting and instructing legal actions taken against public administrators and politicians in cases of corruption (Speck, 2011; Power and Taylor, 2011).

The Randomized Audits Program

Shortly after its creation, the CGU launched an anti-corruption program targeted at municipal governments. The program, named *Programa de Fiscalização por Sorteios Públicos* (Monitoring Program with Public Lotteries), consists of random audits of municipalities for their use of federal funds. It originally started with the audit of 26 randomly selected municipalities across different states, but then shortly moved towards auditing 60 municipalities chosen by lottery. The lotteries are held publicly in conjunction with the national lottery in Brasília, and all municipalities with a population of up to 500,000 inhabitants are eligible for selection.[6] As of February 2015, there have been 2,241 audits across 40 lotteries in 1,949 municipalities and over R$22 billion dollars worth of federal funds audited.

Once a municipality is chosen, the CGU gathers information on all federal funds transferred to the municipal government during the previous three to four years and issues a random selection of inspection orders. Each one of these orders stipulates an audit task for a specific government project (e.g. school construction, purchase of medicine, etc.) within a specific sector.[7] Once these inspection orders are determined, 10 to 15 auditors are sent to the municipality for one to two weeks to examine accounts and documents, to inspect for the existence and quality of public work construction, and to verify the delivery of public services. These auditors are hired based on a competitive public examination and earn highly competitive salaries, thus their incentives for corruption are lower than those of other bureaucrats in the federal level administration. Moreover, the inspections are done by a team which reduces the opportunity for corruption among individual

[6]This eligibility criterion has changed slightly over time.

[7]Auditors do not have discretion in auditing other projects. If they find clear evidence of corruption in their fieldwork, they need to notify the central unit of the CGU who will then decide whether to issue a new inspection order.

auditors.[8] After the inspections are completed, a detailed report describing all the irregularities found is submitted to the central CGU office in Brasília. The central unit unifies the information and publishes a report on the internet. These reports are also sent to the Federal Courts of Accounts (TCU), the Federal Prosecutors' Office (MPF), the local judiciary, the Federal Police, and to the municipal legislative branch.

Over time the program has changed in order to improve the auditing capabilities of the CGU. Because larger municipalities receive substantially more transfers, the CGU decided in August 2005 to target a limited number of randomly selected sectors in larger municipalities. For example, in the 17^{th} lottery that took place in August 2005, the CGU chose to audit the sectors of social assistance, crime prevention, and industrial policies. Municipalities smaller than 20,000 inhabitants are still subject to audits in all sectors.

Although these changes affect the areas in which municipalities can be audited, they do not affect a municipality's audit probability. Lotteries are done by state and so the probability of being audited is constant for municipalities within the same state. For smaller states such as Alagoas, only 1 or 2 municipalities are typically drawn in a single lottery, whereas for a large state like Minas Gerais, with over 853 municipalities, as many as 8 municipalities have been drawn in a single lottery. Once audited, the municipality can only be audited again after several lotteries have elapsed.[9] Overall, as we see from Table 3.1, the implied audit probabilities in any given lottery are quite small, with the average being only 1.3 percent (s.d.= 0.005) in a given lottery. But given the frequency of the lotteries, the probability of being audited in one's political term can be quite high, ranging anywhere from 8.6 percent for the state of Minas Gerais to 26.4 percent in the case of Rio de Janeiro.

Note that even though audit probabilities are known at the time of a lottery, there are two important sources of uncertainty that can affect a mayor's perception of audit risk. First, the number of municipalities audited per state changes over time and this information is only provided right before the lottery takes place. For example, consider the state of Ceará: at the beginning the program, the CGU only selected 3 municipalities per lottery. After the 9^{th}, this number decreased to 2 municipalities, only to then increase back to 3 after the 22^{nd} lottery. The number then changed again to 4 starting in the 34^{th} lottery. Similar changes have occurred in other states. Second, and most importantly, due to fluctuations in the federal budget, it is extremely difficult for mayors to anticipate how many lotteries will take place during their term in office. As we document in Figure 3.1, the number of lotteries held per year has varied substantially over the course of the program. In some years, the program carried out as many as 7 lotteries in given year – leading to as many as 400 municipalities being audited – while in other years the program only carried out a single audit. For these reasons, it is reasonable to assume that mayors are uncertain about future audit risk.

By various accounts, the program has served as an important weapon in Brazil's fight against political corruption. The information obtained from the CGU audits has been widely used in political campaigns and in voters' selection and sanctioning of municipal politicians (Ferraz and Finan,

[8]Ferraz and Finan (2008) find no evidence that auditors manipulate the audit reports. In a recent study of Brazil's federal government, Bersch et al. (2016) found the CGU to be one of the government's most autonomous and least politicized agencies.

[9]This rule has changed over time going from 3 to 12 lotteries.

2008). The federal police and federal prosecutors have also exploited the audits to better target their investigations, and to help build their cases against corrupt politicians and public servants. Consequently, since 2004 Brazil has witnessed a steady increase in the number of legal actions involving political corruption, evidence of which can be seen in Figure 3.2.

Panel A of Figure 3.2 plots the number of police crackdowns, called Operações Especiais (Special Operations), aimed at uncovering municipal corruption. These crackdowns, which have increased over time and to date total 199 cases throughout Brazil, are the result of a direct collaboration between the federal police and the CGU. The number of civil court cases of individuals charged with misconduct in public office has also increased since 2004. In Panel B, we plot the number of mayors convicted of misconduct in public office who are banned for running for any public office for at least five years. As the figure depicts, fewer than 50 mayors were convicted of irregularities in 2004, but more than 400 were convicted in 2009. Although the CGU is not solely responsible for the increase in anti-corruption crackdowns and convictions, it has undoubtedly increased the costs of corrupt practices in Brazil, and as we will document below, its random audit program has played a significant role in this increase.

Together with the increasing number of prosecutions and anti-corruption crackdowns by the Federal Police, the local media has also contributed to the program's effectiveness. Local media is an important source of information for both politicians and voters to learn about the audits of nearby municipalities, as well as the likelihood of future legal actions. For example, on March 31, 2010, the Federal Police arrested the mayor of Satubinha, Maranhão after the CGU had discovered that he had diverted funds from over 23 procurement contracts. According to a political activist blog, when the radio announced his arrest, the mayor of São Bento, a neighboring municipality, was seen leaving on a small airplane afraid that he would be arrested next.[10]

The radio will often report on the audit results of neighboring municipalities. For example, on September 28, 2012, Radio Três Fronteiras, located in the municipality of Campos Sales, Ceará, ran a radio program to discuss the audit results of the neighboring municipality of Arneiroz.[11] The radio station Rádio Pajeú AM 1500, which covers 23 municipalities in the states of Pernambuco and Paraíba, also airs programs about municipal audits. On December 15^{th}, they ran a show on the CGU's audit of the municipality of Afogados, to highlight the large number of irregularities found in the implementation of the Conditional Cash Transfer program Bolsa Familia.[12]

Data

We build measures of mismanagement and corruption from a database managed by the CGU. In our analysis, we focus on corruption occurring in the 2004-2008 and 2008-2012 electoral terms. Hence, our main estimation sample consists of all audits conducted between July 2006 and March 2013 (lotteries 22 through 38).

[10] See http://isanilsondias.blogspot.com.br/2010/04/policia-federal-no-encalco-de-prefeitos.html. Retrieved December 12, 2016.

[11] See http://tresfronteirasam.com.br/radio/noticias.php?noticia=1003. Retrieved December 12, 2016.

[12] See http://www.radiopajeu.com.br/portal/pente-fino-da-cgu-no-bolsa-familia-prefeitura-de-afogados-emite-nota/. Retrieved December 12, 2016.

The dataset includes the coding of all irregularities found by the auditors for each inspection order. Although all audit reports are posted online, starting with the 20^{th} lottery in March 2006, the CGU began to code the information used for the reports. For each inspection order, the dataset contains information on the sector and government program, the amount transferred to the municipality, and a list of findings. For each finding, the auditors describe the irregularity found and classify it as: 1) an act of mismanagement (e.g. documents were not properly filled out, or improper storage of food supplies and medical equipment), 2) act of moderate corruption, 3) act of severe corruption.[13]

While the CGU's distinction between acts of mismanagement and acts of corruption is clear, the difference between moderate versus severe corruption is less obvious. To illustrate this, consider for example the municipality of Chaval in Ceará, which was audited in the 20^{th} lottery. The auditors went to the municipality with 25 inspection orders, one of which involved the financing of school buses for students attending primary schooling. They discovered two irregularities – one moderate and the other severe. For the moderate irregularity, a representative of the mayor withdrew R$1,200 without proving how the money was spent. The severe irregularity took place during the procurement of transportation services. The contract was awarded to a firm that did not match the original proposal, and the value of the contract was for a different amount than what was offered. While the second irregularity is arguably more severe, the CGU also classified as moderate the following irregularity discovered in Urbano Santos in Maranhão: There auditors visited three schools to check whether a school lunch program had been provided. Despite the fact that the municipality had received the money to pay for the program, school lunches had not been delivered for an entire year in one school, and had gone missing for a month in the other two schools. Given these types of examples, we had decided to use as our main measure the combination of both moderate and severe irregularities.

Based on this information, we construct measures of corruption and mismanagement at the municipality-lottery level. Our measure of corruption is the number of irregularities classified as either moderate or severe. Our measure of mismanagement is the number of irregularities associated with administrative and procedural issues. In Figure 3.3, we plot the distributions of irregularities associated with corruption and mismanagement per service order. The audits discovered on average 2.5 acts of corruption and 0.88 acts of mismanagement per service order, suggesting that 73.6 percent of the irregularities found during an average audit involves some act of corruption. To put these figures in perspective, Bandiera et al. (2009) estimate only 20 percent of waste found in Italy's public procurement process was due to corruption. Similarly, Olken (2007) argues that the main reason why audited villages in Indonesia did not significantly reduce their corruption is because the audits mostly reveal acts of mismanagement as opposed to acts of malfeasance. Similar to Bandiera et al. (2009) we do not find any evidence that active and passive waste are positively

[13]These data are similar to those used by Zamboni and Litschig (2015), except that our dataset spans a longer period of time. It is also worth noting that the CGU's distinction between moderate and severe irregularities does not map directly onto the categories used either by Ferraz and Finan (2008) or Brollo et al. (2013). Because the CGU classifies the irregularities based on potential losses accrued to the government, many of their "moderate" irregularities are typical examples of the corrupt practices used in the analysis by Ferraz and Finan (2008) and Brollo et al. (2013). See Zamboni and Litschig (2015) for a discussion of this point.

correlated (correlation coefficient = 0.02). In Figure 3.4, we plot the average number of irregularities associated with corruption and mismanagement by lottery. While our measure of corruption has been increasing steadily over time, the number of acts of mismanagement has varied more, particularly in recent audits. Given the changes to the auditing protocol over time, one should be cautious to interpret this temporal variation. In the regression results, we control for time trends in audit practices and exploit only within-audit variation.

Four other data sources are used in this paper. The political outcome variables such as re-election, vote shares, and mayor characteristics come from the Tribunal Superior Eleitoral (TSE), which provides results for the 2004-2012 municipal elections. These data contain vote totals for each candidate by municipality, along with various individual characteristics, such as the candidate's gender, education, occupation, and party affiliation. With this information, we match individuals across elections to construct measures of reelection and whether mayors are serving on a first versus second term.

We constructed the data on the joint CGU-Federal Police crackdowns using information available on the CGU homepage, as well as internet searches.[14] For each year starting in 2003, the CGU lists the name of the Special Operations and a description of the target. For each crackdown, we searched for the name of each operation together with the names of the targeted municipalities and keywords such as "mayor" or "corruption". We created a dataset comprised of the municipality targeted, a description of the findings, and whether the mayor or public servants of the targeted municipalities were involved in and/or arrested during the crackdown. We then create an indicator equal to one if a municipality was subject to a crackdown in a given year and whether the mayor was involved in the irregularities and/or arrested.

Data on the convictions of mayors for misconduct in public office was obtained from the Cadastro Nacional de Condenações Cíveis por ato de Improbidade Administrativa e Inelegibilidade. This database, administered by the National Council for Justice (CNJ), includes the names of all individuals charged with misconduct in public office. We downloaded the data in 2013 so the dataset includes all agents convicted up to that point. For each individual we observe the type of irregularity (e.g. violation of administrative principles or diversion of resources), the court where the conviction took place, and the date. These data are matched to the electoral data based on where the individual was a mayor and the period he/she served in office. Individuals on this list are banned from running for any public office for at least five years.

Data on municipal characteristics come from the Brazilian Institute of Geography and Statistics (Instituto Brasileiro de Geografia e Estatística (IBGE)). The 2000 population census provides several socioeconomic and demographic characteristics used as controls in our regressions. Some of these key variables include income per capita, income inequality, population density, share of the population that lives in urban areas, and share of the population that is literate.

To control for different institutional features of the municipalities, we also use information from the municipality survey, *Perfil dos Municípios Brasileiros: Gestão Pública*, which is conducted annually from 1999-2010. This municipal survey characterizes not only various aspects of the public administration, such as budgetary and planning procedures, but also more structural features

[14]See http://www.cgu.gov.br/assuntos/auditoria-e-fiscalizacao/acoes-investigativas/operacoes-especiais.

such as whether the municipality has a judge. Moreover, the survey provides our key measures of media availability, namely the number of radio stations and the number of daily newspapers.

Table 3.2 presents summary statistics for the municipalities in our sample, by whether they were audited previously or not. For each characteristic, we also present the difference between these characteristics. As expected from the random assignment, there are few differences in the characteristics of places audited for a first time versus those that had been audited previously. Importantly, included among these characteristics is the number of service orders. The fact that the number of service orders is balanced between treatment and control verifies the fact that the CGU does not adjust the number of service orders based on a previous audit.[15] Out of 15 characteristics, only one is statistically significant at the 10 percent level. We also fail to reject the hypothesis that all the variables are jointly significant (F-test=1.17; p-value=0.30). Overall the results from Table 3.2 suggest that the lottery used by the CGU was effective.

3.3 Model

To disentangle the channels through which audits reduce corruption, we consider the following model of political accountability, which builds on the career concerns model (Holmström, 1999; Persson and Tabellini, 2002). In our framework, audits reduce future corruption through a *selection effect* by altering the expected ability of reelected mayors. We then expand the framework so that observing audits causes mayors and voters to update their beliefs over the audit probability. Hence, the history of audits within a municipality will also have a *disciplining effect*: mayors who have observed relatively more audits will reduce corruption due to perceived increases in legal and electoral incentives.

The Model Without Learning

We consider an infinite horizon economy in discrete time. To simplify notation, we will omit time subscripts because the environment is stationary.

The Mayor. Mayors differ along a single continuous dimension, which we label ability, that is constant throughout their tenures in office. Mayors with higher levels of ability extract more rents than those with low ability. The ability of the mayor is a function of his observable characteristics X_i (gender, education, occupation) and an unobservable characteristic ε_i. Thus, we have:

$$\text{Ability}_i = X_i'\alpha + \varepsilon_i \tag{3.1}$$

The mayors' observable characteristics are common knowledge to both mayors and voters, and are drawn i.i.d. before the first-term mayor selects his action from a distribution with mean zero. The

[15] This is expected given the way inspection orders are issued. As we mentioned, within sectors inspection orders are issued based on a random selection of government projects from the last 3 to 4 years.

unobservable characteristic ε_i is drawn from a normal distribution with mean zero and variance σ_ε^2. The ε_i draw is privately observed by the first-term mayor after he chooses his action.

Mayors face a two-term limit. Let $T \in \{F, S\}$ denote whether the mayor is in his first term (F) or second term (S). Rents in term T for mayor i are given by the sum of the mayor's rent extraction effort e_i^T and his ability:

$$r_i^T = e_i^T + X_i'\alpha + \varepsilon_i \tag{3.2}$$

In each term, after the mayor chooses his extraction effort, an audit is drawn independently from a Bernoulli distribution with probability q_i. Let $a_i^T = 1$ if an audit is drawn in term T and $a_i^T = 0$ otherwise. The mayor seeks to maximize the discounted sum of rents r, net of the costs of rent extraction c over his tenure. The mayor's per-period utility in term T is given by

$$u^T(e_i^T, X_i, \varepsilon_i, a_i^T) = e_i^T + X_i'\alpha + \varepsilon_i - c(e_i^T, a_i^T) \tag{3.3}$$

Mayors incur the costs to rent extraction through two channels. First, the mayor incurs the expected cost of having a legal action taken against him, which is increasing in the effort placed into rent-seeking. We refer to this channel, captured by variation in c, as legal discipline. Second, outlined in the derivation of the voter's strategy in the following section, the mayor's expected reelection probability is decreasing in the rents he extracts. We refer to the latter as the electoral discipline channel.

We describe here the legal discipline channel. We assume that a legal action is taken against the mayor with probability $(\gamma_0 + \gamma_1 a_i^T)e_i^T$, where $\gamma_1 > 0$ implies that legal actions are more likely when mayors are audited. We assume that the cost of the legal action is given by $b_0 + b_1 e_i^T$, so that $b_1 > 0$ implies that punishment is increasing in the mayor's corrupt action. Thus the mayor's cost function can be written as

$$c(e_i^T, a_i^T) = b_0(\gamma_0 + \gamma_1 a_i)e_i^T + b_1(\gamma_0 + \gamma_1 a_i)(e_i^T)^2 \tag{3.4}$$

Assuming that $b_0, b_1, \gamma_0, \gamma_1 > 0$, this function is strictly increasing and strictly convex in e_i^T.

The Voter. We assume that there is a representative voter and adapt the standard probabilistic voting model. The voter in municipality i only chooses an action if there is a first-term mayor ($T = F$). The voter's decision, whether or not to reelect the incumbent, depends on the following factors: the mayor's observable characteristics, the voter's belief about the mayor's unobservable characteristic $\tilde{\varepsilon}_i$, and the mayor's popularity. The mayor's popularity is given by $X_i'\xi + \delta_i$, where the mayor's popularity shock δ_i is drawn independently from a uniform distribution F_D with mean μ_D and density σ_D. The voter's per-period utility when there is a first-term mayor is given by $v_i^F = -r_i^F$ with the added popularity shock $X_i'\xi + \delta_i$ if he chooses to reelect the incumbent, while the voter's per-period utility when there is a second-term mayor is $v_i^S = -r_i^S$.

The voter observes contemporaneous rents with probability $\chi_i^T \equiv \chi_0 + \chi_1 a_i^T$. Setting $\chi_1 > 0$ implies that voters are more likely to observe rents and punish mayors when an audit occurs in the same term. After observing the popularity shock and, possibly, rents, the voter chooses whether to reelect the incumbent or select a challenger who is drawn at random from the pool of candidates.

Equilibrium. The timing of the game is as follows; (1) Given his observable characteristics, the first-term incumbent chooses his effort level; (2) his unobserved ability draw is realized and first-term rents are extracted; (3) the audit draw, the voter's rent signal draw, and the incumbent's popularity shock are realized; (4) elections are held; if the incumbent loses, the game continues with step (1) with a randomly drawn first-term mayor, otherwise; (5) the second-term incumbent chooses his effort level, the second-term audit draw is realized and second-term rents are extracted; the game continues with step (1) with a randomly drawn first-term mayor.

We solve for the perfect Bayesian equilibrium in pure strategies. A strategy for the mayor is a sequence of choices $e_i^T(q_i, X_i)$ for each term T conditional on the audit probability q_i and his observable characteristics X_i. A strategy for the voter is the choice $R_i(\tilde{\varepsilon}_i, \delta_i, q_i, X_i)$ of whether to reelect the mayor conditional on his belief about the mayor's type $\tilde{\varepsilon}_i$, the popularity shock δ_i, the audit probability and the mayor's observable characteristics. Formally, a perfect Bayesian equilibrium is a sequence of mayor and voter strategies and voter beliefs such that: 1) the mayor's strategy is optimal given the voter's strategy, 2) the voter's strategy is optimal given the mayor's strategy, and 3) the voter's beliefs are consistent with the mayor's strategy on the equilibrium path.

We begin by considering the equilibrium strategy of the second-term mayor. The second-term mayor faces no reelection incentives and thus only maximizes his expected second-term utility (see equation (3.3)). We assume that $1 > b_0(\gamma_0 + \gamma_1 q_i)$ so that there is a unique interior solution. The first-order condition yields the second-term mayor's equilibrium strategy:

$$e^{S*}(q_i) = \frac{1 - b_0(\gamma_0 + \gamma_1 q_i)}{2b_1(\gamma_0 + \gamma_1 q_i)} \tag{3.5}$$

We next consider the voter's equilibrium strategy. Given his belief over the mayor's type, the voter chooses whether or not to reelect the incumbent by considering which option maximizes his expected lifetime utility. In equilibrium, the voter's value function when selecting a random first-term mayor is given by

$$\begin{aligned} V(q_i) = \int & v^{F*}(X_i, \varepsilon_i, q_i, \delta_i) + \\ & \beta \left[p(X_i, \varepsilon_i, a_i^F, q_i) \left(v^{S*}(X_i, \varepsilon_i, q_i) + \beta V(q_i) \right) + \left(1 - p(X_i, \varepsilon_i, a_i^F, q_i)\right) V(q_i) \right] \, \mathrm{d}\mathbf{F} \end{aligned} \tag{3.6}$$

where β is the discount factor, p denotes the equilibrium probability of reelection, v^{F*} and v^{S*} denote equilibrium per-period voter utilities, and $\mathbf{F}$ denotes the joint distribution of $(X_i, \varepsilon_i, \delta_i, a_i^F)$.

Let $\tilde{\varepsilon}_i$ denote the voter's belief about the mayor's type. The voter reelects the incumbent if

$$\delta_i \geq -h(X_i) + \beta \left((1-\beta)V(q_i) + e_i^{S*} + \tilde{\varepsilon}_i \right) \tag{3.7}$$

where $h(X_i) \equiv X_i'\xi - \beta X_i'\alpha$ denotes how much voters value the mayor's characteristics when accounting for their effects on both rents and popularity. It follows that in equilibrium, the probability that a mayor of type (ε_i, X_i) is reelected is[16]

$$p\left(X_i, \varepsilon_i, a_i^F, q_i\right) = F_D\left(2\mu_D + h(X_i) - \beta[(1-\beta)V(q_i) + e_i^{S*} + (\chi_0 + \chi_1 a_i^F)\varepsilon_i]\right) \tag{3.8}$$

[16]We derive this equation in Appendix 3.8.

Thus, since audits increase the probability of detection by the voter ($\chi_1 > 0$), the equilibrium reelection probability is higher when there is an audit if and only if the mayor's unobservable ability is below average ($\varepsilon_i < 0$).

We next solve for the first-term mayor's maximization problem. His problem is to choose, conditional on the voter's strategy, the effort levels $(e_i^F, e_i^S) \in \mathbb{R}_+^2$ which maximize his expected utility:

$$\max_{e_i^F, e_i^S} \int u^F(e_i^F, X_i, \varepsilon_i, a_i^F) + \beta \mathbb{P}(R_i = 1 | e_i^F, \varepsilon_i, X_i, a_i^F, q_i) u^S(e_i^S, X_i, \varepsilon_i, a_i^S) \quad d\mathbf{G} \tag{3.9}$$

where $\mathbb{P}(R_i = 1 | e_i^F, \varepsilon_i, X_i, a_i^F, q_i)$ is the probability the mayor is reelected,[17] and $\mathbf{G}$ denotes the joint distribution function of $(a_i^F, a_i^S, \varepsilon_i)$. Assuming an interior solution, the mayor's equilibrium first-term action is

$$e^{F*}(q_i, X_i) = \frac{1 - b_0(\gamma_0 + \gamma_1 q_i) - \beta^2(\chi_0 + \chi_1 q_i)\sigma_D U^{S*}(q_i, X_i)}{2b_1(\gamma_0 + \gamma_1 q_i)} \tag{3.10}$$

where $U^{S*}(q_i, X_i)$ denotes the equilibrium expected payoff for the mayor's second term.

Therefore, equilibrium first-term rents, $r_i^{F*} = e_i^{F*} + X_i'\alpha + \varepsilon_i$, are determined by three factors. First, they are decreasing in expected legal costs, which are captured by the legal cost parameters b_0 and b_1, and the legal action probabilities γ_0 and γ_1. Second, the possibility of reelection reduces the effort spent on rent extraction. The magnitude of the reduction is increasing in the probability that the voter observes rents (captured by χ_0, χ_1, and q_i), expected term 2 utility, the density of the popularity shock, and the mayor's patience as captured by the discount factor β. Third, selection over ability plays a role through observable characteristics X_i and the unobservable trait ε_i.

In contrast, equilibrium second-term rents, $r_i^{S*} = e_i^{S*} + X_i'\alpha + \varepsilon_i$, are only determined by legal costs and selection. Elections will improve the selection of mayors who are reelected, and will do so to a greater extent when an audit occurs prior to the election.[18]

The Model With Learning

In the model outlined above, whether a municipality has been audited in the past has no long-term consequences on corruption. Since there is a two-term limit, an audit only affects rents in the following term through its effect on selection. Otherwise, since the expected costs of an audit do not depend on the municipality's audit history, mayor and voter strategies will not depend on past audits. This result is not unique to our framework as it also follows from other typical models of political agency.[19] However, if mayors and voters are not perfectly informed about the ex-ante

[17] See Appendix 3.8 for the derivation.

[18] To be precise, selection on observables improves with reelection if and only if voters do not have a sufficiently strong taste for observable characteristics which increase rents.

[19] For example, in a framework where politicians differ on whether they have social preferences for the voter's welfare or are purely self-interested (Besley, 2007), audits will only affect future corruption through a change in the selection of types who are reelected. This is also the case if, instead, politicians differ along a dimension of responsiveness to voters (Banks and Sundaram, 1993). See Bobonis et al. (2015) for a discussion of the effects of audits in various modeling frameworks of political agency.

audit probability within a given electoral term, as we argued in Section 3.2, it is plausible that they update their beliefs over the audit risk through the information they acquire from their own and others' audit experiences.[20]

Setup. We extend our model with the following framework of Bayesian learning to rationalize why the history of audits may affect the behavior of mayors and voters (henceforth, agents). For agents in municipality i, we assume that prior beliefs over the probability of an audit in a given term are distributed $Beta(\beta_{0i}, \beta_{1i})$. The mean of the prior is $\hat{q}_i := \frac{\beta_{0i}}{\beta_{0i}+\beta_{1i}}$ and the strength of the prior is captured by the sum $\beta_{0i} + \beta_{1i}$. We assume that $\beta_{0i} + \beta_{1i} < \infty$, so that there is uncertainty in the prior and thus agents will take their own (and neighboring) experiences into account when forming beliefs.

Every term, an audit is drawn in each municipality from an i.i.d. Bernoulli distribution with probability q. After each term, the agents observe their own draw and the draws of their neighbors if local media is present. Let N_i denote the set of municipalities neighboring i, where N_i is empty if local media is absent. Then the number of audit draws observed during a term in municipality i will follow a binomial distribution with sample size $n_i := |N_i| + 1$ and number of successful draws $y_i := a_i + \sum_{j \in N_i} a_j$. After observing these draws, the agents update their beliefs about the audit probability using Bayes' rule.

Although we model learning over the audit probability, it could also be the case that agents learn about the costs associated with audits. In the model, this would be the case if agents were uncertain and learned about γ_1 and χ_1 instead of q. In Appendix 3.9, we explicitly solve for such a model and estimate it with the same data. Although the structure of the learning process differs between the two models, in both cases, agents are learning about the expected costs of the audit program. Thus, perhaps unsurprisingly, we find similar results when estimating either model.[21]

Another possibility is that audits affect objective costs rather than beliefs. However, if audits only affect costs, it is difficult to rationalize why audits cause spillover effects across municipalities only in the presence of local media. This seems especially unlikely for the costs associated with legal actions, as it would imply that the legal penalties for corruption are higher when neighbors have been previously audited, but only in the presence of local media. Moreover, the learning models are consistent with the narrative evidence we presented in Section 3.2.

Equilibrium. We outline here the pure strategy perfect Bayesian equilibrium in Markov strategies when incorporating learning into the model. For the technical details, see Appendix 3.8.

The state of municipality i is given by the vector $\omega_i := (\beta_{0i}, \beta_{1i}, n_i)$, where β_{0i} and β_{1i} parametrize the prior of the agents in the municipality, and n_i denotes the number of audit draws observed in

[20] Moreover, recent empirical findings suggest that in fact agents place at least some weight on their experiences when forming beliefs (Kleven et al., 2011; Malmendier and Nagel, 2011). Our learning framework is similar to the one developed in Gallagher (2014). This study finds a significant increase in insurance take-up in communities following the experience of a flood. Similarly to the spillover effects we find, Gallagher (2014) also shows that insurance take-up increases when neighboring communities which share a TV media market are flooded.

[21] Formally, using the Vuong closeness test, we cannot reject the null that the two models are equally close to the true data generating process (p-value $= 0.423$).

a term. In the following period, due to Bayesian updating, the state is given by $\omega_i' = (\beta_{0i}', \beta_{1i}', n_i')$, where $\beta_{0i}' = \beta_{0i} + y_i$, $\beta_{1i}' = \beta_{1i} - y_i + n_i$, and $n_i' = n_i$. We analogously define the state in the subsequent period by ω_i''.

Then, when solving the second-term mayor's maximization problem, we must take his beliefs about the audit probability into account. Similarly, for the first-term mayor, we now consider his beliefs about the audit probability in the current and next term. Thus, the equilibrium first-term and second-term effort levels, which are now a function of the state vector ω_i, are

$$e^{F*}(X_i, \omega_i) = \frac{1 - b_0(\gamma_0 + \gamma_1 \hat{q}_i) - \beta^2 \sigma_D W(X_i, \omega_i)}{2b_1(\gamma_0 + \gamma_1 \hat{q}_i)} \tag{3.11}$$

$$e^{S*}(\omega_i) = \frac{1 - b_0(\gamma_0 + \gamma_1 \hat{q}_i)}{2b_1(\gamma_0 + \gamma_1 \hat{q}_i)} \tag{3.12}$$

where $W(X_i, \omega_i) := (\chi_0 + \chi_1)\hat{q}_i U^{S*}(X_i, \omega_i, a_i^F = 1) + \chi_0(1 - \hat{q}_i)U^{S*}(X_i, \omega_i, a_i^F = 0)$, and U^{S*} denotes the mayor's equilibrium expected second-term payoff, conditional on his characteristics X_i, the state ω_i and the audit draw a_i^F. These effort levels are similar to the ones we derived for the model without learning, with the key difference being that the belief about the audit probability will discipline mayors in both terms.

The equilibrium probability that a mayor of type (X_i, ε_i) is reelected conditional on whether an audit is drawn and the state ω_i is:

$$p\left(X_i, \varepsilon_i, \omega_i, a_i^F = 1\right) = F_D\left(2\mu_D + h(X_i) - \beta\left[G(\omega_i) + (\chi_0 + \chi_1)\varepsilon_i\right]\right) \tag{3.13}$$

$$p\left(X_i, \varepsilon_i, \omega_i, a_i^F = 0\right) = F_D\left(2\mu_D + h(X_i) - \beta\left[G(\omega_i) + \chi_0 \varepsilon_i\right]\right) \tag{3.14}$$

where $G(\omega_i) := V(\omega_i) + e_i^{S*}(\omega_i) - \beta \mathbb{E}_{y_i|\omega_i} V(\omega_i')$, and $V(\omega_i)$ denotes the value function of the voter when a random first-term mayor is selected. Notably, this function will depend on the state ω_i:

$$V(\omega_i) = \int v^{F*}(X_i, \varepsilon_i, \delta_i, \omega_i) + \beta \left\{ p(X_i, \varepsilon_i, a_i^F, \omega_i') \left[v^{S*}(X_i, \varepsilon_i, \omega_i') + \beta V(\omega_i'') \right] + \left[1 - p(X_i, \varepsilon_i, a_i^F, \omega_i')\right] V(\omega_i') \right\} d\mathbf{F} \tag{3.15}$$

where δ_i is the popularity shock, v^{T*} denotes equilibrium per-period voter utility, and $\mathbf{F}$ is the joint distribution function for the vector $(X_i, \varepsilon_i, \delta_i, a_i^F, y_i, y_i')$.

In sum, there are two objects in the model which are directly affected by the belief over the audit probability. First, consider the expected legal costs faced by the mayor, which are given in this case by $b_0(\gamma_0 + \gamma_1 \hat{q}_i)e_i + b_1(\gamma_0 + \gamma_1 \hat{q}_i)e_i^2$. In municipalities in which mayors and voters have observed larger proportions of audits, mayors will expect higher legal costs and extract less rents. Second, the belief over the audit probability also affects the probability of voter detection. The mayor will choose his action taking the probability of detection to be $\chi_0 + \chi_1 \hat{q}_i$. Thus, in municipalities in which mayors and voters have observed larger proportions of audits, first-term mayors will be disciplined by higher perceived electoral costs of corruption. Therefore, in this framework, audits will not only affect future corruption through a selection effect, but also a disciplining effect.

3.4 Research Design

Before structurally estimating the model, we examine whether the audits reduce future corruption in the reduced-form using the random variation induced by the lotteries. To test this hypothesis, we need to overcome the fact that we only observe corruption once a municipality has been audited. We do so by exploiting municipalities that have been audited multiple times. As we see in Figure 3.5, out of the 1,949 municipalities that have been audited, 14 percent of them have been audited multiple times: 253 audited twice, 18 three times, and 1 municipality 4 times. For a given round of audits, we compare the corruption levels of municipalities that had been audited prior to this audit to those that had not (and are thus being audited for the first time).

Figure 3.6 shows the number of control and treated municipalities for each lottery in our estimation sample. As expected, the number of municipalities that have been audited more than once increases over time. For instance, in the 22^{nd} lottery, only 6 out of 60 municipalities had been audited in the past, compared to 22 out of 60 in the 38^{th} lottery. Given the structure of the data, we estimate the following model for municipality m in state s, audited at date t.

$$\text{Corruption}_{mst} = \alpha + \beta \text{Past Audit}_{mst} + Z'_{ms}\gamma + f(nos)_{mst} + \nu_s + \mu_t + \varepsilon_{mst} \tag{3.16}$$

where Corruption_{mst} is the log of the number of corrupt irregularities detected in municipality m during audit t, and Past Audit_{mst} is an indicator for whether at date t the municipality had been audited in the past. The vector Z'_{ms} consists of a set of municipal characteristics (e.g. population, income per capita, income inequality, etc.) measured in 2000. These controls allow us to account for any socio-economic differences across municipalities prior to the start of the program. The variable nos_{mst} denotes the number of service orders that auditors were sent to investigate. Because audits with more service orders tend to discover more irregularities, it is important to account for these differences in a flexible manner. In our preferred specification, the number service orders is controlled for non-parametrically. The error term, ε_{mst}, captures unobserved (to the econometrician) determinants of corruption.

Importantly, our model also adjusts for two classes of fixed-effects. We include state intercepts, ν_s, to capture the fact that the randomization is stratified by state.[22] We also include lottery fixed effects, μ_t, which are important for two reasons. First, municipalities are more likely to become treated over time, but within a given lottery the probability a municipality had been audited in the past is the same for municipalities within a state. Second, starting in the 20^{th} lottery, the CGU began to audit funds in selected areas and programs. It is thus difficult to compare corruption levels over time, and hence we restrict our analysis to variation within audits. Because municipalities are audited at random, we can interpret the coefficient β as the causal effects of the audits on corruption.

In addition to estimating the effects on corruption, we also test whether an audit increases the likelihood of a federal conviction or investigation. Because we do not need to restrict the sample to only audited municipalities, we can compare audited places to non-audited places with

[22]Given the population density of North Brazil, when CGU draws municipalities for audit, this region, which includes the states of Acre, Amapá, Amazonas, Pará, Rondônia, Roraima and Tocantins, is treated as a single state.

the following specification:

$$\text{Legal}_{mt} = \alpha + \beta \text{Audited}_{mt} + \nu_m + \mu_t + \varepsilon_{mt} \tag{3.17}$$

where Legal_{mt} is an indicator for whether a legal action (e.g. crackdown involving political corruption or the mayor was convicted for corruption) occurred in municipality m in year t. Our treatment variable, Audited_{mt}, which is equal to one after the municipality has been audited for the first time, estimates the causal effect of being audited on the likelihood of incurring a subsequent legal action. The regression adjusts for municipal and year fixed effects, and the error term is clustered at the level of the municipality.

3.5 Results

Reduced-form Estimates

Effects of the Audits on Corruption and Mismanagement. Table 3.3 presents OLS regression results from estimating several variants to Equation 3.16. The specification in the first column estimates the effects of having been audited on the log of the total number of irregularities discovered in the audit, controlling for state and lottery intercepts, as well as the number of service orders. Column 2 extends this specification to include various socio-economic characteristics of the municipality. Our preferred specification is presented in Column 3, which modifies the specification in Column 2 to control for the number of service orders in a nonparametric manner. Our estimation sample includes all audits from lotteries 22 to 38.

The results in columns 1-3 suggest that municipalities that had been audited in the past commit significantly fewer irregularities than those that had not been previously audited. Once we control for municipal characteristics and service-order fixed-effects, we estimate a reduction of 5.8 percent. We also find that the number of irregularities correlates with several of the socio-economic characteristics that we have come to expect from the cross-country literature (e.g. Treisman (2000)). For example, we see strong negative associations with income per capita and literacy rates, as well as positive correlations with income inequality and population.

As we discussed above, there is an important distinction to be made between corruption and mismanagement. We do this in columns 4-9. In columns 4-6, we replicate the previous specifications using as a dependent variable the log of total acts of mismanagement. In columns 7-9, we use the log of total acts of corruption as the dependent variable.[23]

We do not find any evidence that audits affect mismanagement. Under our preferred specification, the point estimate is small and statistically indistinguishable from zero (coefficient = -0.023, robust standard error=0.041). In contrast, we find that having been audited in the past leads to a significant reduction in corruption. Municipalities that had experienced a previous audit committed 7.9 percent fewer acts of corruption compared to those that had not. Visually, the effects of

[23]We also estimate the effects of the audits on the totals acts of corruption and mismanagement, using a negative binomial regression model. We present the marginal effects in Table 3.4. Overall, the findings are quite similar.

the treatment can be seen in Figure 3.7. The figure plots the residuals from a regression of log corruption on state, lottery, and service order fixed effects. The figures compares the distribution of these residuals between treatment and control municipalities. From this comparison, we see that the audits reduced corruption at the upper tail of the distribution. For treated municipalities, the 99^{th} percentile of the corruption distribution corresponds to approximately the 91^{st} percentile of the corruption distribution in control municipalities. The left tails of the corruption distributions are comparable between treatment and control municipalities.

To interpret this magnitude, consider that the average municipality in our sample receives 15 million reais in federal transfers per year. Based on our estimates of a random sample of audit reports, 30 percent of the funds audited were found to be diverted, implying that audits reduced corruption by R$355,000 per year per municipality. The municipal characteristics are also quite predictive of corruption levels: for example, a 10 percent increase in per capita income is associated with a 1.8 percent decline in corruption.[24]

Spillover Effects. The estimates presented in Table 3.3 are likely to represent a lower bound on the effects of the audits. If control municipalities are learning about the audits either through the media, from an audited neighbor, or from their partisan network, then they too might refrain from corruption. We explore these possibilities in Table 3.5. In column 1, we re-estimate Equation 3.16, adding the number of neighboring municipalities that have experienced an audit as an additional independent variable. To account for the fact that municipalities have different numbers of neighbors, we also control non-parametrically for the number of neighbors. In columns 2 and 3 we introduce an interaction term for whether local media is present in the municipality. Because neighboring municipalities typically share a media market, a municipality is more likely to learn about its neighbors' audits if it has local media. In Section 3.2, we presented anecdotal evidence in support of this claim.

In column 1, we estimate that for each additional treated neighbor, a municipality reduces its corruption by 2.0 percent, but this effect is not statistically significant. The coefficient on our main treatment variable is nearly identical to those presented in Table 3.3, suggesting that even when controlling for spillover effects from neighboring municipalities, fewer acts of corruption are uncovered in municipalities that have been audited in the past. In columns 2-3, we test for whether the spillover effects are more pronounced in places with local media. For both AM radio (column 2) and television (column 3), we find evidence of significant spillover effects. An additional audited neighbor decreases corruption by 7.5 percent when AM radio is present, and by 10.4 percent for local television. We find no evidence of spillover effects in municipalities without the presence of the media.

In column 4, we further explore whether information about the effects of an audit is also transmitted through partisan networks. Within a state, political parties will sometimes facilitate interactions between their mayors through annual meetings and discussions with federal deputies, senators and governors. If these partisan networks are strong, then mayors might learn from the

[24]We also test whether the effects of the audits vary according local characteristics, but find little evidence of heterogeneous effects (see Table 3.6).

audits experienced by other mayors within their network. To test for this, we add to the specification presented in column 3 the number of times a mayor from the same party within the state had been audited in the past. To account for any differences in the strength of the partisan networks, we also included party fixed effects. The results in column 4 suggest that parties do play a relatively small but statistically significant role in information diffusion. For each additional mayor audited from their partisan network, mayors decreased their corruption levels by 0.4 percent. The spillover effects of local media also remain strong even after allowing for the effects of partisan networks. In column 5, we re-estimate the equation allowing for the spillover effects to vary by the share of the population with a college degree, income per capita, and the share of urban population. Even after allowing for differential effects along these other characteristics, the heterogeneous effects by AM radio, local television, and party remain robust.[25]

Overall, these findings suggest that we are underestimating the audit program's true impact on corruption. Municipalities that are presumably learning about the potential effects of the audits are engaging in less corruption, even if they had not experienced an audit themselves.

Effects of the Audits on Legal Actions. In Table 3.7, we investigate the effects of being audited on the likelihood that the municipality faces a subsequent legal action. In columns 1-6, we estimate variants of Equation 3.17 with three sets of dependent variables: an indicator for whether a police crackdown involving political corruption occurred (columns 1 and 2), an indicator for whether a mayor was convicted for corruption (columns 3 and 4), and an indicator for either a crackdown occurred or a mayor was convicted (columns 5 and 6). Because we are not limited to municipalities that have been audited at some point in time, we estimate these specifications for the entire sample of municipalities eligible for an audit.

Compared to non-audited municipalities, places that have experienced an audit are much more likely to face a subsequent legal action, as measured by either a police crackdown or a mayor conviction. Municipalities that have been audited in the past are 0.5 percentage points more likely to face a legal action than those that have not been audited. This effect implies that the audits led to an increase of approximately 30 legal actions from a base of 140 among control municipalities. In columns 2, 4 and 6, we find that the effects of the treatment are largely concentrated in places with a judiciary district. Among these municipalities, the treatment increased the likelihood of a legal action by 35.4 percent, relative to control municipalities with a judiciary district.

While informative, the specifications presented in columns 1-6 would ideally also condition on the level of corruption in the municipality. In columns 7-9 we regress our measures of legal action on log acts of mismanagement and log corruption for the sample of municipalities that have been audited. As expected, we find that corruption is strongly associated with the likelihood of a legal action. For example, a 1 percent increase in number of corruption acts is associated with a 8.8 percent increase in the likelihood of a legal action. In contrast, acts of mismanagement are not associated with any subsequent legal actions. Overall these findings suggest that the legal costs of engaging in corruption are substantial.

[25]We also replicate these findings when using a dummy for the presence of at least one neighboring audit, instead of the number of neighboring audits (see Table 3.8).

Mechanisms

Thus far, the evidence suggests that audits reduce future corruption and increase the likelihood of a legal action. In Section 3.3, we discussed several reasons why the audits may reduce corruption. One reason is political selection. If audits allow voters to punish corrupt mayors and reward good ones, then we would expect better politicians in places where the incumbent was audited prior to the election and still re-elected. Another channel is electoral discipline. If audits increase the perceived future probability of being exposed to voters, then mayors who have re-election concerns will refrain from corruption. A third is what we have termed a legal or non-electoral disciplining effect. Mayors may refrain from corruption even in the absence of re-election incentives, lest they incur reputation or legal costs. A final possibility is a political entry effect, which would occur if audits changed the type of mayors who run for office.

In this section, we present reduced-form tests of these various mechanisms, and isolate their effects under the assumption that they are constant and additive. In Section 3.6, we instead disentangle the channels by structurally estimating the model.

Electoral and Legal Disciplining Effects. To isolate the effects from electoral and legal discipline, we consider the set of municipalities in which a mayor experiences an audit early in his term (often over funds that he did not administer), and is then audited again in the same term.[26] In these cases, no election has occurred, which rules out the possibility of any audit-induced political selection or entry effect. Any difference in corruption levels between these municipalities and those that have not been audited (control group) can only be due to electoral or legal disciplining effects.

To further distinguish between electoral and legal disciplining, we estimate two additional specifications. We first test whether the effects of the audits vary by whether the mayor was in his first or second term. If second-term mayors, who are term limited, do not have further re-election incentives, then they should only respond to legal costs, whereas first-term mayors will respond to both types of costs. The second specification tries to relax the assumption that second-term mayors do not have further career concerns, given that they may be inclined to run for higher office. To account for this possibility, we first estimate a mayor's propensity to run again for a future office using data from all elections held during 2000 to 2012. To compute this propensity score, we estimate a Logit model based on a mayor's gender, education, previous occupation, vote share and campaign spending in the past election.[27] We then test whether the effects of the audits were more

[26]Note that the audit may have even occurred in the subsequent term, but the funds audited referred to those administered under the previous term.

[27]Specifically, we estimate the follow equation:

$$\log \frac{\mathbb{P}(\text{Ran higher}_i)}{1-\mathbb{P}(\text{Ran higher}_i)} = \beta_0 + \beta_1 \text{Male}_i + \beta_2 \text{campaign spending}_i + \beta_3 \text{vote share}_i + \eta_e + \theta_o \tag{3.18}$$

where Ran higher$_i$ is an indicator for whether the mayor ran for a higher office, namely elections for state and national legislature, governor, or president. Male$_i$ indicates whether the mayor is male, campaign spending$_i$ measures the amount of money the mayor spent is his election, vote share$_i$ denotes the share of votes he received in his election, η_e represent a set education intercepts, and θ_o represents a set of occupation dummies at the 1-digit level. The results from estimating this equation are presented in Table 3.9.

pronounced for mayors who were more likely to run for a future office.

We perform these comparisons in columns 1-3 of Table 3.10. In column 1, we compare non-audited places to municipalities in which the mayor experienced multiple audits within the same term. We find that the audits led to 12.7 percent reduction in corruption, which can be attributed to either an electoral disciplining effect, a legal disciplining effect, or both. In column 2 and 3, however, we do not find a statistically significant differential effect based on whether the mayor is in his second term or is more likely to run for a higher office. For example, based on a one standard deviation increase in the propensity to run for a higher office, the estimates reported in column 3 suggest that the differential effects of the audits led to only an additional 0.3 percent reduction in corruption.

Political Selection. The effect of political selection on corruption stems from voters reelecting at greater rates the mayors who are less corrupt. To test for the existence of the political selection channel, we compare mayors who were audited and re-elected to mayors who were not audited but were also reelected. If, as documented in Ferraz and Finan (2008), the audits enable voters to punish corrupt politicians and reward non-corrupt ones, then the reelected mayors who had been audited prior to the election should be, on average, more positively selected than the reelected mayors who had not been audited prior to the election.

We present this comparison in column 4. Among municipalities where the mayor was re-elected, corruption levels were 14.9 percent lower in audited municipalities compared to non-audited municipalities. This difference in corruption levels, however, reflects both the change in the composition of mayor types (political selection), as well as a legal disciplining effect. But given our previous estimates of the disciplining effects (in columns 1-3), these results suggest that political selection is actually playing a relatively minor role in how these audits are reducing corruption.

One concern with this interpretation is the lack of statistical precision for some of our estimates. If, for example, we used the lower bound estimate of the 95 percent confidence interval in column 3, the differential effects of the audits would imply an additional 9.8 percent reduction in corruption levels for mayors with an one standard deviation increased propensity to run for higher office. Similarly, although the results in columns 1 and 4 imply a political selection of only 2.2 percent, given our standard errors, the political selection effects could also be as large as 15.2 percent.

Another potential concern with our comparison between discipline versus selection effects is in the timing of the audits. If the effects of the audits differ depending on how much time had elapsed since the last audit, perhaps due to recency bias, then the comparison between the effects in columns 1 and 4 would also incorporate this additional effect. In columns 5 and 6, we present two specifications to test for this possibility: 1) we allow for the effects of the audits to vary flexibly by the number of terms since the last audit; 2) we control for the log of the number of years since the last audit (re-centered at the sample mean). In both specifications, we find no evidence of a differential effect based on how much time had elapsed since the last audit.

Political Entry. A fourth channel through which audits may reduce corruption is political entry. This would be the case if the audits induced better candidates to enter politics. We test for this mechanism by comparing corruption levels in places that were audited at $t-1$ to those that were not, conditional on having a new mayor in time t due to an open seat election. By focusing on open-seat elections, we obviate the direct effects of the audits on any potential candidate, given that the audits had taken place on a term-limited mayor. As such, any effects of the audits would have to come from changes to the political environment more generally. Conditional on having a new mayor at time t, there are three effects that could be driving this difference: electoral and legal disciplining effects, and a political entry effect. With estimates of the first two effects, we can isolate the effects of audits through political entry.

In column 6, we find that compared to non-audited municipalities, corruption is 12.2 percent lower in places that were audited in the prior administration. Once we net the effects estimated in column 1, these results suggest that the political entry effect is zero, and provide additional support for the importance of legal disciplining.

As a further test for political entry, we examine whether the audits impacted the types of candidates that ran for office during open-seat elections. In Table 3.11, we examine whether the audits impacted the competitiveness of the elections, as well as the characteristics of the candidate pool and elected mayor. Consistent with a negligible entry effect, we find no evidence that the audits affected any of these election characteristics.

In sum, the results from Tables 3.10 and 3.11 suggest that the audits' impact on corruption were driven mostly by legal disciplining effects. There are, however, two important caveats to this interpretation. First, we need to assume additive and constant treatment effects in order to compare the effects of the audits across the various subsamples. Second, large standard errors cloud some of our comparisons. In light of these limitations, it is useful to complement our reduced-form findings with structural analysis, in which we can better disentangle the various channels contributing to corruption.

Alternative Mechanisms

An alternative interpretation of our reduced-form findings is that audits simply teach politicians how to better hide corruption. In this case, corruption has not necessarily been reduced, but perhaps displaced. Although we cannot rule out this interpretation completely, there are at least two reasons why we do not think displacement is the primary mechanism. First, the set of programs and sectors that are subject to an audit vary over time, making it difficult for mayors to predict which specific areas and programs will be audited in the future. Second, based on the audit reports, we can classify how the corruption occurred. If places that have been audited in the past learned how to displace corruption, then we might expect an audit to affect the type of corruption committed in subsequent audits.

We test for these explanations in Table 3.12. Here, we estimate the effects of having been audited in the past on the share of corrupt acts associated with embezzlement, procurement contracts, and over-invoicing; the three most common forms of corruption.[28] In column 1-3, we find no evidence that the audits induced mayors to shift away from or into these forms of corruption. In columns 4-6, we restrict the sample to consider only those cases in which the mayor experienced multiple audits, which presumably is where the learning effects would be easiest to detect. But again, we find no evidence that the audits affected the nature of corruption in these places.

To further test the displacement hypothesis, we explore whether, across municipalities that are audited multiple times, less corruption is uncovered when the same sectors are audited. We estimate in column 7 the association between the amount of corruption detected during a second audit and the share of sectors investigated in both this and the municipality's previous audit. If mayors in treated municipalities are learning to better hide corruption, then presumably less corruption should be uncovered in places where the audits investigated funds from the same sectors. But as the result in column 7 indicates, the correlation, instead of negative, is positive and not statistically significant.

As a final test of displacement, we examine whether the audits affect how municipalities spend their budgets. If local governments are displacing corruption by shifting their expenditures to sectors where corruption is harder to detect, then expenditure shares should be different in municipalities that have been audited in the past. In particular, we might expect mayors who experienced an audit to shift expenditures away from sectors that are more prone to corruption. For each audit, we compute the share of public expenditures spent in each sector during the given year. We also aggregate the share of public expenditures spent in education, health, and welfare, which are the sectors in which almost 78% of the corruption occurs during a first time audit.

The results of public expenditures are presented in Table 3.13. In column 1, we do not find any evidence that the audits led mayors to shift their expenditures away from high corruption sectors towards sectors that are less corruption prone. In the remaining columns, we disaggregate expenditures further, and again do not find any evidence that the treatment affected the manner in which municipalities allocated their budgets. In light of our previous discussion that mayors cannot anticipate which sectors and projects will be audited in the future, this result is not surprising.

Another impact of the audits may have come from a reduction in the amount of block grants a municipality receives from the federal government. If this response in turn lowered the opportunity for mayors to engage in corruption, then this could explain the reduction in corruption we observe among previously audited places. As shown in Table 3.14, we do not find any evidence that having been audited in the past leads to a reduction in subsequent block grants.[29]

[28]For each audit report, we create these shares by first counting keywords which are associated respectively with embezzlement, procurement contracts, and over-invoicing. We then divide the counts by the number of corrupt acts and finally we normalize the measures.

[29]We also do not find an effect when we interact the treatment with amount of corruption discovered in the audits (see Table 3.14).

3.6 Structural Estimation

We structurally estimate the model to complement our reduced-form analysis in two ways. First, an empirical challenge is that a decrease in rents in treated municipalities could equally be caused, on the one hand, by legal discipline, or on the other, by the combination of electoral discipline and selection. The structural estimation directly tackles this issue without restricting the sample. Instead, we jointly estimate an equation for the responsiveness of voters to corruption with equations derived for the mayor's equilibrium strategy taking the voter's strategy into account. At the cost of imposing some structure to the relationships, this approach allow us to estimate the parameters required to quantify the importance of each channel. Second, the structural model embeds the learning process caused by the realization of audits which we formulated in Section 3.3. Thus, in addition to data on corruption and elections, the structural estimation exploits data on neighboring audits and media presence.[30] Moreover, this approach allows us to recover the parameter estimates needed to analyze policy counterfactuals.

Data and Estimation

We estimate the model for the same sample of audits used in the reduced-form estimation, except that we remove the second audit in cases where the same mayor is audited twice in the same term. Each observation i consists of the vector $\mathbf{Y}_i := (T_i, r_i, R_i, a_i^F, X_i, Z_i, \omega_i)$, where $T_i \in \{F, S\}$ indicates the mayor's term, r_i is the log of acts of corruption in the audit report (the same measure as used in the reduced-form estimation), R_i is a dummy for whether the mayor is reelected, a_i^F denotes whether the municipality was audited in the *previous* term if the current mayor is in his second term, X_i denotes the vector of mayor characteristics, Z_i denotes the vector of municipal characteristics, and ω_i is the vector that determines the beliefs over the audit probability.

To compute ω_i for each observation, we use data on the past history of audits of each municipality indexing time with t. We set $t = 1$ to be the 2001-2004 mayoral term, the first which was subjected to the audit program, and let $t = 2$ denote the 2005-2008 term and $t = 3$ denote the 2009-2012 term. In the first term when the program was implemented, i.e. when $t = 1$, we assume that prior beliefs over the audit probability follow the distribution $Beta(\beta_0, \beta_1)$. We set the mean of the prior, $\frac{\beta_0}{\beta_0+\beta_1}$, equal to the objective probability of an audit in our sample. This pins down one of the two free parameters which determine the prior. To pin down the remaining parameter, we set the number of pseudo-observations of the prior to $\beta_0 + \beta_1 = 20$. Our main results decomposing the effects of audits into channels are robust to this assumption. However, the effects of our counterfactual policies on the audit probability are affected by the choice of pseudo-observations. A larger number implies less uncertainty in the initial prior and hence smaller changes in beliefs due to experience, which in turn leads to the estimation of larger effects for changes in the audit probability on rent extraction. Finally, for the subsequent two time periods, we compute the prior using Bayes' rule. Hence we obtain $\omega_i = (\beta_{0i}, \beta_{1i}, n_i)$ for each observation.

[30] Despite the reduced-form evidence of spillover effects within a mayor's political network, we opted for parsimony and decided not to explicitly model this channel as it would require introducing parties.

The vector X_i includes mayor characteristics (gender, education and occupation), number of service orders, number of neighbors and state and lottery intercepts. We set the density of the popularity shock $\sigma_D = 2$, and the discount factor $\beta = 0.6561$, which represents an annual discount factor of 0.9, in order to identify χ_0 and χ_1. We set the cost parameters to $b_0 = 8$, $b_1 = 4$, such that the penalty of a legal action is equal to the equivalent of two terms of rents plus four times the amount of rents captured. These assumptions are made to identify γ_0 and γ_1: they do not substantially affect our results other than by scaling our estimates for these two parameters.

We estimate the vector of parameters $\theta := (\gamma_0, \gamma_1, \chi_0, \chi_1, \mu_\delta, \sigma_\varepsilon, \alpha', \eta', \lambda')$ using Maximum Likelihood. For a municipality i where the mayor is in his first term, the likelihood function is given by

$$L(\theta|r_i, R_i, X_i, Z_i, T_i = F, \omega_i) = f_\varepsilon(\varepsilon_i^F|\theta) p(X_i, \varepsilon_i^F, \omega_i, a_i = 1|\theta)^{1\{R_i=1\}} \left(1 - p(X_i, \varepsilon_i^F, \omega_i, a_i = 1|\theta)\right)^{1\{R_i=0\}}$$

where $\varepsilon_i^F = r_i - e_i^{F*}(X_i, \omega_i) - X_i'\alpha - Z_i'\lambda$ is the mayor's unobserved ability shock conditional on equilibrium play. We include the municipal characteristics Z_i additively and linearly in this term in order to control for heterogeneity across municipalities. Here, f_ε denotes the density of the shock, and p denotes the equilibrium probability of reelection, where we set a_i equal to 1 because an audit is realized for each of these observations.

If the mayor is in his second term, then the likelihood function is given by

$$L(\theta|r_i, R_i, X_i, Z_i, a_i^F, T_i = S, \omega_i) = f_\varepsilon(\varepsilon_i^S|\theta) p(X_i, \varepsilon_i^S, \omega_i, a_i = a_i^F|\theta)$$

where $\varepsilon_i^S = r_i - e_i^{S*}(\omega_i) - X_i'\alpha - Z_i'\lambda$ again denotes the mayor's unobserved ability shock. Note that for second-term mayors, whether the municipality was audited in the previous term enters the likelihood function by altering the probability of reelection as a function of ability, hence creating a selection effect. Thus, the probability of reelection is conditioned on whether an audit was realized in the previous term (a_i^F).

Let $\mathbf{Y} = (\mathbf{Y}_1', ..., \mathbf{Y}_n')$ denote the data. We estimate the vector of parameters θ which maximizes the likelihood function:

$$\mathscr{L}(\theta|\mathbf{Y}) = \prod_i L(\theta|\mathbf{Y}_i)$$

We estimate the asymptotic covariance matrix of the maximum likelihood estimator by evaluating the Hessian of the likelihood function, and we use the Delta method when needed to evaluate standard errors.

Results

Identification. Formally, the parameter vector θ is identified if for any other parameter vector $\theta' \neq \theta$, for some data $\mathbf{Y}$, $\mathscr{L}(\theta'|\mathbf{Y}) \neq \mathscr{L}(\theta|\mathbf{Y})$. In this section, we provide an informal discussion of the variation used to identify the parameter vector.

First, consider the legal parameters γ_0 and γ_1. Ignoring selection on the unobservable for now, the parameter γ_0 is identified because we observe the rents of second-term mayors, while γ_1 is identified because we can back out the mean perceived probability $\hat{q}_i$ from the data ω_i (see our discussion on Bayesian learning and equation (3.12)). For instance, $\gamma_1 = 0$ would imply that second-term rents are uncorrelated with the perceived audit probability. Second, χ_0 is identified since we jointly observe rents and reelection outcomes as well as the difference in rents between first and second-term mayors (see equations (3.11)-(3.14)). This will determine the size of the selection effect on second-term rents that was necessary to pin down γ_0 and γ_1. Since χ_0 is identified, the parameter χ_1 is also identified as we observe $\hat{q}_i$. Next, the vectors (α', λ') and η are identified by variation, respectively, in rents and reelection probabilities as a function of observable characteristics. Finally, the parameter σ_ε follows from the empirical distribution of rents and μ_D follows from the distribution of reelection rates.

Parameter estimates. Table 3.15 reports maximum likelihood estimates for our parameters of interest. The first two rows present the estimates for the probability of legal action. The estimate for the constant γ_0 is 0.0245. This implies that for a mayor who extracted average rents in the data ($r = 3.9825$), the probability of legal action when no audit occurs is 9.8 percent. This estimate is close to the mean number of legal actions which occur during a mayoral term ($0.029 \times 4 = 11.6$ percent, reported in Table 3.7). The positive, statistically significant estimate for γ_1 of 0.0052 implies that the realization of an audit increase the probability of legal action by 2.1 percentage points for a mayor who extracted average rents. This represents a 21 percent increase from the baseline probability when no audit occurs, which is close to the 20 percent increase we estimated with the legal action data in the reduced-form section. Therefore, these results suggest that the history of audits in a municipality and its neighbors, through its effect on the perceived threat of non-electoral costs, significantly affects corruption.

The next two rows of Table 3.15 report estimates for the probability that the voter observes rents. The estimate for the constant χ_0 is 0.0147, which implies that, if no audit is realized, the probability that the voter observes rents is approximately 1.5 percent. The estimate for χ_1 implies that this probability increases by 8.77 percentage points if an audit is realized. This result is consistent with the hypothesis that audits affect electoral discipline and selection. In the final two rows we report estimates for two more structural parameters. We estimate the standard deviation of the ability shock to be 0.3366. Since it is significantly larger than zero, the estimate implies that there is scope for voters to select mayors who extract less rents during elections. The final estimate reported is for the mean of the popularity shock. The estimate is positive, but not statistically significant. Thus, in the current sample, we do not find evidence for an incumbency advantage, which is consistent with the empirical literature for Brazilian municipalities (Klašnja and Titiunik, 2014).

In the rents and reelection terms within the likelihood equation, we also include the vector of mayoral characteristics. We report the coefficients for each characteristic in Table 3.16. In column 1, we find that rents are uncorrelated with gender and negatively correlated with education and quality of occupation (captured by a dummy for white collar occupations). However, these estimates are statistically indistinguishable from zero. In column 2, we find that the mayor's popularity is positively related to education, white collar occupation and male gender, but again none of these coefficients are statistically different from zero. Since we also did not find any reduced-form evidence that candidate characteristics depend on the history of audits in a municipality, it is unlikely that candidate entry explains why audits reduce corruption. We return to this point in the counterfactuals section.

Equilibrium outcomes. Given the maximum likelihood estimates, we compute predicted rents (log acts of corruption) for all mayors in the sample. The average predicted rents for mayors are 3.9825, the same as average rents in the estimation sample. To assess goodness-of-fit, we perform a Likelihood Ratio Test comparing the unrestricted model to a restricted model where only γ_0, σ_ε, μ_D and the lottery and state dummies are estimated. The restricted model is essentially one where a constant determines rents and a separate constant determines the reelection rate. We strongly reject the hypothesis that the restricted model is true ($\chi^2 = 159.37$, p-value $< 10^{-16}$).

To assess the out-of-sample fit of the model, we use data from the most recent audits which were not used in the structural estimation (i.e. the audits uncovering corruption from the 2012-2016 term). We test whether the structural model predicts out-of-sample corruption more accurately than an OLS model with the same set of explanatory variables. Using the parameter estimates for each model computed with the same sample of 839 observations, we compute predicted rents for the additional 239 observations from the most recent audits. We find the mean squared deviation between predicted and observed rents to be 0.140 when using the structural estimates compared to 0.161 when using the OLS estimates. Thus, the structural model outperforms the OLS model when fitting out-of-sample data on corruption.[31] We plot the data against rents predicted by the structural model in Figure 3.8.

To assess the fit of the Bayesian learning model, we regress rents on mayor and municipal characteristics, number of service orders, number of neighbors, lottery and state fixed effects. We repeat the regression with the mean belief about the audit probability as the dependent variable. Figure 3.9 presents the residuals of these regressions in a binned scatter plot. Recall that the mean of the posterior about the audit risk increases when agents within a municipality observe a larger proportion of audits than would be predicted by their prior. This plot shows that in such cases, mayors extract less rents. Likewise, in municipalities with histories where agents observe a smaller proportion of audits, the figure suggests that mayors extract more rents. Overall, the relationship between rents and the mean belief about the audit risk appears to be well approximated by a linear fit. Moreover, the R^2 of a linear regression (with the aforementioned controls) of rents on the mean belief is larger than that obtained from a linear regression of rents on the number of audits observed in the municipality.

Decomposing the effects of the audits. We decompose the effect of the audits on rents through legal discipline, electoral discipline and selection, and report the results in Table 3.17. The effect of legal discipline is computed by setting $\gamma_1 = 0$ for all observations and computing predicted rents under this condition. The condition implies that mayors are choosing their actions as if the probability of legal action were only γ_0 instead of $\gamma_0 + \gamma_1 \hat{q}_i$, that is, as if the agents were in a

[31] The structural model also outperforms the restricted model described in the previous paragraph, which yields a mean squared deviation of 0.179. Furthermore, we find a similar result when using absolute deviations instead of squared deviations. The mean absolute deviation between predicted and observed rents is 0.280 when using the structural estimates compared to 0.291 for the OLS model and 0.317 for the restricted model.

counterfactual setting where audits do not affect the probability of legal action. We then compare mean predicted rents in this counterfactual setting to those derived using our estimated parameters. We find that rents are on average 13.8 percent lower due to the effect of audits on legal discipline.

We quantify the effect of audits on the electoral discipline and selection channels using a similar methodology. We eliminate both channels by setting $\chi_1 = 0$. To back out electoral discipline, we then compare the counterfactual first-term rents under this condition to those predicted by our maximum likelihood estimates. We do not compare second-term rents as our model restricts electoral discipline to first-term mayors. We find that audits reduce rents through electoral discipline by 5.3 percent.

Next, we measure selection by comparing second-term rents when $\chi_1 = 0$ to those predicted by our estimates. This channel captures the effect of audits on the distribution of the ability of second-term mayors. The comparison shows that selection plays a negligible role: rents are on average less than 0.1 percent lower due to this channel. While the negligible selection effect may appear surprising at first, it can be explained by the fact that few municipalities in our sample are affected by the selection effect of audits, whereas all are affected by its disciplining effect. This is because only 30 percent of our sample are second-term mayors, of which only 10 percent were audited in the previous term. If we restrict our analysis to this subsample of affected mayors, we find that audits reduce rents by 2.4 percent due to selection over unobserved ability. Thus while audits do affect selection, few municipalities are subject to this effect.

Overall, the above results suggest that in our sample approximately 72 percent of the reduction in rents caused by audits is due to legal discipline, 28 percent is due to electoral discipline and less than 1 percent is due to selection. The importance of the legal discipline channel in reducing rents is consistent with our reduced-form findings.

This decomposition excludes the possibility that audits reduce the rents of second-term mayors through electoral discipline. We consider two extensions to the model which incorporate this channel. First, we estimate the model including in the equilibrium effort equations a term for the propensity to run for a higher office and its interaction with the mean of the prior for the audit probability. We do not find significant effects on the coefficients for these terms (see Table 3.18, column 1). Second, in Appendix 3.8, we consider an extension to the model where the second-term mayor values the voter's belief about his type when exiting office. Again, we do not find evidence that audits reduce the corruption of second-term mayors through electoral discipline (see Table 3.18, column 2).

Policy counterfactuals. We parameterize the model with our structural estimates and conduct a number of policy simulations. The results are presented in Table 3.19. We begin by simulating changes in the audit probability. Since mayors and voters are assumed to have a rational mean prior, increasing the audit probability amounts to increasing the mean of the prior distribution by the same amount. This increases the perceived audit probability for all mayors in the sample. We find that a 10 percentage point increase in the audit probability, roughly equivalent to doubling the audit probability, reduces corruption by an average of 14.6 percent for first-term mayors and by 9.3 percent for second-term mayors. The slightly larger effect for first-term mayors stems from electoral discipline reducing first-term rents more rapidly than the selection effect reduces second-term rents.

Assuming that the effect of the audit probability on rent-seeking is linear, our results are similar to those found in the literature. For instance, Zamboni and Litschig (2015) find that a 20 percentage points increase in the objective audit probability for a group of Brazilian municipalities decreased corruption by approximately 20 percent. Moreover, Bobonis et al. (2015) find that in the context of a long-standing audit program of municipalities in Puerto Rico, releasing audit reports just prior to an election induces a reduction in corruption by 67 percent. Olken (2007) finds that an increase in the audit probability from 4 to 100 percent for construction projects in Indonesian villages led to a reduction in missing expenditures by 30 percent. Although we find similar results, we caution that our estimates are sensitive to the assumptions in the learning framework we have used to model the effects of audits, and in particular, the parametrization of the prior distribution.

We next study the extent to which mayors can be disciplined by increasing the legal penalties associated with corruption. Recall that legal costs are assumed to have the linear functional form $b_0 + b_1 e_i^T$ and that expected legal costs are given by the product of the legal costs and the probability of legal action. We simulate percent increases in the parameter b_1, which in practice would map to increases in the percentage of resources stolen which must be paid when one is caught. We find similar, substantial effects for mayors in both terms: increasing the legal cost on rents extracted by 10 percentage points reduces average rents by 9.8 percent for first-term mayors and by 9.7 percent for second-term mayors.

Given the importance of the media in disseminating information and the large spillover effects we document in Section 3.5, a third policy prescription we study is a change in access to information about neighboring audits. We simulate the model under the assumption that every municipality has access to information from its neighbors–equivalently, we simulate the model under the assumption that every municipality has access to local radio which reports on neighboring audits. We find that on average, first-term rents are 2.39 percent lower and second-term rents are 1.31 percent lower under this counterfactual setting. The comparatively modest effects stem from the fact that with more access to information, mayors and voters are more likely to update their priors about the audit risk in both directions. Thus, some agents will acquire additional information which leads them to overestimate the expected costs of corruption, while others will acquire information which leads to underestimation. Unless agents have biased priors or do not update their beliefs using Bayes' rule, the effects of this policy on rents will be comparatively minor compared to the first two policy counterfactuals we considered.

Another mechanism which has garnered much attention is political entry. We consider whether significant gains could be made in curbing corruption by instituting formal requirements to run for office. The following counterfactuals are at best suggestive as mayor characteristics may capture unobserved heterogeneity in the estimation, in which case our results are likely upper bounds for the true effect sizes. We find modest effects however. Requiring mayors to have a college degree only decreases average rents by 1 percent, whereas requiring mayors to have previously been employed in a white collar occupation reduces average rents by about 3 percent.

3.7 Discussion

This paper shows that anti-corruption audits can be an effective policy in the fight against corruption. We find that, in the case of Brazil's municipalities, corruption is 8 percent lower in places that have been audited in the past compared to those that had not. Naturally, this estimated impact captures only partial, short-term equilibrium effects. In the presence of spillovers, our estimates are likely to represent underestimates of the true impact, and we provide some evidence of this by showing that corruption is lower in municipalities where a neighbor was audited and local media is present to diffusive the information. We also show that audits increase the legal actions taken against corrupt mayors by increasing the chances of a police crackdown or a conviction in court.

By highlighting how audits can help spur legal sanctions, our findings offer important policy implications. While the existing literature has shown that information obtained through audits can help promote electoral accountability, this channel alone might not be sufficient to reduce corruption in the long run, especially if in response, public officials are able to adjust their electoral strategies or find alternative forms of corruption (Bobonis et al. (2015), Olken and Pande (2012)). A sustainable reduction in corruption may instead require policies aimed at improving the state's capacity to detect and prosecute corrupt politicians (e.g. Besley and Persson (2011)). Our results suggest that channeling resources to anti-corruption agencies who can implement well-executed random audits may be an important step towards this direction.

Although we have emphasized the importance of legal accountability for reducing political corruption, our understanding of how best to improve a country's legal system remains limited, particularly in a context where corruption is endemic. More research is needed to better understand how we can improve the selection of public prosecutors and judges, and the incentives they face to punish corrupt politicians.

3.8 Appendix I: Additional Material

Derivation of Equations in Model Section

We solved for the equilibrium reelection probability, equation (3.8), as follows.

In the main text, we showed that the voter reelects the incumbent if

$$\delta_i \geq -h(X_i) + \beta \left((1-\beta)V(q_i) + e_i^{S*} + \tilde{\varepsilon}_i \right)$$

where $h(X_i) \equiv X_i'\xi - \beta X_i'\alpha$ denotes how much voters value the mayor's characteristics when accounting for their effects on both rents and popularity.

Let $s_i^T \in \{0,1\}$ denote whether the voter observes the rent signal in term T. Suppose $s_i^F = 1$. Then the voter's posterior belief about the mayor's type is $\tilde{\varepsilon}_i = \varepsilon_i + e_i^F - e_i^{F*}$ by equations (3.1) and (3.2). The probability that the voter reelects an incumbent conditional on the mayor's type and his action e_i^F is given by

$$\mathbb{P}\left(R_i = 1 | s_i^F = 1, X_i, \varepsilon_i, e_i^F, q_i\right) = F_D\left(2\mu_D + h(X_i) - \beta[(1-\beta)V(q_i) + e_i^{S*} + \varepsilon_i + e_i^F - e_i^{F*}]\right)$$

Now consider the case $s_i^F = 0$, where the voter does not observe the rent signal. In this case, the voter reelects the mayor with probability

$$\mathbb{P}\left(R_i = 1 | s_i^F = 0, X_i, \varepsilon_i, e_i^F, q_i\right) = F_D\left(2\mu_D + h(X_i) - \beta[(1-\beta)V(q_i) + e_i^{S*}]\right)$$

We then integrate over the probability that the voter receives the signal to obtain the ex-ante probability that the voter chooses to reelect the mayor:

$$\mathbb{P}\left(R_i = 1 | X_i, \varepsilon_i, e_i^F, a_i^F, q_i\right) = F_D\left(2\mu_D + h(X_i) - \beta[(1-\beta)V(q_i) + e_i^{S*} + (\chi_0 + \chi_1 a_i^F)(\varepsilon_i + e_i^F - e_i^{F*})]\right)$$

Hence the equilibrium reelection probability follows immediately by setting $e_i^F = e_i^{F*}$:

$$p\left(X_i, \varepsilon_i, a_i^F, q_i\right) = F_D\left(2\mu_D + h(X_i) - \beta[(1-\beta)V(q_i) + e_i^{S*} + (\chi_0 + \chi_1 a_i^F)\varepsilon_i]\right)$$

Details of Equilibrium with Learning

The timing of the game is as follows; (1) Given the state of the world ω_i, and his characteristics X_i, the first-term incumbent chooses his effort level; (2) his ability draw is realized and first-term rents are extracted; (3) the audit draw, the voter's rent signal draw and the incumbent's popularity shock are realized; (4) elections are held; if the incumbent loses, the game continues with step (1) with a randomly drawn first-term mayor and state ω_i', otherwise; (5) the second-term incumbent chooses his effort level, the second-term audit draw is realized and second-term rents are extracted; the game continues with step (1) with a randomly drawn first-term mayor in state ω_i''.

We solve for the pure strategy perfect Bayesian equilibrium in Markov strategies. A strategy for the mayor is a sequence of actions $e_i^T(\omega_i, X_i)$ for each term T conditional on the state ω_i and his observable characteristics X_i. A strategy for the voter is the choice $R_i(\tilde{\varepsilon}_i, \delta_i, \omega_i, X_i)$ of whether to reelect the mayor conditional on his belief about the mayor's type $\tilde{\varepsilon}_i$, the popularity shock δ_i, the state, and the mayor's observable characteristics. Formally, the equilibrium is a sequence of mayor and voter strategies and voter beliefs such that: 1) the mayor's strategy is optimal given the voter's strategy, 2) the voter's strategy is optimal given the mayor's strategy, and 3) the voter's beliefs are consistent with the mayor's strategy on the equilibrium path. The solution concept restricts equilibrium strategies to be stationary in the payoff-relevant state vector.

We begin by considering the equilibrium strategy of the second-term mayor. The second-term mayor faces no reelection incentives and thus only maximizes his expected second-term utility. The first-order condition yields the second-term mayor's equilibrium strategy as a function of his belief $\hat{q}_i$:

$$e^{S*}(\omega_i) = \frac{1 - b_0(\gamma_0 + \gamma_1 \hat{q}_i)}{2b_1(\gamma_0 + \gamma_1 \hat{q}_i)} \tag{3.19}$$

We next consider the voter's equilibrium strategy. Given his belief over the mayor's type, the voter chooses whether or not to reelect the incumbent by considering which option maximizes his expected lifetime utility. In equilibrium, the voter's value function when selecting a random first-term mayor is given by

$$V(\omega_i) = \int v^{F*}(X_i, \varepsilon_i, \delta_i, \omega_i) + \beta \left\{ p(X_i, \varepsilon_i, a_i^F, \omega_i') \left[v^{S*}(X_i, \varepsilon_i, \omega_i') + \beta V(\omega_i'') \right] + \left[1 - p(X_i, \varepsilon_i, a_i^F, \omega_i')\right] V(\omega_i') \right\} d\mathbf{F}$$

where δ_i is the popularity shock, $\mathbf{F}$ is the joint distribution function for the vector $(X_i, \varepsilon_i, \delta_i, a_i^F, y_i, y_i')$, $p(X_i, \varepsilon_i, a_i^F, \omega_i')$ denotes the equilibrium probability of reelection conditional on the mayor's type and the audit draw, and v^{F*} and v^{S*} denote equilibrium per-period voter utilities. We note here that the draws y_i and y_i' will determine the future states ω_i' and ω_i''. Furthermore, the probability of reelection will depend on the state ω_i' rather than ω_i because the voter has observed the audit draw and updated his belief about the audit probability when the election occurs.

Let $\tilde{\varepsilon}_i$ denote the voter's belief about the mayor's type. Conditional on the state ω_i, the voter reelects the incumbent if

$$\delta_i \geq -h(X_i) + \beta \left(V(\omega_i) + e^{S*}(\omega_i) - \beta \mathbb{E}_{y_i|\omega_i} V(\omega_i') + \tilde{\varepsilon}_i \right)$$

where $h(X_i) \equiv X_i'\xi - \beta X_i'\alpha$ denotes how much voters value the mayor's characteristics when accounting for their effects on both rents and popularity. The term $\mathbb{E}_{y_i|\omega_i} V(\omega_i')$ denotes the expected value for V in the following term, which depends on what the state will be (ω_i').

Following the steps analogous to those described in Appendix 3.8, the equilibrium probability that a mayor of type (X_i, ε_i) is reelected conditional on the state ω_i and an audit being drawn is:

$$p\left(X_i, \varepsilon_i, \omega_i, a_i^F = 1\right) = F_D\left(2\mu_D + h(X_i) - \beta \left[V(\omega_i) + e^{S*}(\omega_i) - \beta \mathbb{E}_{y_i|\omega_i} V(\omega_i') + (\chi_0 + \chi_1)\varepsilon_i \right]\right)$$

and conditional on no audit being drawn is:

$$p\left(X_i, \varepsilon_i, \omega_i, a_i^F = 0\right) = F_D\left(2\mu_D + h(X_i) - \beta \left[V(\omega_i) + e^{S*}(\omega_i) - \beta \mathbb{E}_{y_i|\omega_i} V(\omega_i') + \chi_0\varepsilon_i \right]\right)$$

Finally, we solve the maximization problem of the first term mayor. This problem differs from the model without learning because the mayor is not only uncertain about the audit probability in the current term, but is also uncertain (and will update his belief) about the audit probability in the following term. We solve his maximization problem by taking the first-order condition, which yields the equilibrium action as a function of the state ω_i:

$$e^{F*}(X_i, \omega_i) = \frac{1 - b_0(\gamma_0 + \gamma_1\hat{q}_i) - \beta^2\sigma_D \left[(\chi_0 + \chi_1)\hat{q}_i U^{S*}(X_i, \omega_i, a_i^F = 1) + \chi_0(1 - \hat{q}_i) U^{S*}(X_i, \omega_i, a_i^F = 0) \right]}{2b_1(\gamma_0 + \gamma_1\hat{q}_i)}$$

where $U^{S*}(X_i, \omega_i, a_i^F)$ denotes the mayor's equilibrium expected second-term payoff, conditional on his known characteristics, the state in his first term, and whether he is audited in his first term. This value is computed by integrating the mayor's second-term utility over his posterior belief about the audit probability after substituting in (3.19) for his effort level.

Finally, we numerically solve for $V(\omega_i)$ and the equilibrium reelection probabilities.

Model Extension: Electoral Incentives for Term-Limited Mayors

In this section, we extend the model in Section 3.3 so that term-limited mayors also have electoral incentives. To keep the problem tractable, we assume that if the mayor chooses to run for higher office after his second term, the future electorate will only be informed of any signal about the mayor's type from his last term in office. Since we are focusing on the possibility of electoral incentives for second-term mayors, we assume that first-term mayors do not run for higher office if they are not reelected.

We assume that the second-term mayor's per-period utility is

$$u_i^S(e_i^S, X_i, \varepsilon_i, a_i^S) = e_i^S + X_i'\alpha + \varepsilon_i - c(e_i^S, a_i^S) - W\pi\tilde{\varepsilon}_i^S(e_i^S, a_i^S) \tag{3.20}$$

where $\tilde{\varepsilon}_i^S$ denotes the voter's posterior belief about the mayor's type at the end of the term, π denotes the propensity score of the mayor to run for higher office, and W denotes the marginal benefit from having a better reputation after the second term, conditional on running for higher office. The propensity score is obtained from estimating equation (3.18).

Given beliefs about the audit probability implied by ω_i, we maximize (3.20) with respect to e_i^S. This yields the second-term mayor's equilibrium term 2 action:

$$e^{S*}(X_i, \omega_i) = \frac{1 - b_0(\gamma_0 + \gamma_1\hat{q}_i) - W\pi(\chi_0 + \chi_1\hat{q}_i)}{2b_1(\gamma_0 + \gamma_1\hat{q}_i)}$$

The remaining equilibrium strategies are derived as in the original model. The first-term mayor's problem is the same as before, with the exception that we adjust the expected utility from being reelected. Similar adjustments are made for the voter's reelection problem and the value function.

We estimate this model and report the results in the second column of Table 3.18. If second-term mayors place value on completing their mayoral careers with a good reputation, then we would expect W to be positive. Surprisingly, we find a negative and statistically insignificant estimate for W. The remaining parameters estimated are very close to those we obtained without considering this additional channel. Thus, our results suggest that second-term electoral incentives are not a principal channel through which audits reduce future corruption.

3.9 Appendix II: Alternative Model with Learning about Costs

In this section, we modify the model so that mayors and voters learn about the costs of audits instead of the audit probability. In both models, mayors and voters learn about the *expected costs* of the audit program, but the structure of the learning process differs between the two. In the original model considered in Section 3.3, agents update their priors about the audit probability based on the number of observed draws which are audits. In contrast, in the model we consider below, the agent updates his priors about the audit costs based on whether, conditional on observing an audit, the costs drawn are higher or lower than the agent's priors. Although the updating process is different in this model, we find similar results when estimating it.

Setup

In each municipality, in each term, there is a fixed probability q that an audit is drawn, which is known by all players.

There is also a fixed probability that the voter observes the rent signal. If the municipality is audited, the rent signal is drawn from the $Bernoulli(\chi_T)$ distribution, and otherwise it is drawn from the $Bernoulli(\chi_C)$ distribution. Therefore, the probability that the voter in municipality observes the rent signal is

$$\chi_i = \begin{cases} \chi_C & \text{if } a_i = 0 \\ \chi_T & \text{if } a_i = 1 \end{cases} \tag{3.21}$$

where a_i indicates whether the municipality is audited.

The probability that the voter observes the rent signal conditional on no audit, χ_C, is common knowledge. However, the agents are uncertain about the probability of observing the rent signal conditional on an audit. We assume that the prior for χ_T in municipality i is distributed $Beta(\beta_{0i}, \beta_{1i})$.

We now consider how agents update their beliefs in some municipality i. The number of audits observed is $y_i = a_i + \sum_{j \in N_i} a_j$, where a_i is a dummy for the realization of an audit, and N_i denotes the set of neighbors that agents in municipality i observe. Similarly, we denote the number of rent signals observed by z_i. Therefore, the prior for χ_T in the following period will be distributed $Beta(\beta_{0i} + z_i, \beta_{1i} - z_i + y_i)$.

We model learning over the probability of legal action in the same manner. Agents are certain of the probability of legal action under no audit (γ_C), but are uncertain of the probability of legal action under an audit (γ_T). Agents in municipality i have a prior distributed $Beta(\beta_{2i}, \beta_{3i})$ for γ_T. Denoting the number of legal actions observed by agents in municipality i by w_i, it follows that the prior in the next term is distributed $Beta(\beta_{0i} + w_i, \beta_{1i} - w_i + y_i)$. Note that here, the cost of a legal action is given by $b_0 e_i + b_1 e_i^2$.

Equilibrium

The derivation of the equilibrium is very similar to that for the model with learning over the audit probability. We redefine for this section only the state vector for municipality i, $\omega_i := (\beta_{0i}, \beta_{1i}, \beta_{2i}, \beta_{3i}, n_i)$. Through Bayesian updating, this state transitions in the next term to $\omega_i' = (\beta_{0i}', \beta_{1i}', \beta_{2i}', \beta_{3i}', n_i')$, where $\beta_{0i}' = \beta_{0i} + z_i$, $\beta_{1i}' = \beta_{1i} - z_i + y_i$, $\beta_{2i}' = \beta_{2i} + w_i$, $\beta_{3i}' = \beta_{3i} - w_i + y_i$, and $n_i' = n_i$. To keep the notation consistent with previous sections, we let $\gamma_0 = \gamma_C$, $\chi_0 = \chi_C$, $\gamma_1 = \gamma_T - \gamma_C$, and $\chi_1 = \chi_T - \chi_C$.

The timing of the game is as follows; (1) Given the state of the world ω_i, and his characteristics X_i, the first-term incumbent chooses his effort level; (2) his ability draw is realized and first-term rents are extracted; (3) the audit draw, the voter's rent signal draw, the legal action draw and the incumbent's popularity shock are realized; (4) elections are held; if the incumbent loses, the game continues with step (1) with a randomly drawn first-term mayor and state ω_i', otherwise; (5) the second-term incumbent chooses his effort level, the second-term draws are realized and second-term rents are extracted; the game continues with step (1) with a randomly drawn first-term mayor in state ω_i''.

We solve for the pure strategy perfect Bayesian equilibrium in Markov strategies. A strategy for the mayor is a sequence of actions $e_i^T(\omega_i, X_i)$ for each term T conditional on the state ω_i and his observable characteristics X_i. A strategy for the voter is the choice $R_i(\tilde{\varepsilon}_i, \delta_i, \omega_i, X_i)$ of whether to reelect the mayor conditional on his belief about the mayor's type $\tilde{\varepsilon}_i$, the popularity shock δ_i, the state, and the mayor's observable characteristics. Formally, the equilibrium is a sequence of mayor and voter strategies and voter beliefs such that: 1) the mayor's strategy is optimal given the voter's strategy, 2) the voter's strategy is optimal given the mayor's strategy, and 3) the voter's beliefs are consistent with the mayor's strategy on the equilibrium path.

We begin by considering the equilibrium strategy of the second-term mayor. The second-term mayor faces no reelection incentives and thus only maximizes his expected second-term utility. The first-order condition yields the second-term mayor's equilibrium strategy as a function of his belief $\hat{\gamma}_{1i}$:

$$e^{S*}(\omega_i) = \frac{1 - b_0(\gamma_0 + \hat{\gamma}_{1i} q)}{2b_1(\gamma_0 + \hat{\gamma}_{1i} q)} \tag{3.22}$$

We next consider the voter's equilibrium strategy. Given his belief over the mayor's type, the voter chooses whether or not to reelect the incumbent by considering which option maximizes his expected lifetime utility. In equilibrium, the voter's value function when selecting a random first-term mayor is given by

$$\begin{aligned} V(\omega_i) = \int & v^{F*}(X_i, \varepsilon_i, \delta_i; \omega_i) \\ & + \beta \left\{ p(X_i, \varepsilon_i, a_i^F, \omega_i') \left[v^{S*}(X_i, \varepsilon_i, \omega_i') + \beta V(\omega_i'') \right] + \left[1 - p(X_i, \varepsilon_i, a_i^F, \omega_i') \right] V(\omega_i') \right\} d\mathbf{F} \end{aligned} \tag{3.23}$$

where δ_i is the popularity shock, p denotes the equilibrium probability of reelection, v^{F*} and v^{S*} denote equilibrium per-period voter utilities, and $\mathbf{F}$ is the joint distribution function for the vector $(X_i, \varepsilon_i, \delta_i, a_i^F, y_i, y_i', w_i, w_i', z_i, z_i')$. We note here that the draws $(y_i, y_i', w_i, w_i', z_i, z_i')$ will determine the future states ω_i' and ω_i''. Furthermore, the probability of reelection will depend on the state ω_i' rather than ω_i because the voter has observed the audit draw and updated his beliefs about the audit costs when the election occurs.

Let $\tilde{\varepsilon}_i$ denote the voter's belief about the mayor's type. Conditional on the state ω_i, the voter reelects the incumbent if

$$\delta_i \geq -h(X_i) + \beta \left(V(\omega_i) + e^{S*}(\omega_i) - \beta \mathbb{E}_{\mathbf{y}_i|\omega_i} V(\omega_i') + \tilde{\varepsilon}_i \right) \tag{3.24}$$

where $h(X_i) := X_i'\xi - \beta X_i'\alpha$, and $\mathbf{y}_i = (y_i, w_i, z_i)$. Following the steps analogous to those described in Appendix 3.8, the equilibrium probability that a mayor of type (X_i, ε_i) is reelected conditional on an audit being drawn in the state ω_i is:

$$p\left(X_i, \varepsilon_i, \omega_i, a_i^F = 1\right) = F_D \left(2\mu_D + h(X_i) - \beta \left[V(\omega_i) + e^{S*}(\omega_i) - \beta \mathbb{E}_{\mathbf{y}_i|\omega_i} V(\omega_i') + (\chi_0 + \hat{\chi}_1)\varepsilon_i \right] \right) \tag{3.25}$$

and conditional on no audit being drawn is:

$$p\left(X_i,\varepsilon_i,\omega_i,a_i^F=0\right)=F_D\left(2\mu_D+h(X_i)-\beta\left[V(\omega_i)+e^{S*}(\omega_i)-\beta\mathbb{E}_{\mathbf{y}_i|\omega_i}V(\omega_i')+\chi_0\varepsilon_i\right]\right) \tag{3.26}$$

Finally, we solve the maximization problem of the first term mayor. We solve his maximization problem by taking the first-order condition, which yields the equilibrium action as a function of the state ω_i and characteristics X_i:

$$e^{F*}(X_i,\omega_i)=$$
$$\frac{1-b_0(\gamma_0+\hat{\gamma}_{1i}q)-\beta^2\sigma_D\left[(\chi_0+\hat{\chi}_{1i})qU^{S*}(X_i,\omega_i,a_i^F=1)+\chi_0(1-q)U^{S*}(X_i,\omega_i,a_i^F=0)\right]}{2b_1(\gamma_0+\hat{\gamma}_{1i}q)}$$

where $U^{S*}(X_i,\omega_i,a_i^F)$ denotes the mayor's equilibrium expected second-term payoff, conditional on his known characteristics, the state in his first term, and whether he is audited in his first term.

Finally, we solve for $V(\omega_i)$ and the equilibrium reelection probabilities with the approximation that $V(\omega_i)=\mathbb{E}_{\mathbf{y}_i|\omega_i}V(\omega_i')$.

Estimation

This setup introduces eight parameters which govern learning, $\beta_0,\beta_1,\beta_2,\beta_3,\chi_0,\chi_1,\gamma_0$, and γ_1, in lieu of the six parameters estimated in the original model with learning about the audit probability. Also, notably, the econometrician does not observe the realizations $\{z_{it},w_{it}\}_{t=1}^3$. We address the two issues as follows.

In order to estimate the eight parameters, we make the following assumption. We fix β_0 so that the mean of the prior for χ_T is given by χ_C, and fix β_2 so that the mean of the prior for γ_T is given by γ_C. Thus, we assume that since mayors (and voters) are uncertain about the new audit program, they start with the prior that in expectation, the costs of rent extraction are the same whether an audit occurs or not. As they observe more audits, their beliefs about the costs of audits will converge through Bayesian updating to the true costs. Therefore, we estimate only the learning parameters $\{\beta_1,\beta_3,\chi_0,\chi_1,\gamma_0,\gamma_1\}$. In this setup, β_1 and β_3 identify the strength of the priors (respectively, $\beta_0+\beta_1$ and $\beta_2+\beta_3$), whereas the remaining parameters will capture the electoral and non-electoral costs of rent extraction.

The second econometric issue is that we do not observe the realizations of rent signals and legal actions. In words, we do not know whether in municipality i in period t, voters have observed the rent signal, or a legal action has taken place against the mayor. To address this, for each observation in the data, we sum over the likelihood of the observation given each possible history of rent signals and legal actions leading up to that point.

More precisely, let $\pi(\omega_i|m_i)$ denote the probability that the state in a municipality is ω_i, conditional on the total number of observed audit draws as of the current mayor's term being m_i. This number is given by the data, $m_i:=\sum_{s=1}^{t-1}y_{is}$. Then we can write the likelihood function as:

$$\prod_i\sum_{\omega_i}L(\theta|r_i,R_i,X_i,Z_i,a_i^F,T_i,\omega_i)\pi(\omega_i|m_i) \tag{3.27}$$

where, given the independence of the two cost shocks,

$$\pi(\omega_i|m_i) = \pi_0(\beta_{0i}, \beta_{1i}|m_i)\pi_1(\beta_{2i}, \beta_{3i}|m_i) \tag{3.28}$$

and

$$\pi_0(\beta_0 + z, \beta_1 - z + m_i|m_i) = \binom{m_i}{z}(\gamma_T)^z(1-\gamma_T)^{m_i - z} \quad \text{for } z = 0, \ldots, m_i \tag{3.29}$$

$$\pi_1(\beta_2 + w, \beta_3 - w + m_i|m_i) = \binom{m_i}{w}(\chi_T)^w(1-\chi_T)^{m_i - w} \quad \text{for } w = 0, \ldots, m_i \tag{3.30}$$

and $\pi_0 = 0$ for any other combinations of (β_{0i}, β_{1i}), and likewise $\pi_1 = 0$ for any other combinations of (β_{2i}, β_{3i}). Here, the fact that we have use the Beta-Binomial functional form for beliefs reduces the computational burden of calculating the likelihood function. If $m_i = 0$, the we have instead $\pi(\beta_0, \beta_1, \beta_2, \beta_3, n_i) = 1$, i.e. ω_i is set to the prior described above.

Results

Table 3.20 presents the results of the structural estimation. Column 1 reports the results of the present estimation, where mayors and voters learn about costs (the "cost model"). We compare these results to those obtained in the main text, which we replicate in column 2 (the "probability model"). The point estimates for the parameters are very similar for the two models. The main difference is in the estimate for γ_1, the added probability of a legal action when there is an audit, which is larger in the cost model. This difference is due to the fact that we separately estimate the strength of the prior for the legal action probability and the prior for the voter's rent signal probability in the cost model. It turns out that the estimated strength of the prior for the legal action ($\hat{\beta}_2 + \hat{\beta}_3 = 38.3$) is larger than that for the voter's rent signal ($\hat{\beta}_0 + \hat{\beta}_1 = 6.2$), which translates into a larger estimate for γ_1 to compensate for each audit causing relatively smaller shifts in the beliefs about legal actions.

Despite the minor differences in the parameter estimates, both models suggest a similar breakdown of the effects of audits between the three channels. The bulk of the reduction in rents is due to discipline in both models, with legal discipline accounting for 82 percent of the share in the cost model compared to 72 percent in the probability model. In both frameworks, electoral discipline is the second most important in channel, accounting for respectively 18 and 28 percent of the reduction in rents. Finally, selection plays a minimal role in both models.

Unfortunately, the cost model is ill-suited to quantify the effects of a change in the audit probability on absolute levels of corruption. The setup allows for the comparison of rents between municipalities with different audit histories, but it cannot address the extent to which corruption is lower across all municipalities because of the audit program itself. Thus, we cannot compare the two modeling frameworks with respect to their predictions about the effects of counterfactual audit probabilities.

We conclude this section by comparing how the models fit the sample and out-of-sample data. First, both models fit the sample data similarly. The log-likelihood of the cost model is -679.31, compared to -682.01 for the probability model. Using the Vuong closeness test, we cannot reject the null that the two models are equally close to the true data generating process (p-value $= 0.423$).

In terms of out-of-sample fit, the two models also have similar performances. Using our estimates from the cost model, we compute the predicted rents for the additional 239 observations from the most recent audits. The mean squared deviation between predicted and observed rents is 0.139, compared to 0.140 for the probability model. The absolute squared deviation is 0.280 for both models. Hence, both models perform very similarly in fitting out-of-sample corruption.

3.10 Figures and Tables

Table 3.1: Probability of Being Audited

	Lottery	Year	Term
Alagoas	1.4	4.9	14.7
Bahia	1.1	4.3	12.5
Ceará	1.6	5.5	16.3
Espírito Santo	1.3	5.3	14.7
Goiás	1.1	4.5	11.8
Maranhão	1.1	4.0	12.0
Minas Gerais	0.8	3.1	8.6
Mato Grosso do Sul	1.6	6.4	17.2
Mato Grosso	1.3	5.2	13.6
North	1.7	6.3	16.3
Paraíba	1.1	3.9	11.6
Pernambuco	1.4	4.7	14.6
Piauí	1.1	4.1	11.8
Paraná	0.8	3.4	9.2
Rio de Janeiro	2.3	11.5	26.4
Rio Grande do Norte	1.5	5.2	16.1
Rio Grande do Sul	0.9	3.5	9.7
Santa Catarina	0.8	3.7	9.6
Sergipe	1.8	5.7	17.2
São Paulo	0.8	3.2	9.1

Notes: This table shows the share of audited municipalities by state for a given time period, for the full duration of the program (from 2003 to 2015). Column 1 is the probability of being audited in a given lottery. Column 2 is the probability of being audited in a given year. Column 3 is the probability of being audited in a given term.

Table 3.2: Mean Comparisons Between Audited and Non-audited

	Control		Treatment		
	Mean (1)	Std Dev. (2)	Mean (3)	Std Dev. (4)	Difference (5)
Population	22992.72	45069.94	26000.85	43799.66	436.7
					[3153.1]
Share female	0.495	0.015	0.496	0.014	0.000
					[0.001]
Share urban	0.574	0.235	0.576	0.234	0.008
					[0.016]
Human Development Index	0.507	0.105	0.492	0.101	-0.002
					[0.005]
Income inequality (Gini)	0.550	0.068	0.563	0.069	0.003
					[0.005]
Income per capita (log)	5.575	0.580	5.499	0.582	-0.001
					[0.026]
Share poor	0.445	0.229	0.486	0.215	0.005
					[0.009]
Share illiterate	0.247	0.136	0.268	0.134	0.003
					[0.006]
Share of bureaucracy with a college degree	0.192	0.123	0.180	0.118	-0.007
					[0.008]
Share of population with a college degree	0.021	0.021	0.020	0.023	0.001
					[0.001]
Has AM radio	0.211	0.408	0.243	0.430	0.017
					[0.032]
Has a Judiciary District	0.447	0.497	0.523	0.501	0.002
					[0.037]
Effective Number of Candidates for Mayor	2.150	0.550	2.204	0.648	0.044
					[0.047]
Reelection rates for mayors	0.405	0.491	0.437	0.497	0.026
					[0.040]
Mayor’s years of education	12.009	4.194	11.868	4.355	-0.229
					[0.341]
Share of votes mayor received	0.561	0.125	0.564	0.133	0.006
					[0.010]
Number of Service Orders	25.205	9.264	24.802	9.983	-0.169
					[0.610]
N	881		222		

Notes: This table shows means and standard deviations of various municipal characteristics by places that have been audited in the past (treatment) and places that have not been audited in the past (control). The difference and corresponding standard error (in brackets) are computed based on a regression that controls for both state and lottery fixed effects. All of these characteristics are based on information collected in 2000, except for the share of the bureaucracy with a college degree, which is based on a 2005 survey.

Table 3.3: The Effects of the Audits on Corruption and Mismanagement

	Number of Irregularities			Acts of Mismanagement			Acts of Corruption		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Audited in the past	-0.034	-0.045*	-0.058*	0.010	0.001	-0.023	-0.059*	-0.070*	-0.079*
	[0.021]	[0.021]	[0.021]	[0.040]	[0.040]	[0.042]	[0.028]	[0.027]	[0.028]
	(0.09)	(0.03)	(0.01)	(0.60)	(0.52)	(0.30)	(0.03)	(0.03)	(0.01)
Population (log)		0.057*	0.064*		0.047*	0.037+		0.053*	0.064*
		[0.011]	[0.011]		[0.020]	[0.022]		[0.014]	[0.015]
Income inequality (Gini)		0.337*	0.361*		0.137	0.177		0.449*	0.459*
		[0.140]	[0.138]		[0.272]	[0.276]		[0.185]	[0.188]
Income per capita (log)		-0.085*	-0.102*		0.111	0.103		-0.158*	-0.176*
		[0.041]	[0.042]		[0.076]	[0.079]		[0.052]	[0.054]
Illiteracy		0.003+	0.003+		0.001	0.000		0.004*	0.005*
		[0.002]	[0.002]		[0.003]	[0.003]		[0.002]	[0.002]
Share of urban population		0.123*	0.118*		-0.056	-0.068		0.190*	0.182*
		[0.050]	[0.050]		[0.109]	[0.113]		[0.072]	[0.072]
Controls	N	Y	Y	N	Y	Y	N	Y	Y
f(Service Orders)	log	log	nonpar	log	log	nonpar	log	log	nonpar
R^2	0.655	0.675	0.704	0.472	0.480	0.509	0.597	0.616	0.644
N	983	983	983	982	982	982	983	983	983

Notes: This table reports the effects of being audited in the past on corruption and mismanagement. The dependent variable in columns 1-3 is the log of the total number of irregularities discovered in the audit. In columns 4-6, the dependent variable is the log of total acts of mismanagement, and in columns 7-9 the dependent variable is the log of total acts of corruption. In addition to the controls presented in the table, each regression controls for state and lottery fixed effects. In columns 3, 6, 9 the number of service items audited is controlled for in a fully nonparametric fashion by including a vector of indicators for each possible number. In the other columns, we control for the log of the number of service items audited. P-values based on randomization inference reported in the parentheses. The p-values were computed based on 1,000 random draws. Robust standard errors are reported in brackets, + p<0.10, * p<0.05.

Table 3.4: Effects of the Audits on Corruption and Mismanagement - Negative Binomial

	Number of Irregularities			Acts of Mismanagement			Acts of Corruption		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Audited in the past	-0.031	-0.043*	-0.057*	0.015	0.006	-0.019	-0.060*	-0.071*	-0.081*
	[0.020]	[0.020]	[0.019]	[0.033]	[0.033]	[0.033]	[0.026]	[0.025]	[0.024]
Controls	N	Y	Y	N	Y	Y	N	Y	Y
f(Service Orders)	log	log	nonpar	log	log	nonpar	log	log	nonpar
N	983	983	983	983	983	983	983	983	983

Notes: This table reports the effects of being audited in the past on corruption and mismanagement, using a negative binomial regression model. The dependent variable in columns 1-3 is the total number of irregularities discovered in the audit. In columns 4-6, the dependent variable is the total acts of mismanagement, and in columns 7-9 the dependent variable is the total acts of corruption. In addition to municipal controls, each regression controls for state and lottery fixed effects. In columns 3, 6, 9 the number of service items audited is controlled for in a fully nonparametric fashion. In the other columns, we control for the log of the number of service items audited. Robust standard errors are reported in brackets, + $p<0.10$, * $p<0.05$.

Table 3.5: Spillover Effects of Neighboring Audits on Acts of Corruption

	Acts of Corruption				
	(1)	(2)	(3)	(4)	(5)
Audited in the past	-0.078*	-0.081*	-0.086*	-0.093*	-0.094*
	[0.028]	[0.028]	[0.028]	[0.028]	[0.028]
Neighbors Audited	-0.020	0.003	0.010	0.006	0.098
	[0.015]	[0.016]	[0.016]	[0.016]	[0.162]
AM Radio		0.065	0.050	0.044	0.065
		[0.046]	[0.046]	[0.046]	[0.046]
Neighbors Audited × AM Radio		-0.075*	-0.050+	-0.052+	-0.073*
		[0.028]	[0.030]	[0.030]	[0.034]
TV			0.012	0.013	0.032
			[0.054]	[0.055]	[0.055]
Neighbors Audited × TV			-0.083*	-0.081*	-0.094*
			[0.036]	[0.036]	[0.038]
Same Party Audited				-0.005*	-0.005*
				[0.002]	[0.002]
Full Set of Interactions	N	N	N	N	Y
R^2	0.65	0.65	0.65	0.67	0.67
N	983	983	983	983	983

Notes: This table reports the indirect effects on corruption of one's neighbor or one's political party being audited. The dependent variable is the log of the total acts of corruption discovered in the audit. The independent variable ”Same Party Audited” is the number of times in a given term a mayor from the same party and from within the same state was audited. In addition to the municipal controls presented in Table 3.3, each regression controls the following set of fixed effects: state, lottery, service order, number of neighbors, and political party (for columns 4 and 5). In column 5, we interact Neighbors Audited with the full set of municipal controls. Robust standard errors are reported in brackets, + p<0.10, * p<0.05.

Table 3.6: The Effects of the Audits on Corruption and Mismanagement By Local Characteristics

	Acts of Mismanagement						Acts of Corruption					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Audited in the past	-0.023	-0.022	-0.024	-0.022	-0.029	-0.032	-0.079*	-0.080*	-0.086*	-0.081*	-0.076*	-0.079*
	[0.042]	[0.042]	[0.043]	[0.042]	[0.042]	[0.044]	[0.028]	[0.028]	[0.029]	[0.028]	[0.028]	[0.029]
Treatment Interacted with												
Radio	-0.036					-0.067	-0.058					-0.006
	[0.093]					[0.113]	[0.070]					[0.079]
% pop with a college degree		0.001				0.004		-0.017				-0.009
		[0.017]				[0.028]		[0.014]				[0.021]
Income per capita (log)			-0.009			-0.059			-0.063			-0.001
			[0.072]			[0.110]			[0.049]			[0.070]
Share of urban population				0.093		0.236				-0.166		-0.131
				[0.176]		[0.245]				[0.116]		[0.147]
Judiciary District					0.016	0.018					-0.023	-0.003
					[0.081]	[0.088]					[0.052]	[0.056]
P-value (Joint test)						0.87						0.46
R^2	0.510	0.510	0.509	0.510	0.517	0.518	0.645	0.646	0.645	0.645	0.647	0.650
N	982	982	982	982	974	974	983	983	983	983	975	975

Notes: In columns 1-6, the dependent variable is the log of total acts of mismanagement, and in columns 7-12 the dependent variable is the log of total acts of corruption. In addition to the interaction terms presented in the table, each regression controls for the direct effect, the controls presented in 3.3, as well as state, lottery, service order fixed effects. Robust standard errors are reported in brackets, + p<0.10, * p<0.05.

Table 3.7: The Effects of the Audits on Legal Actions

	Crackdowns		Convictions		Legal Action		Crackdowns	Convictions	Legal Action
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Audited	0.00139	-0.0000887	0.00443+	0.000195	0.00562*	0.000241			
	[0.001]	[0.001]	[0.002]	[0.003]	[0.003]	[0.003]			
Audit × Judiciary District		0.00325+		0.00933*		0.0119*			
		[0.002]		[0.004]		[0.005]			
Corruption (logs)							0.0369+	0.0601*	0.0882*
							[0.021]	[0.029]	[0.035]
Mismanagement (logs)							-0.0116	-0.00647	-0.0146
							[0.016]	[0.02]	[0.024]
Control group mean	0.003	0.003	0.025	0.025	0.029	0.029	0.047	0.202	0.240
N	70,902	70,902	70,902	70,902	70,902	70,902	982	982	982

Notes: This table investigate the effects of the audits on the occurrence of a legal action. In columns 1, 2 and 7, the dependent variable is whether a police crackdown on political corruption was conducted in the municipality in a given year. In columns 3, 4, and 8, the dependent variable is whether a mayor was prosecuted for corruption in a given year. In columns 5, 6 and 9, the dependent variable is whether a police investigation or a conviction occurred. In columns 1-6, we control for municipality and year fixed effects. Robust standard errors are reported in brackets and are clustered at the municipality-level in columns 1-6. + $p<0.10$, * $p<0.05$.

Table 3.8: Spillover Effects of At Least One Neighboring Audit on Acts of Corruption

	Acts of Corruption				
	(1)	(2)	(3)	(4)	(5)
Audited in the past	-0.078*	-0.079*	-0.081*	-0.089*	-0.089*
	[0.028]	[0.028]	[0.028]	[0.028]	[0.028]
Neighbor Audited Dummy	-0.015	0.016	0.028	0.013	0.048
	[0.029]	[0.031]	[0.031]	[0.031]	[0.300]
Radio AM		0.074	0.045	0.040	0.053
		[0.049]	[0.050]	[0.050]	[0.050]
Neighbor Audited Dummy × Radio AM		-0.158*	-0.094	-0.097	-0.112
		[0.061]	[0.064]	[0.064]	[0.071]
TV			0.069	0.073	0.086
			[0.059]	[0.059]	[0.060]
Neighbor Audited Dummy × TV			-0.270*	-0.274*	-0.292*
			[0.079]	[0.081]	[0.081]
Same Party Audited				-0.005*	-0.005*
				[0.002]	[0.002]
Full Set of Interactions	N	N	N	N	Y
R^2	0.65	0.65	0.65	0.67	0.67
N	983	983	983	983	983

Notes: This table reports the indirect effects on corruption of one's neighbor or one's political party being audited. In these specifications, "Neighbor Audited Dummy" is equal to one if at least one neighbor has been audited. The dependent variable is the log of the total acts of corruption discovered in the audit. The independent variable "Same Party Audited" is the number of times in a given term a mayor from the same party and from within the same state was audited. In addition to the municipal controls presented in Table 3.3, each regression controls the following set of fixed effects: state, lottery, service order, number of neighbors, and political party (for columns 4 and 5). In column 5, we interact Neighbor Audited with the full set of municipal controls. Robust standard errors are reported in brackets, + p<0.10, * p<0.05.

Table 3.9: Probability of Running for a Higher Office

Dependent variable:	Ran for higher office
	(1)
Some Primary School	1.489*
	[0.694]
Primary School	2.247*
	[0.695]
High School	1.980*
	[0.711]
Some College	2.278*
	[0.683]
College	2.714*
	[0.707]
More than College	3.061*
	[0.688]
Male	-0.055
	[0.100]
Vote Share in previous election	-1.205*
	[0.385]
Campaign Spending in last election	0.042*
	[0.008]
Occupation dummies	Y
N	2222

Notes: The dependent variable equals one if the mayor ran again for a higher office and zero otherwise. The regression also controls for 1-digit occupation codes. Campaign spending is measured in R$100,000s. Robust standard errors are reported in brackets, + p<0.10, * p<0.05.

Table 3.10: The Effects of the Audits on Corruption

	Acts of Corruption						
	Same Term			Reelected	Full Sample		Open Seat
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Audited in the past	-0.127*	-0.113+	-0.133*	-0.149*		-0.079**	-0.122
	[0.050]	[0.058]	[0.056]	[0.064]		[0.028]	[0.075]
	(0.060)	(0.133)	(0.057)	(0.01)		(0.01)	(0.22)
Second-term mayor		-0.032					
		[0.032]					
Audited in the past × Second-term mayor		-0.050					
		[0.095]					
Audited in the past × Propensity to seek higher office			-0.025				
			[0.315]				
Propensity to seek higher office			-0.066				
			[0.105]				
Audited one term ago					-0.078*		
					[0.035]		
					(0.056)		
Audited two or more terms ago					-0.074+		
					[0.041]		
					(0.078)		
Number of years since last audit (logs)						-0.011	
						[0.058]	
						(0.32)	
R^2	0.63	0.63	0.63	0.68	0.64	0.64	0.67
N	821	821	821	596	983	983	665

Notes: This table reports the effects of being audited in the past on corruption. The dependent variable is the log of the total acts of corruption discovered in the audit. In addition to the municipal controls presented in 3.3, each regression controls for state, lottery, service order fixed effects. P-values based on randomization inference reported in the parentheses. The p-values were computed based on 1,000 random draws. Robust standard errors are reported in brackets, + $p<0.10$, * $p<0.05$.

Table 3.11: The Effects of the Audits on Entry

				Characteristics of the Candidate Pool				Mayor Characteristics		
	Win Margin	Number of Candidates	Number of Parties	Elementary School	High School	College	Campaign Spending	White Collar	Male	College
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Audited in the past	0.010	-0.002	0.000	-0.024	0.026	0.000	-0.018	-0.009	-0.027	0.030
	[0.018]	[0.029]	[0.028]	[0.025]	[0.030]	[0.029]	[0.071]	[0.044]	[0.027]	[0.046]
R^2	0.07	0.30	0.31	0.15	0.23	0.21	0.65	0.24	0.11	0.12
N	665	684	684	684	684	684	672	662	679	685

Notes: This table reports the effects of being audited on the candidate pool. The dependent variable is specified at the top of each column. The number of candidates, number of parties, and campaign spending are measured in logs. In addition to the municipal controls presented in 3.3, each regression controls election and state fixed effects. The sample is restricted to open-seat elections. Robust standard errors clustered at the municipality level are reported in brackets, + p<0.10, * p<0.05.

Table 3.12: The Effects of the Audits on Displacement

	Full Sample			Same Term			
	Embezzlement (1)	Procurement (2)	Over-invoicing (3)	Embezzlement (4)	Procurement (5)	Over-invoicing (6)	Acts of Corruption (7)
Audited in the past	0.031	0.011	-0.026	-0.132	0.117	-0.050	
	[0.069]	[0.060]	[0.033]	[0.123]	[0.123]	[0.069]	
Share of same sectors audited							0.200
							[0.299]
R^2	0.18	0.28	0.07	0.18	0.27	0.08	0.82
N	983	983	983	821	821	821	217

Notes: This table reports the effects of being audited in the past on type of corruption detected. Embezzlement, Procurement, Over-invoicing correspond to the number of acts of corruption involving these procedures as a share of the total number of corrupt violations. In columns 1-3, the regressions are estimated for the entire sample. In columns 4-6, the treatment is restricted to those mayors that were audited twice in a single term. In addition to the municipal controls presented in 3.3, each regression controls for state, lottery, service order fixed effects. Robust standard errors are reported in brackets, + p<0.10, * p<0.05.

Table 3.13: The Effects of Audits on Public Spending

	High Corruption	Education	Health	Administration	Housing	Welfare	Transportation	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Audited in the past	-0.006	-0.006	-0.006	0.009	0.005	0.000	-0.004	0.001
	[0.008]	[0.006]	[0.005]	[0.008]	[0.005]	[0.003]	[0.003]	[0.004]
R^2	0.537	0.598	0.235	0.204	0.290	0.275	0.502	0.421
N	773	773	773	773	773	773	773	773

Notes: This table reports the effects of being audited in the past on public spending. Public spending data are obtained from the IPEA. The dependent variable in columns 2-8 is the share of public spending on one of seven mutually exclusive categories: education, health, administration, housing, welfare, transportation, and other spending. In addition to the municipal controls presented in 3.3, each regression controls for state, lottery and service order fixed effects. The sample size is less than 983 due to missing data on public spending, in particular for audits which occurred in 2012 as the public spending data ends in 2011. Robust standard errors are reported in brackets, + p<0.10, * p<0.05.

Table 3.14: The Effects of the Audits on Federal Block Grants

	Number of Block Grants		Amount of Block Grants		Share of Funds Disbursed	
	(1)	(2)	(3)	(4)	(5)	(6)
Audited in the past	-0.015	-0.010	-0.017	-0.324	0.012	0.022
	[0.047]	[0.372]	[0.085]	[0.735]	[0.020]	[0.162]
Corruption (logs)		-0.057		-0.138		0.032
		[0.059]		[0.117]		[0.024]
Corruption (logs) × Audited in the past		-0.004		0.072		-0.005
		[0.092]		[0.178]		[0.040]
R^2	0.60	0.65	0.49	0.52	0.39	0.43
N	794	775	794	775	793	774

Notes: This table reports the effects of being audited in the past on the amount of blocks grants the municipality received in the subsequent years of the administration. The dependent variables in columns 1-4 are expressed in logs. In addition to the municipal controls presented in Table 3.3, each regression controls for state and lottery fixed effects. Columns 2, 4 and 6 also include service order fixed effects. Robust standard errors are reported in brackets, + p<0.10, * p<0.05.

Table 3.15: Structural Estimates of Interest

	Parameter Estimate (1)
Probability of legal action	
constant (γ_0)	0.0245
	[0.0003]
audit (γ_1)	0.0053
	[0.0025]
Probability of voter observing rents	
constant (χ_0)	0.0147
	[0.0076]
audit (χ_1)	0.0877
	[0.0496]
Standard deviation of ability shock (σ_ε)	0.3366
	[0.0075]
Mean of popularity shock (μ_D)	0.0028
	[0.0113]
N	839

Notes: This table reports the maximum likelihood estimates. The first two rows report parameter estimates for the probability of legal action. The constant denotes the probability of legal action conditional on the realization of no audit. The audit coefficient denotes the increase in the probability of legal action when an audit is realized. Rows 3 and 4 report analogous parameter estimates for the probability of the voter observing the rent signal. Log likelihood -682.01.

Table 3.16: Structural Estimates for Mayor Characteristics

	Rents Equation (1)	Reelection Equation (2)
Education	-0.0032	0.0009
	[0.0036]	[0.0027]
White Collar	-0.0520	0.0093
	[0.0300]	[0.0228]
Male	0.0005	0.0246
	[0.0477]	[0.0316]

Notes: This table reports the maximum likelihood estimates on the mayor characteristics for the rents and reelection equations. Education is measured in years of schooling. Both equations also include controls for state, lottery, number of neighbors and number of service orders. Column 1 also controls for municipal characteristics (population, Gini coefficient, GDP per capita, share college educated, share urban).

Table 3.17: Reduction in Rents Due to Audits by Channel

	Average Difference in Rents (1)
Due to:	
Legal discipline	0.138
	[0.067]
Electoral discipline	0.053
	[0.030]
Selection	0.0007
	[0.0004]
Total	0.192
	[0.057]

Notes: This table reports the difference in average rents between those predicted by the maximum likelihood estimates and those predicted by varying counterfactuals. Each row reports the difference for a separate counterfactual, where audits do not affect the respective channel, with the final row denoting the sum of the first three rows. Standard errors are computed using the Delta method.

Table 3.18: Estimates for Extensions Including Electoral Incentives for Term-Limited Mayors

	Parameter Estimates	
	(1)	(2)
Panel A: Rents equation parameters		
Propensity to run for higher office	0.1079	
	[0.4264]	
Propensity to run for higher office × Audit probability prior	-0.1702	
	[3.1160]	
Education	-0.0043	-0.0040
	[0.0039]	[0.0036]
White Collar	-0.0471	-0.0486
	[0.0307]	[0.0300]
Male	0.0025	0.016
	[0.00476]	[0.00477]
Panel B: Other parameters		
Marginal utility from reputation (W)		-0.2113
		[0.1796]
Probability of legal action		
constant (γ_0)	0.0245	0.0245
	[0.0003]	[0.0003]
audit (γ_1)	0.0053	0.0055
	[0.0026]	[0.0025]
Probability of voter observing rents		
constant (χ_0)	0.0144	0.0147
	[0.0077]	[0.0077]
audit (χ_1)	0.0853	0.0851
	[0.0501]	[0.0498]
Standard deviation of ability shock (σ_ε)	0.3364	0.3364
	[0.0076]	[0.0075]
Mean of popularity shock (μ_D)	0.0028	0.0029
	[0.0114]	[0.0113]
Log likelihood	-681.67	-681.28
Observations	839	839

Notes: This table reports the maximum likelihood estimates for two exensions to the baseline structural model which account for potential electoral incentives for term-limited mayors. In the first column, the model includes in the equilibrium effort equations a term for the propensity to run for a higher office and its interaction with the mean of the belief about the audit probability. In the second column, we estimate the model presented in Appendix 3.8, which includes electoral incentives for term-limited mayors through the parameter W.

Table 3.19: Counterfactuals

	Average Decrease in Rents (%)	
	First-term	Second-term
	(1)	(2)
Audit probability		
10 percentage point increase	14.6	9.3
	[3.8]	[4.3]
20 percentage point increase	28.5	18.3
	[7.1]	[8.3]
Legal cost (b_1)		
10 percentage point increase	9.8	9.7
	[0.1]	[0.1]
20 percentage point increase	19.1	18.9
	[0.2]	[0.1]
Local radio access to neighbors		
All municipalities have radio	2.39	1.31
	[0.61]	[0.60]
Mayor characteristics		
All mayors college educated	1.08	1.09
	[1.22]	[1.23]
All mayors white collar	3.48	3.25
	[1.99]	[1.87]

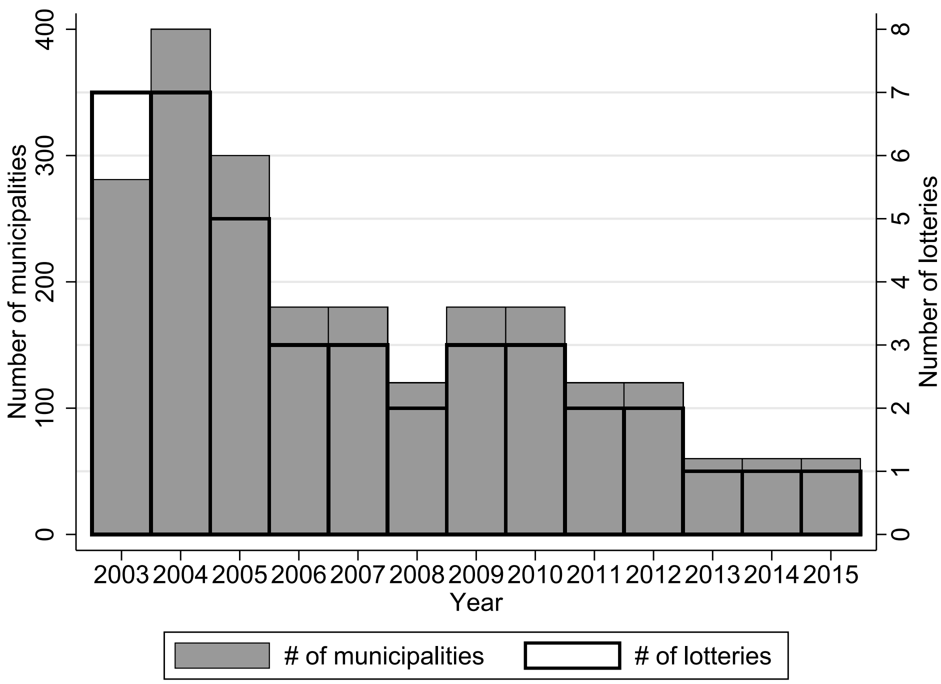
Notes: This table reports the difference in average predicted rents between the maximum likelihood estimates and the following set of policy counterfactuals. The first set increases the audit probability of all municipalities. The second increases the legal cost parameter b_1 associated with rent extraction. The third sets all municipalities to have access to information about neighboring audits. The fourth alters the characteristics of incumbent mayors. Standard errors are computed using the Delta method.

Table 3.20: Comparison of Learning Models

	Audit Cost Model (1)	Audit Probability Model (2)
Panel A: Parameter estimates		
Probability of legal action		
constant (γ_0)	0.0251	0.0245
	[0.0001]	[0.0003]
audit (γ_1)	0.0596	0.0053
	[0.0349]	[0.0025]
Probability of voter observing rents		
constant (χ_0)	0.0034	0.0147
	[0.0037]	[0.0076]
audit (χ_1)	0.0890	0.0877
	[0.0511]	[0.0496]
Standard deviation of ability shock (σ_ε)	0.3231	0.3366
	[0.0077]	[0.0075]
Mean of popularity shock (μ_D)	-0.0014	0.0028
	[0.0116]	[0.0113]
Panel B: Effects of audits on equilibrium rents, by channel (%)		
Legal discipline	82.1	71.9
	[15.0]	[18.5]
Electoral discipline	17.7	27.8
	[12.8]	[18.3]
Selection	0.3	0.4
	[5.0]	[0.2]
Log likelihood	-679.31	-682.01
Observations	839	839

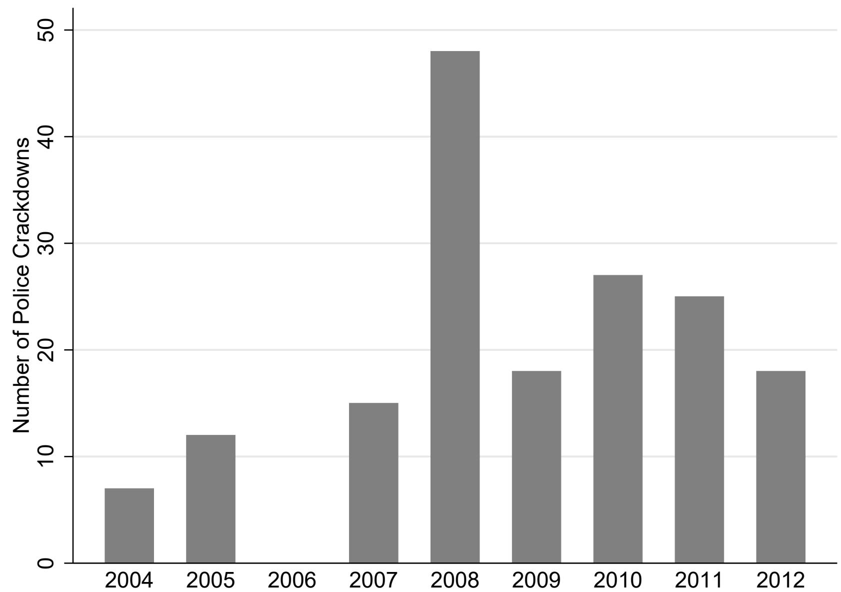
Notes: This table reports the maximum likelihood estimates for the model with learning about audit costs in column 1 and the model with learning about the audit probability in column 2. Panel A reports parameter estimates of interest. Panel B reports the percent decrease in rents due to each of the three respective channels.

Figure 3.1: Number of Lotteries and Municipalities Audited Per Year

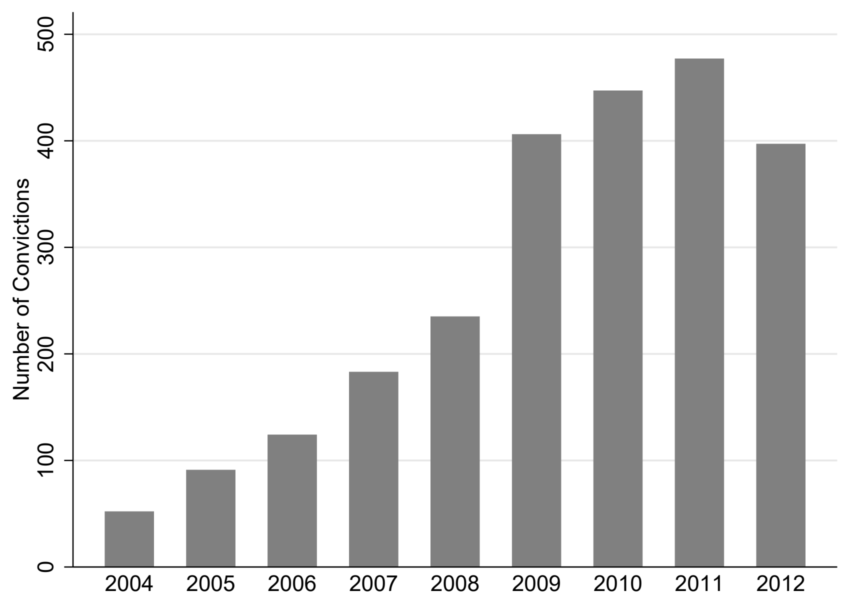


Notes: This figure plots the number of lotteries and the number of municipalities that have been audited for the full duration of the program (from 2003 to 2015).

Figure 3.2: Number of Legal Actions over Time



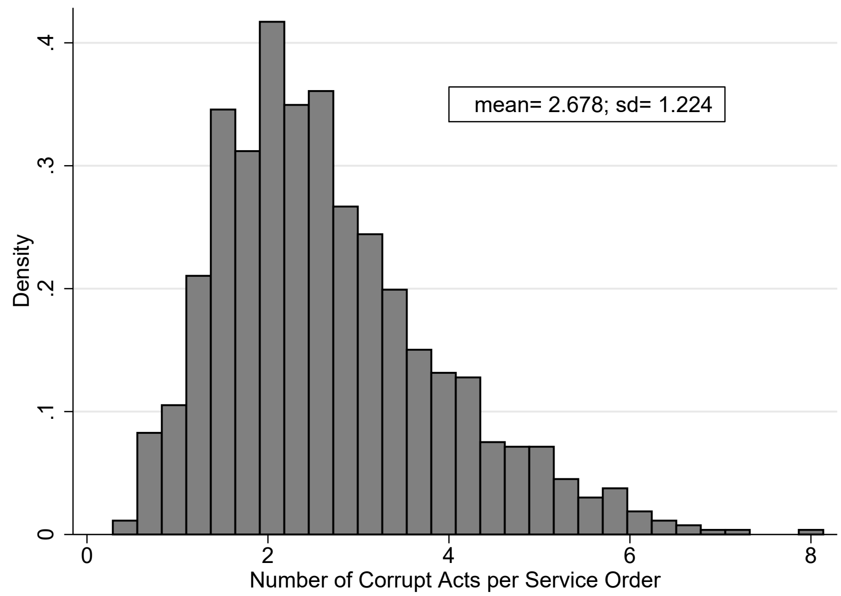
Panel A: Police Crackdowns



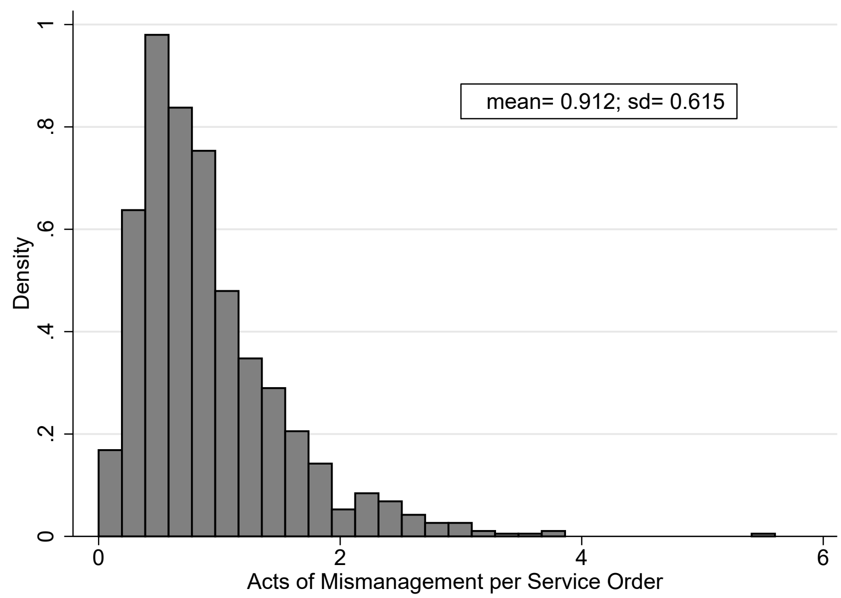
Panel B: Convictions

Notes: This figure plots the number of police crackdowns and convictions involving political corruption during the period 2004 to 2012.

Figure 3.3: Distribution of Irregularities Associated with Corruption and Mismanagement



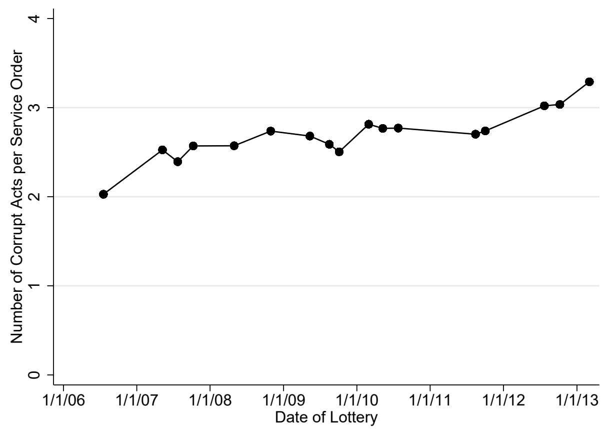
Panel A: Corruption



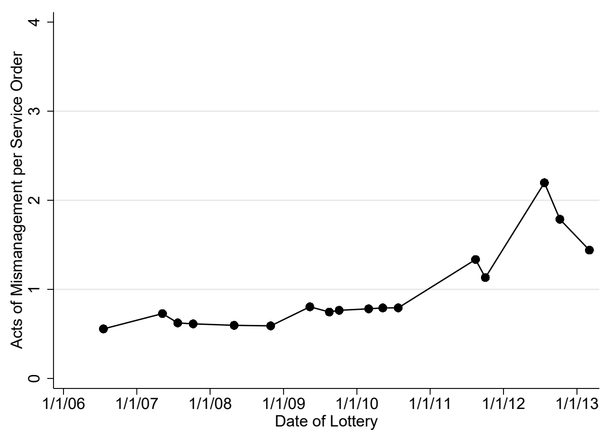
Panel B: Mismanagement

Notes: This figure displays the distribution of irregularities per service order associated with corruption and mismanagement. These data are based on the audits conducted in our estimation sample, from July 2006 to March 2013.

Figure 3.4: Average Number of Irregularities By Lottery



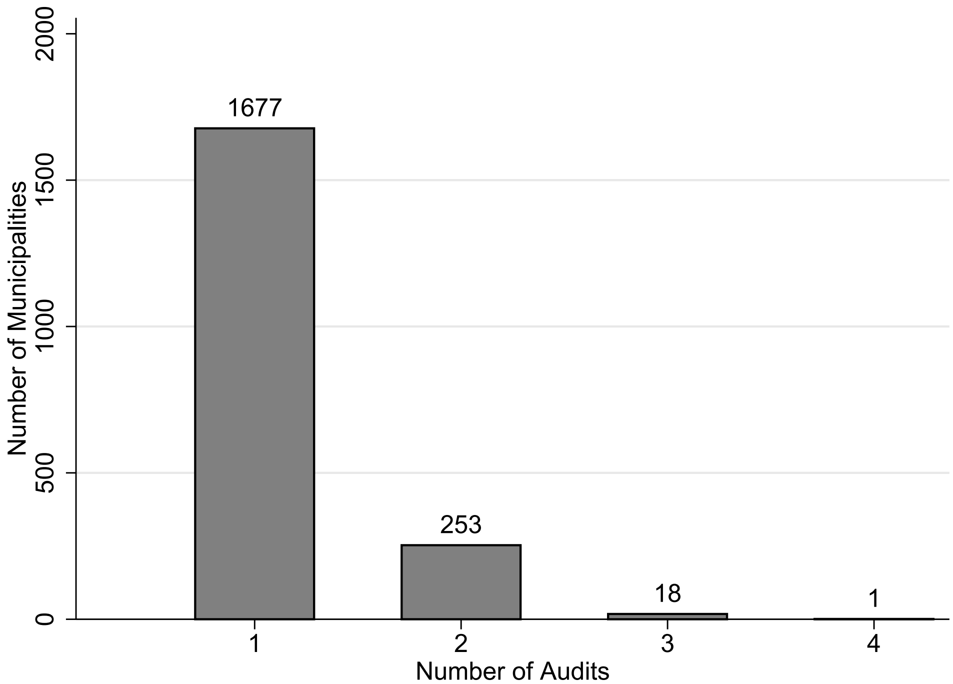
Corruption



Mismanagement

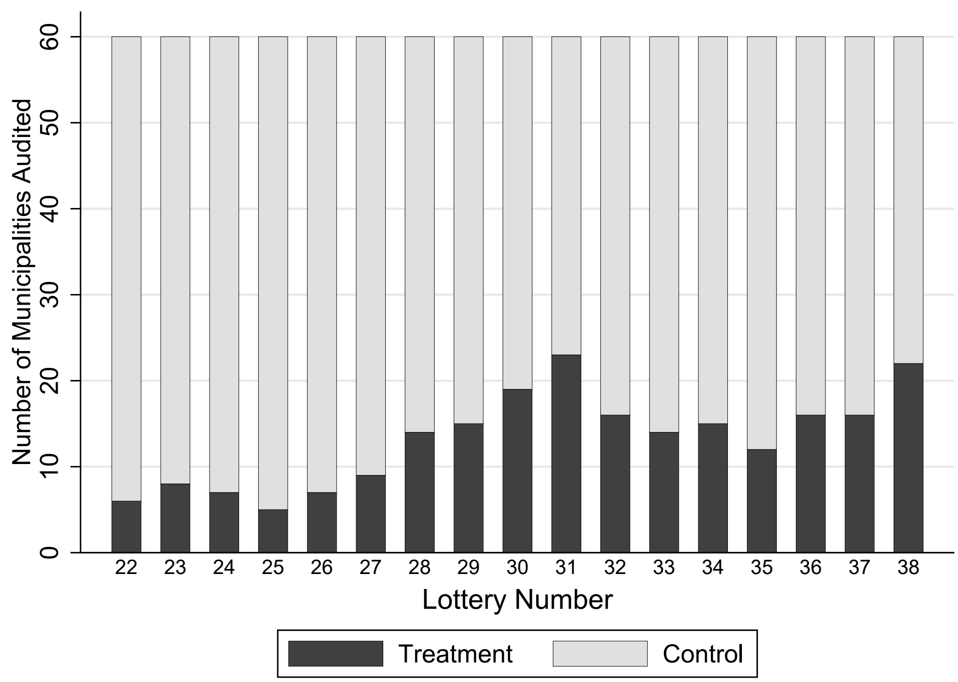
Notes: This figure displays by lottery the average number of irregularities per service order associated with corruption or mismanagement. These data are based on the audits conducted in our estimation sample, from July 2006 to March 2013.

Figure 3.5: Distribution of Times a Municipality has been Audited



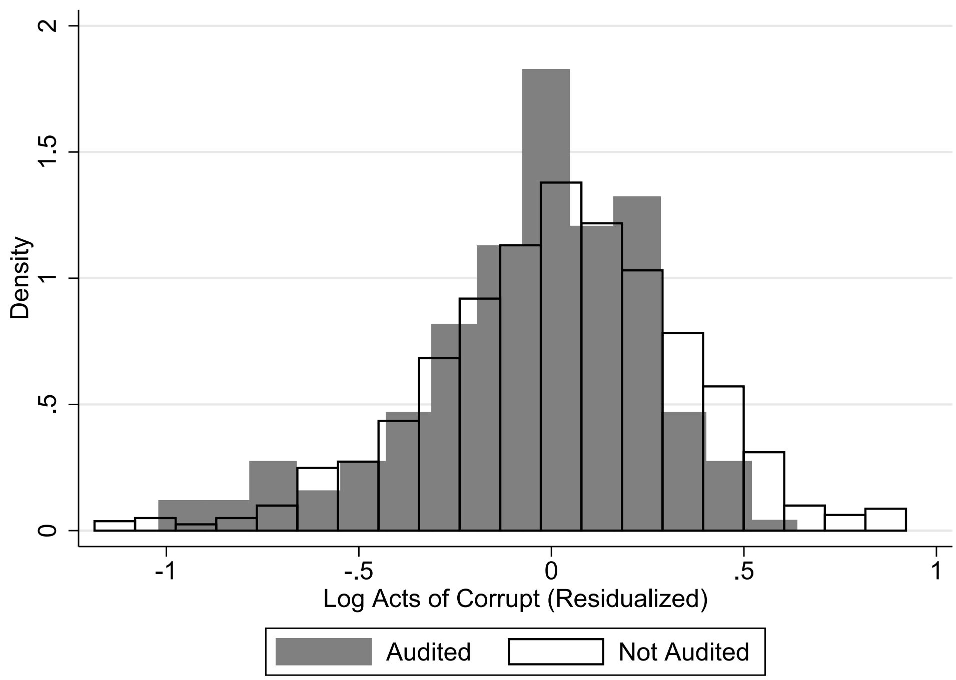
Notes: This figure plots the distribution of the number of times a municipality has been audited for the full duration of the program (from 2003 to 2015).

Figure 3.6: Distribution of Control and Treatment Municipalities Over Time



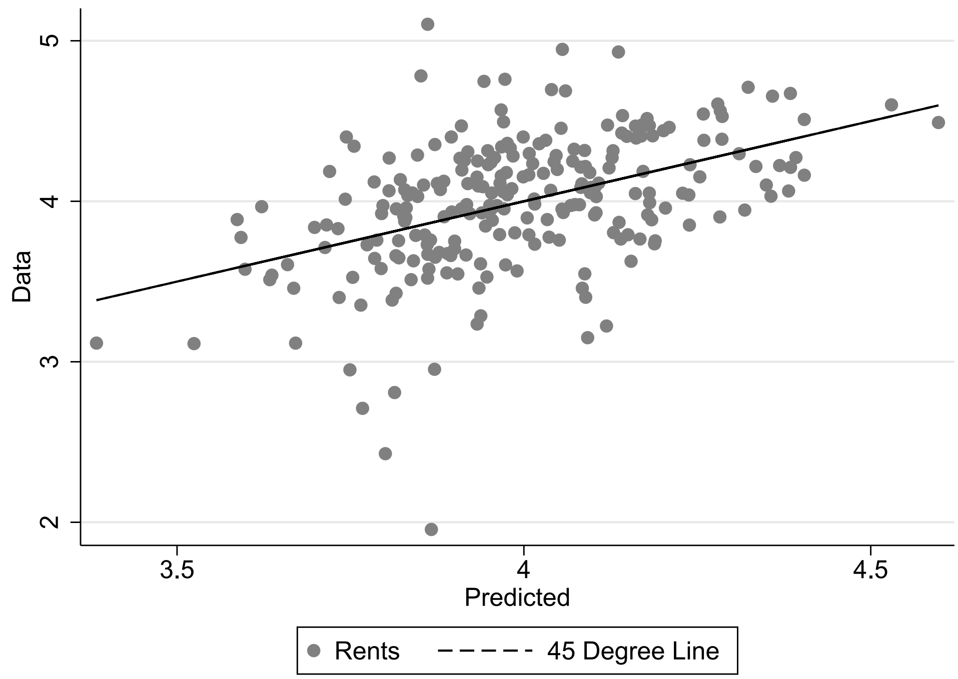
Notes: This figure plots the number of control and treated municipalities for each lottery in our estimation sample. The dark blue bars denote the number of treated municipalities (i.e. previously audited). The light blue bars denote the number of control municipalities (i.e. not previously audited).

Figure 3.7: Effects of the Audits on Corruption



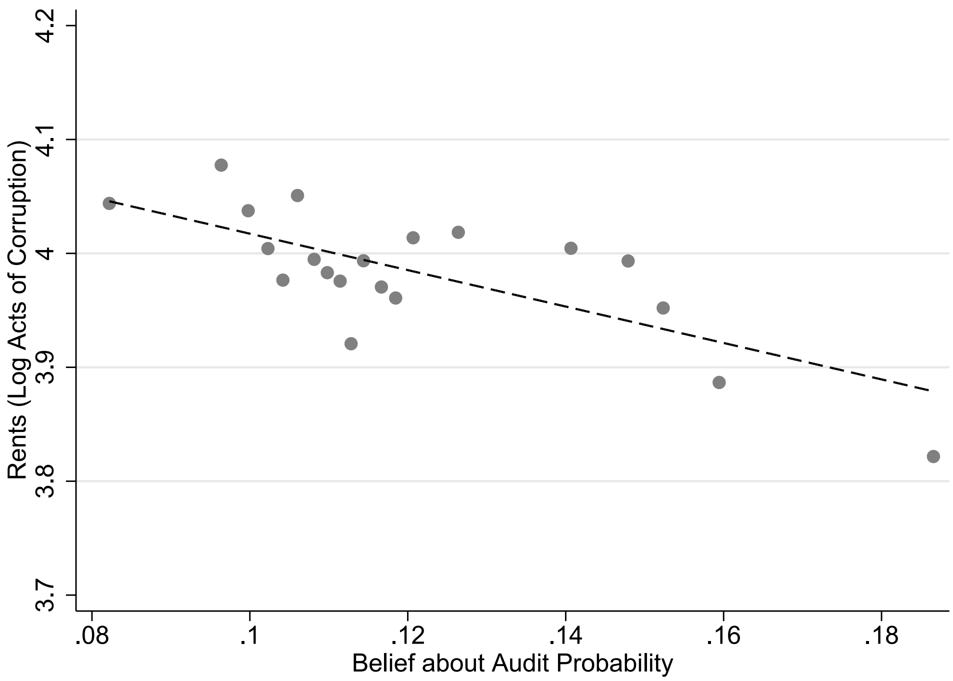
Notes: This figure compares the distribution of corruption between treatment and control municipalities. Specifically, it plots the residuals from two separate OLS regressions (one for treatment municipalities, the other for control municipalities) of log corruption on state, lottery, and service order fixed effects.

Figure 3.8: Out-of-Sample Fit for Rents



Notes: This figure displays predicted and actual rents for 239 audits which occurred in the period following those used in our estimation sample. Predicted rents are computed using the maximum likelihood estimates.

Figure 3.9: Rents and the Belief over the Audit Probability



Notes: This figure displays a residuals binned scatter plot of rents on the mean belief over the audit probability. The regressions control for mayor and municipal characteristics, number of service orders, number of neighbors, lottery and state fixed effects.

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