The London School of Economics and Political Science

The Political Economy of Government Formation and Local Public Goods

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Declaration

I certify that the thesis I have presented for examination for the MPhil/PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

I declare that my thesis consists of about 46300 words.

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Abstract

This thesis examines three questions: first, do national government coalitions favour local governments connected to them to receive local public goods? Secondly, does favouritism in the allocation of public goods imply large welfare losses? Finally, how national governments form, and what are the consequences of this for national policy making? These questions are answered in the particular context of Brazil, where rich data on national politics and local public good allocation is available.

The first chapter of the thesis summarizes aspects of the Brazilian context that are relevant for the rest of the thesis – covering aspects of Brazilian national politics, and of the rules for allocation of funds for local public goods. The chapter also discusses the disaggregated data on the universe of matching grant transfers from the Brazilian national government to municipalities, used in the second and third chapters.

The second chapter answers the following question: are regions connected to the national government favoured to receive funding for local public goods? While a broad literature shows that "politically connected" regions receive more funds from national governments, it is unclear whether this reflects favouritism, or simply connections allowing the national government to know better the needs of regions connected to them. The chapter finds evidence broadly consistent with favouritism.

The third chapter then examines the welfare losses associated with favouritism. I build a model of grant requests by cities and approvals by the national government and provide estimates of the model's parameters. Despite ample evidence of favouritism, if the only source of conflict between the national government and society is due to favouritism, the welfare losses for society due to favouritism are of the order of 0.24% of the budget for grants.

The second and third chapters suggest large effects of the national coalition over local public good provision. The fourth and final chapter, instead, analyses how national coalitions interact with national policies. More precisely, do government coalitions form to include legislators ideologically close to the executive, or ideologically unattached legislators

whose votes are "easier to buy"? Moreover, what are the consequences of this for policy making at the national level – in particular, for roll call votes in the chamber of deputies?

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1. Introduction

This thesis contains three chapters on the effects of connections/association with the national government over policy making in Brazil between 2009-16. The next chapter (chapter 2) covers the Brazilian context. In particular, it covers information on Brazilian national level politics, interactions between national and local politics, and implications for the allocation of public funds across regions. It also covers the data source used for two out of the three following chapters in the thesis.

Chapter 3 then moves to answer two questions. First, do political connections of local politicians - namely, shared partisan identities between local mayors and national politicians - affect the allocation of public goods across regions? Secondly, are regions connected with the national government being favoured to receive public goods, or are connections creating some type of "second best" allocation - by either providing information about local needs to the national government, or means of enforcement?

The chapter provides answers to these questions using data on the universe of grant requests and approvals in Brazil between 2009-16, documenting effects for a broad cross section of public goods without aggregation. Secondly, the setting allows me to have significant variation in connections within a city-year, across the 27 different ministries in charge of approving grant requests for the different types of public goods - for one example, a city might be connected to the ministry of health, which provides hospitals and sanitation; while not being connected to the ministry of urban planning, which provides grants for urban roads and housing. Finally, I can explore exogenous variation in connections arising only from national level changes in the partisan identity of each of the ministers, holding fixed the mayor.

This setting allows me to detect potentially welfare relevant effects of connections that would not be detectable without disaggregated data

on different types of public goods: if all cities were connected to different ministries and connections were relevant, it could well be that all cities get the same amounts of grants. Yet, because of their particular connections to particular ministries, some cities would only get hospitals, and some other cities would only get roads, when it is possible that the additional roads for the second group of cities doesn't substitute for not getting hospitals. By observing disaggregated data, the chapter is able to detect such effects of connections, and by observing a broad cross section of public goods, the chapter is able to present estimates that are representative across many different types of public goods. Similarly, if only some cities get connections to obtain all types of public goods, once again the setting allows for the identification of the effect for each type of public good, which can then be summed up to obtain a total effect over overall expenditures.

The results suggest that connected cities request and get 15% more funds than unconnected cities. These effects are not driven by ideological differences between mayors of different parties: even after controlling for proxies of ideological distance between the mayor and the minister, results remain largely unchanged. Similarly, the results are unlikely to be driven by information effects: even for a subset of grants where allocation is relatively less ideological, and where I can observe the reaction to publicly known shocks to local demands, I show that connected cities still get 17% more funds than unconnected cities, suggesting that differences in information are not driving the effects of connections. This lack of information effects is further confirmed by the fact that this potential information doesn't seem to spillover to bureaucrats analysing the grants in preliminary stages; and that whatever information about mayoral competence is available from past connected ministers, this information doesn't seem to be learned by future ministers. I interpret these results, instead, as consistent with favouritism in the allocation of grants to connected mayors.

Given the evidence in chapter 3 for favouritism in the allocation of grants, how do we compute the aggregate welfare losses associated with favouritism? In particular, do the gains obtained by connected cities compensate for the losses for unconnected cities? Chapter 4 answers this question. The key challenges in answering this question are that, first of all, we often don't observe measures of local welfare obtained from many

types of public goods - consider the relative welfare gains of grants for sports courts in one city versus in another city. Even if we did, and even if we could compute whether grant amounts have decreasing or increasing returns on local welfare, we often don't know the extent to which society has inequality aversion. Finally, we often don't observe society's welfare weights across different regions, and heterogeneous demands for public goods across different regions.

I first provide a model of grant requests and approvals where both municipalities and the national government have separable preferences across different types of public goods.[1] The model is then used to separate between the losses for unconnected cities and gains for connected cities. Based on information about the parameters of this model, the chapter computes the costs of political connections from the viewpoint of a non-partisan ministry. I argue this is a useful welfare criterion, as it serves as a plausible lower bound on the welfare costs of delegating these decisions to the political ministries relative to first best: if this is the right welfare function, the claim is obvious, while if there are even more sources of divergence between the partisan ministers and society, the costs of delegating the decisions to partisan ministries is higher than the cost I compute in the chapter. By doing so, the chapter deals with the lack of knowledge about the welfare effects of grants by computing plausible bounds on the welfare losses associated with partisan ministers.

I show that despite the evidence of favouritism, if the only source of distortion in the allocation of public funds is the one stemming from political connections, the welfare losses from connections based allocations are of the order of 0.24% of the typical ministry's budget, or 1 hospital per year in a country of 5500 cities. The reason for that is that ministers are estimated to behave as if grants had close to linear returns. This implies that the marginal return on the dollar removed from the unconnected city is similar to the marginal return on the dollar given to the connected city, and consequently welfare losses are relatively small. Moreover, this implies that under the assumption of separability of preferences, ministries behave as if different types of public goods are close substitutes.

While chapters 3 and 4 analyse the effects of connections to the na-

[1]Note that this still allows for several patterns of complementarities and substitution, such as Cobb Douglas or CES preferences.

tional government over local provision of public goods, chapter 5 turns to understand how alliances between parties at the national level affect national policies - in particular, roll call votes in Congress. While in western democracies, checks and balances are often viewed as an important mechanism to prevent the executive from unilaterally implementing its will, it is not clear that checks and balances are always effective: the executive can form coalitions with like-minded congressmen only, or rely on congressmen whose votes are easy to change/easy to buy. Chapter 5 then discusses (i) how coalitions are formed - which types of ideological interests it includes, and whether easy to buy votes are included more often in the coalition, (ii) what are the consequences of this for voting in the legislature: in particular, does the legislature effectively check the executive?.

The key challenge in answering this question relies on estimating whether coalitions change votes of congressmen (by buying off votes), or whether coalition members voting together reflects only sorting on ideology - that is, whether the coalition is formed by including congressmen that think alike. This chapter addresses this question by relying of moves of congressmen and parties in and out of the governing coalition, while controlling for an arbitrary dimensional ideological space.

The paper first present a model where the executive forms a coalition with congressmen in the legislature by offering cabinet positions in exchange for more favourable votes in congress. The model suggests that when a substantial share of congressmen are ideologically similar to the executive, the executive prefers to form coalitions with congressmen who are hard to buy off, and instead prefers to form a coalition with congressmen that think alike the executive. In these situations, the coalition has a small effect over congressional votes. When the executive is, on the other hand, ideologically distant from most congressmen, the coalition involves instead easy-to-buy legislators, and the executive then relies on putting pressure on the coalition to change their votes.

The model further suggests a voting equation where congressmen's votes depend on their ideologies and a "coalition pressure" term, with parameters that can be identified from the large number of congressmen, large number of bills voted on, and from switches of parties into and out of the coalition. Moreover, from this voting equation, we can estimate what is each congressmen's sensitivity to pressure (or, how easy is it to

"buy-off" the congressman).

The model estimates that while PT is a political party that is ideologically distant from most parties in congress, PMDB is ideologically similar to a large group of parties (typically referred to as "Centrão"). Consistently with the theoretical model, while PT is in power, the president forms a coalition with congressmen who are sensitive to pressure and are easy-to-buy. When PMDB gets to power, instead, the president forms a coalition with congressmen who are ideologically similar to the president and who are less sensitive to pressure. The model is further validated by the fact that the estimated amounts of pressure are well explained by a linearised first order condition for the executive, which was not used to estimate the voting model. Finally, the estimated model suggests that during the PT years, without coalitional pressure, Congress would have approved 79.5% of the bills the executive pressed against; with coalitional pressure, Congress instead approved 66.7% of these bills. During the PMDB years, on the other hand, coalitional pressure had no effect over votes, and both with and without coalitional pressure, Congress (would have) approved 81.6% of bills the executive pressed against. This suggests that while the executive can undermine checks and balances by "buying off" congressmen's votes in the process of coalition formation, Congress still effectively checks the executive by approving bills against the executive's will in 66.7% of bills the executive presses against.

2. National government and local public goods in Brazil: Context

2.1 Context

2.1.1 The agents

For future reference, it's useful to establish beforehand the active players in the context analysed in the paper. On the national government (also referred to as federal government), ministries in the executive branch are the ones in charge of approving and rejecting grant requests. In the context of this thesis, I consider grants requested by the 5565 municipal governments in Brazil.

2.1.2 National politics

At the national level, the Brazilian government follows a presidential system. The executive branch is headed by a President who is directly elected every four years by a run-off voting system, where, if no presidential candidate gets more than 50% of the national votes in the first round, a run-off election with the two front runners from the first round happens, and the majority winner from the run-off then wins the presidency. Once elected, the president appoints ministers, each in charge of different areas, such as health, urban planning, and so on; among their many tasks, they have substantial power to approve and reject grant requests aimed at financing local public goods in their area - so that the ministry of health has power to approve grant requests for hospitals and sanitation in particular areas, the ministry of urban planning has power to approve grant requests for urban roads and housing, and so on. While

the number of ministries varies over time, for the time-span of this study, we focus on 27 ministries that could legally allocate matching grants to local governments.

As in typical presidential systems, the executive branch faces some checks from the legislative branch, in particular, when approving yearly budgets and other pieces of legislation. The legislature in Brazil is bicameral: the Senate contains three representatives per State serving 8-year long terms. Every four years, on the same day as the first round of the presidential election, either one or two senators are elected by simple plurality, so that the one or two candidates for the senate with largest vote shares get elected in a single election round. Similarly, on the same day as the first round of the presidential election, citizens vote for candidates running for the Chamber of Deputies. Each state elects a number of deputies that is roughly proportional to the state's population[1] for a total of 513 deputies nationwide; and within each state, congressmen are elected according to an open list proportional voting system. That is, in each state, citizens vote on congressional candidates (or on the party). Then, within each state, seats are allocated to parties according to the party vote share at the state level, and within the party seats, the most voted on congressional candidates for that party get allocated seats first. Importantly, due to regulatory constraints, political parties can only campaign on TV and radio by using an allowance of free airtime, where each party's endowment is proportional to its number of seats in the Chamber of Deputies (see law 9504/1997).

This type of electoral system often encourages a large number of parties to be elected to the Chamber of Deputies (see, for instance, Duverger (1954) and Morelli (2004)). Moreover, the regulations on campaigning by political parties imply that the large number of parties in the chamber of deputies get to campaign on TV and radio for all elections, implying a large number of parties also in the Senate. Consistently with this, in 2009, 15 political parties held seats in the Senate and 21 political parties held seats in the Chamber of Deputies in 2009, since then, the number of parties in both chambers has increased. This implies that the executive, effectively, never has a majority in either chamber, and con-

[1]Each state has a number of deputies that is proportional to population, as long as they get at least 8 deputies and at most 70 deputies. Effectively, this implies that northern states with small populations get 8 deputies, the large state of São Paulo gets 70 deputies, and most other states have a number of seats proportional to population.

sequently, needs to build a government coalition - often a super-majority - by allocating different ministries to different parties. This implies, then, that most ministries responsible for grants are headed by ministers affiliated with political parties, often different from the presidents' party. In particular, the median cabinet in my sample has ministries coming from 7 different political parties (for more details on the cross party distribution of ministries, see figure 2.2).

Also consistently with this, most changes in the parties heading ministries happen together with changes in the presidential and congressional terms. In the sample period, new presidential and congressional terms have started in January 2011 and January 2015 - due to elections -, and in May 2016 - due to the impeachment of Dilma Rousseff. Given that these periods come with substantial changes in the composition of Congress and of government coalitions, it can be seen in graph 2.3 that these are also the years that face the largest changes in ministerial party affiliation. In particular, over 60% of the changes in ministerial affiliation came during the years with changes in the presidential term. These years are also the years with most changes in number of ministries moving from one party to another, and with most ministries moving from non-partisan ministers to partisan ones.

2.1.3 Interactions of local and national politics

Given that ministries in the executive power have substantial discretion in deciding whether to approve or reject a grant, political connections with ministries might play a role in allocation of grants in this context. This section reviews the process and timing through which cities might become more or less politically connected with the ministries throughout a given mayoral term.

While presidential changes and most ministerial changes happen in 2011, 2015 and 2016 during this sample period, new mayors start their terms in January 2009 and January 2013. Mayors are also elected from the same political parties that are present in the legislature. Due to this timing of mayoral terms, midway through their terms (typically in the third year), mayors lose and gain "political connections" with ministries - measured as being in the same party of the minister -, and this is only due to changes related to the national level politics.

One possible concern with this source of variation might be that, given

that the estimates happen on the sample of switchers, future connections can substitute for current connections. Table 2.1 displays the share of cities that have been connected for at least one year between 2009-15, 2009-16 and 2003-16 with each of the largest 6 ministries (in terms of budget for grants). Between 97% and 59% of cities were never connected with each of these ministries throughout the 7 years between 2009-15. Including the year of the impeachment, between 79% and 52% of cities are never connected with the largest 6 ministries throughout 8 years. Going further back between 2003-16, between 79% and 43% of cities have remained unconnected with the largest 6 ministries throughout 14 years. For the average ministry, 64% of cities remain unconnected throughout 14 years. This suggests that for a large sample of cities, they do not manage to replace current connections with future connections for time horizons of 14 years.

This might raise the concern that the source of variation is not representative of the full sample of cities. The fact, though, that I have a sample of 27 ministries occupied by a median of 7 parties allows me to cover in the set of switchers a significant share of the cities in the sample. The second to last row of table 2.1 shows the share of cities that are connected with at least one ministry for at least one year. Between 2009-15, 86% of cities had some political connection with at least one ministry. Including the impeachment year, the share of cities having at least one year of connections with at least one ministry rises to 97% of cities.

Given that future connections with a given ministry are not common, connected mayoral candidates often campaign on the promise to bring resources on the basis of their political alignment. In Pelotas, RS, Fernando Marroni, affiliated with PT (the president's party), when asked in an interview during his 2012 campaign about the differences between him and his rival, answered

> *"we represent the real change. A government aligned with the federal government and that can benefit from the resources, projects and programs to bring development to Pelotas."* (Leboutte and Macedo, (2012))

In Rio de Janeiro, RJ, Eduardo Paes, affiliated with PMDB (the main coalition partner during the period), mentioned during a radio interview during his 2008 campaign that

> *"The time of political isolation is over. I have support from the state governor Sérgio Cabral, and I can guarantee to voters that the mayor will also work together with the federal government."* (G1, Rio de Janeiro, (2008))

Mayors also expect their connections to bring in extra attention and projects from the naitonal government. In an interview with Revista Piauí from June 2017, Fernando Haddad, affiliated with PT and São Paulo's mayor between 2013 and 2016, reports on a quick meeting with president Dilma Rousseff the morning after his electoral victory, where he

> *"insinuated to the president that he thought the federal government should treat São Paulo in a singular manner, given the city's importance"* (Haddad (2017))

The report further mentions his expectations for a meeting with President Dilma Rousseff in the beginning of his mayoral term:

> *"My expectation was to have a first meeting with strategic ministries to establish what Brasília* [the federal government] *could do to change São Paulo."* (Haddad (2017))

The reasons behind this expectation are hard to pin down. Brollo and Nannicinni (2012) show evidence that for two-candidate election, victories with small vote margins by connected candidates were associated with higher re-election rates, suggesting that the grants coming with connections could increase re-election rates of connected mayors. The extra funding could also be a way to facilitate campaign donations to the political party. Anecdotally, the interview by Fernando Haddad also suggests the national government might want other policy responses from the mayor:

> *"What I heard was the extreme oposite demand: what would São Paulo do to help the national government? [...] The issue of* [bus] *prices had become a problem for the team in charge of the economy, who was fighting against inflation. I knew that I would be demanded to do something hard: the price maintenance after it had been kept unchanged for two years"* (Haddad (2017))

The expectation that these political connections might bring in money to cities motivated a number of rules and institutions regulating federal transfers. To prevent these transfers from happening in the eve of elections, the article 73 of the Law 9504/97 prevents the national government from approving new funding 3 months before an election. The same law forbids usage of public property for campaigning, hiring and firing of public workers, concession of free non-programatic gifts from the public purse and other actions suggestive of usage of the public machine for electoral purposes. Similarly, to prevent preferential targeting of education funds, the FUNDEF and FUNDEB programs created formulaic rules regulating cross-city allocation of funds for education.

Graph 2.6 show evidence on grant requests according to the timing of elections. During the years with either mayoral or presidential elections, grant requests significantly increase early in the year and take a dip during the 3 month window forbidding new spending. This pattern doesn't repeat itself during the surrounding non-electoral years, when there are few grant requests in the beginning of the year, and they accumulate throughout the year.

By contrast, grant approvals follow a markedly different pattern: graph 2.7 show evidence that a substantial part of grant approvals happens in the end of the year, when national budgets expire. If we apply blindly the evidence from Liebman, Mahoney (2017) to this context, this might indicate some inefficiency in allocation of grants. During years with either mayoral or presidential elections, some grants are approved midway through the year, right before the 3-month window established in the law. Grant approvals fall partially during the 3-month window, and pick up again by the end of the year. In contrast, in the years surrounding the electoral year, there are virtually no grant approvals during the middle of the year, and most approvals happen by the end of the year. This suggests that the law might be only partially effective in restricting new grants during the 3-month window, and that it is not fully effective in preventing electoral budget cycles.

2.1.4 Allocation of federal grants

This thesis considers data on the universe of matching grants from the Brazilian national government to municipalities. Among the national government's transfers to local governments, these are a subset of the

discretionary transfers (awarded at the national government's discretion) where the grants, once conceded, are administered by municipal governments and where municipalities are required to copay for the project.

These grants are indexed to specific capital investment projects as explained below. Moreover, they seem to be an important tool for local governments to be able to make capital expenditures: for the average city in 2009, 89% of all of the municipality's revenues - inclusive of local taxes, and intergovernmental transfers - went into payments for personnel, pensions and materials for day to day operations, with small space leftover for capital expenditures. These matching grants, in turn, are an important tool for municipalities to execute such capital expenditure projects. The rest of this section explains how these grants are awarded to city governments.

Every four years, the federal level Executive branch proposes, and the federal Legislature approves, a Multi-Year Plan (*Plano Pluri-Anual*). These plans specify long term policy goals, including *programs/actions* specifying the types of public goods that will be considered for grants - for example, including programs/actions for provision of hospitals, or for investing in rural sanitation.

Once these programs are set in the Multi-Year Plans, the yearly budget (LOA) is approved in the national legislature, specifying the allocation of funds for each program in that year. For our purposes, this means that in each year, each ministry in the Executive branch has a particular set of programs/actions - say, a program/action for construction of hospitals -, each of them with a pre-specified budget to allocate in funds for local governments.

Municipal governments, then, can request grants from a particular program, using a central website - SICONV - taking grant requests for all programs from all ministries. The online request must include a description of the project to be implemented with the grant, and indicate the municipality's need for the project and capacity to complete it. If the grant happens to be approved, officially, the municipality cannot use the grant for expenses unrelated to the project. Beyond this, the municipality includes in its online request the amount of money requested from the national government and a matching fund the municipality will invest in the project with own resources - typically, a small share of the overall project's budget. Finally, municipalities include timetables of

project implementation, and at some moment before the approval of the request, the municipality must submit online an expense plan, indicating the prices and quantities of services and goods to be purchased with the grant.

These requests seem to be of non-trivial cost. Not only they require a wealth of information to be submitted online, but also, a substantial share of cities do not apply for grants. More precisely, for the 6 largest ministries, in the average year, an average of 49.9% of cities do not apply for grants. For the overall set of ministries, for the average year, an average of 85% of cities do not make grant requests. This suggests making grant requests is not something of trivial costs, nor prohibitive. At the same time, as expected, it indicates that cities make requests more often to ministries with budgets to approve requests.

Once the request is submitted, the ministry initiates the approval process. Bureaucrats review whether the grant request is consistent with the goals of the program the request was submitted to, whether the municipality has the capacity to implement the project and whether the city looks like it's requesting more money than needed. Depending on this, the ministry can either accept the grant request, reject it, or request amendments. In the data, while we see many amendments, I do not observe any changes in amounts requested by cities.

Cities do not seem to perfectly anticipate approvals: in particular, while there are 507889 requests made throughout 2009-16, only 81799 requests were approved (or 16.1% of the requests made). For the cities who actually get grant requests approved, the median approval process takes 145 days. This varies considerably, however, with the 25 percentile of duration till approval of 77 days and the 75-th percentile of 213 days. Less than 3% of projects take more than 10 months till approval, and the histogram 2.1 shows the sharp drop in approval rates 10 months after the date of proposal. The reason for that is that 95% of requests get approved in the year they were made, typically by the end of the year (see figure 2.7), and few requests are submitted in January-February (see figure 2.6).

Once a grant is approved, the municipality implements the project. The national government sends money to a joint account with the municipality, the municipality sends its matching funds to the same account, and every expenditure needs to be reported online and right after the ex-

penditure, in a way that the central government can audit. The median approved grant is completed within 2 years and 8 months (more precisely, 971 days) after accounting for deadline extensions. This naturally has some variation: the 25th percentile of time-to-completion is of 720 days - or approximately 2 years - while the 75th percentile of time-to-completion is of 1308 days - approximately 3 years and 8 months.

Finally, after the project is completed, there is a final audit stage. The ministry first chooses whether to audit the project implementation - on average, 37% of the approved grants are audited. Once the audit is finalized, the project accounts can be either "approved", "approved with reservations" or "rejected". Mayors might appeal the outcomes - typically, appeals only happen after rejected accounts -, and the ministry might revise the audit outcomes or leave it unchanged - most often, outcomes are not revised.

2.2 Data

The main data source used in chapters 3 and 4 of this thesis is the administrative data from SICONV. As mentioned above, SICONV is the online system kept by the national government, where municipalities can request grants from different programs, where bureaucrats in the ministries review the grants, grants are accepted, and where the ministry monitors bills and expenses made with grants. The system started operating in the middle of 2008, and from 2012 onwards, all ministries were required to send their previous administrative data to SICONV, and to use SICONV for all future grant requests.

The website for SICONV provides open microdata on several stages of grant approval. In particular, for this thesis, I use, first of all, the data on grant requests - including data on values requested, values of matching funds - and the programs and ministries the requests are associated with. I also use data on the bureaucratic approval process - including data on the occurrence of different parts of the review of the grant request, on ministries' requests for amendments by municipalities, and timing until approval. Finally, I use data on grants approved, and post implementation audits by the ministries in charge of the grants. While results for final amounts spent on grants are considered, the data on this is incomplete - partially, because many of the projects (specially

the ones approved towards the end of the sample period) are still being implemented.

From this data, I also classify, for one of the exercises, grants for disaster prevention and relief. I define these to be the grants conceded by *Ministério da Integração Nacional* under budgetary programs 1027, 1029 and 2040. These grants consist of 39.28% of the grants requested from this ministry, and 18.21% of the grants approved by this ministry - during the sample period, the ministry has approved 11% of the grants requested.

Additional data on mayors' political parties and their elections come from the electoral court (*Tribunal Superior Eleitoral - TSE*). Data on the identity of the ministers' come from the webpages of the Office of the President of the Republic and its section on previous presidents. Ministers' party affiliations come from CPDOC - FGV (when ministers were politically active prior to 2010), from ministers' webpages in the Chamber of Deputies or Senate (when they were Congressmen prior to becoming ministers) and from public news sources. I use this data to build the measure of political connections used throughout this thesis, by coding a mayor as connected to the ministry if the mayor and the minister are in the same party.

City-level characteristics, including poverty shares in 2000, income per capita per household in 2000 and population (rural, urban and total) in 2007, come from IPEADATA.

Finally, information on registers of natural disasters by the Ministério da Integração Nacional come from *Sistema Integrado de Informações sobre Desastres*. In particular, I use it's digital archive (*Arquivo Digital*), where the ministry records officially recognized disasters or newspaper articles mentioning disasters between 2009 and 2015.[2] While this source might be biased towards registering disasters in politically connected cities, this comes with the advantage of taking into account that disasters might be happening not only due to natural events, but also due to poor infrastructure, lack of local capacity to react to events, and things alike.

This database records 21689 cases of city-years facing natural disasters during the sample period, affecting 4639 cities. Droughts account for 55.3% of the events documented, floods account for 28.3% of events, storms account for 10.8% of events, while the leftover 5.6% of events

[2]Data for 2016 is still unavailable.

include events such as landslides, fires, dam ruptures and building collapses, among others.

Tables

Table 2.1: Share of cities connected with ministries

Share of cities connected between:	2009-15	2009-16	2003-16
Urban Planning	.23	.35	.45
Tourism	.41	.41	.52
Sports	.03	.21	.21
Health	.38	.44	.54
Agriculture	.32	.39	.46
Int. Nacional	.38	.48	.57
Avg. across all ministries	.23	.30	.36
Connected to some ministry	.86	.97	.99
Number of cities		5556	

Figures

Figure 2.1: Days till grant approval

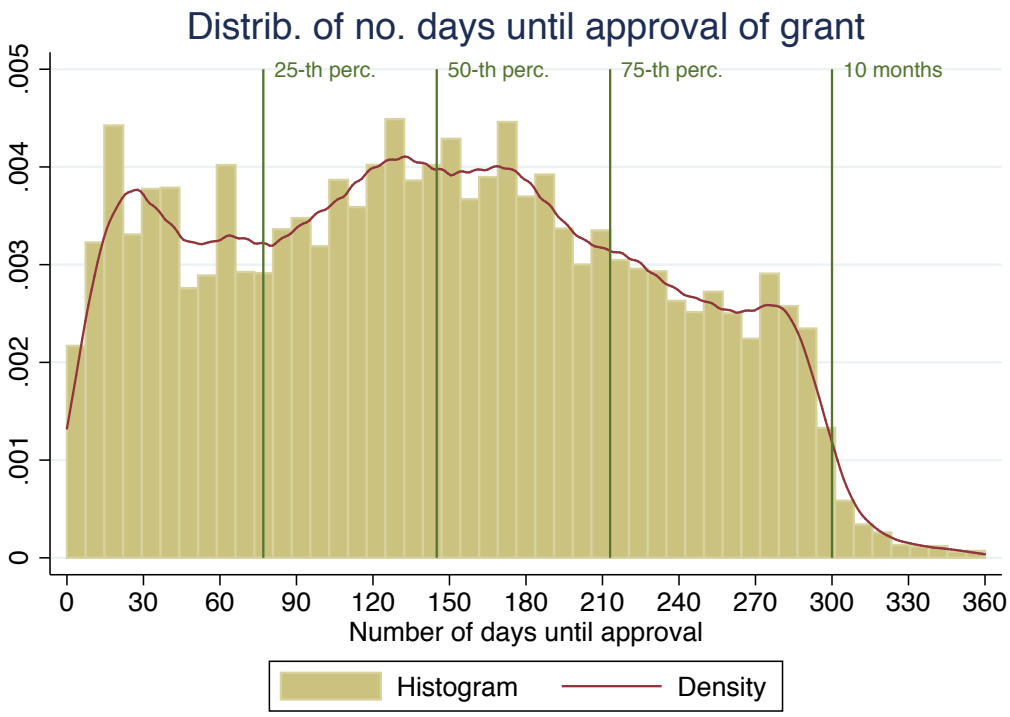


Figure 2.2: No. of ministries of each political party over time

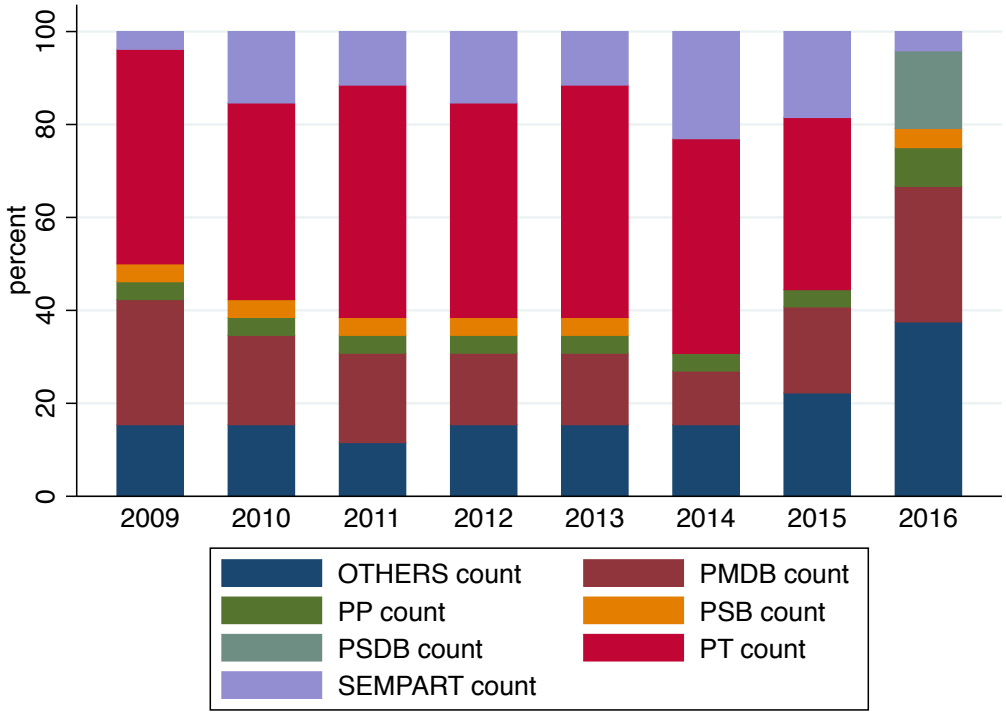
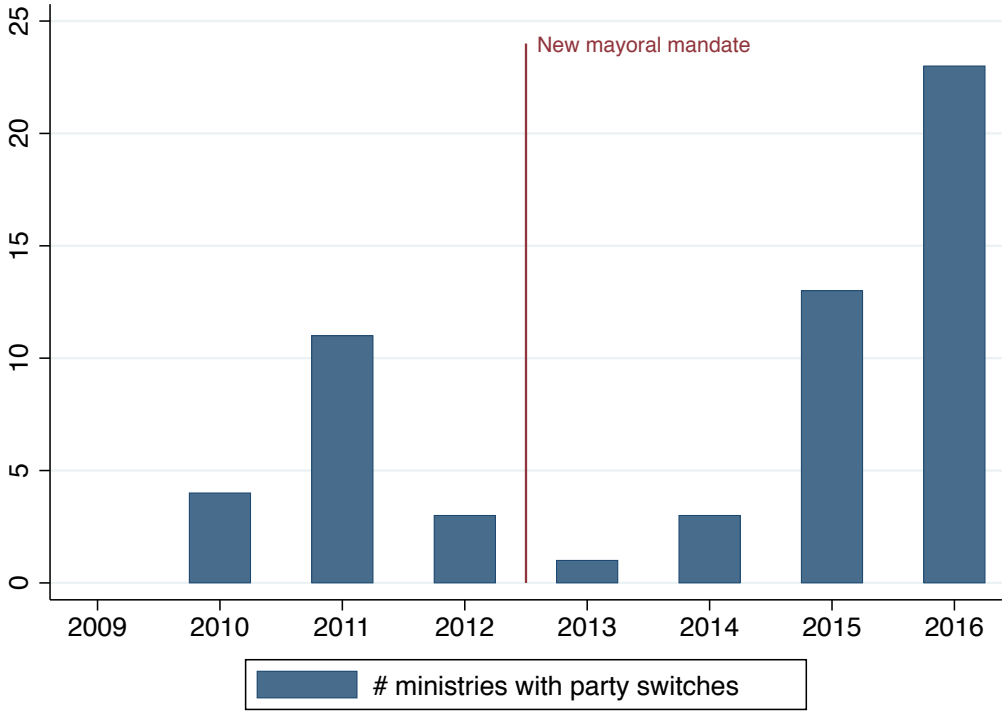


Figure 2.3: No. of ministries facing changes in party of minister



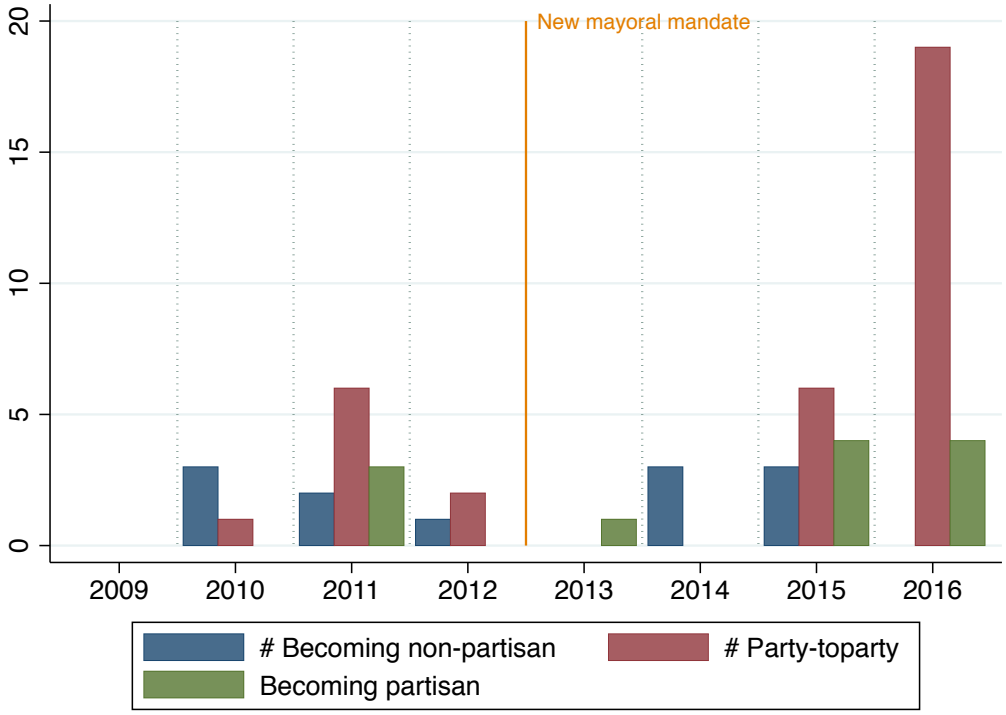
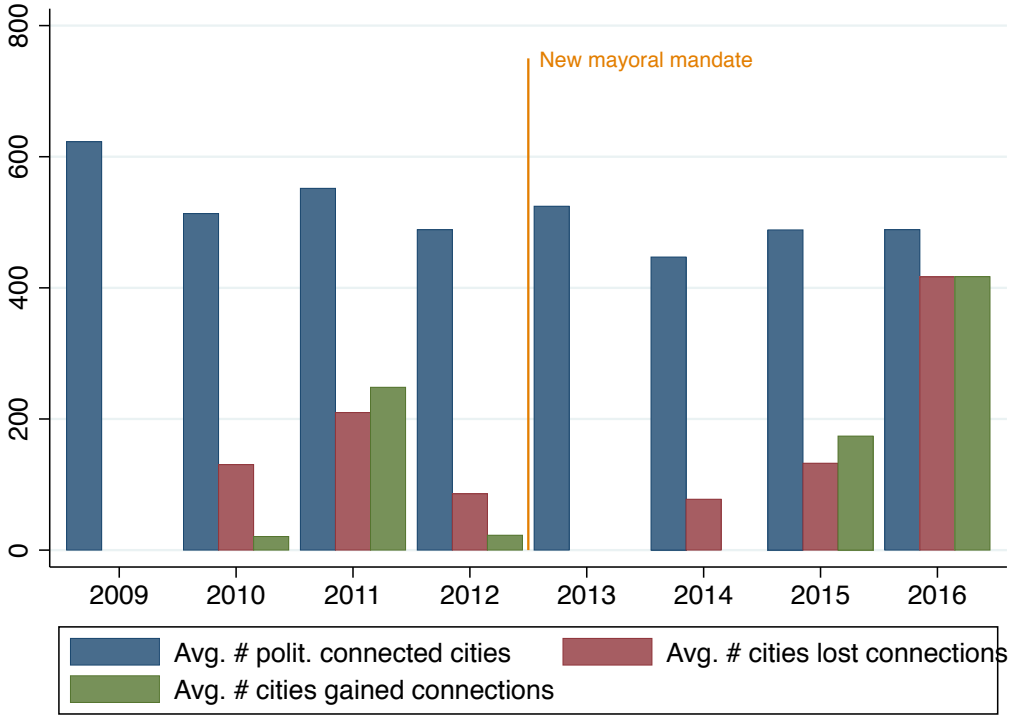


Figure 2.4: Panel A: number/change in number of cities connected with an average ministry. Panel B: number of cities connected with at least one ministry



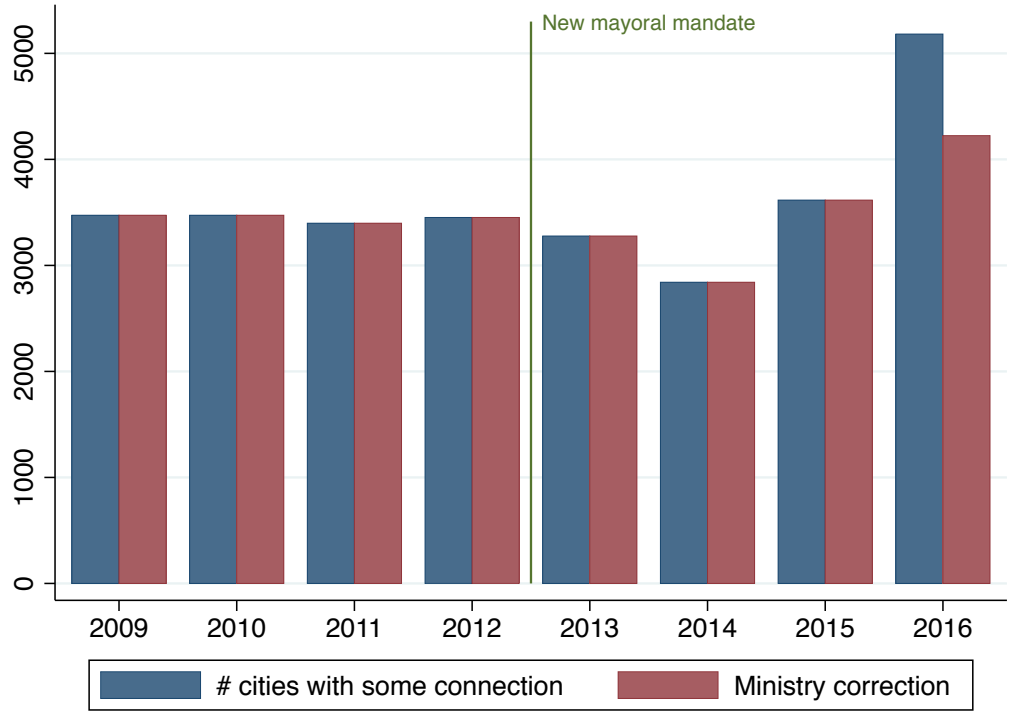


Figure 2.5: Duration of connections between cities and average ministry

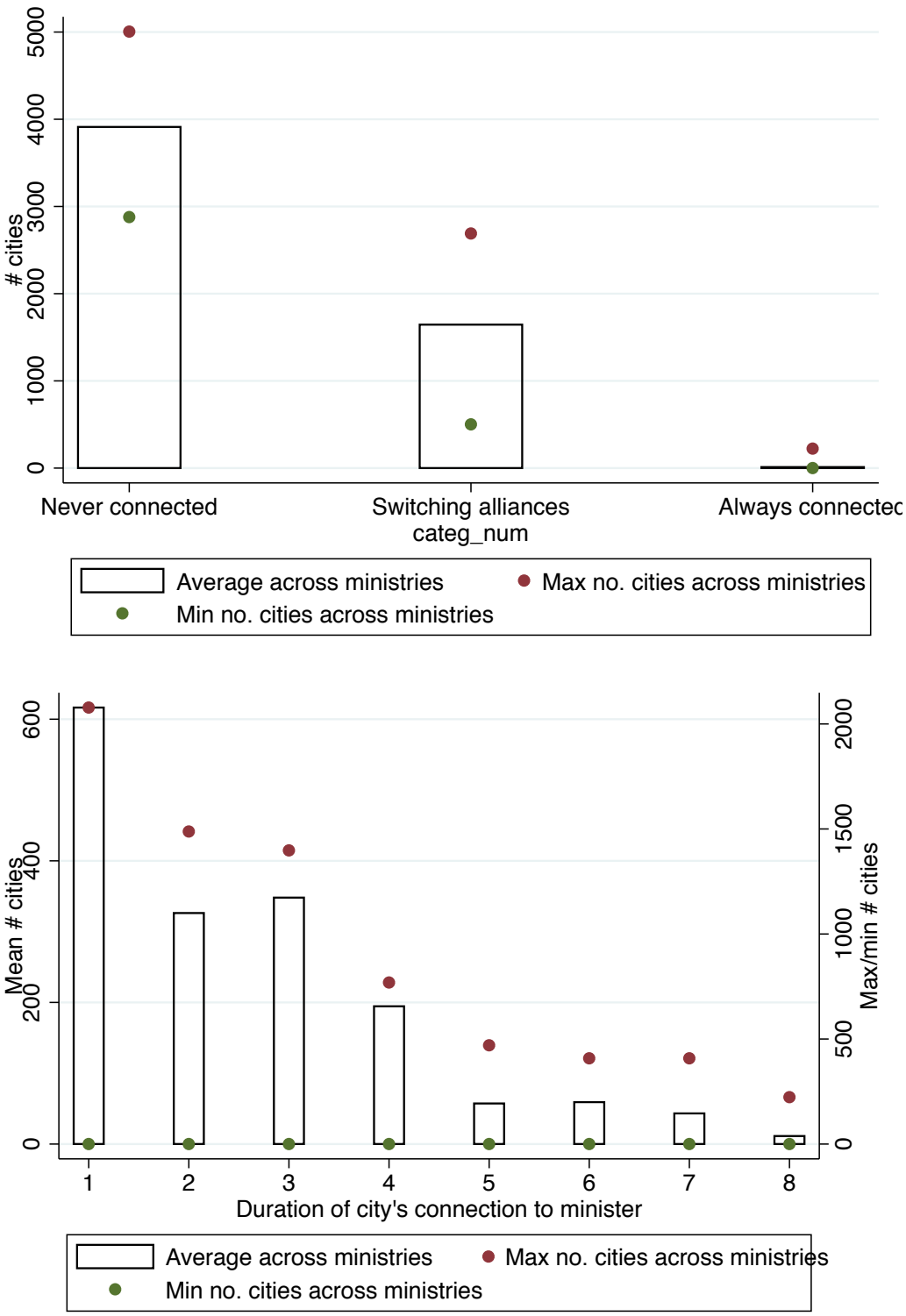
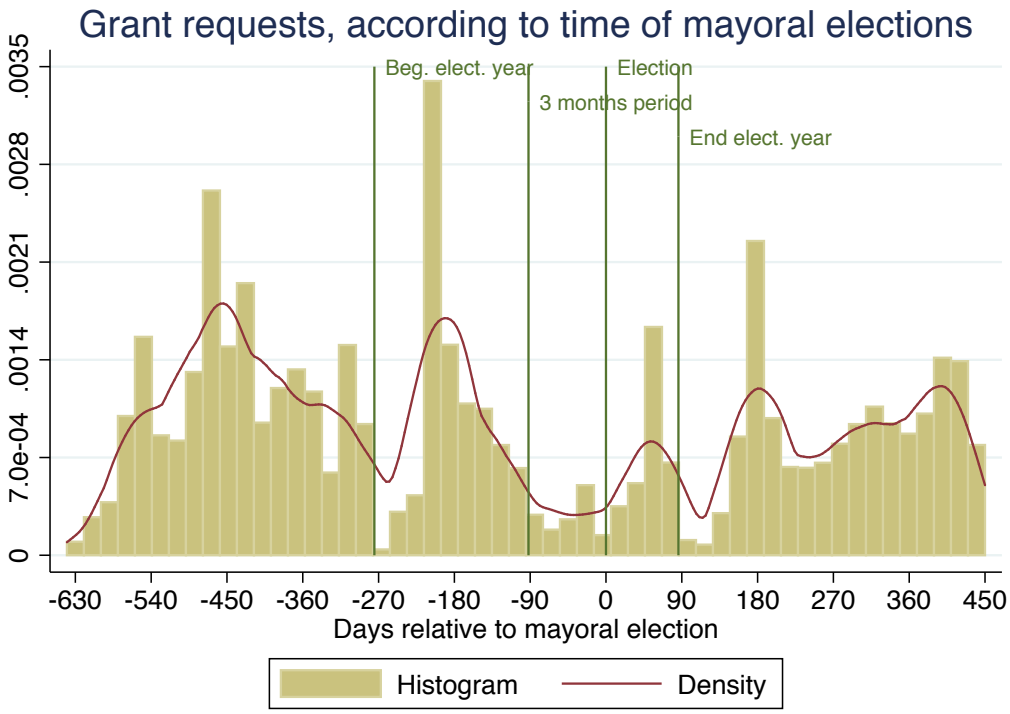


Figure 2.6: Timing of grant requests and mayoral/presidential elections



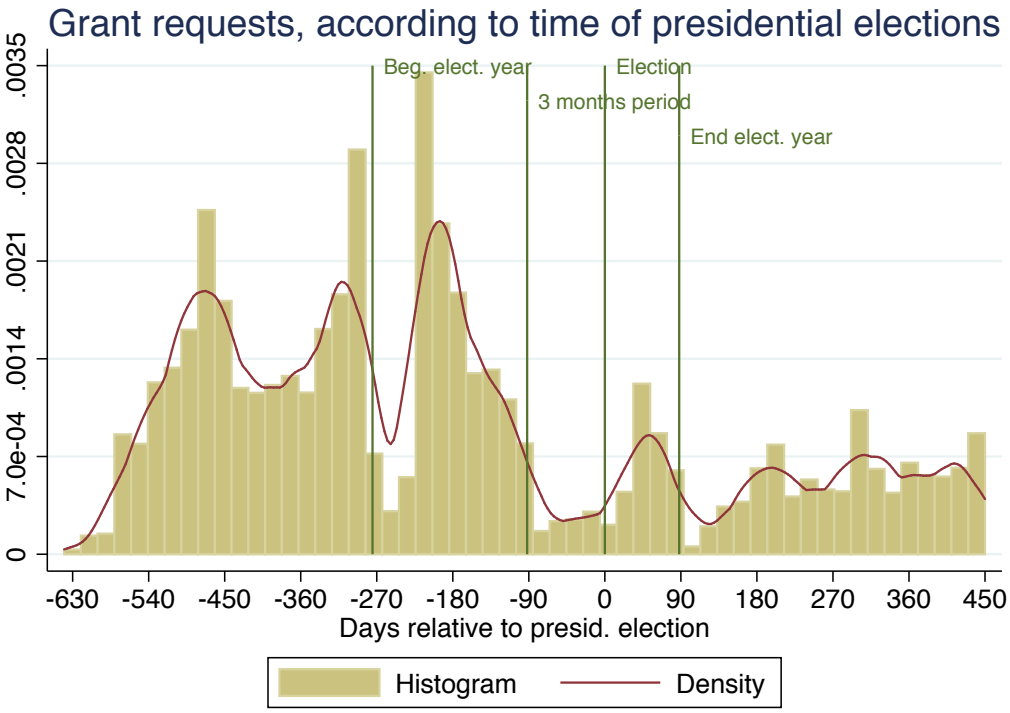
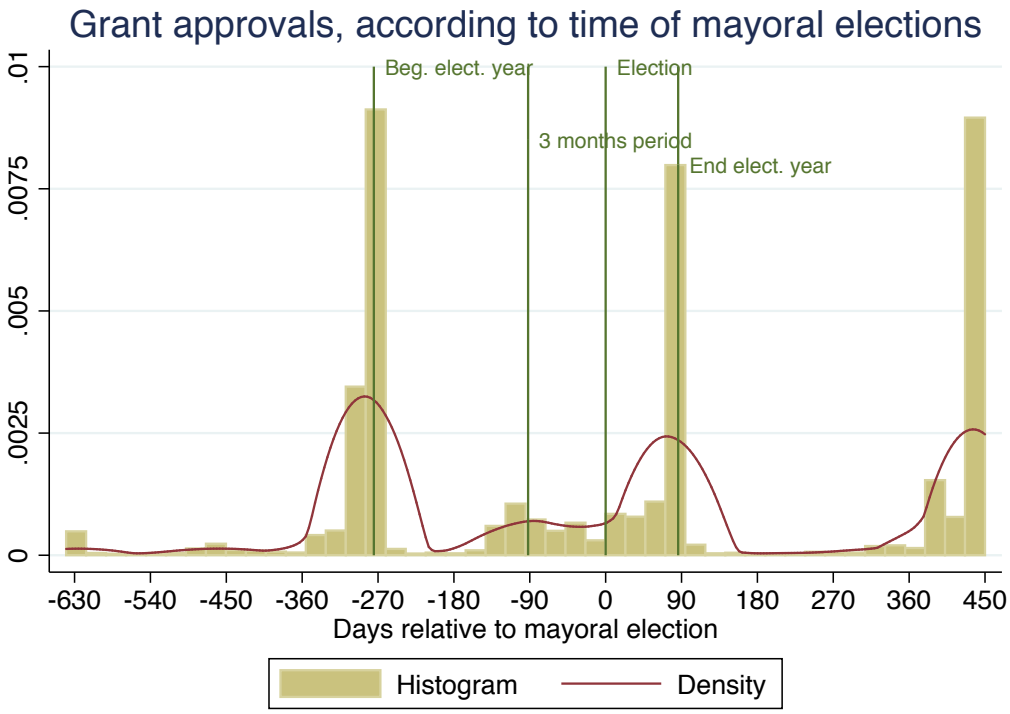
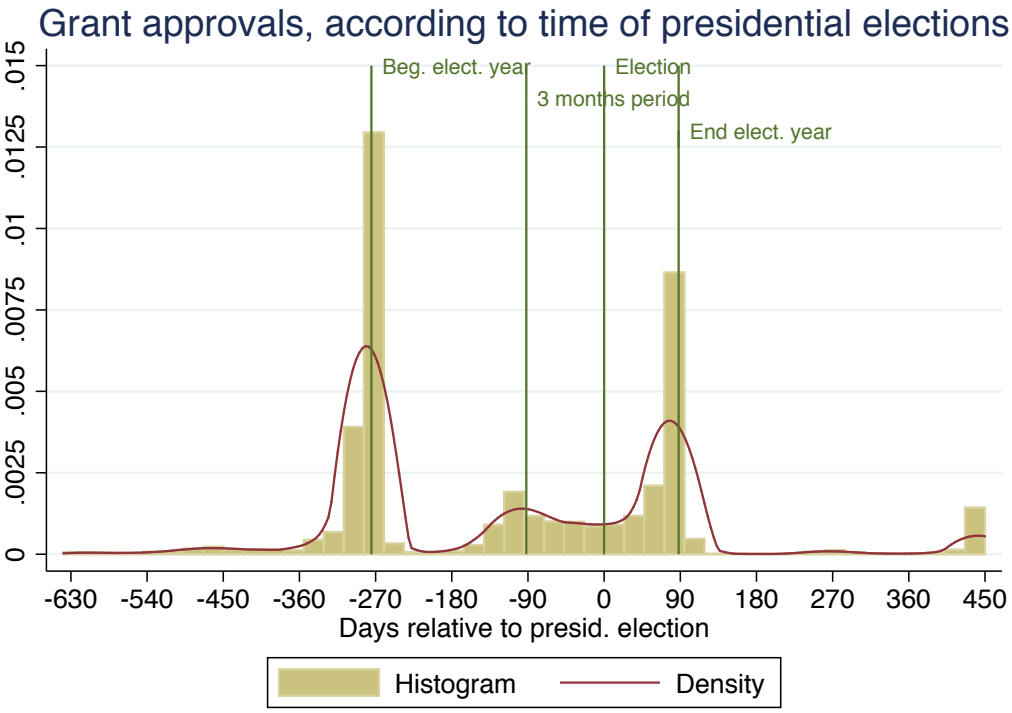


Figure 2.7: Timing of grant approvals and mayoral/presidential elections





3. Political Connections and Local Public Goods in Brazil: Favouritism vs. Information

3.1 Introduction

When allocating public goods across regions, it is desirable for governments to target regions in an efficient manner - for instance, by maximizing some social welfare function, or by sending public goods to regions with high local demands. Despite this, in many contexts, public goods and funds are instead allocated to "politically connected" regions. That is, often, co-ethnics of the President get more roads (Burgess et al, 2015), U.S. districts represented by important committee chairs get more pork (for a review, see Evans, 2011), and in non-democratic countries, the regions of birth of national leaders have higher economic activity (Hodler and Raschky, 2015).

Particularly in democratic federations, we commonly see regional and national level politicians sharing partisan affiliations. Despite the potential for these types of connections to be politicized and to drive the allocation of grants across regions, with few exceptions (see Brollo and Nannicini (2012) and Sollé-Ollé and Sorribas-Navarro (2008)), they have been relatively understudied. With this in mind, how much do partisan connections between lower level and upper level politicians drive the allocation of grants at the national level? Secondly, are connections distorting the allocation of public goods by creating favouritism, or are they simply creating second best mechanisms for information transmission?

At least three difficulties hinder answers to these questions. First of all, in democracies, there is often a limited amount of exogenous variation in political connections, since political leaders are chosen endogenously.

Even when there is exogenous variation to explore, this is typically restricted to few switches in connections or small treatment and control groups. Secondly, it is often hard to get measures of a comprehensive set of public goods. Instead, most studies need to limit their analysis to a small set of types of public goods, or aggregate across many different types of public goods by looking at final expenditures or economic performance. This either faces a problem of making unclear how comprehensive the problem is, or misses the possibility that different types of public goods might not substitute for each other. Thirdly, and relatedly, we often don't observe good proxies for relative demands across regions, nor proxies for ex-post welfare coming from many types of public goods - such as sports courts or renovations of public squares. This in turn prevents us from directly observing whether on the margin, the returns from public good provision in connected cities are higher or lower than in unconnected cities.

To deal with these difficulties, this chapter looks at the context of allocation of grants from the Brazilian national government to municipal governments. In this context, data availability and institutional features allows me to address the issues raised above. First of all, decisions on grant approvals are made by 27 different ministries in the national government, each in charge of different types of public goods - for instance, the Health ministry often invests in sanitation, and overall infrastructure for medical treatment; the Urban Planning ministry often invests in urban roads, housing, and small amounts in sanitation. The fact that different ministries are involved in approving grants for mostly different purposes means that a city can be politically connected with one minister and not with another. That is, within a city, there is variation in political connections across ministries. It also provides scope for many switches in connections.

Moreover, these ministers are often picked among the ranks of several different political parties, as a mechanism to build legislative majorities. In particular, presidential and congressional elections take place simultaneously every four years, at the end of the second year of mayoral terms. By then, new national coalitions form with little influence from any given city, and new ministers come to power then. This implies that a city's political connections with given ministries often switch exogenously midway through the mayoral term, and this variation can be used

for identification.

Secondly, the national government gives access to data from the universe of grants requests by cities and approvals by all ministries to all cities from 2009 to 2016. This allows me to obtain the effect of connections for a large spectrum of different types of public goods being provided, while not needing to aggregate across public goods that are potentially not substitutable (such as hospitals and urban roads). This setting allows for the detection of potentially welfare relevant effects of connections that would not be detectable without disaggregated data on different types of public goods: if all cities were connected to different ministries and connections were relevant, it could be the case that all cities get the same amounts of total grants. Yet, because of their particular connections to particular ministries, some cities would only get hospitals, and some other cities would only get roads, when it is possible that from the view point of the second group of cities, the additional roads don't substitute for not getting hospitals. By observing disaggregated data, the chapter is able to detect such effects of connections, and by observing a broad cross section of public goods, the chapter is able to present estimates that are representative across many different types of public goods. Similarly, if only some cities get connections to obtain all types of public goods, once again the setting allows for the identification of the effect for each type of public good, which can then be summed up to obtain a total effect over overall expenditures.

This in turn allows me to provide credible identification of the effect of partisan connections between lower and upper layers of a federal government over allocation of grants. Figure 3.1 summarizes the identification strategy used here. Consider first Panel A, where I take as an example the years between 2010-11, when the new presidential term started. The ministry of Health switched from being occupied by the PMDB party to being occupied by the PT party, while the ministry of Urban Planning remained in the hands of the PP party. Due to this ministerial switch, the city of Fortaleza, governed by a PT mayor from 2009-12, became connected with the ministry of Health, while remaining unconnected with the ministry of Urban Planning. This allows me to run, within a city, the difference-in-differences estimate of the effect of connections by comparing what happens to grants from the Health ministry to Fortaleza relative to grants from the Urban Planning ministry

to the same city. Doing so allows me to control for all city-time varying specific characteristics driving demands for grants from all ministries.

This simple difference-in-differences, however, might be confounded by the fact that different ministries might be evolving differently over time. Panel B, then, illustrates the strategy to deal with this issue: we can use a placebo city, such as Belo Horizonte, which had a mayor from the PSB party between 2009-12. Due to this, Belo Horizonte didn't become connected with either the ministry of Health nor with the Ministry of Urban Planning between 2010-11. By running a placebo difference-in-differences for Belo Horizonte, then, I'm able to clean out differences in ministry-specific time effects from the estimate from the previous paragraph.

The resulting triple-differences strategy then deduces the first difference in difference estimate from the placebo in the previous paragraph. This can, then, be used to obtain the causal effect of political connections over grant allocation. Cities becoming politically connected with a ministry due to a ministerial change request 15% more resources from the ministry, and get 14.8% more resources approved. The share of grant requests approved rises by 7%.

These results are consistent with favouritism in the allocation of grants. That being said, the results can be due to many alternative theories. The results could be driven by an ideological agreement effect, where a political party thinks the ministry of Health should focus on sanitation investments, while another party thinks the ministry of Health should focus on building hospitals. The shared party affiliations between mayor and minister might just imply agreement on the types of public goods to request and provide. Similarly, the results could be due to an information effect, since political parties could be providing additional channels for the mayor to communicate with the minister about local needs, or for the minister to communicate to the mayor what the ministry is willing to approve. In either one of these last two cases, political connections could have an effect over allocation of grants without necessarily implying any distortion in allocation of funds.

To deal with this, I first analyse a subset of grants that are relatively less ideological, and for which I observe comprehensive publicly known shifters of local demands. More precisely, I look at a subset of post-natural disaster prevention and relief grants. Post disaster relief is a

much less ideological issue, as political parties seem to agree on the need to provide help to cities affected by natural disasters. Secondly, many of the potential sources of differences in information about local demands mentioned above can be controlled for and are known to be publicly observed by the minister, since there is data available on which natural disasters were recognized by the minister.

This, in turn, allows me to use the fact that natural disasters typically affect many cities simultaneously to control for many of these differences in local demands. In particular, I can look at the effect of changes in connections due to ministerial switches only within the set of cities affected by the same natural disaster. This allows me to control for a significant share of the variation in local needs post disaster - if not for all of this variation. Moreover, given that the ministry in charge of these grants publicizes the cities who are recognized to have been affected by a given disaster, this allows me to hold constant at least one important determinant of the ministry's willingness to approve grants - it's recognition of the disaster - that might have been private information in other contexts.

Even after controlling for a significant share of determinants of local demands and of important parts of the ministry's willingness to approve grants, and considering cases where ideological agreement issues are unlikely to drive results, cities becoming politically connected after a natural disaster do not request more grants, but receive 17% more money than other cities affected by the same natural disaster. Despite the extensive controls for many of the sources of differences in needs, despite the fact that information on needs is relatively public, and despite the lack of prominence of ideological disagreement as a driver of grant allocations, effects are, if anything, slightly larger than the baseline effects for all grants.

These findings suggest favouritism is more likely to drive the effect of connections over grant approvals, at least when it comes to post natural disaster prevention and relief grants. How much do these findings generalize to other types of grants? First, by controlling for 3 different measures of ideological distance between mayors and ministers, I show that the effects of connections become, if anything, stronger once we control for ideological differences. While the effect of ideological differences between mayors and ministers is often close to zero, when it has a significant effect, it is positive, suggesting that conditional on the minister

sending money to mayors outside his own party, the minister prefers to send money to mayors who are ideologically distant from him - possibly, to reduce political competition from similar parties. With this in mind, the effects of connections are unlikely to be due to ideological proximity - since ideological proximity doesn't bring additional funds to a mayor outside the minister's party.

Similarly, while it is hard to obtain measures of performance, I can provide evidence from indirect tests for whether differences in information drive the effects of connections for non-disaster prevention and relief grants. If the differences in information concern the mayors' capacity to implement projects specific to that ministry, then future ministers can learn about mayors that were connected in the past at least by observing which of them received grants (and potentially through additional sources). This would imply that mayors that are currently unconnected but were connected in the past should also receive more grants. Instead, unconnected mayors who were connected in the past receive no more grants than mayors who were always unconnected. Moreover, the effect of current connections, if anything, becomes slightly stronger. This suggests there is little scope for learning across ministers from different parties about mayoral competence.

Similarly, if ministers have information about the quality of grants from connected mayors, this information should potentially spillover to the bureaucrats analysing the grants. Yet, the approval process of grants from connected and unconnected mayors is very similar, up until the very last stages: grants from connected and unconnected mayors are equally likely to be analysed by bureaucrats, to face amendments, and take only 2 to 6 days shorter amount of time to be approved (with the typical grant for unconnected cities taking approximately 5 months to be approved). This suggests that the effects of partisan connections only happens at the final stages of grant approval, and doesn't reach the bureaucrats reviewing the grants, and that whatever additional information ministers have about connected mayors, they do not spill over to the bureaucrats analysing the grants.

Finally, on the few performance measures I have available - audit outcomes and time till completion - grants from connected cities and unconnected cities are statistically and economically very similar. That being said, it is hard to argue that I would have power to detect dif-

ferences in audit outcomes given that there are very few grants with irregularities detected in audits.

That is, current connections encourage the ministers to give more to mayors, even for subsets of grants where information about local needs is relatively public and can be controlled for. Moreover, connected mayors get more despite there being little evidence of learning about mayors that were connected in the past, there being little evidence of information on connected mayors spilling over to the bureaucrats analysing the grants, and relatively few observable differences in performance. Finally, there is no evidence that the effects of connections are driven by differences in ideological agreement on what types of public goods to provide. I interpret this as evidence of favouritism in the allocation of public funds across regions.

This chapter relates to three strands of literature. First, a group of papers tries to understand the cost and benefits of social and political connections in different environments. Fisman and Wang (2015) analyse the costs of political connections on the enforcement of labor standards. Xu (2017) and Jia, Kudamatsu and Seim (2015) show evidence, respectively, on the costs and benefits of political connections in the selection of bureaucrats and local governants. Fisman (2001) shows that in Indonesia during Soeharto, firms' political connections had strong implications for their performance. Khwaja and Mian (2005) show that firms' political connections give them easier access to credit in Pakistan. Relative to this literature, this chapter analyses the costs of connections in the context of government grants and funds. Often, this is a harder case to analyse, given the lack of measures of local demands. What this chapter does to address this is to control for measures of local demands when available, and to look at more indirect tests to generalise the findings to other types of grants.

Secondly, this chapter relates to a broad literature on politicized allocation of public funds. For the U.S., Evans (2011) provides a review of evidence, and three recent references discussing politicized allocation of public funds are Knight (2005), Kriner and Reeves (2015) and Dynes and Huber (2015). For developing countries, see Franck and Rainer (2012), Kramon and Posner (2013), Hodler and Raschky (2014), Burgess et al (2015) and Kramon and Posner (2016). For two cases with settings closely related to mine, see Sollé-Ollé and Sorribas-Navarro (2008) for

grants in Spain and Brollo and Nannicinni (2012) for grants in Brazil. Relative to this literature, I document a setting from a developing country with considerable variation in connections due to switches in connections. This chapter also manages to provide evidence for a comprehensive set of different types of public goods without aggregation. Finally, I provide new tests to distinguish between the possibility that connections might be bringing information about the relative value of grants across regions, or creating enforcement of better performance for grants.

The next section shows the main identification strategy for the effect of connections over the allocation of public funds, and presents the results and robustness checks. The third section presents the results on favouritism versus information, with evidence on (i) post natural disaster prevention and relief grants, (ii) learning across ministries throughout the mayoral term, (iii) evidence on the measures of performance available. Finally, the last section concludes the chapter.

3.2 Political Connections and Allocation of Local Public Goods

In this section, I show evidence on the effects of political connections between mayors and ministers from the national government over the allocation of public funds - where I define that a mayor is connected to a minister if they're both in the same party. In this section, I focus on the effects of these connections both through changes in the ministerial approval criterion, and through mayors deciding to request more or less due to political connections. I discuss the separation between these two channels in the structural estimates.

This section has the following structure: first, I show the results on the effects of political connections over grant allocations. After this, I move to the discussion of whether it is likely that results are driven by favouritism, or whether it is likely that political connections are bringing differences in partisan agreement, information or enforcement across cities.

3.2.1 Identification

In analysing the reduced form effects in this section, at least two challenges to identification need to be tackled. First of all, cities who need

more grants might endogenously select more or less connected mayors, so that the correlation between political connections and grants might not reflect the causal effect of political connections discussed above.

Secondly, city-level time varying characteristics might affect the city's propensity to receive grants. In particular, a city's demands for grants, it's importance and influence from the viewpoint of the national government might change over time, and these might be correlated both with connections and with grant allocations.

These concerns motivate the following triple differences research design. Firstly, to control for endogenous mayoral selection, I use the fact that ministries often change midway through the mayoral term - often due to changes in the president's and congressional terms - to explore variation in connections only due to national level changes, holding fixed the mayoral term.

Secondly, I explore the fact that many ministries decide on the allocation of grants to look at variation in connections within cities, across ministries. This strategy can be visualised in the top half of figure **??**: say that midway through the mayoral term, the Ministry of Health moved from the party PMDB to the party PT. Meanwhile, say that the Ministry of Urban Planning stayed in the hands of PP. We can then look at the change in grants from the Ministry of Health to the city of Fortaleza, governed by PT, relative to the change in grants from the Ministry of Urban Planning to Fortaleza.

These differences in grants might reflect differences in time trends across ministries. To control for this, the bottom half of figure **??** looks at the same difference-in-differences from the previous paragraph, but instead, with the placebo city of Belo Horizonte, governed by PSB.

This design is implemented with the following regression framework:

$$y_{cmt} = \beta s_{cmt} + d_{cmT} + d_{mt} + d_{ct} + \epsilon_{cmt}$$

where c stands for a city, m stands for a ministry, t stands for an year, and T stands for a mayoral term, and y_{cmt} is a dependent variable under consideration (either grant requests, or grant acceptance). The dummy s_{cmt} indicates that the mayor's party in city c at the time of his election is the same as the party ahead of ministry m in time t - or that the mayor in city c is aligned, or connected with the minister m (these terms are used interchangeably). Standard errors are clustered at the ministry level and

at the municipality level, following Cameron, Gelbach and Miller (2011).

This strategy deals with the first identification concern - of endogenous mayoral selection - by controlling for the city-ministry-mayoral term effect d_{cmT}, and by holding the mayor's party fixed at his beginning of term party. With this, this strategy estimates β only on the sample of city-ministries that face some change in s_{cmt} only due to changes in the minister's party. The crucial identification assumption here is that voters' mayoral selection is not done in anticipation to changes at the ministry level that typically happen around the third year of the mayoral term. To check for the plausibility of this assumption, I further run several placebo tests in section 4.2.2 to check whether the cities facing changes in connectedness seem to have gains from connectedness before the ministerial change.

This strategy also deals with the second concern by looking at variation across ministries, within a city. By doing so, I manage to control for city-time specific effects d_{ct}, including the city's overall demand for grants from different ministries and the city's importance and influence over the overall national government, which might be correlated both with connections and grants received.

Finally, by controlling for d_{mt}, this strategy controls for any factors shifting the ministerial's budget over time, and overall demands in the country for the ministry's particular type of public good.

3.2.2 Main results

Table 3.1 displays estimates of the effect of political connections over grant requests by cities, using the identification strategy described above. While non-connected mayors request, on average, 0.42 proposals from a given ministry, political connections increases the number of grants requested from a given ministry by 0.066, an increase in 15.7% in the number of grants requested. In the same way, the probability that a given city requests a grant from a given ministry increases by 6.6%. The 3rd-5th columns, in turn, analyse semi-elasticities of monetary amounts requested from a given ministry with respect to connections, by looking at the log of dependent variables plus 1. Column 3 shows that the overall grant size increases by 15% when a mayor becomes connected with the ministry. The size of the grant financed by the ministry increases also by 15%, while the size of the co-payment by the city increases by 12.2%.

These larger grant requests do translate into larger amounts of grants approved. Table 3.2 shows results of final grants obtained by a given city (that is, cities that didn't request any grant from a given ministry are taken to receive zero grants). Relative to a baseline average number of grants from a given ministry of 0.066 for non-connected mayors, connected mayors get 0.023 more grants, an increase of 34.8% in the number of approved grants. In a similar way, the probability that a city gets a grant approval from a given ministry increases by 22.9%. Columns 3-6 analyse semi-elasticities of monetary amounts approved with respect to connections as before. When a mayor becomes politically connected with a city, he obtains 14.8% larger grants approved. The monetary amounts financed by the ministry increase by 14.7%, while the amounts co-paid by the city increase by 11.4%. The final amounts of money spent from the grant increase by 9.8%. While we likely cannot reject the hypothesis that the effects over final amounts spent and the effects over total grant size are the same, it is useful to highlight that we might not observe completely the amounts spent from grants approved after late 2015, since the execution of these grants might not yet be finalized.

Table 3.3 shows the effects of political connections over grant approvals on the sample of city-ministry-years that have some grant request. While, on average, a non-connected mayor that made at least one grant request has 0.433 grants approved, connected mayors have 0.11 higher number of grants approved, an increase in 25.6% in the number of grants approved. Similarly, the probability that at least one grant is approved increases by 0.037, an increase of 11.7% relative to the probability that an unconnected mayor gets at least one grant approved (2nd column). While 20.1p.p. of the grants requested get approved for non-connected mayors, 1.2p.p. more grants get approved for connected mayors, so that connected mayors have 6% higher rate of request approval than unconnected mayors. Columns 4-7 show semi-elasticities of grant sizes. Among the cities making grant requests, connected cities get 53.1% larger grant sizes (4th column), 52.9% larger monetary values financed by the ministry (5th column), 42.8% larger co-payments (6th column) and 40.6% larger final amounts spent (7th column).

3.2.3 Robustness

Endogenous ministerial choice: These results might be partially driven by endogenous ministerial choice. In particular, it might be that cities that particularly need larger grants from a given ministry are able to pressure the national government to change the party ahead of that ministry. To check for this possibility, I check whether the results still hold in a subset of cities that are likely not to have strong influence over the national government. In particular, table 3.4 shows (a summarized subset of) the results for the subset of cities with less than median population size. Brazil has a population of almost 200 million individuals, and cities with less than median population size have less than 10,677 inhabitants, so each of these individual cities are unlikely to have large effects over the national government choice.

In fact, most results still hold for this subset of cities: politically connected mayors make 16.2% larger number of grant requests (1st column) and request 16% larger project values (2nd column). Among the cities making grant requests, connected mayors also get 10% larger number of grants approved (3rd column), get 21.4% larger grant sizes (4th column), and have 15.2% larger amounts spent (5th column), though this last result is imprecisely estimated. We use these results as suggestive that it is unlikely that endogenous ministerial choice drives the results from the previous section.

Alternative control groups: A second concern for identification is that the baseline results do include cities that are unconnected to all ministries as part of the control group. If these cities follow substantially different trends from cities that are connected with at least one ministry, or cities whose mayors are in parties in the national government coalition, this might bias the results under our identification strategy. To take this into account, table 3.5 shows results including only cities that are politically connected with at least one ministry.

Once again, the results are unaffected by the choice to exclude the cities unconnected with all ministries. Politically connected mayors make 21.9% larger number of grant requests (1st column) and request 19.3% larger grant amounts (2nd column). Among those who made some grant request, politically connected mayors request 23.5% larger number of grants (3rd column), 51.8% larger monetary amounts (4th column) and 43.7% larger amounts spent (5th column).

Is this driven by a few parties only? A third concern might be that the results might be driven only by a few political parties, as opposed to being something systematic. In particular, it might be that the result is completely driven by being in the president's party. Table 3.6 shows the results by political party - given the large number of parties, though, I show the results for the 5 parties with most changes in alignment in the sample, and group all other parties in the "others" group.

In fact, throughout most of the period, the presidency was under PT, and surprisingly, we cannot estimate any significant effects for mayors from PT. A few reasons for this might be due to the fact that PT occupies several small ministries that allocate relatively few grants (political connections seem to play less of a role in small ministries, results not shown); that PT might be more willing to avoid favouring connected mayors as an extra tool in building coalitions; or that PT might be benefited by all ministries due to presidential influence. It is hard, however, to disentangle between these alternatives. Quantitatively, we observe largest effects for PT's main coalition partners throughout 2009-2015, namely, PMDB, PP and PSB. PSDB is a party that was outside the government coalition (and unconnected with all ministries) between 2009-15, and who got ahead of several ministries only in May 2016 with the impeachment of President Rousseff. Typically, during the few months ahead of mayoral races, the number of grant requests and approvals fall somewhat (see figures 2.6 and 2.7), and this might explain why we see relatively little effect from connections with PSDB. Overall, we can robustly reject the hypothesis that the effects are jointly zero, while we can only reject the hypothesis of no heterogeneous effects in 3 out of 5 of the regressions displayed.

Common trends: Finally, there might be a concern that treatment and control groups do not follow common trends throughout the mayoral terms. If they don't follow common trends, this becomes a serious threat to the identification strategy. The typical test for this common trends assumption is to check whether there are pre-trends. Doing so in this setting, however, presents two difficulties: first, I have relatively few periods per unit, so that it is hard to check whether there are long running common trends between treated and control groups. Secondly, in the current setting, we observe mayors both switching into and out of connections, and doing so from the 2nd to the 4th year of the term. If we

use the typical strategy of using leads and lags of treatment to estimate differential pre-trends, we'd estimate the effects of leads on a different sample from the effects of lags.

To get around this, first, define $sw^s_{c,m}$ to be the group of city-ministries switching alliance in period s. In particular, I let $sw^s_{c,m} = 1$ if the city-ministry pair c, m wasn't connected in period $s-1$ and becomes connected in period s. Similarly, I let $sw^s_{c,m} = -1$ if the city-ministry pair c, m was connected in period $s-1$ and stops being connected in period s, and finally, I let $sw^s_{c,m} = 0$ if connectedness doesn't change. I then run the following specification:

$$y_{c,m,t} = \sum_{s=2}^{4} \sum_{v=2}^{4} sw^s_{c,m} \mathbf{1}(t = v)\beta_{s,v} + d_{c,m,T} + d_{c,t} + d_{m,t} + \epsilon_{c,m,t}$$

More specifically, for the intuition in this specification, ignore at first the differences in time trends across cities and ministries. Then, note that $\beta_{s,v}$ has the following interpretation: it is the growth in the outcome $y_{c,m,t}$ between period v and period 1 for the groups of city-ministry pairs switching alliances in period s, relative to the growth in $y_{c,m,t}$ between periods v and 1 for the groups of city-ministry pairs not switching alliances in any period. Adding the terms $d_{c,t}$ and $d_{m,t}$ then just changes the interpretation to the triple differences previously described.

Also note that the coefficient $\beta_{s,v}$, with $v \geq s$, captures the effect of becoming permanently connected after period s. With this, all coefficients $\beta_{s,v}$, with $v \geq s$, represents post treatment effects, and those with $v < s$ represent pre-trends. Given that we're estimating 9 different coefficients in each equation, we focus on the joint significance of the pre-trends coefficients $(\beta_{3,2}, \beta_{4,2}, \beta_{4,3})$, and the joint significance of the post-trends coefficients $(\beta_{2,2}, \beta_{2,3}, \beta_{2,4}, \beta_{3,3}, \beta_{3,4}, \beta_{4,4})$.

Table 3.7 shows common trends tests for grant requests. Clearly, the effects of post trends are only significant for those city-ministry pairs that change alliances by the 2nd year of the mayoral term. With some exceptions, pre-trends coefficients are almost never significant. The joint significance tests confirms this: while the pre-trends coefficients are not jointly statistically different from zero, the post trends coefficients are typically jointly statistically different from zero at a p-value of 10% (similarly to the baseline regressions in table 3.1).

Table 3.8 shows the same common trends tests for grant approvals

in the full sample. Now, it can be seen that post trends coefficients are often significant for mayors switching alliances in the 2nd, 3rd and 4th year of the term, while pre-trends coefficients are only significant for those mayors changing alliances in the 4th year of the term. Once again, we cannot reject the hypothesis that all pre-trends coefficients are jointly zero at conventional significance levels, while we can reject, for most specifications, the null hypothesis that all post-trends coefficients are jointly different from zero. A similar pattern shows up in table 3.9, where we run the common trends test for grant approvals on the sample of city-ministry-years with some grant request.

Finally, table 3.10 shows the same specifications as the main specification, but using the proposal level data. Indeed, given the sample of proposals, politically connected mayors request larger project values, with more financing by the ministry and with larger matching funds. A given grant request also has a higher probability of being approved. That being said, conditional on project approval, grants approved for connected mayors are not larger than the grants approved for unconnected mayors, suggesting that the effect of political connections is on the extensive margin - where connections lead to a larger share of approved projects.

3.3 Favouritism vs. information

These results show that when ministers change and mayors become/stop being politically connected with them, the allocation of grants for public goods increase/decrease significantly. On one hand, this could be due to favouritism: that is, that a politically connected mayor could get more grants than unconnected mayors, even without inherent differences in demands for public goods in the two cities.

There are, however, at least four alternative explanations for the findings above suggesting that political connections could affect grant allocations without being harmful. First, political connections could proxy for partisan agreement in what types of public goods to provide. In particular, it could be that a given party believes the health ministry should prioritize sanitation, while another party believes the health ministry should prioritize investments in mobile health units. The agreement between the mayor and the minister on what to provide could be a driver

of the effect of connections.

Secondly, partisan connections could serve as a tool for informal flows of information. That is, the minister could use the party connections to communicate better with mayors about what types of requests the ministry is willing to accept. In a similar way, the mayors could use the party connections to communicate better to ministers about local needs.

Thirdly, ministers might be better able to enforce effort by connected mayors. This could serve as a mechanism to improve the welfare value of connected mayors' grant requests. This could, in turn, provide a reason for a benevolent minister to allocate more grants to politically connected mayors.

Finally, connected mayors might, overall, have grants with different characteristics, and these characteristics might not be fully captured with the structure of fixed effects included in the specifications above. The next few exercises provide some evidence on these alternative explanations.

3.3.1 Differences in ideology

A first potential driver of the effects of political connections is that politicians in the same political party share political ideologies, and as a consequence, tend to agree more often on the types of public goods to deliver. To check for this possibility, I use the measures of ideology obtained in the chapter 5 of this thesis and the measure by Power and Zucco (2011) for the national congress in 2009. From these measures, I compute the average party ideology in the Chamber of Deputies averaged across 2008-2016. Based on these, I compute the euclidean ideological distance between the mayors' parties and the ministers' parties, running the following specification:

$$y_{cmt} = \beta s_{cmt} + \gamma I_{cmt} + d_{cmT} + d_{mt} + d_{ct} + \epsilon_{cmt}$$

where I_{cmt} is the Euclidean distance between the mayor's ideology in city c, time t (as measured by the ideology of his party) and the ministry's ideology in ministry m, time t.

Table 3.11 shows the result using an estimate of a two dimensional ideology measure from the structural model in chapter 5, but assuming

there is no pressure from the government on the government coalition.[1] If anything, the effects of connections become slightly stronger: becoming politically connected increases the number of requests made by 22.6% of the baseline average, and increases the amount of funds requested by 25%. Similarly, increases the number of grants approved by 50% of the baseline average, and the amount of money approved by 22.8%. Consistently with this, mayors that are ideologically more distant from ministers are, if anything, more likely to get more grants.

These results are robust to different measures of ideological distance between the mayor and the minister. Table 3.12 use an estimate of a two dimensional ideology measure from the full structural model in chapter 5, and 3.13 use a unidimensional measure of ideology from Power and Zucco (2011). The results are largely unaltered.

Finally, many of these results might reflect measurement error of ideologies. With this in mind, I run the regression using ideologies measured as usual from roll call voting data, instrumenting ideological distance jointly with the two-dimensional measure from the structural model and with the measure from Power and Zucco (2011). Once again, the results remain largely unaltered.

This suggests that ideological differences are not a driver of the effect of political connections over the allocation of public funds across regions. Instead, very often, ideological distance between mayors and ministers often have zero effects over the allocation of grants, and when there are significant effects, they are positive. This suggests that, if anything, conditional on giving money to unconnected mayors, ministers prefer to give money to political parties that are ideologically distant from them. Potentially, this might be a mechanism to avoid political competition from political parties that are ideologically near the minister's party, and might steal the minister's party's votes.

3.3.2 Partisan agreement and information: evidence from natural disaster prevention and relief grants

To check whether the effects are driven by differences in partisan agreement or in information, the first exercise here considers the case of dis-

[1] This is the usual ideology measure based on roll-call votes.

aster prevention and relief grants, focusing specially on grants after natural disasters. These grants are given by the *Ministério da Integração Nacional*, one of the ministries that has most conceded grants during this period. Not only these constitute a substantial share of the grants requested and conceded by this ministry, but this is also an economically important case to analyse. According to the *Perfil dos Municípios Brasileiros 2013* from the national statistics institute *IBGE*,[2] between 2008-13, 1543 cities faced gradual floods leading to 1,406,713 individuals displaced, while 1574 cities faced flash floods leading to 777,546 displaced individuals. Yet, 48% of the municipalities do not have the instruments surveyed aimed at dealing with natural disasters.[3] With this in mind, grants and overall assistance by the national government might be an important way for cities cope with these events.

Compared to the rest of grants analysed in the paper, there is less disagreement across parties on types of grants needed. First of all, political parties seem to agree on the need to help regions affected by natural disasters. Secondly, the city's needs for a project are relatively technical: for instance, after droughts, it might be relatively clear whether the city needs a grant to buy a water truck.

In addition, many differences in information can be controlled for when looking at these types of grants. Given that many natural disasters affect many cities simultaneously, this allows me to control for one important determinant of local demands for these grants by looking only at variation within the set of cities affected by a given natural disaster. Also, given that the *Ministério da Integração Nacional* publishes data on which cities it recognized to have faced a disaster, at least one important factor in the ministry's willingness to allocate grants - the recognition of the disaster - is public and controlled for once I look only at a group of cities who were recognized to have faced the same disaster. Finally, given that some cities face many natural disasters, this allows me to control for the cities' typical exposure and reaction to disasters.

As a consequence, this exercise analyses a setting in which the extra information and partisan agreement brought by political connections are relatively less important in driving the results. If political connections

[2]This database surveys municipalities' administrative capacities.

[3]See *Pesquisa de Informações Básicas Municipais: Perfil dos Municípios Brasileiros 2013*, tables 20, 21 and graph 44. In ftp://ftp.ibge.gov.br/Perfil_Municipios/2013/munic2013.pdf .

affect resource allocation only by providing additional partisan agreement and information transmission, it would be expected that the effect of political connections over grant allocation would become smaller as the attention is restricted to the post disaster grants analysed here. Instead, if political connections affect the allocation of post natural disaster grants by as much or more than they affect overall grants, it would be harder to claim that political connections affect grant allocation by bringing additional information and partisan agreement.

To evaluate the effect of connections over post disaster grants, I first group records by the ministry as a *disaster event* by assuming that events of the same type,[4] recorded by the ministry to have happened in the same day and in the same state are the same event. That is, if I observe two cities in a given state facing a drought in the same day, I record this as a single event.

I then build a database of event-cities e, c, and record the number of disaster prevention and relief grants obtained by the cities up to two years before the disaster and up to two years after the disaster. With this in hands, I look at the following specification:

$$\begin{aligned} y_{cer} =& \beta_{pre} s_{cer} \mathbf{1}(r < 0) + \beta_{post} s_{cer} \mathbf{1}(r \geq 0) + \\ &+ \gamma_{pre} o_{cer} \mathbf{1}(r < 0) + \gamma_{post} o_{cer} \mathbf{1}(r \geq 0) d_{ceT} + d_{er} + d_{cr} + \epsilon_{cer} \end{aligned}$$

where c stands for a city, e stands for a disaster event, r stands for the year relative to the disaster date (that is, $r = -2$ when the year is 2 years before the disaster's year, $r = 0$ at the disaster date, and then on). Once again, T stands for the mayoral term. The variable y_{cer} stands for variables relating to disaster grants obtained by city c, r years after event e has happened. As before, s_{cer} captures whether the mayor from city c is in the same party as the minister from *Ministério da Integração Nacional* (the ministry in charge of disaster grants) r periods after the event e. The variable $o_{c,e,r}$ captures the number of other ministries the mayor from city c is connected with, r periods after event e.

By controlling for d_{er}, I control for how the disaster e affects the group of cities facing it over time, effectively capturing exposure to the disaster

[4]Events are grouped by *COBRADE*, and classified by *Ministério da Integração Nacional*, into 3 digit code event (for instance, at the 3-digit level, events can be classified as long droughts, short droughts, forest fires and low air humidity - all classified as droughts at the 2 digit level).

and intensity of the disaster affecting a group of cities. This controls both for an important shifter of local demands - namely, the intensity of the disaster throughout the years - and for the ministry's recognition of the disaster for this group of cities. As a consequence, this controls for many of the factors that mayors and ministers might inform each other about.

By further controlling for d_{cr}, the strategy controls for the city's capacity to react to disasters, the city's typical exposure to natural disasters, and a number of (not all, though) time varying city characteristics that might create omitted variable bias. Finally, by controlling for d_{ceT}, I allow for the city to be permanently different from other cities affected by the disaster e, prominently, controlling for factors such as proximity to geographic features affecting the city-specific disaster intensity. At the same time, the fact that this effect is only constant within a mayoral term guarantees that I only use variation in connections due to ministerial changes. While these controls might not capture all factors that cities and the ministry need to inform each other about, they capture many important shifters of local demands and the ministry's recognition of the disaster.

The coefficient β_{post} captures, then, the effect of changes in connections due to ministerial changes over disaster response grants (or, disaster grants after the disaster has happened). I focus on this coefficient for the rest of this section.

Note that, due to the control for d_{cr}, this specification only uses cities facing more than one disaster that affected many cities. While for most regressions, this is not too restrictive, at times, this reduces the sample considerably. With this in mind, I also run specifications replacing d_{cr} by $d_c \times \mathbf{1}(r \geq 0)$: that is, instead of controlling for the full disaster response by a given city, I allow the city to respond to disasters only by having a pre and post response. This allows me to keep looking only at disasters affecting many cities, while still allowing me to keep cities that faced only one such disaster.

Table 3.15 shows results for this specification for grant requests. In fact, mayors who become politically connected to the Ministério da Integração Nacional do not request more grants neither before nor after the disasters.

While connected mayors are not requesting more grants after the

disaster, table 3.16 shows that after disasters, mayors becoming politically connected get more than double the number of requests approved, doubled probability that a single request is approved, 17% higher approved project sizes, 17% higher financing from the ministry and 13.3% higher matching grants.

We can also look only at the cities who made some proposal r periods after the event e. The results are presented in table 3.17. While the more demanding specification controling for d_{cr} has large estimated effects, they are imprecise due to the small sample size - to control for d_{cr}, the specification needs to keep only cities facing mroe than one disaster affecting many cities. Under the more parsimonious specification, controlling only for $d_c\mathbf{1}(r \geq 0)$, mayors becoming politically connected get, once again, more than double the number of requests approved, and more than double the probability of getting at least one request approved. The share of grant requests approved also more than doubles. Project sizes and financing by the ministry increase by approximately 131%, while matching funds increase by 96%.

In this sense, at least when looking at disaster prevention and response grants, the estimated effects of political connections are quantitatively similar to (if not slightly larger than) the effect estimated for overall grants. This is the case despite the fact that there is less margin for partisan disagreements to drive the effect of connections for these grants. Similarly, this is the case despite the fact that I control for many factors that could drive differences in information between connected and unconnected mayors: namely, 1) many factors shifting local demands for these grants, and 2) an important factor in the approval decision by the ministry, the recognition that the city faced the disaster.

3.3.3 Partisan agreement and information: ministerial learning

How much does this finding of favouritism generalize to other types of grants from other ministries? That is, when looking at other types of grants, information to play more of a role?

While the lack of performance measures for most grants prevents me from fully answering this question, I can check whether current ministers learn information from past ministers. In particular, if ministers know better which connected mayors are competent to deliver projects from

that ministry, past grant concessions should indicate to current ministers that a particular mayor is competent. With this in mind, we can check whether mayors that are currently unconnected but were connected in the past request more and whether they get more grants. More precisely, I run:

$$y_{cmt} = \beta s_{cmt} + \beta_{past} s_{cm(0)-(t-1)}(1 - s_{mt}) + d_{cmT} + d_{ct} + d_{mt} + \epsilon_{cmt}$$

where $s_{cm(0)-(t-1)}$ indicates whether the mayor in period t was connected in any of the previous years of his current term. With this in hands, β still indicates the effect of current connections, while β_{past} indicates the effect of past connections for a mayor given that he is currently unconnected.

Table 3.18 show the results for the effects of past connections over the currently unconnected in terms of grant requests. The results suggest that the effect of current connections becomes stronger: mayors becoming connected due to national level changes increase their number of requests by 15% relative to baseline, the probability of at least one request by 10% relative to baseline, the size of project requested by 20.3%, the amount of money requested from the ministry by 20.3%, and the amount of money to copay by 15.4%. At the same time, past connections never have a statistically significant effect over current grant requests.

Table 3.19 shows results for the effects of past connections when currently unconnected over grants approved. Once again, the effects of current connections become, if anything, stronger: current connections increase the number of grants received by 40.9% relative to baseline, increase the probability of getting at least one grant by 27.1% relative to baseline, the share of grant requests approved by 6.5% relative to baseline, the amounts of money approved for the project by 17.6% and the amounts coming from the ministry by 17.5%. Once again, past connections have no statistically significant effect over the unconnected, and the point estimates suggest an effect of past connections amounting to at most 1/3 of the effect of current connections.

This suggests that current ministers aren't learning from past ministers about the competence of mayors to deliver ministry-specific projects. If the effects of connections were driven by information, and learning about mayoral competence could happen across ministers from different parties - say, by observing past grant allocations - this would imply that connected mayors receive more money due to information effects, and

they keep receiving more money after they lose connections. Instead, there is no evidence of this in the data.

3.3.4 Differences in enforcement

An alternative explanation for the results is that political connections might bring extra sources of enforcement of effort by the mayors. If this is the case, ministries could be justified to approve more grants to connected mayors.

While I cannot provide evidence on all forms of enforcement of effort, I can provide evidence on at least two important dimensions of effort: first, I can check whether ministers in fact audit connected mayors more often or less often. This is one tool the ministry can use to discipline mayors, and they might need to use it less often on connected mayors if connected mayors are expected to be more disciplined. Secondly, I can check the actual audit outcomes to see whether connected mayors are more likely to spend the money according to plan and without corruption. Finally I can provide evidence on whether connected mayors take different times to complete their projects.

Table 3.20 runs the following specification using the data at the level of approved grant requests, looking at audits data:

$$y_{icmt} = s_{cmt}\beta + d_{cmT} + d_{ct} + d_{mt} + \epsilon_{icmt}$$

Once again, while there are 81799 grants approved during the sample period, the specification above only uses observations from city-ministry pairs that have more than one grant approved within the mayoral term.

The results show, first of all, how often grant requests are audited and how often they show irregularities. In fact, among unconnected mayors, 59.1% of them never get audited. For around 10.5% of the audited grants, we don't observe any results because audits are still under way. For 87.5% of the audited projects from unconnected mayors, the accounts are either approved or approved with reservations. Only 2% of the audits reject the project's reports due to irregularities - and 65% of these rejected reports face appeals from mayors, requesting the result to be reviewed. That is, among unconnected mayors, while there is a reasonable chance of being audited, only a very small fraction of audits report irregularities.

Moreover, connected and unconnected mayors do not face differences

in their audit probabilities. The results show not only that the differences in audit probabilities are not statistically significant, but also that they are quantitatively small and precisely estimated: under these standard errors, a 2.6p.p. (or 5%) difference in audit probabilities would have been detected as statistically significant. This show that while there might be other methods for ministers to differentially enforce effort from connected and unconnected mayors, audits are not being used as a method to differentially enforce effort from connected and unconnected mayors.

Similarly, there are no statistically significant differences between connected and unconnected mayors in the probabilities that the audit gets finished, that the audit rejects the mayoral accounts, nor that the audited accounts are accepted. All that can be observed is a 1p.p. (or 3.2% of audits) difference in probabilities of accounts accepted with reservations, significant only at the 10% level. I interpret this as showing that there are only small and non-robust differences between connected and unconnected mayors in terms of probabilities of irregularities detected.

Table 3.21 looks at results on time to completion of a project, using the same specifications used for the audit results. The first column shows that while on average, non connected mayors take 1071 days to complete a project, mayors becoming politically connected at the time of request take 16 days less to complete their projects. These results are not statistically significant, and estimated precisely. These results could be due to connected mayors getting approvals for projects that are more complex and take longer to complete. With this in mind, columns 2 and 3 look instead at time to completion relative to other projects in the same budgetary program. [5] Once again, the results indicate that mayors becoming politically connected do not take longer or shorter times to complete their projects than the average time it takes to complete the type of project they are implementing.

3.3.5 Overall differences in grant characteristics

Are the grants requested by connected mayors inherently different? This section shows evidence on inherent differences in grant requests as measured by the bureaucratic procedures the grant requests go through.

Table 3.22 provides evidence on this using the same specification as

[5] While on column 2, the sample keeps all projects, column 3 only keeps budget programs that approved more than one project.

above, in the data of all grant requests. The evidence is largely mixed here. There are no statistically significant differences in the probability that the ministry analyses the grants of connected and unconnected mayors, and the point estimates are small and precisely estimated. There are also no statistically significant differences in the probability of requests for amendments nor in the number of amendments made to the grant request. Naturally, connected mayors still get the work-plan for their grant requests approved more often - a necessary step for a grant to be eventually approved. While there are statistically significant differences in the time till grant approval, these differences are relatively small: among those with approved projects, connected mayors get projects approved 6 days quicker than unconnected mayors, which represents an improvement of 4.1% in the time till approval.

3.4 Conclusion

This section has documented a strong and robust effect of political connections over the allocation of funds to finance local public goods in Brazil: mayors becoming politically connected due to a ministerial change get 14.8% more money approved for local public goods. To which extent does this represent a distortion in the allocation of public goods? More precisely, is this driven by favouritism, or is it due to political connections allowing for information transmission, political agreement, enforcement of effort, or inherently different grant requests?

I first showed that in a particular case of disaster prevention and relief grants, political connections still increase the amount of money received after a disaster by 17%. That is, even after controlling for many of the factors generating differences in information or partisan agreement for a particular type of grant, the magnitude of the effect of political connections doesn't change, or even slightly increases. With this in mind, it is unlikely that the effects of political connections over the allocation of grants are only due to differences in information or partisan agreement. To some extent, this result extends to other grants: if there were differences in information about, for instance, the mayoral competence at delivering the projects from a given ministry, there should be learning over time, and mayors who are currently unconnected but were connected in the past should also get more grants. Yet, there is no evidence that

this happens, and instead, the effects of current connections become, if anything, stronger.

I also showed evidence that, to the extent that differences in enforcement should be reflected in audit outcomes and time to completion, there is no evidence suggesting that political connections bring differences in enforcement. Similarly, there is scarce evidence on inherent differences in grant requests from connected and unconnected mayors - other than the fact that connected mayors request more money, and get approvals more often.

I take this as evidence that political connections affect grant allocations and that, to some extent, this must reflect favouritism.

Tables

Table 3.1: Requests for matching grants by ministry-municipality-year

	# requests	> 0 Request	Log $	Log $, Ministry	Log $, Copay
$same\_party_{cmTt}$	0.066	0.010	0.150	0.150	0.122
	(0.029)**	(0.005)*	(0.076)*	(0.076)*	(0.061)*
R^2	0.67	0.67	0.68	0.68	0.69
N	1,148,238	1,148,238	1,148,238	1,148,238	1,148,238
Avg. y, unconnected	0.420	0.151	2427822	2315878	113190
Municip.-year FE	YES	YES	YES	YES	YES
Ministry-year FE	YES	YES	YES	YES	YES
Mun.-min.-term FE	YES	YES	YES	YES	YES

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

Table 3.2: Approvals of matching grants by ministry-municipality-year

	# Grants	> 0 Grant	Log \$	Log \$, Ministry	Log \$, Copay
$same\_party_{cmTt}$	0.023	0.011	0.148	0.147	0.114
	(0.010)**	(0.005)**	(0.067)**	(0.066)**	(0.051)**
R^2	0.54	0.51	0.51	0.51	0.52
N	1,148,238	1,148,238	1,148,238	1,148,238	1,148,238
Avg. y, unconnected	0.066	0.048	667853.329	607495.678	61226.959
Municip.-year FE	YES	YES	YES	YES	YES
Ministry-year FE	YES	YES	YES	YES	YES
Mun.-min.-term FE	YES	YES	YES	YES	YES

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

Table 3.3: Approvals of matching grants by ministry-municipality-year among those requesting

	No. Grants	Some grant	% approv.	Appr. money	Grant	Match. grant	Spent
$same\_party_{cmTt}$	0.110	0.037	0.012	0.531	0.529	0.428	0.406
	(0.016)***	(0.008)***	(0.005)**	(0.115)***	(0.114)***	(0.097)***	(0.084)***
R^2	0.67	0.65	0.68	0.66	0.66	0.66	0.65
N	137,936	137,936	137,936	137,936	137,936	137,936	137,936
Avg. y, unconnected	0.433	0.316	0.201	12.823	12.732	9.670	12.476
Municip.-year FE	YES	YES	YES	YES	YES	YES	YES
Ministry-year FE	YES	YES	YES	YES	YES	YES	YES
Mun.-min.-term FE	YES	YES	YES	YES	YES	YES	YES

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

Table 3.4: Matching grants by ministry-municipality-year, sample: population below 10677

	# Requests	Log $, req.	# Grants	Log $, Grants	Spent
$same\_party_{cmTt}$	0.055	0.160	0.035	0.214	0.152
	(0.024)**	(0.082)*	(0.016)**	(0.120)*	(0.135)
R^2	0.69	0.68	0.65	0.66	0.66
N	573,601	573,601	59,264	59,264	59,264
Avg. y, unconnected	0.340	1387119.41	0.356	362481.55	277909.34
Sample	All	All	Some prop.	Some prop.	Some prop.
Municip.-year FE	YES	YES	YES	YES	YES
Ministry-year FE	YES	YES	YES	YES	YES
Mun.-min.-term FE	YES	YES	YES	YES	YES

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

Table 3.5: Matching grants by ministry-municipality-year, sample: always in national govt. coalition

	# Requests	Log $, req.	# Grants	Log $, Grants	Spent
$same\_party_{cmTt}$	0.094	0.193	0.106	0.518	0.437
	(0.038)**	(0.090)**	(0.015)***	(0.125)***	(0.097)***
R^2	0.68	0.69	0.65	0.65	0.65
N	572,782	572,782	74,682	74,682	74,682
Avg. y, unconnected	0.429	2456170.35	0.451	706964.42	357907.67
Municip.-year FE	YES	YES	YES	YES	YES
Ministry-year FE	YES	YES	YES	YES	YES
Mun.-min.-term FE	YES	YES	YES	YES	YES

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

Table 3.6: Matching grants by ministry-municipality-year, per party

	# Requests	Request $	# Grants	Appr. $	Spent
$same\_party_{cmTt}$, PMDB	0.070	0.115	0.099	0.581	0.512
	(0.035)*	(0.086)	(0.024)***	(0.129)***	(0.100)***
$same\_party_{cmTt}$, PSDB	-0.023	0.106	0.185	0.590	-0.391
	(0.035)	(0.177)	(0.072)**	(0.405)	(0.340)
$same\_party_{cmTt}$, PT	0.039	-0.025	0.062	-0.087	-0.034
	(0.044)	(0.059)	(0.039)	(0.328)	(0.287)
$same\_party_{cmTt}$, PP	0.144	0.869	0.236	0.965	0.870
	(0.098)	(0.382)**	(0.077)***	(0.348)**	(0.293)***
$same\_party_{cmTt}$, PSB	0.231	0.745	0.144	1.390	0.978
	(0.092)**	(0.257)***	(0.047)***	(0.330)***	(0.253)***
$same\_party_{cmTt}$, Others	0.070	0.276	0.155	1.171	0.230
	(0.036)*	(0.139)*	(0.059)**	(0.439)**	(0.547)
N	1,148,238	1,148,238	137,936	137,936	137,936
P-val., joint signif.	0.080	0.030	0.000	0.000	0.000
P-val., no heter.	0.069	0.020	0.201	0.116	0.049
Avg. y, unconnected	0.420	2427822.35	0.433	667853.33	324090.18
Sample	All	All	Some prop.	Some prop.	Some prop.
Municip.-year FE	YES	YES	YES	YES	YES
Ministry-year FE	YES	YES	YES	YES	YES
Mun.-min.-term FE	YES	YES	YES	YES	YES

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

Table 3.7: Common trends: Grant requests

	# Requests	> 0 requests	Log $	Log $, Ministry	Log $, Copay
Change connect., 2nd yr.					
Year 2	0.094	0.005	0.099	0.097	0.098
	(0.049)*	(0.006)	(0.078)	(0.077)	(0.065)
Year 3	0.175	0.027	0.399	0.398	0.313
	(0.076)**	(0.011)**	(0.159)**	(0.159)**	(0.126)**
Year 4	0.181	0.024	0.364	0.363	0.298
	(0.081)**	(0.009)***	(0.128)***	(0.127)***	(0.108)**
Change connect, 3rd yr.					
Year 2	-0.002	0.000	0.000	0.000	0.018
	(0.039)	(0.006)	(0.085)	(0.084)	(0.074)
Year 3	0.089	0.013	0.196	0.196	0.167
	(0.056)	(0.011)	(0.160)	(0.160)	(0.134)
Year 4	0.116	0.017	0.257	0.256	0.213
	(0.062)*	(0.013)	(0.179)	(0.178)	(0.146)
Change connect, 4th yr.					
Year 2	0.026	0.005	0.068	0.068	0.058
	(0.019)	(0.003)	(0.045)	(0.045)	(0.035)
Year 3	0.029	0.001	0.021	0.021	0.039
	(0.017)*	(0.006)	(0.079)	(0.079)	(0.058)
Year 4	0.048	0.008	0.121	0.120	0.110
	(0.019)**	(0.006)	(0.076)	(0.075)	(0.057)*
R^2	0.67	0.67	0.68	0.68	0.69
N	1,148,238	1,148,238	1,148,238	1,148,238	1,148,238
P-value, pre-trends	0.401	0.183	0.170	0.169	0.223
P-value, post-trends	0.085	0.088	0.067	0.068	0.040
Avg. y, unconnected	0.420	0.151	2427822.35	2315877.64	113189.98
Municip.-year FE	YES	YES	YES	YES	YES
Ministry-year FE	YES	YES	YES	YES	YES
Mun.-min.-term FE	YES	YES	YES	YES	YES

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

Table 3.8: Common trends: Grant approvals

	# Grants	> 0 grant	Share approv.	Log $	Log $, ministry	Log $, copay
Change connect, 2nd yr.						
Year 2	0.027	0.016	0.213	0.211	0.175	0.201
	(0.013)*	(0.008)**	(0.104)*	(0.104)*	(0.083)**	(0.105)*
Year 3	0.042	0.024	0.318	0.317	0.248	0.277
	(0.020)**	(0.011)**	(0.153)**	(0.152)**	(0.120)**	(0.142)*
Year 4	0.048	0.026	0.352	0.350	0.270	0.336
	(0.023)**	(0.012)**	(0.166)**	(0.166)**	(0.129)**	(0.175)*
Change connect, 3rd yr.						
Year 2	0.009	0.007	0.087	0.086	0.070	0.097
	(0.017)	(0.006)	(0.078)	(0.077)	(0.064)	(0.086)
Year 3	0.036	0.019	0.250	0.248	0.192	0.181
	(0.020)*	(0.011)*	(0.148)	(0.147)	(0.115)	(0.111)
Year 4	0.044	0.023	0.303	0.301	0.235	0.249
	(0.019)**	(0.010)**	(0.130)**	(0.129)**	(0.103)**	(0.108)**
Change connect, 4th yr.						
Year 2	0.011	0.006	0.074	0.073	0.054	0.080
	(0.008)	(0.004)	(0.052)	(0.051)	(0.040)	(0.049)
Year 3	0.015	0.007	0.092	0.091	0.072	0.104
	(0.007)**	(0.004)*	(0.053)*	(0.052)*	(0.040)*	(0.052)*
Year 4	0.022	0.010	0.132	0.132	0.101	0.104
	(0.011)*	(0.005)*	(0.074)*	(0.073)*	(0.052)*	(0.049)**
R^2	0.54	0.51	0.51	0.51	0.52	0.50
N	1,148,238	1,148,238	1,148,238	1,148,238	1,148,238	1,148,238
P-value, pre-trends	0.130	0.335	0.338	0.339	0.234	0.202
P-value, post-trends	0.121	0.016	0.017	0.017	0.034	0.072
Avg. y, unconnected	0.066	0.048	211198.31	192111.14	19299.50	103003.24
Municip.-year FE	YES	YES	YES	YES	YES	YES
Ministry-year FE	YES	YES	YES	YES	YES	YES
Mun.-min.-term FE	YES	YES	YES	YES	YES	YES

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

Table 3.9: Common trends: grant approvals among those requesting

	# Grants	> 0 grant	Share approv.	Log $	Log $, ministry	Log $, copay	spent
Change connect, 2nd yr.							
Year 2	0.103	0.061	0.020	0.845	0.838	0.723	0.866
	(0.063)	(0.027)**	(0.017)	(0.361)**	(0.356)**	(0.303)**	(0.281)***
Year 3	0.217	0.096	0.046	1.328	1.318	1.080	1.525
	(0.050)***	(0.021)***	(0.015)***	(0.288)***	(0.286)***	(0.227)***	(0.228)***
Year 4	0.195	0.099	0.019	1.382	1.367	1.152	1.560
	(0.047)***	(0.028)***	(0.019)	(0.361)***	(0.359)***	(0.281)***	(0.296)***
Change connect, 3rd yr.							
Year 2	0.004	0.018	0.013	0.221	0.219	0.181	0.267
	(0.050)	(0.014)	(0.009)	(0.189)	(0.185)	(0.170)	(0.228)
Year 3	0.125	0.051	0.020	0.710	0.706	0.581	0.558
	(0.037)***	(0.018)***	(0.011)*	(0.241)***	(0.238)***	(0.203)***	(0.191)***
Year 4	0.147	0.051	0.011	0.742	0.737	0.608	0.749
	(0.043)***	(0.019)**	(0.011)	(0.252)***	(0.248)***	(0.218)**	(0.216)***
Change connect, 4th yr.							
Year 2	0.057	0.032	0.027	0.414	0.411	0.310	0.391
	(0.031)*	(0.016)*	(0.010)**	(0.211)*	(0.209)*	(0.184)	(0.169)**
Year 3	0.065	0.037	0.024	0.465	0.462	0.344	0.553
	(0.034)*	(0.017)**	(0.011)**	(0.229)*	(0.226)*	(0.176)*	(0.177)***
Year 4	0.107	0.046	0.036	0.634	0.633	0.461	0.529
	(0.029)***	(0.015)***	(0.014)**	(0.196)***	(0.196)***	(0.131)***	(0.177)***
R^2	0.67	0.65	0.68	0.66	0.66	0.66	0.66
N	137,936	137,936	137,936	137,936	137,936	137,936	137,936
P-value, pre-trends	0.214	0.177	0.045	0.198	0.196	0.272	0.039
P-value, post-trends	0.000	0.000	0.014	0.000	0.000	0.000	0.000
Avg. y, unconnected	0.433	0.316	0.201	211198.31	192111.14	19299.50	103003.24
Municip.-year FE	YES	YES	YES	YES	YES	YES	YES
Ministry-year FE	YES	YES	YES	YES	YES	YES	YES
Mun.-min.-term FE	YES	YES	YES	YES	YES	YES	YES

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

Table 3.10: Grant requests data: main specifications

	Log $, req.	Log $ req, min.	Log $ req, copay	Prob. approv.	Log $, approv	Log $ approv, min.	Log $ approv, Copay
$same\_party_{cmTt}$	0.034	0.035	0.049	0.011	0.008	0.017	-0.032
	(0.012)***	(0.012)***	(0.028)*	(0.004)***	(0.031)	(0.031)	(0.060)
R^2	0.46	0.46	0.55	0.38	0.66	0.66	0.72
N	480,779	480,779	480,779	480,779	51,562	51,562	51,562
Avg. y, unconnected	873817.951	833574.603	40243.348	0.156	487420.811	443404.074	44016.737
Municip.-year FE	YES	YES	YES	YES	YES	YES	YES
Ministry-year FE	YES	YES	YES	YES	YES	YES	YES
Mun.-min.-term FE	YES	YES	YES	YES	YES	YES	YES

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

Table 3.11: Summary of results, baseline two-dimensional measure of ideology from roll-call voting data

	# Requests	Log $ req.	# Grants	Log $ approv.
$same\_party_{cmTt}$	0.095	0.250	0.033	0.228
	(0.040)**	(0.114)**	(0.014)**	(0.093)**
I^0_{cmt}	0.018	-0.011	0.001	0.007
	(0.015)	(0.052)	(0.007)	(0.052)
I^1_{cmt}	0.019	0.301	0.025	0.232
	(0.064)	(0.167)*	(0.014)*	(0.107)**
R^2	0.68	0.69	0.56	0.53
N	922,609	922,609	922,609	922,609
Avg. y, non-aligned	0.420	2427822.351	0.066	667853.329
Municipality-year FE	YES	YES	YES	YES
Ministry-year FE	YES	YES	YES	YES
Municip.-ministry-term FE	YES	YES	YES	YES

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

Table 3.12: Summary of results, measure of ideology from structural model for roll call voting data

	# Requests	Log $ req.	# Grants	Log $ approv.
$same\_party_{cmTt}$	0.097	0.274	0.034	0.238
	(0.040)**	(0.122)**	(0.014)**	(0.096)**
I^0_{cmt}	0.006	0.069	0.007	0.072
	(0.022)	(0.044)	(0.003)**	(0.033)**
I^1_{cmt}	0.009	0.027	0.001	0.007
	(0.006)	(0.025)	(0.002)	(0.014)
R^2	0.68	0.69	0.56	0.53
N	922,609	922,609	922,609	922,609
Avg. y, non-aligned	0.420	2427822.351	0.066	667853.329
Municipality-year FE	YES	YES	YES	YES
Ministry-year FE	YES	YES	YES	YES
Municip.-ministry-term FE	YES	YES	YES	YES

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

Table 3.13: Summary of results, measure of ideology from Power and Zucco (2011)

	# Requests	Log $ req.	# Grants	Log $ approv.
$same\_party_{cmt}$	0.079	0.188	0.024	0.152
	(0.036)**	(0.087)**	(0.010)**	(0.067)**
I^{PZ}_{cmt}	-0.000	0.018	-0.001	-0.000
	(0.008)	(0.041)	(0.005)	(0.030)
R^2	0.68	0.70	0.57	0.54
N	857,498	857,498	857,498	857,498
Avg. y, non-aligned	0.420	2427822.351	0.066	667853.329
Municipality-year FE	YES	YES	YES	YES
Ministry-year FE	YES	YES	YES	YES
Municip.-ministry-term FE	YES	YES	YES	YES

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

Table 3.14: Summary of results, IV for measures of ideology

	# Requests	Log $ req.	# Grants	Log $ approv.
$same\_party_{cmt}$	0.084	0.247	0.028	0.201
	(0.041)*	(0.122)*	(0.013)**	(0.090)**
I^0_{cmt}	0.009	0.085	0.012	0.115
	(0.037)	(0.111)	(0.009)	(0.059)*
I^1_{cmt}	-0.000	0.111	-0.010	-0.055
	(0.060)	(0.342)	(0.038)	(0.231)
R^2	0.68	0.70	0.57	0.54
N	857,498	857,498	857,498	857,498
Avg. y, non-aligned	0.420	2427822.351	0.066	667853.329
Municipality-year FE	YES	YES	YES	YES
Ministry-year FE	YES	YES	YES	YES
Municip.-ministry-term FE	YES	YES	YES	YES

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

Table 3.15: Disaster grants by ministry-municipality-year: grant requests

	# Requests	> 0 Requests	Log $	Log $, ministry	Log $, Copay
s_{cer}× pre	0.129	0.018	0.301	0.300	0.246
	(0.101)	(0.019)	(0.270)	(0.270)	(0.203)
s_{cer}× post	0.018	0.002	0.027	0.027	0.012
	(0.046)	(0.011)	(0.158)	(0.157)	(0.115)
o_{cer}× pre	0.002	0.008	0.101	0.101	0.071
	(0.015)	(0.003)**	(0.047)**	(0.047)**	(0.034)**
o_{cer}×post	0.001	0.001	0.011	0.011	0.007
	(0.006)	(0.002)	(0.025)	(0.025)	(0.016)
R^2	0.72	0.73	0.73	0.73	0.74
N	62,595	62,595	62,595	62,595	62,595
Avg. y, unconnected, pre-disast.	0.704	0.260	6957760.748	6574199.812	383560.936
Avg. y, unconnected, post-disast.	0.370	0.153	8804951.192	8304860.934	500090.258
Municip.-post disast. FE	YES	YES	YES	YES	YES
Event-yr. from disast. FE	YES	YES	YES	YES	YES
Munic.-event FE	YES	YES	YES	YES	YES

	# Requests	> 0 Requests	Log $	Log $, ministry	Log $, Copay
s_{cer}×pre	0.175	0.004	0.142	0.142	0.138
	(0.140)	(0.024)	(0.335)	(0.334)	(0.247)
s_{cer}×post	-0.027	0.004	0.045	0.044	0.022
	(0.057)	(0.013)	(0.186)	(0.186)	(0.132)
o_{cer}×pre	0.007	0.006	0.073	0.073	0.051
	(0.016)	(0.003)*	(0.048)	(0.047)	(0.035)
o_{cer}×post	-0.001	0.000	-0.004	-0.004	-0.006
	(0.009)	(0.002)	(0.036)	(0.036)	(0.024)
R^2	0.79	0.81	0.82	0.82	0.82
N	56,083	56,083	56,083	56,083	56,083
Avg. y, unconnected, pre-disast.	0.704	0.260	6957760.748	6574199.812	383560.936
Avg. y, unconnected, post-disast.	0.370	0.153	8804951.192	8304860.934	500090.258
Municip.-yr from disast. FE	YES	YES	YES	YES	YES
Event-yr. from disast. FE	YES	YES	YES	YES	YES
Munic.-event FE	YES	YES	YES	YES	YES

Table 3.16: Disaster grants by ministry-municipality-year: grant approvals

	# Grants	> 0 Grant	Log $	Log $, Ministry	Log $, Copay
s_{cer}×pre	0.014	0.018	0.246	0.245	0.178
	(0.011)	(0.009)*	(0.126)*	(0.126)*	(0.094)*
s_{cer}×post	0.014	0.011	0.165	0.164	0.131
	(0.005)***	(0.004)***	(0.054)***	(0.053)***	(0.042)***
o_{cer}×pre	0.002	0.002	0.023	0.023	0.015
	(0.001)	(0.001)*	(0.014)	(0.014)	(0.011)
o_{cer}×post	-0.000	-0.000	-0.003	-0.003	-0.003
	(0.001)	(0.000)	(0.005)	(0.005)	(0.004)
R^2	0.62	0.62	0.62	0.62	0.62
N	62,595	62,595	62,595	62,595	62,595
Avg. y, unconnected, pre-disast.	0.021	0.019	109849.309	101236.903	8612.406
Avg. y, unconnected, post-disast.	0.013	0.012	106206.990	99164.114	7042.876
Municip.-post disast. FE	YES	YES	YES	YES	YES
Event-yr. from disast. FE	YES	YES	YES	YES	YES
Munic.-event FE	YES	YES	YES	YES	YES

	# Grants	> 0 Grant	Log $	Log $, Ministry	Log $, Copay
s_{cer}×pre	0.018	0.019	0.255	0.255	0.181
	(0.012)	(0.010)**	(0.128)**	(0.128)**	(0.096)*
s_{cer}×post	0.015	0.012	0.170	0.170	0.133
	(0.005)***	(0.005)***	(0.062)***	(0.062)***	(0.047)***
o_{cer}×pre	0.003	0.003	0.036	0.036	0.025
	(0.001)**	(0.001)***	(0.014)***	(0.014)***	(0.011)**
o_{cer}×post	0.001	0.001	0.012	0.012	0.007
	(0.001)	(0.001)	(0.009)	(0.009)	(0.007)
R^2	0.75	0.74	0.74	0.74	0.75
N	56,083	56,083	56,083	56,083	56,083
Avg. y, unconnected, pre-disast.	0.021	0.019	109849.309	101236.903	8612.406
Avg. y, unconnected, post-disast.	0.013	0.012	106206.990	99164.114	7042.876
Municip.-yr from disast. FE	YES	YES	YES	YES	YES
Event-yr. from disast. FE	YES	YES	YES	YES	YES
Munic.-event FE	YES	YES	YES	YES	YES

Table 3.17: Disaster grants by ministry-municipality-year: grant approvals among those requesting

	# Grants	> 0 Grants	% Approv	Log $	Log $, Ministry	Log $, Copay
s_{cer}×pre	-0.043	0.002	0.003	-0.067	-0.065	-0.088
	(0.060)	(0.044)	(0.014)	(0.572)	(0.570)	(0.431)
s_{cer}×post	0.108	0.093	0.053	1.311	1.313	0.959
	(0.040)***	(0.036)**	(0.022)**	(0.501)***	(0.498)***	(0.386)**
o_{cer}×pre	-0.031	-0.033	-0.011	-0.456	-0.453	-0.368
	(0.022)	(0.019)*	(0.009)	(0.260)*	(0.258)*	(0.206)*
s_{cer}×post	-0.016	-0.018	-0.004	-0.262	-0.261	-0.214
	(0.014)	(0.011)*	(0.006)	(0.150)*	(0.149)*	(0.117)*
R^2	0.70	0.74	0.73	0.74	0.74	0.74
N	5,435	5,435	5,435	5,435	5,435	5,435
Avg. y, unconnected, pre-disast.	0.083	0.073	0.035	109849.309	101236.903	8612.406
Avg. y, unconnected, post-disast.	0.085	0.075	0.040	106206.990	99164.114	7042.876
Municip.-post disast. FE	YES	YES	YES	YES	YES	YES
Event-yr. from disast. FE	YES	YES	YES	YES	YES	YES
Munic.-event FE	YES	YES	YES	YES	YES	YES

	# Grants	> 0 Grants	% Approv	Log $	Log $, Ministry	Log $, Copay
s_{cer}×pre	-0.004	-0.001	0.010	-0.107	-0.110	-0.058
	(0.051)	(0.048)	(0.014)	(0.703)	(0.700)	(0.550)
s_{cer}×post	0.246	0.173	0.155	2.612	2.643	1.818
	(0.200)	(0.164)	(0.119)	(2.320)	(2.308)	(1.828)
o_{cer}×pre	0.002	-0.008	-0.007	-0.089	-0.088	-0.068
	(0.017)	(0.017)	(0.006)	(0.230)	(0.228)	(0.177)
o_{cer}×post	0.018	0.006	0.000	0.080	0.080	0.048
	(0.014)	(0.012)	(0.004)	(0.169)	(0.168)	(0.131)
R^2	0.92	0.92	0.91	0.92	0.92	0.92
N	3,499	3,499	3,499	3,499	3,499	3,499
Avg. y, unconnected, pre-disast.	0.083	0.073	0.035	109849.309	101236.903	8612.406
Avg. y, unconnected, post-disast.	0.085	0.075	0.040	106206.990	99164.114	7042.876
Municip.-yr from disast. FE	YES	YES	YES	YES	YES	YES
Event-yr. from disast. FE	YES	YES	YES	YES	YES	YES
Munic.-event FE	YES	YES	YES	YES	YES	YES

Table 3.18: Learning about mayors from past ministers: Requests

	# Requests	> 0 Requests	Log $	Log $, Ministry	Log $ Copay
$same\_party_{cmt}$	0.064	0.014	0.203	0.203	0.154
	(0.027)**	(0.006)**	(0.082)**	(0.081)**	(0.063)**
Only past connections	-0.003	0.007	0.098	0.098	0.060
	(0.026)	(0.005)	(0.072)	(0.072)	(0.053)
R^2	0.67	0.67	0.68	0.68	0.69
N	1,148,238	1,148,238	1,148,238	1,148,238	1,148,238
Avg. y, never connected	0.421	0.152	364082.398	347157.647	16924.751
Municip.-year FE	YES	YES	YES	YES	YES
Ministry-year FE	YES	YES	YES	YES	YES
Mun.-min.-term FE	YES	YES	YES	YES	YES

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

Table 3.19: Learning about mayors from past ministers: Approvals

	# grants	>0 grants	% grants approv.	Log $	Log $, Ministry	Log $ Copay
$same\_party_{cmt}$	0.027	0.013	0.013	0.176	0.175	0.131
	(0.011)**	(0.005)**	(0.007)*	(0.066)**	(0.065)**	(0.049)**
Only past connections	0.008	0.004	0.001	0.052	0.052	0.031
	(0.007)	(0.004)	(0.015)	(0.051)	(0.051)	(0.038)
R^2	0.54	0.51	0.68	0.51	0.51	0.52
N	1,148,238	1,148,238	137,936	1,148,238	1,148,238	1,148,238
Avg. y, never connected	0.066	0.048	0.200	31913.768	28995.418	2918.350
Municip.-year FE	YES	YES	YES	YES	YES	YES
Ministry-year FE	YES	YES	YES	YES	YES	YES
Mun.-min.-term FE	YES	YES	YES	YES	YES	YES

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

Table 3.20: Grants requests data: auditing

	Not audited	Not finalized	Approved	Approv. w/ reserv.	Reject	Reject, appeal
same_party	-0.010	0.006	0.013	-0.010	0.003	0.003
	(0.013)	(0.009)	(0.012)	(0.005)*	(0.004)	(0.002)
R^2	0.75	0.53	0.75	0.49	0.52	0.49
N	51,562	51,562	51,562	51,562	51,562	51,562
Avg. y, non-aligned	0.591	0.052	0.333	0.025	0.008	0.005
Sample	Approved grants	Approved grants	Approved grants	Approved grants	Approved grants	Approved grants
Municip.-year FE	YES	YES	YES	YES	YES	YES
Ministry-year FE	YES	YES	YES	YES	YES	YES
Mun.-min.-term FE	YES	YES	YES	YES	YES	YES

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

Table 3.21: Grants requests data: time to completion

	Time to compl.	Time relat. to avg	Time relat. to avg
same_party	-16.023	-1.875	-0.906
	(21.323)	(13.014)	(13.448)
R^2	0.67	0.59	0.60
N	51,562	51,562	45,590
Avg. y, unconnected	1071.228	1.294	1.397
Sample	Approved	Approved	Appr., similar grants
Municip.-year FE	YES	YES	YES
Ministry-year FE	YES	YES	YES
Mun.-min.-term FE	YES	YES	YES

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

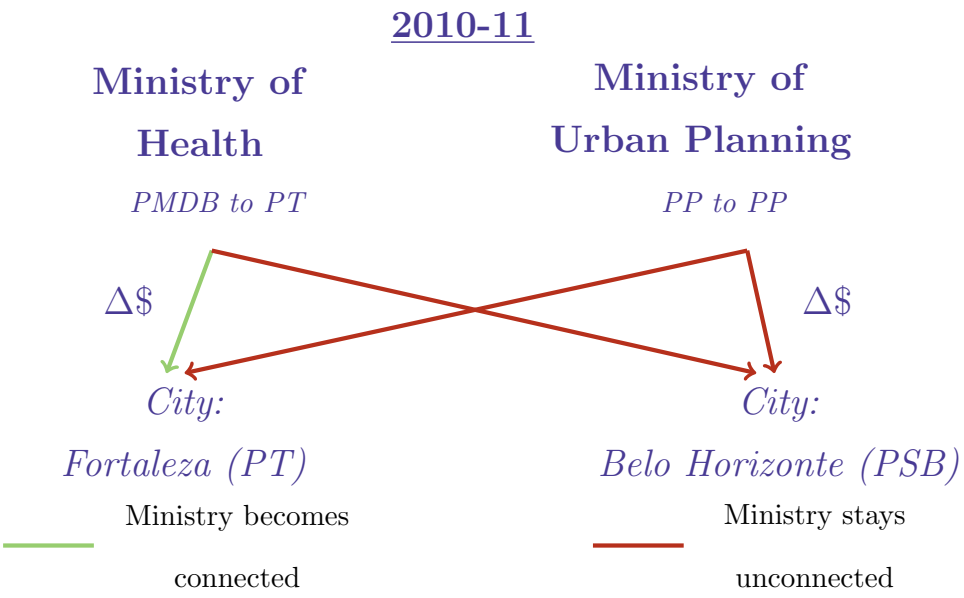
Table 3.22: Grants requests data: bureaucratic process

	Proj. analysed	Workplan appr.	Ammend	# Ammend.	Duration	Duration
same_party	0.008	0.006	0.007	0.017	-2.435	-6.056
	(0.005)	(0.003)**	(0.005)	(0.011)	(0.954)**	(3.439)*
R^2	0.55	0.42	0.40	0.37	0.36	0.62
N	480,779	480,779	480,779	480,779	480,779	51,562
Avg. y, unconnected	0.481	0.185	0.244	0.480	335.436	148.478
Sample	All	All	All	All	All	Appr. req.
Municip.-year FE	YES	YES	YES	YES	YES	YES
Ministry-year FE	YES	YES	YES	YES	YES	YES
Mun.-min.-term FE	YES	YES	YES	YES	YES	YES

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

Figures

Figure 3.1: Identification strategy



4. The Welfare Costs of Political Connections

4.1 Introduction

A large body of evidence suggests public funds are often misallocated across regions: in particular, connected regions are often favoured and receive more funds than unconnected regions (the previous chapter of this thesis documents this for the Brazilian setting, further references include Hodler and Raschky (2015), Burgess et al (2015), Brollo and Nannicinni (2012)). Given these types of allocation often generate disagreements and conflicts between regions, this often brings demands for rules forcing uniformity and equality in policy making, and for non-partisan allocations.

Yet, these policies are often costly: it is unclear that non-partisan actors could be made accountable to people, and it is possible that different regions have different needs for funds that are hard to be captured by rules. With this in mind, what are the aggregate social welfare costs of political connections? Are they large enough to justify potentially costly policy interventions such as the ones above?

The answer to this question relies on, first of all, knowing how much connected cities gain and unconnected cities lose from connections-based allocations. In particular, this implies that we need to know, first of all, whether the social returns to public funds are decreasing, constant, or increasing - including both the returns of public funds in terms of local welfare, and any source of concavity of the welfare function due to inequality aversion. After all, take two cities that, without partisanship, would receive the same amount of funds and get same welfare from it. If the returns to public funds are decreasing, then the losses due to partisanship for unconnected cities end up being larger than the gains from

partisanship for connected cities, and the welfare losses are large. If returns are constant, the losses for unconnected cities are similar to the gains for connected cities, and the welfare losses are small. Second of all, we need to know the degree of heterogeneity in returns to public funds across cities.

If we knew that returns to funds were equal across regions, the shape of returns to public funds, and society's demands for equality across regions, this would be a trivial problem: it becomes a matter of simply computing how much connected cities gain and unconnected cities lose in terms of public funds, and aggregating welfare with a known welfare function. Yet, we often don't directly observe regions' relative demands for public funds, nor have clear measures of society's distaste for inequality. Even more, for many types of public goods - such as sports courts or public squares -, there are few direct measures of welfare that we can use to understand some of the relevant welfare parameters.

To answer this question, this chapter starts by building a model of grant requests by cities and approvals by ministries in a national government. Ministries behave as social planners who care about (i) the returns to grant size, (ii) unobservable differences in returns across cities/ministries, and (iii) who attach an extra welfare weight to politically connected cities - which I'll refer to as the ministerial bias. Cities then anticipate the ministry's propensity to accept a grant request, and decide on requests based on this. This model provides a way to translate the partial equilibrium effects of connections - namely, the differences between connected cities and unconnected cities holding fixed the set of cities that are connected - into an understanding of the amount of public funds connected cities gain vs. unconnected cities lose from political connections.

I argue that given knowledge of the model's parameters, one useful stance to take is to estimate the welfare losses from the viewpoint of a benchmark planner who fully agrees with the ministry, except for not favouring connected cities. This is informative, first of all, of how much a non-partisan/technocratic minister loses from political connections: if these losses turn out to be small, it is unlikely that a non-partisan government would want to prevent partisanship at the cost of creating conflict with potential political allies.

This benchmark planner also provides a lower bound on the welfare

costs of partisan governments to society. The reason for that is that if society doesn't care for political connections, but happens to be fully aligned with the ministry in all other factors, then the benchmark planner is tautologically providing us the social costs of political connections. If society, instead, has additional reasons to diverge from the national government, then the losses of delegating choices to a partisan national government must be larger.

With this benchmark planner serving as the welfare criterion, I then show that the gap in resources between connected and unconnected cities is typically not sufficient to answer the question of aggregate welfare losses. More precisely, I show that in the model, we can justify a given difference in resources between connected and unconnected cities with a model with (i) decreasing returns to grant size and large biases towards connected cities - when the welfare losses from connections would be large -, or (ii) with a model of close to constant returns to grant size and small biases to connected cities - when the welfare losses from connections would be small.

To see this, consider a ministry's with concave returns to grant size. As it becomes biased towards connected cities, connected cities increase their grant requests. Given marginal returns are decreasing, the returns for the ministry of approving a grant to this connected city fall, and cities cannot increase their request further without losing chances that the ministry will approve their request. Consequently, to justify a given difference in requests and approvals between connected and unconnected regions with concave returns, we need relatively large biases towards connected cities. As more funds go to connected cities, this increases the shadow cost of funds for the ministry, who gets then a lower marginal benefit of approving grants to unconnected cities. The first panel of figure 4.1 displays this intuition graphically, together with the fact that under concave returns, the loss in welfare for unconnected cities (the yellow area) turns out to be larger than the gain in welfare for connected cities (the green area).

Assume instead that the returns to grant size are close to linear. Once the ministry becomes even slightly biased towards connected cities, connected cities can increase their grant requests, and because the marginal returns to grant size are close to constant, connected cities can keep increasing the size of their request without marginal returns

falling, and without losing much in terms of probability of approval of the grant request. As a consequence, when marginal returns are close to constant (non-decreasing), we can justify large gaps in resources between connected and unconnected cities even with small biases towards connected cities. Yet, the welfare losses from the viewpoint of the benchmark planner here are small, since the returns to funds for connected and unconnected cities are approximately the same. The second panel of figure 4.1 illustrastes this graphically, also showing that the loss in welfare for unconnected cities (the yellow area) is close to the gain in welfare for connected cities (the green area).

The key challenge, then, is to identify the ministerial bias towards connected cities and the concavity of ministerial returns to funds separately. I do so by exploring rich available data on grant requests from Brazilian municipalities and approvals of these requests by national level ministries between 2009-16. As the intuition above suggests, when the returns to grant size are concave, larger grant requests reduce the probability that the ministry will accept it; while if returns are linear, larger grant requests won't have an effect over the probability that the ministry will accept a request. Estimating this response of probability of approval to project size, jointly with the effect of connections over the probability of approval, is then what allows me to answer the question of interest.

With this in hands, I show that in fact, the ministerial returns to grant size are close to linear, and biases towards connected cities are relatively small. As a consequence, the welfare costs of political connections for the benchmark ministry - that is, when political connections are the only source of distortion in the allocation of public funds across regions - are of the order of 0.24% of the typical ministerial budget for grants.

This chapter is related to the literature on the effects of political connections over the allocation of public funds across regions (see Evans (2011), Knight (2005), Kriner and Reeves (2015) and Dynes and Huber (2015) for the U.S.; Franck and Rainer (2012), Kramon and Posner (2013), Hodler and Raschky (2014), Burgess et al (2015) and Kramon and Posner (2016) review evidence from developing countries; Sollé-Ollé and Sorribas-Navarro (2008) for grants in Spain and Brollo and Nannicinni (2012) for grants in Brazil). This chapter offers methods to aggregate the welfare losses associated with these connections-based allocations

In a related setting, Finan and Mazzocco (2017) study the consequences

of congressmen's electoral incentives over the allocation of pork across regions, and evaluate the welfare consequences of such electoral incentives using a structural model of pork allocation, politicians' decisions to run for re-election, and voters' choice to re-elect politicians. In their setting, maximizing welfare implies (i) sending more money to poorer regions and with higher estimated welfare weights, and (ii) reducing leakage in public funds as estimated in their structural model. Given their model's complexity, they need to estimate the model for only 4 regions. Relative to this, this chapter first provides a simpler model, with the potential to estimate welfare for an arbitrary number of cities (here, 5500 cities) while, as in Finan and Mazzocco (2017), allowing for flexible heterogeneity in the welfare weights across regions. Secondly, this chapter allows for the fact that we don't have good knowledge of the structure of the welfare function, instead providing an estimate of a plausible lower bound on the welfare costs of political connections. On one hand, this allows me to be more agnostic about details of the model that are hard to know, and allows me to express uncertainty in the welfare losses due to political connections due to lack of knowledge of the welfare function; on the other hand, the lack of knowledge of the welfare function gives a low lower bound on the costs of political connections in my setting.

This chapter also relates to a literature on fiscal federalism, in particular, on the difficulties of non-uniform central policy making (see Oates (1972), Persson and Tabellini (1996), Lockwood (2002), Besley and Coate (2003), Alesina, Angeloni and Etro (2005), Kessler (2014), Hoffmann et al (2017)). Relative to this literature, I provide methods to estimate costs of central government discretion (here, discretion awarded to ministries), and the resulting non-uniform policy making. In particular, here, I estimate one cost of such discretion, associated with connections-based allocations by central governments.

4.2 Model

In this section, I build a structural model where a ministry chooses to allocate grants as a planner, and gives higher welfare weights to grant requests coming from connected cities. Anticipating this, cities decide whether to turn their potential grants into grant requests depending on whether the probability of approval is high enough. I use this model

to evaluate equilibrium effects of removing connections, and of imposing policies constraining the ministry, and discuss what can be learned from the model in terms of welfare.

4.2.1 Set-up

Assume that a continuum of cities c have, each of them, a finite number of potential grants i to request from a given ministry m. Each grant has a size g_i, where g_i^m is funded by the ministry and $g_i^c = g_i - g_i^m$ is funded by the city.

If grant i is requested by the city, the ministry can either accept the request ($y_i = 1$), or reject it ($y_i = 0$). I normalize the payoffs from rejecting a request to 0, and assume that if the ministry accepts a request, it gets a utilitarian welfare component of $\tilde{\kappa}^{s_i}\theta_i u(g_i)$. In particular, s_i denotes whether the request came from a politically connected city, $\tilde{\kappa}$ is the extra welfare weight on connected cities, θ_i indexes other differences in welfare weights and local demands for the project, and $u(g_i)$ denotes how the ministry's welfare increases with the project size. At the same time, the ministry has a budget constraint for a given year (determined by the legislative bargaining process between the executive and the legislature) of funds that expire after that year. This imposes on the ministry a shadow cost of funds λ_m. With this in mind, the ministry's payoff from all requests is given by:

$$\int r_i y_i [\underbrace{\tilde{\kappa}^{s_i}}_{Favouritism} \underbrace{\theta_i u(g_i) - \lambda_m g_i^m}_{Remainder}] di$$

where $r_i = 1$ denotes that the request of potential grant i was made. In particular, as seen above, we can interpret the ministerial payoff as a mix of favouritism towards connected areas, and a remainder, capturing all other residual reasons motivating acceptance of grants by the ministry.

Anticipating the ministry's decision, cities decide whether to make requests ($r_i = 1$) or not ($r_i = 0$), the size of the grant g_i, and the amounts financed by the ministry and the city, g_i^m, g_i^c. To incorporate the real-world feature that there are constraints on how much cities need to finance themselves, I assume that cities face a constraint that $\frac{g_i^c}{g_i} \geq \gamma$. This assumption is consistent with the data, where values requested are always exactly equal to the values approved.

When the city gets its request approved, it gets a payoff of $\epsilon_i v(g_i)$. Given the city's budget constraint, the shadow cost of the funds spent by the city on the project are given by $\lambda_c g_i^c$. When the city makes N requests from ministry m, the cost from each request made is given by $\lambda_c F_{cmt}(N)$ - constant across requests, but not necessarily across cities, ministries and time.

To allow for the fact that many requests made are never accepted, I assume that the city cannot fully anticipate the ministry's decision. In particular, I assume that the city only knows that $\theta_i = \theta_{Ki}\theta_{Ui}$, where θ_{Ki} is known to the city and θ_{Ui} is unknown to the city. In particular, I assume that $\log \theta_{Ui} \sim H_\theta(\log \theta_{Ui})$, and that θ_{Ui} is independent of $\epsilon_i, F_{cmt}, \theta_{Ki}$: this assumption captures the idea that the city cannot use the variables it observes to predict θ_{Ui}.[1]

4.2.2 Welfare

Given that I only have information on the ministry's decisions and on the city's decisions, it is hard to evaluate society's welfare directly. With this in mind, I argue instead for the following indirect approach: first, I assume that connections are unrelated to welfare. This is consistent with the interpretation of the reduced form evidence that connections create favouritism.[2]

Still, this doesn't fully define citizen's welfare. Assume then, at first, that citizens welfare is given by the ministry's remainder $\theta_i u(g_i) - \lambda_m g_i^m$ - so that, other than due to connections, the ministerial welfare is fully aligned with society's welfare. If this is the case, once the government's primitives are estimated, we could easily estimate the welfare costs of political connections. We could also use this assumption to compute the welfare effects of policy counterfactuals imposing caps on transfers by ministries to given cities.

We could have instead that society's welfare is unrelated to the ministry's remainder. If this is the case, the ministerial discretion creates

[1]For a micro-foundation for this assumption, assume that the city observes a signal $z_i|\theta_i$ (in particular, let z_i be a vector including ϵ_i, F_{cmt} and extra information). We could take $\log \theta_i = E[\log \theta_i|z_i] + \log \theta_{Ui}$, with $E[\log \theta_{Ui}|z_i] = 0$, and $\log \theta_{Ki} = E[\log \theta_i|z_i]$. Further assumptions on the distribution of the signal, in turn, guarantee θ_{Ui} independent of z_i, and hence of $\theta_{Ki}, \epsilon_i, F_{cmt}$.

[2]The important part of this assumption is that connections affect society's welfare by less than it affects ministry's welfare. While this assumption could be relaxed, I maintain it throughout to simplify the exposition of the exercise.

even more distortions than under the assumption in the previous paragraph. Similarly, there would be even higher welfare gains from the policy counterfactual constraining the ministry.

With this in mind, the assumption that the ministry's remainder is fully aligned with welfare allows us to compute the welfare losses of political connections when this is the only distortion in the ministerial choices of allocation of funds across cities. Similarly, under this assumption, we get a plausible lower bound on the welfare gains from policies imposing constraints on the ministry's possible allocations.

I proceed, then, assuming that society's welfare is given by $\theta_i u(g_i) - \lambda_m g_i^m$.

4.2.3 Model solution

General solution: This model's solution can be easily characterized using backward induction. In the last stage, the ministry approves a grant request i if:

$$\tilde{\kappa}^{s_i}\theta_{Ki}\theta_{Ui} \geq \frac{\lambda_m g_i^m}{u(g_i)} \equiv \theta^{MG}$$

Anticipating this, a given city c makes a request on potential grant i if the fixed cost of requesting this grant is lower than the value function from that grant. In particular, this means that a request is made if:

$$\begin{aligned}\lambda_c F_{cmt}(N) \leq \lambda_c V\left(\frac{\epsilon_i}{\lambda_c}, \gamma, \theta_{Ki}\right) \\ \equiv \lambda_c \max_{\theta^{MG}, g_i, g_i^m} [1 - H(\log\theta^{MG} - \log\theta^{Ki} - s_i\kappa)] \left[\frac{\epsilon_i v(g_i)}{\lambda_c} - g_i + g_i^m\right] \\ s.t.\ g_i^m \leq (1-\gamma) g_i \\ g_i^m = \frac{\theta^{MG} u(g_i)}{\lambda_c}\end{aligned}$$

where $\kappa = \log\tilde{\kappa}$. That is, the problem yielding the value function $V(\cdot)$ is one where the city maximizes the expected value of getting a project approved. It is convenient to write this value function normalized by λ_c as above, so that $V(\cdot)$ can be interpreted as a money equivalent utility function for the city.

If the solution to the city's problem is interior, it can be characterized by first order conditions. In the case of cities not bound by the constraint

on $\frac{g_i^c}{g_i} \geq \gamma$, we get that:

$$\begin{aligned} 1 =& \frac{\epsilon_i v'(g_i)}{\lambda_c} + \frac{\theta^{MG} u'(g_i)}{\lambda_m} \\ \frac{1 - H(\cdot)}{h(\cdot)} g_i^m =& \frac{\epsilon_i v(g_i)}{\lambda_c} - \frac{\theta^{MG} u(g_i)}{\lambda_m} - g_i \end{aligned}$$

The first equation is intuitive: given a choice of a probability of approval (implicit in the choice of θ^{MG}), the project size is chosen to maximize the utilitarian welfare of the city and the ministry at the margin between approving or not the project.

The second equation, in turn, characterizes the city's trade off when choosing its own probability of approval for a given project size g_i. If the city tries to get a marginal increase the probability of approval, it gets a payoff of $\frac{\epsilon_i v}{\lambda_c} - g_i^m$, equivalent to the payoff on the right hand side of the second equation above. On the other hand, to increase the probability of approval by δ while holding the project size g_i constant, the city needs to decrease g_i^m by $\delta \frac{1}{h(\cdot)} g_i^m$. This increases the share of costs paid by the city when the project is approved, which happens with probability $1 - H(\cdot)$.

More generally, for constrained or unconstrained cities, the city will choose project sizes and payments by the ministry according to choice functions:

$$g_i \left(\frac{\epsilon_i}{\lambda_c}, \log\left(\frac{\theta_{Ki}}{\lambda_m}\right), \gamma \right); g_i^m \left(\frac{\epsilon_i}{\lambda_c}, \log\left(\frac{\theta_{Ki}}{\lambda_m}\right), \gamma \right)$$

Particular case: To gain some intuition on the model's implications, consider the case where the city fully knows θ, so that θ_{Ui} can, without loss of generality, be normalized to 1. Similarly, assume for simplicity that $\gamma = 0$. In such a case, the city fully anticipates that the project will be approved if $\tilde{\kappa}^{s_i} \theta_{Ki} \geq \frac{\lambda_m g_i^m}{u(g_i)}$, and chooses optimally to set $\theta^{MG} = \tilde{\kappa}^{s_i} \theta_{Ki}$. Consequently, conditionally on making a request, the city will choose the maximum g_i^m consistent with approval, given by:

$$g_i^m = \min \left\{ g_i, \frac{\tilde{\kappa}^{s_i} \theta_{Ki} u(g_i)}{\lambda_m} \right\}$$

At the same time, the city will choose the project's scale to maximize its payoff, given the choice of g_i^m as above. Note that if the city anticipates $g_i < \frac{\tilde{\kappa}^{s_i} \theta_{Ki} u(g_i)}{\lambda_m}$, the city can get its request approved without any payments, so that the city has incentives to increase project size. The city

will do so up to the point where $g_i \geq \frac{\tilde{\kappa}^{s_i}\theta_{Ki}u(g_i)}{\lambda_m}$.[3] Consequently, we can take $g_i^m = \frac{\tilde{\kappa}^{s_i}\theta_{Ki}u'(g_i)}{\lambda_m}$.

As a consequence, the city will choose its project size to solve the following first order condition:[4]

$$\frac{\epsilon_i v'(g_i)}{\lambda_c} + \frac{\tilde{\kappa}^{s_i}\theta_{Ki}u'(g_i)}{\lambda_m} = 1$$

From this, it is easy to verify the effect of connections over g_i: connections act to increase the level κ faced by the city. A direct application of the implicit function theorem yields the following result:

Proposition 1 *Assume* $\frac{\epsilon v}{\lambda_c} + \frac{\theta u}{\lambda_m}$ *is concave in* g_i *for all* ϵ, θ. *Then, connections increase* g_i. *The effect of connections is higher (i) the larger is* κ_i, *and (ii) the less* $\frac{\epsilon v}{\lambda_c} + \frac{\theta u}{\lambda_m}$ *is concave.*

Proof. Apply the implicit function theorem to the equation above for $g_i(\kappa)$. ■

This result is intuitive. Clearly, as κ increases, the national government becomes more willing to approve a grant from a connected city, without changing its willingness to approve grants to unconnected cities. Given this, connected cities anticipate they can ask for more while still getting an approval.

Similarly, consider a government with a concave objective function u. By now, we know that the ministry pays $g_i^m = \frac{\tilde{\kappa}^{s_i}\theta_{Ki}u(g_i)}{\lambda_m}$, and that connected cities have larger projects g_i (as mentioned in the previous paragraph). But then, this implies that when a connected city increases g_i, it gets less of an increase in g_i^m than an unconnected city would get by increasing g_i. As a consequence, connected cities, on the margin, pay for a higher share of marginal increases in g_i. When $u(g_i)$ becomes less concave, this difference between connected and unconnected cities becomes smaller, and connected cities become more willing to increase g_i. This implies, in turn, that when u is less concave, the difference between connected and unconnected cities becomes smaller.

Finally, the city requests a grant if $F_{cmt}(N) \leq V$. Clearly, given that V is increasing in θ_{Ki}, we can express the decision of the city to make a request as $\theta_{Ki} \geq T\left(\frac{\epsilon_i}{\lambda_c}\right)/\tilde{\kappa}^{s_i}$.

[3] This is possible as long as $\lim_{g_i \to \infty} u'(g_i) < 1$.

[4] One way to understand this solution is to note that, with full knowledge, the optimal choice of θ^{MG} by the city is $\theta^{MG} = \tilde{\kappa}^{s_i}\theta_{Ki}$.

Now, let welfare from a given grant be given by $\frac{\theta_{Ki}u(g_i)}{\lambda_m} - g_i^m$: notice that this is the welfare function discussed previously, but normalized by λ_m so that we express welfare in monetary units. Then, we can write the welfare function from any given allocation as:

$$M \int \left\{ q \int_{\frac{T}{\tilde{\kappa}}}^{\infty} \frac{\theta_{Ki}u(g_i)}{\lambda_m} - g_i^m dH_{\theta_K} + (1-q) \int_{T}^{\infty} \frac{\theta_{Ki}u(g_i)}{\lambda_m} - g_i^m dH_{\theta_K} \right\} dH_{\frac{\epsilon}{\lambda_c}}$$

where q is the share of potential projects coming from connected municipalities and M is the mass of potential projects. Given that $g_i^m = \frac{\tilde{\kappa}^{a_i}\theta_{Ki}u(g_i)}{\lambda_m}$, we obtain in turn that the welfare function from the allocation set by the ministries can be expressed as:

$$\frac{1-\tilde{\kappa}}{\tilde{\kappa}} qME[g_i^m y_i r_i | s_i = 1]$$

I re-state this result, in turn, in the following proposition:

Proposition 2 *The welfare loss from connections, as a share of total resources from the ministry, is given by:*

$$\frac{1-\tilde{\kappa}}{\tilde{\kappa}} R$$

where $R = \frac{qME[g_i^m y_i r_i | s_i=1]}{ME[g_i^m y_i r_i]}$ *is the share of the ministry's approved resources going to connected cities.*

This results highlights a crucial identification problem: any given differences in resources allocated to connected vs. unconnected cities can be driven either by high levels of κ, or by low values of κ with low concavity of $\frac{\epsilon v}{\lambda_c} + \frac{\theta u}{\lambda_m}$. That is, we can explain any given difference in resources allocated either with high favouritism in κ, or with low favouritism but high scalability of projects from the viewpoint of cities and ministries.

Despite this, in this model with perfect knowledge by the city, the welfare losses only depend on $\tilde{\kappa}$ and on the share of resources going to connected cities. In particular, if connections are the only source of distortions in the ministry's decision, and $\tilde{\kappa} \to 1$, the welfare losses from connections go to zero.[5]

In this sense, it is possible that we have low welfare losses due to connections with large differences in approvals to connected and unconnected mayors. The small welfare losses would happen if $\tilde{\kappa}$ is small, and

[5]Note that $R < 1$, while $\frac{1-\tilde{\kappa}}{\tilde{\kappa}}$ goes to 0.

we could still get large differences in approvals by compensating the low $\tilde{\kappa}$ with functions u and v that are sufficiently close to linear.

The identification challenge, then, is, first of all, to be able to differentiate between the shape of u and $\tilde{\kappa}$. Additionally, given that many cities do not request grants, this suggests the presence of fixed costs of making requests; given that many requests are not approved, this suggests that cities cannot fully anticipate whether the ministry will approve grants or not. The second challenge, then, is to estimate the full model, allowing for cities that cannot fully anticipate ministerial decisions, and to compute welfare losses in this more general model.

General case: How do these results apply to the general case where the cities don't know everything in the ministry's objective function?

Relatively straight-forward algebra shows that the welfare function at a given $\epsilon, F_i, \theta_{Ki}$, with given connections s_i and making requests is given by:

$$\frac{1-\tilde{\kappa}^{s_i}}{\tilde{\kappa}} g_i^m \left[1 - H\left(\log \frac{\theta^{MG}}{\theta_{Ki}\tilde{\kappa}^{s_i}}\right)\right] + g_i^m \frac{\theta_{Ki}}{\theta^{MG}} \int_{\frac{\theta^{MG}}{\theta_{Ki}\tilde{\kappa}^{s_i}}}^{\infty} 1 - H(\theta_{Ui}) d\theta_{Ui}$$

The first term is the same term obtained under perfect information: namely, the welfare loss is $\frac{1-\tilde{\kappa}}{\tilde{\kappa}}$ times the amount of resources the ministry approves towards connected cities. Aggregating this expression across $\epsilon_i, F_i, \theta_{Ki}, s_i$, we get that welfare is given by:

$$\frac{1-\tilde{\kappa}}{\tilde{\kappa}} qME[g_i^m y_i r_i | s_i = 1] + E\left[r_i g_i^m \frac{\theta_{Ki}}{\theta^{MG}} \int_{\frac{\theta^{MG}}{\theta_{Ki}\tilde{\kappa}^{s_i}}}^{\infty} 1 - H(\theta_{Ui}) d\theta_{Ui}\right]$$

The first term appears just as it did under perfect information: among the connected cities, $\tilde{\kappa}$ creates a distortion because connected cities get approvals even with relatively low values of $\theta u(g_i)$. This implies a loss of $\frac{1-\tilde{\kappa}}{\tilde{\kappa}}$ of the budget allocated to connected cities.

The second term is new, and only appears under imperfect information: under perfect information, there are only requests when the city is certain that the ministry will approve the grant, so that when $r_i = 1$, $1 - H = 0$. When there is perfect information, on the other hand, $\frac{\theta_{Ki}}{\theta^{MG}} \int 1 - H d\theta_U$ is the virtual income the ministry gets from approving projects with benefits $\theta_{Ki}\theta_{Ui} > \theta^{MG}$ at a cost proportional to θ^{MG}. This term, then creates, relative to an environment without connections, a gain in virtual income earned from connected cities as long as $\frac{\hat{\theta}^{MG}}{\tilde{\kappa}}$ falls

with κ, and a loss in virtual income earned from unconnected cities.

Identifying this extra source of distortion requires identification of $H(\theta_{Ui})$, θKi, θ^{MG} and κ. Conditionally on these, we know welfare losses. On the other hand, the effect of connections over resources allocated still depend, even conditionally on these parameters, on the concavity of $u(g_i)$.

4.3 Identification

To identify this model in the data, I need to argue how the model relates to observables. In a nutshell, the exercise does this by turning the approval decision by the ministry into a binary choice model, and by turning the extensive margin of grant requests into an ordered discrete choice problem. To do so, first of all, I need the following assumption:

Assumption 1 *Let $\beta^{\lambda_m}(t)$, $\beta^{\theta}_m(t)$ and $\beta^{\theta}_c(t)$ be time polynomials. Then,*
(i) $\log \lambda_m = X_m \beta^{\lambda_m}(t)$
(ii) $\log \theta_{Ki} = X_m \beta^{\theta}_m(t) + X_c \beta^{\theta}_c(t) + d_{cmT}$

The first part of the assumption is relatively simple: it says that the shadow cost of funds from the ministry is a ministry-time specific effect. Given that the models estimated turn out to be non-linear, though, the estimation becomes unstable if we add, as in the linear model, ministry-time dummies (which account for 216 ministry-time effects).

The second part of the assumption is more restrictive: it requires that everything observed by the city about the ministry's approval criterion can be captured by ministry-time effects, city-time effects and a ministry-city-mayoral term fixed effect.[6] This assumption is similar to the assumption used in the reduced form part of the paper, and guarantees that I use the same source of variation to identify the effects of political connections.

That being said, this assumption implies that once the observables are controlled for, the only source of variation in g_i and g_i^m comes from ϵ_i, the rules on copayment γ and from mistakes.[7] While in many contexts, this could be a strong restriction on the data, here, it turns out that in fact, g_i and g_i^m are highly correlated, and it isn't too restrictive to assume that ϵ_i and γ explain almost all the variation in g_i and g_i^m.

[6] See the appendix for one microfoundation for this assumption.

[7] While optimization mistakes are not directly in the model, the microfoundation for assumption 1 makes it clear where these mistakes could be coming from.

The second implication of this assumption is that conditional on these observables, there are no other unobservables driving both ministerial approvals and self-selection into making grant requests. This is a restrictive assumption, and with this in mind, I discuss later what are the requirements to identify a more general model with selection on unobservables and a testable implication for this assumption.

For now, I stick with this assumption, and note that if I let $\mathbf{Z}_{cmt} = (s_{cmt}, X_m, X_c)$, assumption 1 guarantees that:

$$\begin{aligned} E[y_i|\log g_i, \log g_i^m, \mathbf{Z}_{cmt}, d_{cmT}, r_i = 1] =& E[y_i|\log g_i, \log g_i^m, \mathbf{Z}_{cmt}, d_{cmT}] \\ =& 1 - H_{\theta_U}\left(\begin{smallmatrix}\log g_i^m - \log u(g_i) - \kappa s_{cmt} \\ + X_c\beta_c(t) + X_m\beta_m(t) + d_{cmT}\end{smallmatrix}\right) \end{aligned}$$

where $\kappa = \log(\tilde{\kappa})$ and the time trends are sums of the time trends in the assumption 1. Assumption 1 also guarantees that conditionally on the observables above, g_i, g_i^m are exogenous to the unobservables driving approvals: after all, the only unobservables driving approvals under assumption 1 are given by θ_{Ui}, the terms unknown by cities at the time they decide on g_i, g_i^m. Further approximate $\log u(g_i) = \sum_{j=1}^{N} \frac{\omega_{Uj}}{j}(\log g_i)^j$ (so that we're approximating g_i as a polynomial in $\log g_i$, and taking the order of the polynomial to infinity as the sample grows).

Under these, identification would be immediate if I could control for the fixed effects d_{cmT}: first, let $\omega_U(g_i) = \frac{u'(g_i)}{u(g_i)}g_i$ be the elasticity of the government's payoff with respect to g_i. Then, note that the marginal effects of $\log g_i$ are given by $h_{\theta_U}(\cdot)\omega_U(g_i)$. Similarly, the marginal effect of $\log g_i^m$ is given by $-h(\cdot)$. But then, the negative of the ratio of these marginal effects is simply $\omega_U(g_i)$ (which is equal to $\sum_{j=1}^{N} \omega_{Uj}(\log g_i)^{j-1}$ under the previous polynomial approximation). For an alternative interpretation, we can identify $u(g_i)$ by looking at the following: if we move $\log g_i$ by a small ψ, and move $\log g_i^m$ in a way that keeps the probability of approval constant, the movement in $\log g_i^m$ necessary to do so is $\omega_U(g_i)\psi$.

The same intuition applies to the identification of κ: if we move a city-ministry pair from connected to unconnected across years, and move $\log g_i^m$ back to keep the probability of approval constant, the movement in $\log g_i^m$ necessary to do so is given by κ.

The things left-over to deal with in identifying this model are (i) dealing with time effects, and (ii) taking into account that I cannot

explicitly control for d_{cmT} due to the incidental parameters problem. That being said, insights due to Manski (1988) allow for the estimation of this model with fixed effects and without functional form assumptions on H_{θ_U}. More precisely, order the grant requests according to some order - say, date of request -, let $\Delta x_i = x_{i+1} - x_i$ and let $l_{cmt} = \kappa s_{cmt} + X_c\beta_c(t) + X_m\beta_m(t) + d_{cmT} = \log \tilde{\kappa} s_i + \log \theta_{Ki}$. Then, Manski (1988) shows that:

$$E[\Delta y_i | \Delta \log g_i, \Delta \log g_i^m, \Delta s_{cmt}, \log g_i, \log g_i^m, s_{cmt}, X_m, X_c] > 0 \\ \leftrightarrow \Delta l_{cmt} < 0$$

and equivalent expressions hold when the expectation is strictly lower than zero. But from this, it is immediate to get the following proposition, which I use to estimate this part of the model:

Proposition 3 *Under assumption 1,*

$$\begin{aligned} E[\Delta y_i | \Delta \log g_i^m, \Delta \log g_i, \Delta Z_{cmt}, \log g_i^m, \log g_i, \mathbf{Z}_{cmt}] = \\ [-\Delta \log g_i^m + \Delta \log u(g_i) + \kappa \Delta s_{cmt} - X_c \Delta \beta_c(t) - X_m \Delta \beta_m(t)] \\ \times m(\Delta \log g_i^m, \Delta \log g_i, \Delta \mathbf{Z}_{cmt}, \log g_i^m, \log g_i, \mathbf{Z}_{cmt}) \end{aligned}$$

where $m(\cdot) > 0$.

Note how this proposition formalizes the intuition above: by guaranteeing that $m(\cdot) > 0$, we guarantee that the only way to keep the probability of approval constant (or, to equate the expectation in the proposition to 0) is by having a movement within a city-ministry pair in the variables that is compensated by a movement in $\Delta \log g_i^m$. Furthermore, if $m(\cdot)$ can be approximated appropriately by a polynomial in the variables, the model above can be estimated by a simple non-linear least squares. In monte-carlo simulations, the point estimates for the main parameters of interest are correct on average even when I assume that m is linear in variables, so I proceed by taking this linear approximation for m.

From this, it is straight-forward to identify $v(g_i)$. In particular, note that by focusing on cities that are unconstrained by the minimum copayment rule, we can re-write the first order condition of the city as:

$$1 - H(\cdot) = h(\cdot) \left\{ \frac{g_i}{g_i^m} \left[\frac{1}{\omega_V(g_i)} - 1 \right] + 1 - \frac{\omega_U(g_i)}{\omega_V(g_i)} \right\}$$

Note that $1-H$ is simply the probability of approval of a project. Similarly, ω_U was just identified in the previous proposition. To identify ω_V, all there is left-over is to identify $h(\cdot)$. Note, however, that $h(\cdot)$ is (minus) the marginal effect of g_i^m over the probability of approval. The difficulty in identifying this marginal effect, however, is that (1) we haven't yet identified the distribution of θ_U nor the distribution of fixed effects, and (2) identifying the distribution of $\theta_U + d_{cmT}$ is not enough to compute this marginal effect. That being said, I can use here the insights from Hoderlein and White (2012) to identify these marginal effects from the same expectation identified in the last proposition. This in turn yields the following result:

Proposition 4 *Define the following terms:*

$$A = E[y_{i+1}|\Delta \log g_i^m, \Delta \log g_i, \Delta Z_{cmt}, \log g_i^m, \log g_i, \mathbf{Z}_{cmt}]$$
$$B = -\frac{\partial}{\partial \psi} E[\Delta y_i|\Delta \log g_i^m = \psi, \Delta \log g_i = 0, \Delta Z_{cmt} = 0, \log g_i^m, \log g_i, \mathbf{Z}_{cmt}]|_{\psi=0}$$
$$= m(0, 0, 0, \log g_i^m, \log g_i, \mathbf{Z}_{cmt})$$

where $m(\cdot)$ is as defined in proposition 3. Then, let $\omega_V(g_i) = \frac{v'(g_i)}{v(g_i)} g_i$ be the elasticity of the city's utility with respect to project size. Then, under assumption 1, we have that:

$$\omega_V(g_{i+1}) = E\left[\frac{B\left[1 - \frac{g_{i+1}^m}{g_{i+1}}\omega_U(g_{i+1})\right]}{A\frac{g_{i+1}^m}{g_{i+1}} + B\left[1 - \frac{g_{i+1}^m}{g_{i+1}}\right]} \,|\, \log g_{i+1}\right]$$

It is useful to discuss the intuition behind the terms A and B: the term A is simply the expectation of the probability of approval of the grant request $i+1$. The term B is more involved: first, consider the expectation in B: it's the expected change in probability of approval given the level of the variables, and given that the only variable moving across requests is $\log g_i^m$, which moves by ψ. Intuitively, when ψ is small, this expectation should be close to the marginal effect of g_i^m times ψ - that is, $-h(\cdot)\psi$. Then, taking derivatives with respect to ψ yields the desired marginal effects.[8]

[8]The insight due to Hoderlein and White (2012) is that when taking the derivative with respect to ψ, the expectation should move both because of the marginal effect of g_i^m and because of the change in the distribution of fixed effects. This second effect, though, goes to zero when ψ is small.

Another interpretation from B is from the following: say that we're able to estimate the expectation in proposition 3 and in B. Then, in a parametric setting with linear effects of the variables, B is the coefficient on $\Delta \log g_i^m$. In a non-parametric setting, this coefficient varies with the value of $\Delta \log g_i^m$. What the proof guarantees is that we need to look at this coefficient when $\psi = 0$. Note that given that the previous proposition provided a method to estimate this expectation inside B, so getting an estimate for B is immediate once we have the estimates from proposition 3.

Once A and B are known, we know all the terms in the first order conditions for the city. The leftover work, then, is just a matter of solving for $\omega_V(g_i)$ given A (or $1-H$), B (or h), $\frac{g_i^m}{g_i}$ and ω_U from before. This will be a function of g_i^m, g_i and all other observables. I then take expectations conditional on g_i to get $\omega_V(g_i)$.

The final object to be estimated here is the extensive margin of grant requests - that is, to obtain an estimable model for the number of requests a city makes. Here, I proceed in an almost reduced form way: first, order the city projects in decreasing order in terms of ϵ_i. Then, note that if we linearize the city's value function, we get that the city makes the grant request i iff:

$$F_{cmt}(N) \leq \eta_1 l_{cmt} + \eta_2(\log \epsilon_i - \log \lambda_c) + \eta_3 \gamma_{ct}$$

where $l_{cmt} = \kappa s_{cmt} + X_c\beta_c(t) + X_m\beta_m(t) + d_{cmT}$ is the latent variable shifting the probability of approval of a city's requests (leaving aside the terms g_i, g_i^m which are exogenous and don't enter the city's value function, naturally), and where γ_{ct} is only allowed to vary at the city-time level because the rules in place only allow for variation at the city-time level. The following assumption, then, disciplines the exercise a bit further:

Assumption 2 *(i)* $F_{cmt}(N) = X_c\beta_c^F(t) + X_m\beta_m^F(t) + d_{cmT}^F + F(N) + F_{cmt}^r$
(ii) $F(N)$ *is an increasing function*
(iii) $F_{cmt}^r | \frac{\vec{\epsilon}_{cmt}}{\lambda_c} = H_F\left(F_{cmt}^r | \frac{\vec{\epsilon}_{cmt}}{\lambda_c}\right)$
(iv) $\log \lambda_c = X_c\beta_c^\lambda(t)$

The first part of the assumption plays the following roles: firstly, it restricts, for the sake of estimation, how does the fixed cost of making requests vary across cities. Secondly, for the purposes of counterfactuals, it imposes the restriction that connections do not affect the fixed cost of

making requests - by doing so, variation in connections can be used as shifters of the probability of getting a grant request accepted, which is useful to compute equilibrium effects.

The second part of the assumption is relatively simpler, and it is helpful to guarantee that cities request a finite number of grants, and that there is a positive probability of requesting any number of grants. The third part of the assumption, finally, disciplines the joint distribution of F^r_{cmt} and the vector $\vec{\epsilon}_{cmt}$ including the values of ϵ_i for all of the city's potential grants. The final assumption just states that the city's shadow cost of funds varies at the city-time level, but not across ministries, consistently with a city's budget constraint.

This assumption is useful to guarantee that the city will make the first N grant requests (with highest ϵ_i values) iff, for the N-th grant request i_N, it is true that:

$$F^r_{cmt} \leq \eta_1 l_{cmt} + \eta_2(\log \epsilon_{i_N} - \log \lambda_c) + \eta_3 \gamma_{ct} - X_c \beta^F_c(t) - X_m \beta^F_m(t) - d^F_{cmt} - F(N)$$

which in turn tells us that:

$$\begin{aligned}\Pr(N \geq n|\cdot) = &\int H_F \left(\begin{matrix} \eta_1 l_{cmt} + \eta_2(\log \epsilon_{i_n} - \log \lambda_c) + \eta_3 \gamma_{ct} \\ - X_c \beta^F_c(t) - X_m \beta^F_m(t) - d^F_{cmt} - F(n) \end{matrix} \right) dH_{\epsilon_{in}} \left(\frac{\epsilon_{i_n}}{\lambda_c} \right) \\ = &M_n \left(\eta_1 l_{cmt} + \eta_3 \gamma_{ct} - X_c \beta_c(t) - X_m \beta_m(t) - d^F_{cmt} - F(n) \right)\end{aligned}$$

so that, to estimate the extensive margin of grant requests, all that is needed to be done is to estimate separate binary choice models for the probability that grant requests exceed n.[9] Once again, if we normalize $\eta_3 = -1$,[10] the coefficient η_1 can be estimated as in proposition 3, and marginal effects of l_{cmt} can be estimated as suggested in proposition 4. These results are, in turn, sufficient to compute counterfactuals as described below.

[9] Note that if the function M_n was independent of n, this would be the usual ordered choice model. Given, however, that the city's value function depends on ϵ_{i_n} - so that the city chooses to request potential grants that it finds most valuable -, the distribution of the relevant shock changes with n.

[10] In expected value, the constraint on minimum contributions has a negative effect over the city's value function. As usual, in discrete choice problems, either a variance normalization or a coefficient normalization is necessary. Here, a coefficient normalization is more convenient.

4.4 Results

Table 4.1 presents results on the estimated parameters of the model, assuming that $\log u(g_i) = \omega_U \log g_i$. The model estimates an almost linear objective function for the national government, with $\omega_U = 0.966$. Given this, by itself, can lead to large differences in resources approved for connected cities, the estimates of $\tilde{\kappa}$ suggest that the extra welfare weight ministries put on connected cities is of 1.7%. Taking $\log v(g_i) = \omega_V \log g_i$, I obtain that in contrast to the ministry's objective function, the city's objective function is considerably more concave, with $\omega_V = 0.413$.

Given the point estimates, it is immediate to do the welfare exercise applying the formula from the simplified model with the estimated parameters. Given that ministries spend 14.3% of their resources on connected cities, that formula implies that if connections are the only distortion in the ministerial allocation decision, the welfare losses from connections are of $\frac{1-\tilde{\kappa}}{\tilde{\kappa}} R = 0.24\%$ of each ministry's budget, or 1.7% of the resources sent to connected cities.

4.4.1 Extensions

Unobservable components anticipated by the city: A concern about the basic results above is that they might be capturing endogenous choices of g_i, g_i^m. In particular, there might be omitted request specific variables that increase the probability of approval and the requested project size g_i. If this was the case, the model would tend to overestimate ω_U, and the high estimated ω_U might lead to low estimated $\tilde{\kappa}$. This could be driving the small estimates for welfare effects of connections.

This alternative could be formalized in the structural model with a slight change in assumption 1 as follows:

Assumption 3 *(i)* $\log \lambda_m = X_m \beta_m^{\lambda_m}(t)$
(ii) $\log \theta_{Ki} = X_m \beta_m^{\theta}(t) + X_c \beta_c^{\theta}(t) + d_{cmT} + \theta_{Ki}^r$

Under this assumption, identification of the structural model becomes considerably harder: first of all, self-selection into grant requests changes the observed expectation of approvals as in typical sample selection models. To deal with this, it would be necessary to have an excluded variable affecting self-selection into grant requests without affecting grant

approvals.[11] Second of all, g_i, g_i^m become endogenous, so that it would also be necessary to observe instruments for both these variables that are independent of $\theta_{Ki}^r, \theta_{Ui}$. While the rule on minimum matching shares could serve as one instrument, it would still be necessary to observe a second instrument. Finally, it would be necessary to deal with the fact that the final model is a panel binary choice model with sample selection - implying the need to deal with the fixed effects d_{cmT} and fixed effects in the sample selection equation, and the need to estimate a double index model for approvals.

Even with propositions on identification of such a model, it is infeasible to credibly identify such a model in this applied context. What is possible to do, though, is to test assumption 3 relative to assumption 1. To see this, say that we select the sample with $g_i \leq \bar{g}$ (or $g_i \geq \bar{g}$). Say that then, I estimate the structural model under assumption 1. Under assumption 1, this amounts to random selection, based only on ϵ_i, observables, γ and mistakes. This in turn implies that the estimated structural coefficients would not change under assumption 1. Under assumption 3, on the other hand, this amounts to selection on unobservables, biasing the estimated coefficients. Hence, one test of assumption 1 relative to assumption 3 is to estimate the original structural model on subsamples selected on the basis of g_i (and the same logic applies to selection based on g_i^m).

Table 4.2 provides the results from this exercise, looking at samples below and above the median g_i, g_i^m and $\frac{g_i^m}{g_i}$. In fact, results are stable across samples: the government's objective function is estimated to have a coefficient ω_U of at most 0.983 and at least 0.964. Similarly, the ministerial extra weight on connected cities is estimated to be of at most 3.6% and at least 1.1%. The implicit welfare loss of connections are estimated to be between 0.15 to 0.49% of the average ministry's budget. I interpret this evidence as suggesting that selection based on variables relating to grant requests do not bias the estimation of the approval equation.

Heterogeneity and scale effects in ministerial utility functions: A second concern with the basic estimates is that results might be driven by heterogeneity in scale effects. In particular, the estimates

[11] Typical choices would be here some policy moving the fixed costs of requests, which are unavailable in this context, or usage of connections at time of request vs. at time of approval, which have a correlation of 0.87, limiting the power to use this to identify the model.

use variation in g_i, g_i^m coming from all cities – irrespectively of city size, income, or connections. If small and large cities face similar marginal effects of g_i, g_i^m over their probability of approval, this would be captured by the model as a close to linear $u(g_i)$ function. This might not necessarily be the case, given that it might take larger project sizes in larger cities (or poorer cities) for marginal product of g_i over $u(g_i)$ to in fact decrease significantly.

To take this into account, I allow, first of all, for heterogeneous return $\log u(g_i) = (\omega_{U0} + \omega_{U0}^{Pop} \log pop + \omega_{U0}^{Inc} \log inc) \log g_i$, allowing for the marginal returns of g_i to depend on a city's population and income. Secondly, I allow for scale effects by specifying $u(g_i) = \omega_{U0} \log g_i + \omega_{U1} (\log g_i)^2$. With this, if $\omega_{U0} > 1$ and $\omega_{U1} < 0$, we guarantee that the government's payoff function u is initially convex and then concave in g_i. Finally, I estimate a third specification where I allow for both scale and heterogeneous returns, according to $\log u(g_i) = (\omega_{U0} + \omega_{U0}^{Pop} \log pop + \omega_{U0}^{Inc} \log inc) \log g_i + \omega_{U1} (\log g_i)^2$.

The results are presented in table 4.3. I present the coefficients ω_U, $\kappa, \tilde{\kappa}$, the average $\omega_U(g_i)$ evaluated at the g_i requested in the data, and the estimated welfare loss of connections using the expression from the full information model. In all three columns, the average $\omega_U(g_i)$ barely changes and shows that cities face, on the margin, a ministerial objective function that is very close to linear.

Not surprisingly, the coefficients $\kappa, \tilde{\kappa}$ indicate that connected cities get an extra welfare weight of between $1.7\% - 1.9\%$. As a consequence, the welfare loss of connections when this is the only distortion is of $0.23\% - 0.27\%$ of the ministry's budget.

4.5 Conclusion

The political connectedness of regions distort the allocation of public funds across in many countries. That being said, often there are few measures of welfare received by different regions of a country to evaluate directly these welfare losses. This chapter discusses how to find lower bounds on the welfare costs of these distortions. While in some contexts, this lower bound could be enough to guarantee that the welfare costs of connections are high, in the context of Brazil between 2009-16, we cannot rule out the possibility that the welfare costs of connections-based

allocations are small, either because of constant/increasing returns to grant size and small concerns for inequality across regions.

The welfare measurement here also suggests that putting in place non-partisan ministries might not be an cost effective policy tool at preventing partisanship in the allocation of funds: such a non-partisan ministry would not lose much from becoming partisan. Moreover, given that political parties are often in charge of deciding on the allocation of public funds in Brazil as part of an effort of coalition formation at the national level, non-partisan ministries could create substantial costs in terms of gridlock and government.

Future research further highlighting upper bounds on the welfare costs of connections-based allocations would be useful for this research agenda. Intuitively, this would require evidence that partisan ministers allocate funds well on certain margins. It is unclear, though, whether this can be done without further welfare measures. Relatedly, research highlighting the benefits of ministerial flexibility might be useful to understand the costs of policies aimed at reducing the role for political connections in the allocation of public funds.

Tables

Table 4.1: Structural model: Basic specification

Parameter	(1)
ω_U	0.966
	(0.011)
κ	0.017
	(0.011)
$\frac{1-\tilde{\kappa}}{\kappa}R$	-0.24%
Sample	All

Table 4.2: Structural model: sample selection test

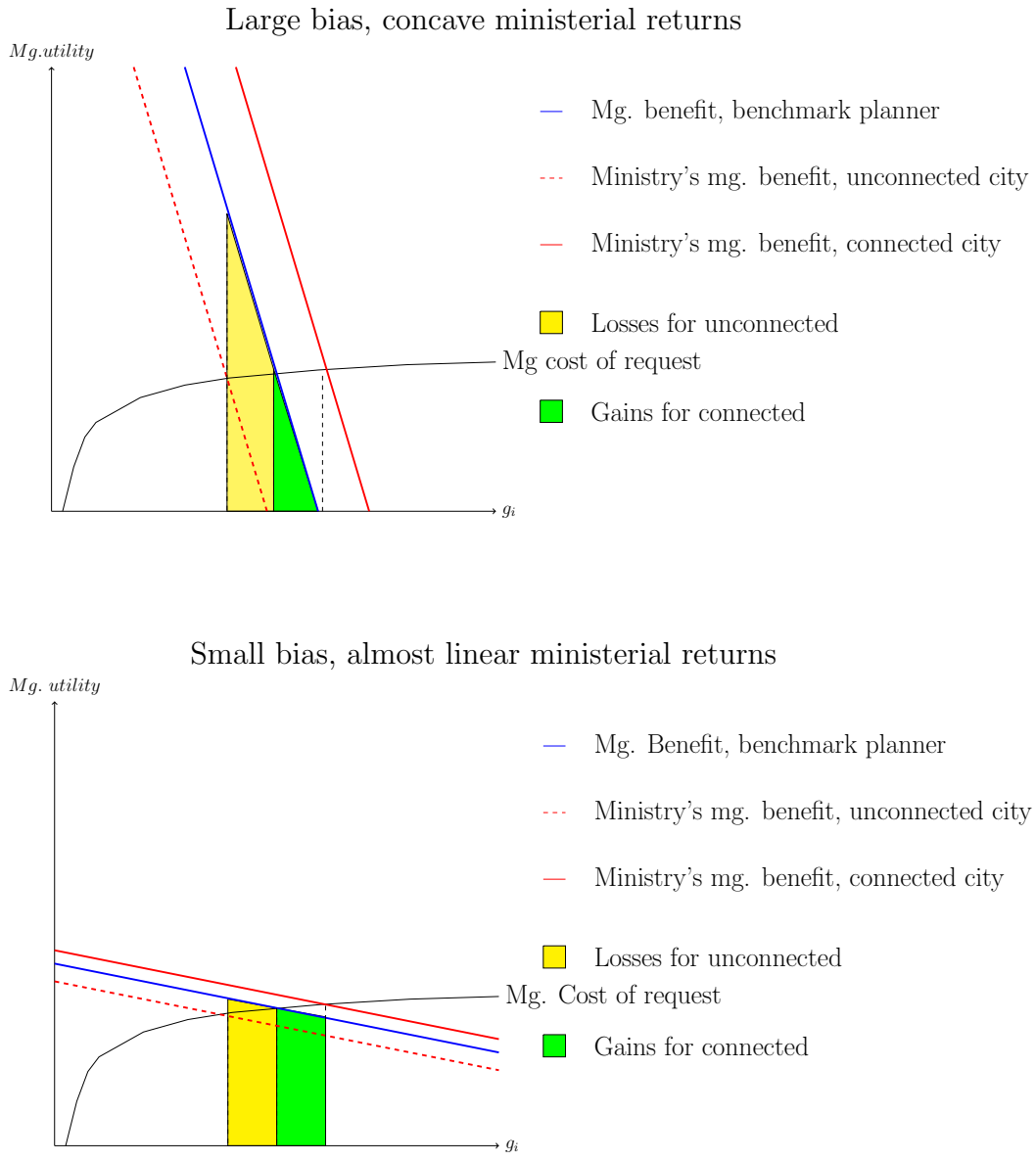
	Selection based on g_i		Selection based on g_i^m		Selection based on $\frac{g_i}{g_i^m}$	
Parameter	(1)	(2)	(3)	(4)	(5)	(6)
ω_U	0.969	0.970	0.964	0.983	0.966	0.973
κ	0.011	0.031	0.017	0.024	0.032	0.035
$\tilde{\kappa}$	1.011	1.032	1.018	1.025	1.032	1.036
$\frac{1-\tilde{\kappa}}{\kappa}R$	0.15%	0.44%	0.25%	0.34%	0.45%	0.49%
Sample	Below 50%	Above 50%	Below 50%	Above 50%	Below 50%	Above 50%

Table 4.3: Structural model: heterogeneity and scale effects

Parameter	(1)	(2)	(3)
ω_{U0}	0.965	1.047	1.184
	(0.011)	(0.042)	(0.104)
$\omega_{U0} * \log(pop)$	0.008		0.013
	(0.005)		(0.005)
$\omega_{U0} * \log(inc)$	-0.008		-0.012
	(0.007)		(0.009)
ω_{U1}		-0.003	-0.008
		(0.001)	(0.004)
			()
			()
			()
κ	0.018	0.019	0.017
	(0.011)	(0.011)	(0.012)
Avg. ω_U	0.965	0.966	0.967
$\tilde{\kappa}$	1.018	1.019	1.017
Welfare loss	0.25%	0.27%	0.23%
Sample	All	All	All

Figures

Figure 4.1: When are the welfare costs of political connections large/small: graphical intuition



5. Ideology, governing coalitions, and checks and balances in the Brazilian national government

5.1 Introduction

In most democracies, institutions often create checks and balances, to ensure that policy making reflects the preferences of many elected officials, and not just the wills of the executive. Yet, in many democracies, the executive can govern not only by making policy concessions, but also by forming coalitions only with those with similar ideologies, or with those who are easy to buy with cabinet appointments, pork, and particularistic benefits. In these cases, it is not clear whether checks and balances are effective at restricting the executive.

With this in mind, to which extent does the executive form coalitions including only ideologically close legislators, or legislators whose votes are easy to buy? Secondly, to which extent do congressmen manage to check the executive's will, by approving bills the legislature likes but the executive dislikes?

This chapter answers this question by, first of all, building a model of coalition formation where the executive forms a governing coalition to influence legislators' votes in congress. The model provides a framework to understand how votes in congress depend on congressmen's ideologies, intensity of policy concerns, and on a "pressure" term, quantifying the extent to which the executive manages to get legislators in the government coalition to vote more favourably in exchange for particularistic benefits. I take this framework to data, where I observe congressmen

voting both in periods when they are in the government coalition, and when they are in the opposition. Given estimates fitting only data on votes in Congress conditional on membership in the governing coalition, then, the model provides testable implications for (i) factors leading the executive to exert more or less pressure on legislators, (ii) the role of ideological proximity and "ease to buy" of a legislator in determining which legislators get included in the coalition, and (iii) allows for counterfactuals to understand how much the executive is effective at changing votes in the legislature.

More precisely, I study a model where an executive offers to a group of legislators - the coalition - some particularistic transfer such as cabinet appointments, pork, election campaigning infrastructure, among others. In exchange for this, the legislators in the coalition, who also care about ideological payoffs coming from a large number of bills to be approved, need to agree to vote more favourably to the executive.

The model provides, first of all, a voting equation that is a simple extension of usual empirical models of ideological voting (see below for a review). It incorporates the usual ideological motivations to vote for each bill, plus a "pressure" term that the executive imposes on coalition members to guarantee they vote in a more aligned way. As expected, the model predicts that when legislators in the coalition (i) get more aligned ideologically with the president, (ii) have less intense policy preferences, (iii) are easier to buy, votes get closer to the president's ideal.

The first main challenge in taking this to data is that it is hard to distinguish between the causal effect of being in the coalition - that is, the effect of the executive exerting pressure on a legislator in the coalition to vote in a given way - from self-selection into the coalition - due to permanent ideological differences between coalition and opposition legislators. To deal with this, this chapter looks at the Brazilian setting, where there are more than 20 parties in Congress throughout the sample period, many of which start in the opposition and switch to spend a few years in the government coalition, and vice versa. With this in hands, by looking at the set of congressmen in the opposition in a given moment, I can identify both (i) unobservable ideological differences across legislators on any D dimensional ideological space, by detecting which legislators tend to vote together and which ones tend to vote differently, and similarly (ii) how these ideological differences interact with bill characteristics,

which determine which subgroups of legislators tend to oppose the bill and which ones tend to approve the bill. With these estimates in hands, then, I can identify the effect of being in the government coalition by looking at how a legislator votes differently on a given bill when he is inside and outside the coalition, holding fixed his ideological profile.

The key identifying assumption behind this is that (i) legislators' ideologies are stable over time, and (ii) the coalition is not changing endogenously because of unobserved changes in the expected bills to be voted on. On the first assumption, at least at the party level, even as new legislators come up, the average ideologies of political parties as measured by traditional roll call voting methods change very little over the sample period. This lends credibility to the first assumption. Moreover, a key advantage of the structural voting model is that it allows for the identification of (i) many unobservable dimensions of ideology and (ii) together with it, many observable policy differences, and this allows for me to control for many unobservable factors that legislators and the executive might be anticipating when deciding to form a coalition.

The second main challenge in taking this to data involves the fact that traditional models of roll call voting in the legislature have many parameters to be estimated: with L legislators voting on B bills and D ideology dimensions, the model needs to estimate DL ideology parameters, and $B(D+1)$ bill characteristics, relying on both large B and large L for asymptotic properties. The empirical model suggested here further adds B government pressure parameters interacted with observable characteristics (a legislator being in the coalition) to be estimated, requiring a total of $DL+B(D+2)$ parameters to be estimated. The paper then develops an *EM algorithm* that allows for quick convergence to local maxima of the likelihood function (see Dempster, Laird and Rubin (1977) for the general proof of properties of the EM algorithm; Imai, Lo and Olmstead (2015) use an application of this algorithm to obtain Bayesian estimates of ideology; I instead focus on classical maximum likelihood).

I then apply this model to data on the Brazilian Chamber of Deputies. This context is one where the question above is particularly relevant: there are political parties (such as PMDB, PP and PSB) that have been in coalitions lead by both relatively more left-wing and more right-wing governments, raising doubts about how much they care about ideological concerns. There is also a common concern that popular presidents can

enact policies with few checks and balances: they can propose "provisional laws" ("Medidas Provisórias") that comes into effect for a short time span up until the legislature holds a vote, potentially changing the status quo before a vote. Throughout my sample period (2008-16), 569 bills were proposed in total, and 252 of them were provisional laws by the executive that came into effect before a vote by the legislature.

The results from the structural model with a 2-dimensional ideology space model in the Brazilian Chamber of Deputies show that the ideology estimates from the model correlate strongly with the measure of left-right wing ideology based on surveys by Power and Zucco (2011) asking legislators about their policy position. Surprisingly, this is captured not by the dimension of ideology that most explains the voting data, but only by the second dimension of ideology. This suggests the model is able to capture well not only the determinants of voting we would expect to observe from other sources, but also other less obvious determinants of congressional votes.

The model also suggests that legislators face some pressure from the executive to vote differently from their ideology. Legislators faced most pressure in the beginning of my sample (2008-10): for half of the bills voted during this period, the government's pressure changed more than 20% of the votes from legislators in the government coalition. The amount of pressure fades over the years, and in 2015-16, in 90% of the bills, the pressure exerted by the government changed less than 10% of the votes from the coalition members.

Given the voting model, I then show that the theoretical implications of the theory of coalition formation are largely seen in the data. I start by running a linearised version of the first order condition for the executive, determining which bills should face more pressure for approval and rejection. The first order condition, determining pressure as a function of (i) a proxy of the president's ideology, (ii) the number of favourable votes to the bill by the opposition, and (iii) the sensitivity of the coalition to pressure (as identified by the voting model), explains around 20% of the variation of government pressure in the data.

The model also predicts that when forming a coalition, the president likes to have parties that are ideologically similar. That being said, when the president includes in the coalition parties that are ideologically far away, the president prefers legislators that are sensitive to pressure

so that he can change their votes. When the president includes in the coalition parties that are ideologically close-by, the president prefers legislators that are insensitive to pressure, since this reduces chance disagreements with the president.

These implications are largely met by the data. In fact, up until May 2016, PT was in the presidency, and the ideology estimates from the model suggest there are few parties that are ideologically similar to them. As a consequence, they formed a coalition with parties that were close to them in the first dimension of ideology, but were far away in the second dimension of ideology. Consistently with the theory, the estimated sensitivity to pressure among coalition members is higher than the sensitivity to pressure among the opposition legislators. After May 2016, PMDB gets to the presidency, and the ideology estimates suggest there are many parties that are ideologically similar to them. As a consequence, they form a coalition with political parties that are more similar to them on both dimensions. Consistently with the model, during this period, the sensitivity to pressure of coalition members is smaller than the sensitivity to pressure of opposition members. This overall pattern is also consistent with the fact that we observe more pressure on legislators in the early years of the sample, and less pressure on legislators in the later years.

Finally, the data suggests that this pattern of coalition formation and pressure over legislators have strong implications for policy. Before May 2016, 12.8% of the bills disliked by the executive were not approved due to pressure. Similarly, 10.8% of the bills liked by the executive became approved as a consequence of pressure. While the number of bills in my sample after May 2016 is small, in this period, there are no detectable bills that the executive disliked, and that they stopped from being approved by applying pressure on coalition members. Similarly, 10.4% of the bills liked by the executive were approved as a consequence of executive pressure on coalition members.

This suggests that when the executive has legislators who are ideologically close to them, they can form coalitions with like-minded legislators, approving policies without pressuring them to switch their votes. When the executive has to form a coalition with ideologically distant legislators, on the other hand, they rely on legislators that are sensitive to pressure, and pressure them to switch their votes on a significant number

of bills. Despite this, the legislature still manages to check the executive a substantial number of times: before May 2016, even with pressure, 66.7% of the bills disliked by the executive were still approved. After May 2016, 81.6% of the bills disliked by the executive were approved.

This chapter contributes to a large literature on coalition formation in governments. Merlo (1997) and Diermeier, Eraslan and Merlo (2003) estimate models of legislative bargaining to understand determinants bargaining power of political parties, of the time until government formation, and the size/duration of government coalitions. Adachi and Watanabe (2008) estimate a model of coalition formation in Japan, with implications for parties' bargaining power and the value of pork intensive vs. "prestige" intensive cabinet positions. More recently, a set of papers also moved to estimate models of the composition of governing coalitions: Gagliarducci and Paserman (2012) discuss the implications of mayoral vs. legislators' gender. Francois, Rainer and Trebbi (2015) discuss the ethnic representativity of governing coalitions in Africa, when facing threats of coups and rebellions. This chapter, instead, focuses on the ideological composition of coalitions, an important feature in most models of legislative bargaining and coalition formation. This chapter further provides implications of the process of coalition formation for policy making, in particular, for the outcomes of votes in the legislature.

Methodologically, this chapter also relates to a large literature on estimation of ideology from roll call votes in legislatures (see Poole and Rosenthal (1985), (1991), Clinton, Jackman and River (2004), Imai, Lo, Olmstead (2016). This chapter is closest to the literature on party pressure (see Krehbiel (1993), Snyder and Groseclose (2000), Jenkins (2000), McCarthy, Poole and Rosenthal (2001), Snyder and Groseclose (2001), Nokken and Poole (2004)), and to the literature documenting the difficulty of capturing ideology from roll call votes in contexts with coalition governments (see Zucco (2009), (2013), Spirling and McLean (2007), Power and Zucco (2012), Hix and Noury (2016)). Relative to this literature, this chapter provides methods to separate between ideology and coalition effects - allowing me to test theories of coalition formation and to analyse counterfactuals.

Dewan and Spirling (2011) provide a theory for government vs. opposition differences in voting in Westminster style politics, and Cox and McCubbins (2005) provides a theory for differences between majority vs.

minority party in the U.S. Congress. This chapter complements this literature understanding differences between government and opposition by focusing on legislators switching from government to opposition within and across a given presidential term. It then argues that a model where the executive forms a coalition in the legislature to govern is helpful to understand the data, in particular to understand which parties/legislators join the government coalition, and the implications of this for roll call votes.

The next section shows the model of coalition formation, characterising voting, the ideological composition of the coalition, and comparative statics. The third section discusses the data used. The fourth section discusses how to identify and estimate the roll call voting model. The fifth section presents (i) reduced form results on the importance of the government coalition for votes, (ii) describes the estimates of the structural voting model, (iii) uses these estimates to test whether the shape of coalitions is consistent with the theory's prediction and to compute counterfactual voting outcomes without the coalition. The last section concludes.

5.2 Model

5.2.1 Set-up

A set of legislators $l = 1, ..., L$ vote over a many bills b - taken here to be a continuum of bills, for convenience. Each legislator has ideology $\tilde{x}_l \in \mathbb{R}^D$, and can vote "yea" or "nay" on each bill. If the bill is approved, it delivers a payoff associated with position x_{by} (for "yea"), while if the bill is rejected, it delivers a payoff associated with position x_{bn} (for "nay").

Legislators care about policy and about particularistic benefits given by the executive t_l - such as cabinet positions, pork, governmental help with elections and campaign finance, and other benefits which are potentially specific to legislator l. For simplicity, I assume that legislators have quasi-linear payoffs in the particularistic benefits t_l, so that given a net policy payoff $\tilde{u}_{bly} - \tilde{u}_{bln}$ in favour of "yea", legislators' final payoffs are given by $t_l + \int (2v_{lb} - 1)(\tilde{u}_{bly} - \tilde{u}_{bln})db$, where $v_{lb} = 1$ indicates a "yea" vote, and $v_{lb} = -1$ indicates a "nay" vote.[1] I assume legislators' payoff

[1] This specification for v_{lb} simplifies the exposition of the empirical implementation of the model.

from "yea" is given by $\tilde{u}_{bly} = u_{bly} + \tilde{\epsilon}_{bly} = -\alpha_l(x_{by} - \tilde{x}_l)^T(x_{by} - \tilde{x}_l) + \tilde{\epsilon}_{bly}$, where α_l stands for how much legislator l cares about policy vis-a-vis particularistic benefits. The payoff from "nay" is analogous to this. I let $\tilde{\epsilon}_{bly} - \tilde{\epsilon}_{bln} = a_b + \epsilon_{bl}$, where a_b stands for overall pressures for all legislators to vote in favour of a bill - such as pressure from lobbies in favour of that bill, or overall agreement across legislators. I'll assume ϵ_{lb} is statistically independent across l, b, and conditional on b, $\epsilon_{lb} \sim \Phi(0, 1)$ and is statistically independent across l.

It is useful to re-express the legislators' net payoff from a "yea" as:

$$\tilde{u}_{bl} = \tilde{u}_{bly} - \tilde{u}_{bln} = a_b + c_b^T x_l + \epsilon_{bl}$$

where $x_l = \alpha_l[1, \tilde{x}_l]$, and the different components of c_b are different combinations of x_{bly}, x_{bln}. As usual, a_b can be interpreted as an overall favourability of the bill for all legislators. The terms c_b can be interpreted as whether the bill shifts legislation to the "right" or to the "left" (which I'll call the bill's polarization): if $c_b > 0$, only legislators with $x_l > 0$ will become more propense to vote for it, if $c_b < 0$, only legislators with $x_l < 0$ will become more propense to vote for it.

The president anticipates this, and chooses (i) which legislators to include in the government's coalition, (ii) how much t_l to offer them, and (iii) what votes v_{lb} to demand from each legislator. The president dislikes higher levels of t_l - for instance, because the president would like to appoint cabinet positions, or wouldn't like to use public funds for pork as much as legislators want - and has quasi-linear preferences in t_l. The president also cares about the overall support for the bill in the government coalition $V_b^{GOV} = \sum_{l \in \mathcal{C}}(2v_{lb} - 1)$ and for the overall support for the bill in the opposition $V_b^{OPP} = \sum_{l \notin \mathcal{C}}(2v_{lb} - 1)$, according to $P(V_b^{GOV}, V_b^{OPP}, u_b^P + \epsilon_{bP})$, where $u_b^P + \epsilon_{bp}$ indexes the president's preferences for the bill.[2] I'll assume that $P(V_b^{GOV}, V_b^{OPP}, u)$ is increasing in V_b^{GOV} when $u > 0$, decreasing in V_b^{GOV} when $u < 0$. I will also assume that $\frac{\partial^2 P(V_b, u)}{\partial V_b^{GOV} \partial u} \geq 0$.

This encompasses a broad range of possible preferences for the president: it allows for a president who wants to only approve or reject some bills if $P_b = u_b^P + \epsilon_{bP}$ once $V_b^{GOV} + V_b^{OPP} > T$, and $P_b = 0$ otherwise.

[2]The only restriction this imposes is that the president's payoffs from bills varies according to a single bill-specific index, and this index in turn depends on the multiple dimensions of the president's ideology.

It also allows for the possibility of a president who tries to buy supermajorities for a bill as a way of preventing the opposition from trying to buy back some legislators after the coalition formation process, as in Snyder and Groseclose (1996). It is important to take such factors into account in the Brazilian context, given the prevalence of supermajority coalitions in Congress. For this, I allow the returns to V_b^{GOV} to take a flexible functional form.

It is also potentially important to allow for V_b^{GOV} not to be perfectly substitutable for V_b^{OPP}. By doing so, I allow for a broad range of potentially plausible scenarios: for instance, votes in the opposition might bring more resources to buy back legislators from the government coalition in a Snyder and Groseclose (1996) framework, and it would be reasonable to expect then that V_b^{OPP} doesn't substitute V_b^{GOV} perfectly. I can also allow for a president who might care for support for a bill as a way of signalling, for instance, that it can effectively pressure and influence legislators in the coalition. In such cases, it is natural to assume that V_b^{OPP} - insensitive to pressure inside the coalition - will not contain the same information as V_b^{GOV} - sensitive to pressure inside the coalition -, and consequently, V_b^{GOV}, V_b^{OPP} won't be perfect substitutes.

The timing of the game is as follows:

1. The president observes $\tilde{x}_l$ for each legislator, and chooses (i) which legislators to form a coalition with, (ii) t_l and (iii) which votes v_{lb} to demand from each legislator (conditional on realized preferences and bills).

2. Legislators accept/reject to be included in the government coalition

3. Legislators choose how to vote on each bill after ϵ_{lb}, x_{by} and x_{bn} are realized.

4. Benefits t_l get delivered to each legislator

I assume that the president has commitment to deliver t_l according to rules pre-specified in the first period of the game. While this assumption might be strong, it stands as a simplification of a model where presidents and legislators interact repeatedly, and where presidents either deliver benefits over time, or where presidents' political parties want to establish a reputation of keeping promises with future coalition partners.

Discussion of assumptions

Three assumptions are worth discussing. First, the solution to the model is the same irrespectively of whether we assume ϵ_{lb} to be privately observed by legislators or publicly observed by everyone. In particular, I'll show that the solution to the relaxed problem ignoring incentive constraints for legislators - and only including participation constraints by legislators - can be implemented in an incentive compatible way, even without communication on ϵ_{lb}, as long as the number of legislators is large enough for the uncertainty on the exact realizations of ϵ_{lb} to be ignored.

Secondly, the model assumes that bill characteristics (in particular, x_{by}, x_{bn}) are exogenous to the president and to the government coalition. In the model, this stands for a process where in each of a continuum of days, shocks happen allowing a given legislator (or the president) to bring a bill to the floor, and shocks to the proposer's perception of what will be approved that day drive the bill offered (such as signals about the values of ϵ_{lb} for all legislators, and consequently shocks about which legislators are pivotal). For the empirical estimation of the model, that won't be a big issue, since the method will control for bill effects. For policy counterfactuals, the results should be interpreted as the effect of the government coalition over which bills are approved/rejected, holding fixed the set of bills proposed.

A third assumption in the model is that legislators vote sincerely according to their policy payoffs, while the president explicitly cares about the bill passing. Given that legislators have a binary choice in the model, pivotality considerations in this model do not change their vote. That being said, governments might demand more or concede more to legislators they expect to be pivotal. This, instead, is partially captured by variation in α_l - the legislator's care for policy payoffs.

5.2.2 Voting within a coalition

Given a government coalition, what are the transfers t_l and votes v_{lb}? To understand this, I start by solving the following relaxed problem, where the president designs t_l and vote rules to maximize his total payoff, subject to a participation constraint by the members of the coalition. Note that, in particular, this ignores incentive constraints for legislators

to vote according to the voting rule v_{lb} suggested by the government. Let $\mathcal{C}$ be the set of legislators in a given coalition. This problem can be written as the choice of votes for legislators in the coalition to solve:

$$\begin{aligned}
&\max \int P(V_b^{GOV}, V_b^{OPP}, u_b^P + \epsilon_{bP})db - \sum_{l \in \mathcal{C}} t_l \\
&s.t.\ t_l + \int (2v_{lb} - 1)[a_b + c_b^T x_l + \epsilon_{lb}]db \geq \\
&\qquad \max_{z_b \in \{-1,1\}} \int (2z_b - 1)[a_b + c_b^T x_l + \epsilon_{lb}]db \ \forall\ l \in \mathcal{C} \\
&\sum_{l \in \mathcal{C}} (2v_{lb} - 1) = V_b^{GOV} \ \forall\ b
\end{aligned}$$

where the right hand side of the constraint (in the last line in the expression above) represents the outside option of legislators: to not get any transfers from the government, and vote according to their ideology.

In this problem, as usual, the constraints bind, yielding an expression for the transfers to each legislator. We can use this to replace for t_l in the president's objective function. This allows the president to choose votes v_{lb} to maximize:

$$\begin{aligned}
\max &\int P(V_b^{GOV}, V_b^{OPP}, u_b^P + \epsilon_{bP})db \\
&\quad + \sum_l \int 2(v_{lb} - v_{lb}^*)[a_b + c_b^T x_l + \epsilon_{lb}]db \\
s.t.\ &\sum_{l \in \mathcal{C}} (2v_{lb} - 1) = V_b^{GOV} \ \forall\ b
\end{aligned}$$

where v_{lb}^* is the legislator's l optimal vote for bill b outside the coalition. The problem is then separable across bills. Letting λ_b be the Lagrange multiplier on the constraint, we obtain that the optimal vote demand v_{lb} by the president is given by:

$$a_b + c_b x_l + \epsilon_{lb} + \lambda_b \geq 0 \leftrightarrow v_{lb} = 1$$

That is: whenever the president wants to increase the number of votes for a bill, it requires congressmen to vote according to their preferences plus a uniform bill level pressure $\lambda_b > 0$. Similarly, when the president wants to reject a bill, it imposes a uniform bill level negative pressure $\lambda_b < 0$.

A few properties of this pressure coefficient are immediate to verify:

$\lambda_b > 0$ only when the legislature would reject the bill if legislators were to vote only according to their ideology. Similarly, $\lambda_b < 0$ only when the legislature would approve the bill if legislators were to vote only according to their ideology.

Finally, if the president's choice of V_b^{GOV} is interior, the president chooses the number of votes V_b^{GOV} to solve the following FOC:

$$P_{V^G}(V_b^{GOV}, V_b^{OPP}, u_b^P + \epsilon_{bP}) = \lambda_b \tag{5.1}$$

where P_{V^G} stands for the derivative of P with respect to V_b^{GOV}. That is: we can interpret λ_b as the marginal cost of demanding an extra vote V_b from legislators in the government coalition. The president then equates λ_b to his marginal benefit of an extra vote in the bill b.

While this characterizes the solution to the relaxed problem, this ignores incentive constraints for the legislators to vote according to the demands of the executive. The next proposition, then, indicates that this can be implemented in an incentive compatible way for legislators:

Proposition 1 *Legislators vote in favour of the bill iff:*

$$a_b + c_b x_l + \lambda_b \mathbf{1}(l \in \mathcal{C}) + \epsilon_{lb} \geq 0$$

and otherwise vote against the bill. Equation 5.1 determines the number of votes for a given bill, and as a function of votes, when the number of legislators grows large, λ_b is determined implicitly by

$$LE_{x_l}[\Phi(a_b + c_b^T x_l + \lambda_b)] = V_b$$

As the number of legislators grows large, λ_b is a deterministic function of $a_b, c_b, x_l, u_b^P + \epsilon_{bP}, \mathcal{C}$ (crucially, it doesn't depend on legislator level shocks ϵ_{lb}).

Finally, the government can implement this voting rule in an incentive compatible way by promising transfers t_l that satisfy the participation constraint if and only if legislators have:

$$\int v_{lb}\lambda_b db \geq T_l$$

and promising zero transfers if legislators' votes don't satisfy the constraint above, where T_l is a number on the interval $(-\int |\lambda_b| db, \int |\lambda_b| db)$.

This proposition is useful for a number of purposes: first, it shows that the relaxed problem's solution indeed satisfies incentive constraints: this is broadly consistent with the ideas in Jackson and Sonnenschein (2007), who show that by linking decisions across independent problems (here, different bills), principals get to extract truthful reports about private information (ϵ_{lb}) from the agents (the legislators). Here, the mechanism takes a particularly simple form: let legislators vote, but promise to give them the benefits of being in government only if they vote sufficiently often in the direction the president wants them to vote, and specially so in more important bills for the president (with higher levels of pressure λ_b).

Secondly, the proposition characterises the voting patterns by legislators in a way that will be useful for empirical implementation: votes depend on ideologies x_l, bill characteristics a_b, c_b, and a government pressure term λ_b that is exogenous to residuals in a voting equation as long as coalitions are exogenous to these residuals. Crucially, this specifies a voting model that is a simple extension of voting models in the literature (such as Poole and Rosenthal (1985),(1991), Clinton, Jackman and River (2004), Imai, Lo, Olmstead (2016)): it includes the usual ideology terms, and an extra term for government pressure. This empirical model is similar to Snyder and Groseclose (2000)'s model of uniform party pressure (across legislators), with the differences that in the current setting, (i) it is reasonable to assume one-sided pressure from the government coalition, without equivalent countervailing pressures from the opposition, and (ii) that we might have pressure also for bills that are sure to pass, or sure not to pass. Consequently, the source of variation identifying this model is different from Snyder and Groseclose (2000)'s source of variation.

The next result summarises comparative statics results about which bills get approved and rejected. For this, parametrise legislators' preferences as:

$$\tilde{u}_{bl} = \kappa[c_b^T x_l + a_b + \epsilon_{lb}] + \frac{\psi}{1-\psi}[u_b^P + \epsilon_{bP}]$$

That is, I'm first parametrizing legislators' preferences as $\kappa[c_b^T x_l + a_b + \epsilon_{lb}]$ so that κ changes the intensity of legislators' preferences. Then, I'm expressing legislators preferences as a convex combination of these re-parametrised legislator's preferences and the president's preferences (nor-

malized by $\frac{1}{1-\psi}$). For $\psi \to 0$, we converge to the original legislator's preferences, and for $\psi \to 1$, the legislator's preferences become the president's preferences. I'll say that the preferences of the coalition *move in the direction of* the president's preferences if ψ increases. I'll say legislator's preferences *become less important* when κ decreases. I'll say the president likes a bill if $u_b^P + \epsilon_{bP} > 0$, and say he dislikes the bill otherwise. Finally, I'll let the president's preferences be parametrized by $\psi_P[u_b^P + \epsilon_{bP}]$, and I'll say that the president's preferences *become more important* when ψ_P rises. With this in hands, we obtain the following result:

Proposition 2 *Assume the optimal V_b^{GOV} is interior. If the president likes the bill (dislikes the bill), the number of votes V_b^{GOV} on a bill increases (decreases) when*

- *The preferences of the coalition move in the direction of the president (ψ increases)*
- *The president's preferences become more important (ψ_P increases)*
- *If the legislator's preferences become less important (κ falls)*

This result is intuitive. If either the legislator's preferences become less important, or they become more aligned with those of the president, the direct loss faced by the legislator when he is pressured to vote according to the president's preferences is smaller, and the president finds it less costly to include the legislator into the coalition while demanding the legislator to vote together with the president.

This proposition highlights how the shape of the coalition might have strong implications for whether the executive enacts policies representing the preferences of many elected officials. The president might end up simply approving his preferred policies by either forming a coalition with legislators with very similar interests and excluding legislators who diverge from him. Similarly, the president might end up simply approving his preferred policies by electing legislators that have small κ, whose votes are "easier to buy".

Once this is done, we can move to analysing who gets into the coalition.

5.2.3 Coalition formation

We start this section by formally characterizing the problem of which legislators to include in a government coalition, and useful properties of the choice of coalition formation for the empirical exercise. We then move to analyse a particular case of the model, to understand the trade-offs involved in coalition formation. The next proposition formalizes the government's coalition formation problem:

Proposition 3 *The decision to include legislator l in the coalition depends on $x_l = \alpha_l[1, \tilde{x}_l]$, on x_P, the distribution of x_l, and the distribution of parameters across bills (in particular, it doesn't depend on particular realizations of ϵ_{lb}).*

If V_b^{GOV}, V_b^{OPP} are perfect substitutes for the president, the marginal effect over the president's payoff from replacing a legislator with ideology x_l in the coalition by one with ideology $x_l + \nu\omega$ (with $\nu \to 0$) is:

$$\int 2(v_{lb} - v_{lb}^*)c_b\omega db$$

where v_{lb} is the legislator's vote under optimal government pressure, v_{lb}^* is the legislator's vote outside the government coalition.

The proposition characterizes a few important issues: first of all, the coalition is built to create support for many bills simultaneously, as highlighted in the previous proposition. As a consequence of this, the coalition doesn't depend on bill specific draws of shocks $\epsilon_{lb}, \epsilon_{Pb}$, but instead, only on the legislator's ideology, on x_P, and on the distribution of a_b, a_b^P, c_b. As a consequence, for the empirical implementation of proposition 1, the model would suggest that conditionally on x_l and a_b, c_b, we can treat the presence of a legislator in the coalition as exogenous to ϵ_{bl} - see the identification section for more on the plausibility of this.

The second result in the proposition characterises a comparative static result: namely, would the president like to replace a coalition member with a given ideology by one with a different ideology? The answer to this question depends on something akin to a "correlation" between vote differences inside and outside the coalition, and c_b. Intuitively, consider a one dimensional policy case: say that the president is more likely to press for approval when $c_b > 0$ (so that $v_{lb} - v_{lb}^* > 0$ in this case), and to press for rejection when $c_b < 0$. If this is the case, $(v_{lb} - v_{lb}^*)c_b > 0$,

and the president gains by replacing legislators x_l with legislators $x_l + \nu\omega$ when $\omega > 0$, since these legislators will be less likely to be pressured to change their vote in response to entering the government coalition, and hence, they would be cheaper to incorporate into the coalition.

To clarify the implications of this for the process of coalition formation and for the policies approved and rejected, we move to analyse a particular example of the model above.

5.2.4 Example

In this section, I highlight the implications of the model with particular examples of the model above. Throughout, I'll consider a case of a president and legislators who face two types of bills: one type of bill is the usual left-right type of bill - such as redistribution or abortion -, where legislators have different ideologies. The second type of bill is a bill where the president disagrees with all legislators - in the context I'm analysing, this can be a bill decreasing the size of the budget for pork projects, bills for pension reforms or bills for fiscal contractions. In other contexts, these could also include bills increasing executive power and reducing the legislature's power, or an impeachment vote where most of the legislature agrees on the impeachment. The president then needs to decide whether to include legislators of given ideologies $\tilde{x}_l$, or to include legislators who care less about policy and whose votes are more easily bought (with lower α_l).

More formally, consider a 2-dimensional policy case where the president has utility $P(V_b, u) = u$ if $V_b > T$, and $P(V_b, u) = 0$ otherwise, with $u_b^P = [c_b^1, c_b^2]^T [x_P^1, x_P^2]$, with $x_P^1, x_P^2 > 0$. That is, the president has preferences similar to legislators, and is right wing in both ideology dimensions. There are two types of bills: a share q of the bills are of type 1 with $c_b^1 \in \mathbb{R}, c_b^2 = 0$, while $1 - q$ of the bills have type 2, with $c_b^1 = 0$ and $c_b^2 = 1$. Both types of bills have $a_b = 0$, so that on average the president wants to approve both types of bills. For simplicity, I'll assume $\epsilon_{Pb} = 0$. With this, the president will choose for all bills type 2 either $\lambda_b = 0$, or $\lambda_b > 0$ with $V_b = T$. For all bills type 1, the president either choose $\lambda_b = 0$, and when it chooses $\lambda_b \neq 0$, we'll have $\lambda_b > 0$ with $V_b = T$ for $c_b^1 > 0$, and $\lambda_b < 0$ with $V_b = T$ for $c_b^1 < 0$.

Legislators, on the other hand, always have $\tilde{x}_l^2 = -1$, and $\tilde{x}_l^1$ taking values on the real line - so that aside from the pressure of the coalition,

legislators always want to reject bill type 2; and on bill type 1, legislators have different ideologies.

I'll say the legislator/president is right wing if $\tilde{x}_l^1 > 0$ (or $x_P^1 > 0$), and that the legislator is left wing if the opposite is true. Finally, if $c_b^1 > 0$, right wing legislators gain from the bill and left wing legislators lose from it, so I'll say that in this case, the bill moves policy to the right. Analogously, if $c_b^1 < 0$, I'll say the bill moves policy to the left.

With this in hands, we can ask whether holding fixed the size of the coalition, whether the president prefers to include in the coalition legislators with whom he agrees more or less (with higher or lower values of x_l^1). Similarly, we can check whether the president prefers to include in the coalition legislators with lower or higher care for policy (lower or higher α_l). This becomes a simple application of the previous proposition to this particular case. The following proposition analyses this.

Proposition 4 *For all possible coalitions, the president always gains by replacing a legislator with lower $\tilde{x}_l^1$ with a legislator with higher $\tilde{x}_l^1$.*

Holding fixed the ideology of the legislator at $\tilde{x}_l^1 > \bar{x}(q)$, the president gains by replacing a legislator with lower α_l by one with higher α_l.

Holding fixed the ideology of the legislator at $\tilde{x}_l^1 < \bar{x}(q)$, the president loses by replacing a legislator with lower α_l by one with higher α_l. $\bar{x}(q) > 0$ and is decreasing in q.

This proposition shows, in this particular case, the trade-offs involved in choosing who is part of the government coalition. Because the president never pressures for rejection of a bill with $c_b^1 > 0$, and never pressures for approval of a bill with $c_b^1 < 0$, right wing legislators (with $x_l^1 > 0$) are less likely to have their votes reversed by government pressure than left wing legislators. As a consequence, right wing legislators have less to lose from joining the coalition, and are cheaper to included in a coalition by a right wing president.

The results are more subtle when it comes to the inclusion of more vs. less policy motivated politicians (higher vs. lower α_l). When the legislator cares more about policy overall, he cares more both about bills where there is agreement with the president (here, the right-left wing margin), and about bills where there is disagreement with the president (here, the second dimension of policy). Because of this, there is a cost of including legislators who care about policy, related to pressuring them to vote with the government on the bills where they disagree with the

president (bills type 2 for all legislators, and bills type 1 for the left wing legislators). As a consequence, a right wing president would prefer to include left wing legislators in the coalition who care less about policy than those who care more about policy.

For right wing legislators, on the other hand, caring more about policy payoffs (higher α_l) implies more disagreement with the president on bills type 2, and less disagreement with the president on bills type 1 - due to a lower number of cases where ϵ_{lb} reverses the legislator's preferences on bills type 1. For legislators who are closer to the center ($x_l \in [0, \bar{x}(q)]$), the rise in α_l doesn't fully prevent disagreements with the president in bills type 1 (due to ϵ_{lb} shocks), and increase disagreement on bills type 2. Consequently, an increase in α_l for these legislators is still costly for the president. For sufficiently right wing legislators ($x_l > \bar{x}(q)$), an increase in α_l is more effective at preventing disagreements on bill type 1, and consequently, including policy motivated extreme right legislators on a right wing president's government is valuable to the president.

This proposition, then, brings about the following conflict: if legislators of different ideology have different degrees of policy motivation α_l, the president might need to choose between those that are ideologically aligned and those that are "easy to buy" because they have low policy motivation (low α_l). That is:

Corollary 1 *The president faces a trade off between including moderate legislators aligned with him ($x_l^1 \in [0, \bar{x}(q)]$) who have high policy motivation (high α_l) versus unaligned legislators ($x_l^1 < 0$) who have low policy motivation (low α_l).*

5.3 Context and data

I look at data on roll calls in the Brazilian Chamber of Deputies between January 2008 until December 2016 (53rd to 55th Congress). Each congress has 513 deputies, in a period that includes the second term of Luiz Inácio Lula da Silva's presidency, the first term of Dilma Rousseff's government, the second term of Dilma Rousseff's government until her impeachment, and the first one year and 7 months of Michel Temer's government. I keep elected deputies who started their term within 4 months of the beginning of the congress, and eliminate from the sample the vice-deputies ("suplentes") replacing deputies after the beginning of

the congressional term.[3]

I focus on five types of votes: "constitutional amendments" are votes to add or replace legislation in the constitution. Its approval requires two votes by the Chamber of Deputies and two votes by the senate, each with a 3/5 supermajority. "Supplemental laws" (leis complementares) are bills regulating specific policy matters allowed by the constitution, requiring an absolute majority for its approval (that is, it requires a 'yea' vote by 257 deputies). "Ordinary laws" (leis ordinárias) include votes on typical legislation, requiring a simple majority for its approval (that is, it requires a 'yea' vote by strictly more than 1/2 of the deputies who show up for the vote). "Provisional laws" (medidas provisórias) are laws enacted by the executive presumably under emergency situations. As soon as the executive enacts these bills, they are valid for 60 days even without a vote by the legislature (and potentially extended for further 60 days). The validity of these bills for longer time periods depends on legislative approval by simple majority. Finally, a "transformation law" (projeto de lei de conversão) are bills changing provisional laws, also requiring approval by a simple majority. Crucially, I eliminate votes on procedural requirements from the data (such as votes on the order of voting and things alike), and focus only on "yea"/"nay" votes for each bill. Some bills faced several votes in the Chamber of Deputies. I keep only the last date the bill is voted on.

I focus on deputies who have at least 50 "yea" or "nay" votes. As will be made clear, the methods to estimate the model require both large number of deputies and bills for consistency; for deputies with few votes, their ideology estimates are unlikely to be well estimated. This eliminates 28 of the legislators included in the sample, or 0.45% of the deputy-bill pairs leftover. For a similar reason, I drop bills that have less than 50 voting deputies. This eliminates 0.82% of the leftover deputy-bill pairs.

Also, as is standard in the literature, I focus on bills with on which there is enough disagreement between congressmen, and eliminate bills with more than 95% or less than 5% 'yes' votes among the non-absent deputies. These votes are unlikely to be informative about parameters of the model since they would be justified as high a_b votes, where the likelihood function becomes flat and uninformative about other model para-

[3]This leaves 509 deputies in legislature 53 (2007-2010), 512 deputies in legislature 54 (2011-2015) and 511 deputies in legislature 55 (2015-17).

meters. Additionally, it's likely that these votes represent other forms of vote trading between the government coalition and opposition and informal norms that are unlikely to be captured by the model presented above.

Finally, I adopt the following definition for deputies who are members of the government coalition: I look at deputies in political parties that have at least one ministry.

Table 5.1 documents the number of deputies and bills analysed in each legislature, and throughout the sample. Throughout 2007-2017, 740 bills are kept in the sample. Given that many deputies are re-elected, a total of 963 deputies throughout the 53rd-55th congresses are kept in sample.

5.4 Identification and estimation

5.4.1 Identification

Proposition 1 shows that a legislator votes "yea" if:

$$a_b + c_b^T x_l + \lambda_b \mathbf{1}(l \in \mathcal{C}) + \epsilon_{lb} \geq 0$$

Can we identify from legislators' votes in each bill the values of a_b, c_b^T, λ_b and x_l? At first, note that we could always change the mean of x_l across legislators, simultaneously change a_b, and get the same latent variable above. To avoid this identification problem, I impose $E[x_l] = 0$. Also, as usual in these models, $c_b^T x_l = c_b^T A A^{-1} x_l$ (where A is any $D \times D$ matrix), so without restrictions on x_l, the model is unidentified. Usual restrictions in this model, which are adopted here, are to impose that (i) the different dimensions of x_l are orthogonal to each other (so that different ideology components represent separate components that do not predict each other), and (ii) that $Var(x_l) = \frac{1}{L}\sum_l x_l^2 = 1$.

Under these (or similar) restrictions, a large literature has evaluated this model with $\lambda_b = 0$: in particular, Poole (2000) shows that the rank of x_l is identified in a single dimensional ideology model, and Peress (2012) shows the more general model is non-parametrically identified.

Identification is slightly more nuanced here: if we observed the same governing coalition throughout the sample period, this model would be impossible to be identified. In particular, let the parameters of the model be given by $a_b, c_b^T, \lambda_b, x_l$, and let $\mu = E[x_l | l \in \mathcal{C}] - E[x_l | l \notin \mathcal{C}]$. This

creates the same voting probabilities as a model with parameters a_b, c_b^T, $\tilde{x}_l = x_l - \delta\mu\mathbf{1}(l \in \mathcal{C})$ and $\tilde{\lambda}_b = \lambda_b + \delta c_b^T \mu$. That is, without variation in the members of the government coalition, it is impossible to separate between the pressure effect of being inside the government coalition λ_b from differences in ideology between the members of the government coalition and the members in opposition.

If we observe exogenous variation in the members of the government coalition - that is, if the distribution of ϵ_{lb} doesn't change when a legislator moves in and out of the government coalition (or, ϵ_{lb} is statistically independent of being in the government coalition) -, things are different. We could use the results in Peress (2012) on the sample of legislators out of the government coalition (permanently or temporarily) to identify these legislators' ideology and bill effects c_b, a_b. Under the assumption that there is always a large enough set of legislators in the opposition, c_b, a_b can be estimated for all bills. As a consequence, this allow us to know $a_b + c_b^T x_l$ for legislators who switch into and out of the government coalition. Based on this, we could identify λ_b from the set of legislators switching in and out of the government coalition, from a binary choice model of legislators' votes for bill b on $a_b + c_b^T x_l$ and on $\mathbb{1}(l \in \mathcal{C})$. Finally, once we know λ_b, we can identify x_l for legislators who are permanently inside the government's coalition from a binary choice model of votes from a given legislator l on variables $a_b + \lambda_b, c_b^T$ (restricting the coefficient on $a_b + \lambda_b$ to be one). The coefficients on c_b^T, then, are the government coalition's legislators' ideology.

Crucially, this argument for identification relies on independence of legislators' participation in the government coalition and ϵ_{lb}. How plausible is this? Crucially, the key challenge is that changes in legislator's preferences for future bills might drive the formation of a coalition. The identification of the model comes from assuming that changes in c_b, a_b over future bills, together with fixed x_l for each legislator, does well enough to capture changes in demands for bills, so by controlling for these effects, we should be controlling for legislator's expected changes in preferences driving the formation of a coalition. Given that the model is specified for a multidimensional ideology space $x_l \in \mathbb{R}^D$, this doesn't seem like a very strong assumption: ultimately, with a large enough number of ideology dimensions, applications of ideology models like the above

explain a substantial portion of the variation in the data.[4]

Beyond this, in the particular application of this chapter, the main changes in governing coalitions during my sample period happen in January 2011, November 2013, January 2015, April 2015 and April-May 2016. In January 2011 and 2015, the changes to government coalitions happen due to new presidential terms and Congress's composition. In November 2013, PSB leaves the government coalition to be able to launch its own candidate for presidency, as supposed to supporting Dilma Rousseff. In April 2015, PROS leaves the coalition because of a fight with the main coalition partner, PMDB. Finally, in 2016, the impeachment - driven by Dilma Rousseff's lack of popularity, economic growth at -4.5% per year, corruption scandals affecting legislators and members of Dilma's party - drove a large change in the government coalition. These changes over this period seem to be less likely to be due to shocks to demands for/against bills driving the formation of coalitions; and more related to changes in the presidency and overall composition of the legislature.

With this in mind, I take as a baseline identification assumption that conditional on a_b, c_b^T, x_l, changes in the coalition are exogenous to ϵ_{lb}. I rely on robustness checks presented later on to confirm this assumption.

It is worth it to mention a possible alternative identification strategy: Snyder and Groseclose (2000) note that if the president only cares about getting bills approved, the model predicts that whenever there is party pressure, the aggregate vote shares should sum to V_b. Hence, if we knew V_b, we could estimate a_b, c_b, x_l from bills with aggregate vote shares far from V_b, and then use these estimates to run a binary choice model of votes for a bill b with aggregate vote share close to V_b on a constant, x_l and $\mathbf{1}(l \in \mathcal{C})$ to recover a_b, c_b and λ_b.

A few issues motivate the different identification strategy presented in this chapter: first, while the president might press for approval of a bill, the president might also press for approval beyond what's necessary for approval as a signal of strength. In other words, we might not know the shape of the function $P(V_b, u_b^P)$, and as a consequence, we might not know the number of votes the president is targeting to obtain. If this is the case, on bills with aggregate vote shares far from V_b, we might be

[4]In applications, estimates of ideology in the U.S. Congress, 1-dimensional ideologies correctly classifies around 80% of votes (Poole and Rosenthal (1985)), and changes in ideology within legislators add relatively little explanatory power to the model (Poole and Rosenthal (1991)).

capturing as ideological components something that is actually presidential pressure, and consequently, we might underestimate pressure on bills close to V_b. Instead, I focus on the identification strategy using variation in the members of the coalition, and verify ex-post whether close bills have significantly higher pressure coefficients λ_b.

5.4.2 Estimation

At first, one could try to estimate this model by maximum likelihood. Yet, with 963 deputies and 569 bills, this implies estimating $963D$ parameters for ideology of deputies, plus $569(D+2)$ parameters for bill effects a_b, c_b, λ_b, with a latent variable that is not linear in parameters (since c_b shows up multiplying x_l.

To deal with this, I estimate the model using the following EM algorithm[5]. At round t of iteration, specify the log-likelihood function of the data as if the latent variable $\tilde{u}_{bl}$ was observed. Under the normality assumption on the residuals, the log-likelihood is:

$$\sum_{l,b} \kappa - [\mathbf{1}(v_{lb} = 1, \tilde{u}_{bl} > 0) + \mathbf{1}(v_{lb} = -1, \tilde{u}_{bl} < 0)](\tilde{u}_{bl} - a_b - \lambda_b \mathbf{1}(l \in \mathcal{C}) - c_b^T x_l)^2 \tag{5.2}$$

Clearly the latent variable $\tilde{u}_{bl}$ isn't observed. We then replace it by its expectation conditional on the parameter estimates from round $(t-1)$, and conditional on v_{lb}. Letting $u_{lb}^{(t-1)} = a_b^{(t-1)} + \lambda_b^{(t-1)} \mathbf{1}(l \in \mathcal{C}) + [c_b^{(t-1)}]^T x_l^{(t-1)}$, it can be shown that:

$$\tilde{u}_{bl}^{e,(t)} = E[\tilde{u}_{bl} | a_b^{(t-1)}, \lambda_b^{(t-1)}, c_b^{(t-1)}, x_l^{(t-1)}, v_{lb}] = u_{lb}^{(t-1)} + v_{lb} \frac{\phi(v_{lb} u_{lb}^{(t-1)})}{\Phi(v_{lb} u_{lb}^{(t-1)})} \tag{5.3}$$

In round (t), then, I estimate the parameters of the model by maximizing the log-likelihood in 5.2, replacing the latent variable $\tilde{u}_{bl}$ by its expectation in 5.3.

In summary, the algorithm proceeds as follows: at stage t

[5]See Dempster, Laird and Rubin (1977) showing that the EM algorithm converges to local maxima of the log-likelihood. Imai, Lo and Olmstead (2016) use the EM algorithm to perform Bayesian estimation of an ideology model. Here, I present an EM algorithm to perform classical estimation of the maximum likelihood estimate of parameters.

1. take initial guesses for the parameters
2. replace $\tilde{u}_{bl}$ in 5.2 by the expectation $\tilde{u}_{bl}^{e,(t)}$ in 5.3
3. choose parameters to maximize 5.2
4. take these parameters as initial guesses for the parameters in the next stage, and move to stage $t+1$

This algorithm is a particular case of Dempster, Laird and Rubin (1977)'s EM algorithm. They show that the following iterative procedure reaches a local maximum of the likelihood function.

Given this, the problem becomes one of iteratively solving for the least squares estimate of $\tilde{u}_{bl}^{e,(t)}$ on the right hand side $a_b + \lambda_b \mathbf{1}(l \in \mathcal{C}) + c_b^T x_l$. If we knew the value of a_b, λ_b, we could run the non-linear regression of $\tilde{u}_{bl}^{e,(t)} - a_b - \lambda_b \mathbf{1}(l \in \mathcal{C})$ on $c_b^T x_l$, and under the identification assumptions above, this is well-known to yield the principal component estimates of factor loadings and scores as estimates of c_b, x_l. At the same time, it is easily verified that the least squares estimates for a_b and λ_b yield:

$$a_b + \lambda_b \frac{\sum_l \mathbf{1}(l \in \mathcal{C})}{L} = \frac{\sum_l \tilde{u}_{bl}^{e,(t)}}{L}$$

$$a_b + \lambda_b = \frac{\sum_l \tilde{u}_{bl}^{e,(t)}}{\sum_l \mathbf{1}(l \in \mathcal{C})} - c_b^T \frac{\sum_l x_l \mathbf{1}(l \in \mathcal{C})}{\sum_l \mathbf{1}(l \in \mathcal{C})}$$

This suggests the following iterative procedure for the least squares problem in stage 3: start with initial guesses of c_b, x_l, compute a_b, λ_b from the system of equations above, and based on this a_b, λ_b, take principal components of $\tilde{u}_{bl}^{e,(t)} - a_b - \lambda_b \mathbf{1}(l \in \mathcal{C})$. Take these new estimates of c_b, x_l, and repeat. In simulations, within a few rounds of iteration - 3 to 5 -, estimates become highly correlated across rounds.

The full procedure, while intricate, gives substantial gains in estimation time: the full model is estimated in under 10 minutes in a dataset of 547,947 observations. This is what allows me to estimate the full model jointly. Additionally, even with a low number of iterations on the EM procedure, parameters are highly correlated with the truth in Monte Carlo simulations.

5.5 Results

5.5.1 Government coalition and voting: reduced form estimates

First, I provide correlations suggesting that legislators that join the coalition members tend to vote more often together with the president. To show this, I run the following regression:

$$y_{lb} = \beta \mathbf{1}(l \in \mathcal{C}_b) + \delta_l + \delta_b + \epsilon_{lb}$$

In the regression above, y_{lb} is a dummy variable indicating when legislator l voted in the same way as the president's party for bill b. For robustness, I define the president's party's vote as "yea" if at least $m\%$ of that party voted "yea", and as "nay" if at least $m\%$ voted "nay", and look at different thresholds m. Legislators in the president's party are excluded from the sample. The term δ_l is a legislator fixed effect, and δ_b is a bill fixed effect. The variable $\mathbf{1}(l \in \mathcal{C}_b)$ indicates whether the legislator l is in a political party that, at the time of the vote on bill b, had a cabinet position. The coefficient of interest here is β. Standard errors are clustered at the bill level and at the deputy level, as in Cameron, Gelbach, Miller (2011).

Table 5.2 shows the results for this regression. Throughout the period, conditional on legislator and bill effects, legislators switching into the government coalition at 10.8p.p. more likely to vote in the same way as a majority of current president's party (see the first two columns). This result is robust to the choice of threshold: if we assume that the president's party only took a position if more than 85% of its members vote in the same way, we still get that members of the government coalition are 12p.p. more likely to vote in the same way as the president's party when more than 85% of the president's party agrees. Finally, while it might be true that a part of the effect is driven by pressuring legislators in the coalition to not be absent, this cannot explain the full correlation: even if the additional legislators in the coalition voting for the bill were to vote always with the president's party, a back of the envelope calculation suggests we'd still have that legislators joining the coalition vote together

with the president's party 8.1p.p. more often.[6]

These results are not driven solely by groups of parties who vote together only because of ideology (as opposed to government pressure) entering and exiting the government coalition simultaneously. To show this, I explore the fact that after the impeachment of Dilma Rousseff in May 2016, there was a significant number of parties switching into and out of the government coalition, while another set of parties stayed inside the government coalition throughout. I then estimate the following regressions:

$$y_{lb} = \beta_{pre}\mathbf{1}(l \in \mathcal{C}_b) \times Pre\_May\_16 + \beta_{post}\mathbf{1}(l \in \mathcal{C}_b) \times Post\_May\_16 + \delta_l + \delta_b + \epsilon_{lb}$$

where y_{lb} now measures whether legislator l votes in the same way as more than $m\%$ of a reference party. I'll look at results for the three largest parties staying in the government coalition throughout the sample period (PMDB, PP and PR), the three largest parties leaving the government coalition after 2016 (PT, PDT and PCdoB), and the three largest parties entering the government coalition (PSDB, DEM, PPS). Once again, I exclude legislators in the reference party from the regression.

Take for instance the regression of a legislator voting with PSDB, a party entering the government coalition in 2016. The coefficient β_{pre} captures whether joining a legislator joining the government coalition prior to 2016 starts voting more often with PSDB. The coefficient β_{post}, in turn, estimates whether a legislator joining the government coalition after 2016 starts voting more often with PSDB. Hence, the difference $\beta_{post} - \beta_{pre}$ stands for the difference in effects when PSDB is inside vs. outside the government coalition.

Table 5.3 shows the results for this regression using a subset of parties leaving the government coalition as reference parties (PT, PDT, PCdoB). Before May 2016, a legislator joining the government coalition became

[6]This is a back of the envelope calculation similar to Lee (2009). Let the share of congressmen not absent among the opposition be a, and let b be the share in the opposition (among those not absent) voting together with the government's party. Let v_A be the share among the government coalition voting with the government's party among those who are not absent neither in government nor in opposition. Let v_B be the share of coalition members voting together with the government among those who turn out to vote only when they are in government (share $a + 0.03$). Using the estimates in columns 1 and 3, the treatment effect $v_A - b = 0.108 + \frac{0.03(b+0.108-v_B)}{a} > 0.081$.

4.9-6 p.p. more likely to vote with PT. After May 2016, when PT left the government coalition, a legislator joining the government coalition became 3.3-4.6 *less* likely to vote together with PT. Once again, the difference $\beta_{post} - \beta_{pre}$ is significantly different from zero at the 1% level. Similar results show up for PDT and PCdoB. While congressmen joining the government coalition often votes more often against PDT's typical position, this effect becomes stronger after May 2016. Finally, when it comes to PCdoB, while before May 2016, congressmen joining the coalition typically increase by 0.9-3.5p.p. how often they vote with PCdoB (non-robustly), after May 2016, congressmen joining the coalition reduce the share of times voting with PCdoB by 9.3-11.3p.p on average.

Table 5.4 shows the results for this regression using a subset of parties joining the government as the reference parties. As expected, before May 2016, legislators joining the coalition became 2.5 to 3.5p.p. less likely to vote with PDSB, -0.5 to 4.4p.p. less likely to vote with DEM, and 3.3 to 3.6p.p. less likely to vote with PPS. After May 2016, when these parties join the governing coalition, legislators joining the coalition become 13.7 to 17.5p.p. more likely to vote with PSDB, 8.1 to 12.3 p.p. more likely to vote with DEM, and 15.8 to 18.2 p.p more likely to vote with PPS.

Finally, table 5.5 shows results using a subset of parties that stayed in the government coalition throughout the sample period (PMDB, PP, PR). As expected, joining the government coalition increases the propensity to vote with these parties both before and after 2016, with an effect ranging from 8.4p.p. to 30.5 p.p.

These results suggests that joining the government's coalition has a strong correlation with a legislator's vote. This correlation is not due to parties moving together in and out of the government coalition: instead, legislators who join the coalition start voting more similarly to parties that stayed inside the coalition, and more similarly to parties concurrently joining the coalition. At the same time, they stop voting similarly to parties that leave the government coalition.

Still, it is hard to know whether expected changes in the propensity of different legislators to vote together that drive the formation of coalitions. In particular, it is hard to guarantee here that there aren't legislator-bill specific effects driving the formation of coalitions and the similarity of votes across legislators. In fact, most basic models of voting would predict such legislator-bill specific effects (here, in $c_b^T x_l$). Moreover, it is hard to

use the current results to perform counterfactual exercises and evaluate the question of interest without estimates of ideology for each legislator, and estimates of government pressure.

5.5.2 Structural model

In this section, I present estimates of the structural model presented above. I estimate the model with a two-dimensional ideology space for legislators using the EM algorithm above. I start by describing the estimates obtained: I show that estimated ideologies are consistent with at least one measure of ideology for political parties in Brazil, and which bills face higher government pressure. I then move to check whether the model's predictions for (i) when does the government exert pressure, and (ii) for who should be included in the government coalition. Finally, I check the consequences of the estimates for policy, showing how often do bills would get approved/rejected without pressure when the president dislikes/likes the bill, and how much presidential pressure changes these numbers.

Main estimates

Ideology estimates: How much do estimated ideology measures correlate with alternative measures for ideology? I start by presenting evidence of the correlation between mean estimated ideologies from the structural model with a proxy for ideology of Brazilian political parties coming from a survey of legislators by Power and Zucco (2011). More precisely, I use the computed ideology from the model, compute average ideology for each political party in a given year, and compare the results with the cross sectional ideology measures from Power and Zucco (2011).

Graph 5.2 shows the results. While the first measure of ideology from the structural model has only a week correlation with the survey measures of ideology, the second dimension of ideology from the structural model is strongly correlated with legislators' self report of preferences. Moreover, while legislators' ideologies are constant over time by assumption in the model, parties have different legislators in different congressional terms. While this could imply that parties' ideologies change across terms, the evidence suggests little variation in average party ideology across terms in the structural model. While a part of the result comes mechanically from

the fact that legislators' ideologies are assumed to be constant, there is substantial turnover in the Brazilian congress across congressional terms, so this result is not immediately obvious.

This result shows that the model is able to both capture measures of ideology that are closely related to at least one alternative proxy of ideology, and other unobservable drivers of legislators' votes that might not be as easily captured. At the same time, the results show that capturing both these dimensions of ideology matter: figure 5.2 shows that before 2016, coalitions were formed on the basis of legislators that had were to the right on the first dimension of ideology, but that were both left and right on the second dimension of ideology (which is more closely related usual ideology proxies). After 2016, on the other hand, the coalition includes parties of all types in the first dimension of ideology, but only relatively right wing legislators in the second dimension of ideology.

This, in turn, highlights the advantages of the structural model: by being able to capture a flexible space of ideologies from roll call data, the model is able look at the effect of being in the government coalition and being pressured to vote in a given way, while controlling for legislators' propensity to vote together. This implicitly allows me to control for, first of all, well known alternative proxies for ideology. Second of all, this allows me to control for other ideological dimensions that are not easily captured by usual ideology proxies, but that matter for voting and coalition formation.

Government pressure estimates: When does the government exert pressure for legislators to vote? Given the estimates do not impose that many of the pressure coefficients might be zero, there might be considerable uncertainty on the estimation of λ_b. Moreover, a given λ_b has different implications for a bill with $a_b = 0, c_b = 0$ (where legislators are divided and the marginal effect of λ_b is large) vs. a bill with a_b high and $c_b = 0$ (where legislators are mostly favourable to the bill, and the marginal effect of λ_b is small). Given this, I present evidence here on the expected number of votes turning from nay to yea due to government pressure:

$$LE_{x_l}\left[\Phi(a_b + c_b^T x_l + \lambda_b \mathbf{1}(l \in \mathcal{C}) - \Phi(a_b + c_b^T x_l)\right]$$

and the expected number of votes reversed due to government pressure

(the absolute value of the expression above).[7]

The evidence is consistent with some government pressure. Figure shows the density of the number of votes/share of votes among voting coalition members turning from nay to yea due to government pressure. Together with this, table 5.6 shows summary statistics of the absolute value of (i) expected number of votes reversed as a result of pressure across bills, (ii) expected share of reversed votes among legislators in the coalition voting for a bill. Throughout the sample period, the government coalition has on average 282 legislators, and for a typical bill, 192 legislators in the government coalition cast a vote for a given bill on average. In 25.3% of these bills between 2008-2016, more than 30 votes among the congressmen voting were expected to be reversed as a result of government pressure on coalition members. In 9.5% of the bills, more than 60 votes were expected to reverse as a result of government pressure on coalition members. As a consequence, for 25% of the bills, more than 17% of the members of the coalition voting for a bill got their votes reversed by government pressure. For 10% of the bills, more than 29.6% of the members of the coalition voting for a bill had their votes reversed due to government pressure.

Table 5.7 documents how the government coalition's pressure has evolved over time. While the government managed to pressure coalition members to vote with them up until 2010 - managing to reverse the votes of more than 30% of coalition members in 25% of the bills -, this pressure falls over time. In particular, in 2015-16, for 90% of the bills, less than 10% of the votes of coalition members were reversed. Finally, table 5.8 suggests that the types of bills facing higher pressure are specially provisional/supplemental laws. These are bills that the president turns into temporary law before a vote by the legislature, and that the president presumably considers to deal with urgent matters.

How can the model explain the estimated pressure levels? If we take the model to mean that the government is only exerting pressure to approve/reject bills (as opposed to pressuring legislators to signal that the coalition is strong, or some other motivation), the model would suggest that whenever the government exerts pressure, the bill is ap-

[7]The number of votes reversed due to government pressure for bill b is $\sum_l |v_{lb} - v^*_{lb}|$. For a given bill b, if $\lambda_b > 0$, $|v_{lb} - v^*_{lb} = v_{lb} - v^*_{lb} > 0$ for all l, and $\sum_l |v_{lb} - v^*_{lb}| \to LE_{x_l}[v_{lb} - v^*_{lb}]$. Similarly, if $\lambda_b < 0$, $|v_{lb} - v^*_{lb}| = v^*_{lb} - v_{lb} > 0$ for all l, and $\sum_l |v_{lb} - v^*_{lb}| \to LE_{x_l}[v^*_{lb} - v_{lb}]$.

proved/rejected by close margins. Given that the model provides estimates of government pressure on all bills (with close margins or lopsided votes), we can see whether the distribution of pressure differs across lopsided votes vs. close votes. To do so, I'll let a bill be a close vote bill if the difference between the share voting yea and the share necessary for approval is above $-C$ and below C. I let C vary over years, so that in each year, 10% of the bills are assumed to be close.[8] With this, then, I compute the density of pressure for bills classified as close votes, and the same density for bills classified as lopsided votes.

Figure 5.3 shows evidence of this: while there are some differences in the density of pressure for close votes and lopsided votes, it is very hard to say that lopsided votes face no government pressure. This result is consistent with presidents exerting pressure on the coalition not only to get bills approved/rejected, but also to obtain large majorities in favour of their position.

Does a more general preference function for the president help in explaining the patterns? The FOC suggests that the government's decision on λ_b depends on the president's preferences for the bill u_b^P, the number of votes for the bill by the opposition, the number of votes by the coalition, and the coalition's sensitivity to pressure.

To make progress on this, I start by proxying the president's preferences for the bill as his party's average preference for the bill. For this, I compute for each legislator in the president's party its expected utility given their votes:

$$E[u_{bl} + \epsilon | v_{bl}] = a_b + c_b^T x_l + v_{lb} \frac{\phi(v_{lb}[a_b + c_b^T x_l + \lambda_b])}{\Phi(v_{lb}[a_b + c_b^T x_l + \lambda_b])}$$

and average this for all members of the president's party.

With this in hands, I then linearise the president's first order condition around $(V^{OPP}, \tilde{u}_b^P, \lambda_b) = (0, 0, 0)$, to obtain:

$$\lambda_b = \frac{1}{\frac{\partial^2 P}{\partial (V^{GOV})^2} \int \phi(a_b + c_b^T x_l) \mathbf{1}(l \in \mathcal{C}) dl - 1} \times \left[-\frac{\partial^2 P}{\partial V^{GOV} \partial V^{OPP}} V^{OPP} - \frac{\partial^2 P}{\partial V_b^{GOV} \partial u} [u_b^P + \epsilon_{bP}] - \frac{\partial P}{\partial V_b^{GOV}} \right]$$

[8] When adopting a fixed cutoff, most close vote bills happen in the last few years of my sample, when pressure is overall lower for potentially other reasons.

where the derivatives are evaluated at $V^{GOV} = \int \Phi(a_b + c_b^T x_l) dl$, $V^{OPP} = 0$ and at $\tilde{u}_b^P = 0$. If V_b^{GOV}, V_b^{OPP} are perfect substitutes, $\frac{\partial^2 P}{\partial (V^{GOV})^2} = \frac{\partial^2 P}{\partial (V^{GOV}) \partial (V^{OPP})}$. That being said, as argued above, this is not necessarily the case: if the president cares about stopping the opposition from buying votes away from the coalition, it isn't clear that opposition votes enter symmetrically to pro-government votes. Similarly, if the government cares about signalling his capacity to pressure effectively and influence the legislators in the coalition, the government will naturally care differently about voting behaviour in the coalition and in the opposition.

Can the model above be identified? For this, we can impose an assumption that ϵ_{bP} is independent $a_b, c_b^T, x_l, \mathbf{1}(l \in \mathcal{C})$: one way of justifying this assumption is that the correlation between legislators' preferences and the president's preferences is fully captured by correlation between $a_b + c_b^T x_l$ and u_b^P, and that the coalition will form not taking into account individual realizations of ϵ_{bP} for individual bills, but instead, on the distribution of ϵ_{bP}. I discuss the possibility of endogenous bill parameters a_b, c_b (due to bill authoring and bills reaching the floor) later in this section.

Once this is done, the model above can be estimated by non-linear least squares, treating $\frac{\partial^2 P}{\partial (V^{GOV})^2}$, $\frac{\partial^2 P}{\partial V^{GOV} \partial V^{OPP}}$, $\frac{\partial^2 P}{\partial (V^{GOV} \partial u}$ and $\frac{\partial P}{\partial V^{GOV}}$ as parameters to be estimated. More precisely, we are identifying $\frac{\partial^2 P}{\partial (V^{GOV})^2}$ from variation in the set of legislators in the government coalition, which is used to obtain variation in the sensitivity of the coalition to pressure $\int \phi(a_b + c_b^T x_l) \mathbf{1}(l \in \mathcal{C}) dl$. To identify $\frac{\partial^2 P}{\partial V^{GOV} \partial V^{OPP}}$, I use variation in the opposition's votes, which are not influenced by the president's efforts to pressure its coalition. The term $\frac{\partial^2 P}{\partial V_b \partial u}$ is identified from variation in the party of the president and its preferences from bills (as proxied by his party's preferences for bills), as well as variation in bill characteristics a_b, c_b that a given president likes and dislikes. Finally, $\frac{\partial P}{\partial V_b}$ is set to capture the mean level of pressure, something that is intuitive from the president's first order conditions. Given that in theory, these parameters depend on $\int \Phi(a_b + c_b^T x_l) \mathbf{1}(l \in \mathcal{C}) dl$, I estimate this model both on the overall sample, and on subsamples of terciles of $\int \Phi(a_b + c_b^T x_l) \mathbf{1}(l \in \mathcal{C}) dl$. I also estimate the model above, and the restricted version where the president only cares about vote totals.

Table 5.9 shows the results for this model.[9] The first column estimates a model forcing V_b^{GOV}, V_b^{OPP} to be perfect substitutes. The point estimates suggests that the presidents returns to votes $\frac{\partial^2 P}{\partial V^2}$ are concave. The second column illustrates why this result comes about: we can see from the estimate for $\frac{\partial P}{\partial V^{GOV} \partial V^{OPP}}$ that for the president, votes by the opposition for the bill mildly reduce the returns to votes by the coalition (suggesting they're substitutes). At the same time, votes by the government face significant increasing returns, as seen from the estimate for $\frac{\partial P}{\partial (V^{GOV})^2}$ (suggesting V_b^{GOV} and V_b^{OPP} are not perfect substitutes). Given that the first column uses the variation in votes from the opposition to identify the coefficient for increasing/decreasing returns for votes inside the government coalition, this drives the first column to significantly underestimate the extent to which there are increasing returns to votes inside the government coalition. The second column is able to prevent this by looking at variation in votes from the government only coming from changes in the government coalition, and separating this from variation in votes coming from the opposition. Doing so also significantly increases the explanatory power of the model, indicating that the more general version of the model is significantly better at explaining the data.

This distinction also has implications for the effects of the president's party preferences over pressure. The first column suggests that the president's party's utility from the bill increases mildly the return to votes by the coalition in $\frac{\partial P}{\partial V^{GOV} \partial u}$. This result gets stronger in the second column. The reason for this is relatively subtle: when there are increasing returns to votes by the coalition, then as the coalition becomes more sensitive to pressure, that increases the coalition's voting in response to pressure, and in turn the returns to votes by the coalition. Because the returns to votes by the coalition are higher, a given change in the preferences for the bill u_b^P has a higher impact over λ_b. Consequently, under increasing returns to votes by the coalition, the impact of u_b^P over λ_b is higher when the coalition is more sensitive to pressure. Analogously, when there are decreasing returns to votes by the coalition, the impact of u_b^P over λ_b is higher when the coalition is less sensitive to pressure. With this in mind,

[9]While I present standard errors clustered at the month level, the regression contain estimated regressors. Instead, joint tests that all coefficients are zero are valid (see Wooldridge (2002)), since the asymptotic distribution of the estimates does not depend on the estimated regressors under the null hypothesis that all coefficients are zero. Consequently, I keep the analysis to interpreting the magnitudes of coefficients and $R-2$ values.

if the data suggests that λ_b is more sensitive to u_b^P when the coalition is more sensitive to pressure, then if we mis-estimate $\frac{\partial^2 P}{\partial (V^{GOV})^2} < 0$, the way the data will capture the interaction between u_b^P and sensitivity to pressure by the coalition is by estimating $\frac{\partial^2 P}{\partial V_b \partial u}$ to be small.

Columns 3-5 allow the coefficients to vary with $\int \Phi(a_b + c_b^T x_l) dl$. Results suggest that the president's party's preferences for the bill have a particularly high effect over the returns to votes when $\int \Phi(a_b + c_b^T x_l) dl$ takes on intermediate values. For the bottom and highest tercile of $\int \Phi(a_b + c_b^T x_l) \mathbf{1}(l \in \mathcal{C}) dl$, the returns to votes for the president don't change too much in response to the president's party's preferences for the bill.

This model uses considerable variation in bill-level parameters a_b, c_b to identify the parameters of the model. Yet, despite this being unmodelled, it is possible that the bill parameters are endogenous: this would be the case if shocks to the president's preferences for bills bring up different bills.

To deal with this, I move to estimating the model excluding first the bills proposed by the executive, and then excluding the bills proposed by any member of the government coalition. If the results remain unchanged, this lends credibility to the idea that the estimates above are not driven by endogeneity of bill-level parameters, but instead by variation across bills in the typical preferences of the government coalition, opposition and the president.

Table 5.10 shows the results. Columns (1) and (2) show the results excluding bills authored by the executive. Once again, allowing the model to have less than perfect substitution between government and opposition votes improves the explanatory power of the model, and yields more intuitive results, in line with the results above. That being said, the explanatory power of the model is small, suggesting that for this subset of bills, the model doesn't explain a substantial part of the variation.

Columns (3) and (4) then move to exclude bills authored by a member of the government coalition (either the executive or a legislator in the coalition). Once again, allowing the model to have less than perfect substitution between government and opposition votes improves the explanatory power of the model substantially. It also yields results in line with the results in the main first order condition. Moreover, the substantial explanatory power of the model suggests that the FOC above helps

in explaining the data on government pressure on bills authored by the opposition.

Taken together, the evidence suggests that the model is informative about the government's decision to put pressure on legislators: the president's FOC has substantial explanatory power of the pressure coefficients estimated from the voting model. It provides evidence that when the government coalition's legislators have more moderate policy preferences (with mid-level $\int \Phi(a_b + c_b^T x_l)\mathbf{1}(l \in \mathcal{C})dl$), the government puts pressure on legislators, specially when the president's party cares a lot about the policy, and when the coalition is sensitive to pressure. Finally, this result seem to be robust to excluding bills authored by the executive, and to looking only at bills authored by the opposition, suggesting that endogenous bill parameter choices are not driving the results.

Who is included in the government coalition?

With this in mind, which legislators are included in the government coalition? Figure 5.4 shows a map of where political parties lie in the two dimensional ideology space, and which parties were in and out of the government coalition. Before the impeachment in May 2016, PT is on the presidency, and besides PCdoB and PDT, most parties are relatively far apart from PT on the ideological spectrum. As a consequence, PT formed a coalition with political parties that were relatively closeby on the first dimension, but far away on the second dimension of politics. After May 2016, PMDB starts occupying the presidency, and many political parties have ideologies similar to PMDB. PMDB then forms a coalition of political parties including parties very nearby them on both dimensions, and including parties that were close on the second ideological dimension (typical correlate with other measures of left-right wing in Brazilian politics), but far away in other dimensions.

The model suggests that when a president forms a coalition with legislators ideologically far away from them - such as PT in 2016 -, they'll prefer to form a coalition with legislators that are more sensitive to pressure and who care less about policy. When the president, instead, forms a coalition with legislators who are closeby, he prefers legislators who are less sensitive to pressure and who care more about policy.

To check for this, I compute for each deputy measures of average sensitivity to pressure: more precisely, I let the legislator's sensitivity to

pressure when he has no pressure be defined as

$$\int \phi(a_b + c_b^T x_l) db$$

That is, this is the derivative of the legislator's probability of voting yea with respect to λ_b, evaluated at $\lambda_b = 0$, then averaged over all bills. I also let the legislator's sensitivity to pressure when he has his actual pressure to be defined as:

$$\int \phi(a_b + c_b^T x_l + \lambda_b \mathbf{1}(l \in \mathcal{C})) db$$

Figure 5.5 shows the density of both measures of sensitivity to pressure before and after May 2016. As can be seen, before 2016, during PT's government, the coalition's legislators' sensitivity to pressure when they have no pressure is higher and substantially more dispersed than the sensitivity to pressure for the opposition's legislators (top left corner). After May 2016, during the PMDB's government, the result reverses, and members of the coalition have significantly less sensitivity to pressure than members of the opposition (top right corner). This is consistent with the theoretical prediction that coalitions with far away legislators should include legislators that are more sensitive to pressure, while coalitions with closeby legislators should include legislators that are less sensitive to pressure.

Interestingly, the bottom graphs show that the government, after selecting legislators in this way, chooses to put enough pressure on the coalition for their sensitivity to pressure to reduce. In this way, both pre and post 2016, legislators in the government coalition are significantly less sensitive to government pressure.

Consequences for policy

FInally, what are the consequences of the model for policy? In particular, how often does the government get bills it likes approved and bills it doesn't like rejected? How different would the numbers be if there was no government pressure?

To do so, first note that the model suggests that $P_v = \lambda_b$, so that whenever the marginal utility to the president of an extra vote for the bill is positive (and by assumption, the marginal utility to extra votes

is then always positive for this bill), then $\lambda_b > 0$. Similarly, when the marginal utility to the president of an extra vote for the bill is negative, $\lambda_b < 0$. Hence, we can look at the number of bills approved when $\lambda_b > 0$, and the number of bills rejected when $\lambda_b < 0$, and how different would the numbers be if there was no government pressure.

Table 5.11 shows results on this. Consistently with the fact that pre May 2016, the government formed a coalition based on pressure on ideologically distant candidates, the government has a moderate effect over policy approval: a congress without a government coalition would have approved 79.5% of the bills the president dislikes (which are bills where the president sets $\lambda_b < 0$). Due to government pressure, the government reduces this number to 66.7% of bills it dislikes. Similarly, among bills the president likes (with $\lambda_b > 0$), 75% would have been approved by congress without pressure. The president manages to increase this to 85.5%.

Post 2016, the government forms a coalition of legislators with closeby preferences and little sensitivity to pressure. As a consequence, it relies on low pressure on legislators. Consistently with this, post 2016, 81.6% of the bills that the president dislikes would have been approved, and this number stays unchanged with presidential pressure. Similarly, among bills the president likes, 37.9% of them would have been approved without any pressure, the president manages to increase this to 48.3%.

5.6 Conclusion

This chapter presented a model of coalition formation and voting in congress to understand (i) what are the ideologies of legislators that the president includes in the coalition, and (ii) what are the implications of these coalitions for votes in congress. The results suggest that while a president with many ideologically similar legislators wouldn't be checked anyway - so he doesn't need to exert pressure, and if anything, focuses on legislators that are less sensitive to it -, a president without many legislators nearby can avoid some of the checks and balances by relying on legislators that are more sensitive to pressure - or to "sell their votes" in exchange for particularistic transfers associated with being part of the government.

Despite this capacity to avoid checks and balances, throughout this

sample period, the legislature has provided strong checks against the executive, often approving bills that the president dislikes, and not approving bills the president likes. Further research focusing on the interaction between institutional rules and the coalitions arising in equilibrium would provide important information on how these rules might affect the capacity of the legislature to check the executive. Similarly, further research delving deeper into the process of bill proposals is important to understand other margins through which the legislature does/does not check the executive - the tools developed in this chapter for identification of bill characteristics can be useful for this agenda.

Proofs

Proof of Proposition 1 It is clear that the solution for votes above satisfies the relaxed President's problem. As a consequence, if it is optimal for legislators in the government coalition to vote in that manner, then the voting equation above satisfies also the President's problem with incentive compatibility constraints.

Before showing this, though, it is useful to show that λ_b is a deterministic function of $a_b, c_b, x_l, u_b^P + \epsilon_{Pb}$ and $\mathcal{C}$ (and in particular, doesn't depend on the values of ϵ_{lb} when the number of legislators grow large. To see this, note that whenever the constraint is binding, we have:

$$\frac{1}{L}\sum v_{lb} = V_b$$

By the central limit theorem, for a given bill, the left hand side converges to $E_{x_l}[\Phi(a_b + c_b^T x_l + \lambda_b \mathbf{1}(l \in \mathcal{C}))]$. As a consequence, conditional on V_b, this pressure is a deterministic function of $a_b, c_b^T, x_l, \mathcal{C}$. Using the FOC from the president, then, the choice of V_b is given by:

$$P_v(V_b, u_b^P + \epsilon_{bP}) = \lambda_b(a_b, c_b^T, x_l, \mathcal{C})$$

Consequently, λ_b is a deterministic function of $a_b, c_b, x_l, u_b^P \epsilon_{Pb}, \mathcal{C}$.

If a legislator decides to get the transfer t_l (that satisfies his particip-

ation constraint), he chooses votes to solve:

$$\max \int (2v_{lb} - 1)[a_b + c_b^T x_l + \epsilon_{bl}]db$$
$$s.t. \int v_{lb}\lambda_b db \geq T_l$$

Treating v_{lb} as a continuous variable, we can set up the Lagrangean for this problem. Let μ_l be the Lagrange multiplier on the constraint. The FOC in v_{lb} is then given by:

$$a_b + c_b^T x_l + \epsilon_{bl} + \mu_l \frac{\lambda_b}{2} \geq 0 \rightarrow v_{lb} = 1$$
$$\left[\int v_{lb}\lambda_b db - T_l\right] \mu_l = 0$$

Note that the value of the left hand side of the constraint is at most $\int |\lambda_b| db$ (if legislators vote $v_{lb} = 1$ when $\lambda_b > 0$, and $v_{lb} = -1$ when $\lambda_b < 0$, as the president wants). In the same way, the minimum value of the constraint is $-\int |\lambda_b| db$ (in case legislators vote in the opposite direction of the pressure of the president in every vote the president exerts pressure).

If $\mu_l \lambda_b = \gamma[a_b + c_b^T x_l + \epsilon_{bl}]$ for all b and this given legislator l, then the first FOC above would reduce to $\mu_l \lambda_b \geq 0$, implying that the legislator votes "yea" whenever $\lambda_b > 0$, and votes "nay" whenever $\lambda_b < 0$. As a consequence, whenever the constraint was possible to satisfy, the legislator would satisfy it. That would imply the lagrange multiplier μ_l is zero. That being said, given ϵ_b is continuously distributed, independent of a_b, c_b, x_l, and there is a continuum of bills, there is probability zero that $\mu_l \lambda_b = \gamma[a_b + c_b^T x_l + \epsilon_{bl}]$ for all b.

As a consequence, with probability 1, the constraint would be binding if $T_l = \int |\lambda_b| db$. Moreover, under this value of T_l, whenever $\lambda_b > 0$, the legislator would have to vote "yea", and whenever $\lambda_b < 0$, the legislator would have to vote "nay". Given ϵ_{lb} is continuously distributed with unbounded support and independent of a_b, c_b, x_l, for this to happen, we'd need to have $\mu_l \rightarrow \infty$.

At the same time, if $T_l = -\int |\lambda_b| db$, then with probability $\Phi(a_b + c_b^T x_l + \mu_l \lambda_b / 2) > 0$, the legislator would vote "yea" with a given $\lambda_b > 0$, and this would be enough to guarantee that the legislator satisfies the constraint. As a consequence, with $T_l = -\int |\lambda_b| db$, the constraint is not

binding, and has $\mu_l = 0$.

Finally, note that conditional on b (and as a consequence, on $u_b^P + \epsilon_{bP}$), ϵ_{lb} is statistically independent across legislators and normally distributed. As a consequence, integrating on the values of ϵ_{lb}, and then on all other bill characteristics conditional on the determinants of λ_b, we get that:

$$\int v_{lb}\lambda_b db = E\left[\Phi\left(a_b + c_b^T x_l + \frac{\mu_l \lambda_b}{2}\mathbf{1}(l \in \mathcal{C})\right)\lambda_b\right]$$

(after all, conditional on the coalition and bill level parameters, λ_b is uncorrelated with ϵ_{lb}). This is continuous in μ_l, and as a consequence, μ_l is continuous as a function of T_l. By the intermediate value theorem, then, there exists a $T_l \in \left(-\int |\lambda_b| db, \int |\lambda_b| db\right)$ such that $\mu_l = 2$. For this value of T_l, conditional on staying in the coalition, the legislator finds it optimal to follow the voting rule suggested by the president's problem.

Finally, we need to verify that the legislator prefers to vote together with the coalition, satisfying the constraint above and receiving t_l, rather than voting according to his ideology, exiting the coalition and not receive t_l. This is guaranteed by the participation constraint. ■

Proof of Proposition 2 Throughout the proof, I'll assume the president likes the bill ($u_b^P + \epsilon_{bp} > 0$), and consider the probability of having the bill approved then. The proof for the probability of rejection when the president likes the bill is analogous.

Under the FOC for V_b, the comparative statics with respect to a parameter $\theta \in \{\psi, \kappa\}$ yields that:

$$\frac{\partial V_b}{\partial \theta} = \frac{\frac{\partial \lambda_b}{\partial \theta}}{P_{vv} - \frac{\partial \lambda_b}{\partial V_b}}$$

For the solution to be interior, the term in the denominator must be negative, so that $\frac{\partial \lambda_b}{\partial \theta}$ fully determines the comparative statics above.

From proposition 1, when the number of legislators is high, λ_b is implicitly given by:

$$LE\left[\Phi\left(\left[a_b + c_b^T x_l + \frac{\psi}{\kappa(1-\psi)}\psi_P[u_b^P + \epsilon_{bP}]\right] + \lambda_b \mathbf{1}(l \in \mathcal{C})\right)\right] = V_b$$

From this, it is immediate that when ψ increases, κ falls or ψ_P increases, and the president likes the bill, λ_b decreases, so that $\frac{\partial \lambda_b}{\partial \psi} < 0$ and V_b rises

with ψ.

For the comparative statics with respect to ψ_P, note that:

$$\frac{\partial V_b}{\partial \psi_P} = \frac{\frac{\partial^2 P}{\partial V_b \partial u}[u_b^P + \epsilon_{bP}] - \frac{\partial \lambda_b}{\partial \psi_P}}{-P_{vv} + \frac{\partial \lambda_b}{\partial V_b}}$$

Once again, when the solution is interior, the denominator is positive. By assumption, $\frac{\partial^2 P}{\partial V \partial u} > 0$ and as derived above, $\frac{\partial \lambda}{\partial \psi_P} < 0$. As a consequence, V_b rises with ψ_P when the president likes the bill. ■

Proof of Proposition 3 Given the coalition, the president has payoff given by:

$$\max_{\{v_{lb}\}_{l,b}} \int P(V_b, u_b^P + \epsilon_{bP})db + \sum_l \int 2(v_{lb} - v_{lb}^*)[a_b + c_b^T x_l + \epsilon_{lb}]db$$

where v_{lb}^* stands for the optimal vote of a legislator outside the coalition, v_{lb} is his vote inside the coalition.

Computing the expectations in the bill effects, we obtain that the above can be re-written as:

$$E_b[P(V_b^*, u_b^P + \epsilon_{bP})] + \sum_{l \in \mathcal{C}} \left\{ \begin{array}{c} 2\left[\Phi\left(a_b + c_b^T x_l + \lambda_b \mathbf{1}(l \in \mathcal{C})\right) - \Phi\left(a_b + c_b^T x_l\right)\right][a_b + c_b^T x_l] \\ + \phi\left(a_b + c_b^T x_l\right) - \phi\left(a_b + c_b^T x_l + \lambda_b \mathbf{1}(l \in \mathcal{C})\right) \end{array} \right\}$$

This in turn shows that the choice of coalition doesn't depend on the realizations of ϵ_{lb} due to the large number of bills.

To evaluate the effect of replacing a legislator in the coalition, we can use the envelope theorem to obtain that the effect of moving x_l in the direction ω. More precisely, the envelope theorem gives us the derivatives with respect to each dimension of c_b^T, and rules for directional derivatives are applied to give us:

$$\int 2(v_{lb} - v_{lb}^*) c_b^T \omega db$$

concluding the proof. ■

Proof of Proposition 4 Given the coalition, the president has payoff

given by:

$$\max_{\{v_{lb}\}_{l,b}} \int \mathbf{1}\left(\sum_l v_{lb} > V_b\right)[a_b^P + c_b^T x_P]db + \sum_l \int 2(v_{lb} - v_{lb}^*)[a_b + c_b^T x_l + \epsilon_{lb}]db$$

where v_{lb}^* stands for the optimal vote of a legislator outside the coalition, v_{lb} is his vote inside the coalition. By the envelope theorem, the effect of increasing $\tilde{x}_l^1$ over the president's payoff is:

$$\begin{aligned}\int 2\alpha_l(v_{lb} - v_{lb}^*)c_b^1 db =& 2\alpha_l q_+ \left\{\int (v_{lb_1} - v_{lb_1}^*)c_b^1 db_1 | c_b^1 > 0\right\} \\ &+ 2\alpha_l q_- \left\{\int (v_{lb_1} - v_{lb_1}^*)c_b^1 db_1 | c_b^1 < 0\right\}\end{aligned}$$

where b_1 stands for bills of type 1, q_+ is the probability of a bill type 1 with $c_b > 0$, q_- is the probability of a bill type 2 with $c_b < 0$, and the second expression follows from the assumption of the distribution of c_b^1. Now, $v_{lb} - v_{lb}^* = 2$ if $a_b + c_b^T x_l + \lambda_b > -\epsilon_{lb} > a_b + c_b^T x_l$, $v_{lb} - v_{lb}^* = -2$ if $a_b + c_b^T x_l + \lambda_b < -\epsilon_{lb} < a_b + c_b^T x_l$, and $v_{lb} - v_{lb}^* = 0$ otherwise. Because $x_P^1 > 0$, when $c_b^1 > 0$ and $a_b = 0$, the president gets positive utility from bills type 1, so he'll never exert negative pressure $\lambda_b < 0$. As a consequence, $v_{lb} - v_{lb}^* \geq 0$, and the first term is positive. Similarly, when $c_b^1 < 0$, the president gets negative utility from bills type 1, and never exerts positive pressure. As a consequence, when $c_b^1 < 0$, $v_{bl} - v_{lb}^* \leq 0$, and $(v_{bl} - v_{lb}^*)c_b^1 > 0$. As a consequence, the term above is always positive, and there is a gain of bringing into the coalition a legislator with higher x_l.

Similarly, we can use the envelope theorem to see the effect of bringing in less ideologically attached legislators. The effect of increasing ideological attachment is:

$$\begin{aligned}\int 2(v_{lb} - v_{lb}^*)c_b^T \tilde{x}_l db =& 2q\left\{\int (v_{lb_1} - v_{lb_1}^*)c_b^1 \tilde{x}_l^1 db_1\right\} \\ &+ 2(1-q)\int (v_{lb_2} - v_{lb_2}^*)c_b^2 \tilde{x}_l^2 db_2\end{aligned}$$

By assumption, $\tilde{x}_l^2 < 0$, and $c_b^2 = 1$ when the bill is type 2. As a consequence, in these cases, the president only puts pressure for approval, and $v_{lb_2} - v_{lb_2}^* \geq 0$. Hence, the last line is negative. As we discussed before, $(v_{lb_1} - v_{lb_2}^*)c_b^1 > 0$ when the president has $x_P^1 > 0$. As a consequence, when

$x_l^1 > 0$, the first and second line are positive, and get larger the larger is x_l^1 (the first and second line go to ∞ when $x_l^1 \to \infty$ as long as there is some government pressure in equilibrium). Similarly, when $x_l^1 < 0$, the first and second line are negative, and the overall derivative above is negative. As a consequence, there exists a threshold $\bar{x}(q) > 0$ such that for $\tilde{x}_l^1 < \bar{x}(q)$, the president's payoff falls in the legislator's care for policy α_l, while for $\tilde{x}_l^1 > \bar{x}(q)$, the president's payoff rises in the legislator's care for policy α_l. Finally, it is immediate to see that $\bar{x}(q)$ is decreasing in q. ■

Tables

Table 5.1: Sample of deputies/bills

Congress	No. of deputies	No. of bills
53 (2008-2010)	497	135
54 (2011-2014)	502	174
55 (2015-2016)	503	260
53-55th (2008-2016)	963	569

Table 5.2: Government coalition and absenteism

	Vote with govt	Vote with govt	Absence
Coalition member	0.108	0.120	-0.030
	(0.019)***	(0.020)***	(0.010)***
R^2	0.35	0.36	0.15
N	215,116	192,005	352,515
Avg. y out of coalition	0.555	0.558	0.296
Threshold	50%	85%	
Congressman FE	YES	YES	YES
Bill FE	YES	YES	YES

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

Table 5.3: Vote with party: parties leaving the govt. coalition in 2016

	PT	PT	PDT	PDT	PCdoB	PCdoB
Coalition, pre May/16	0.049	0.060	-0.051	-0.007	0.009	0.035
	(0.015)***	(0.015)***	(0.016)***	(0.018)	(0.015)	(0.015)**
Coalition, post May/16	-0.046	-0.033	-0.150	-0.093	-0.113	-0.093
	(0.026)*	(0.026)	(0.033)***	(0.037)**	(0.030)***	(0.028)***
R^2	0.40	0.41	0.30	0.38	0.38	0.40
N	212,901	196,089	235,028	151,880	234,634	225,595
Avg. y out of coalition	0.502	0.503	0.590	0.614	0.527	0.537
Equality of effects	0.000	0.000	0.000	0.045	0.000	0.000
Threshold	50%	85%	50%	85%	50%	85%
Congressman FE	YES	YES	YES	YES	YES	YES
Bill FE	YES	YES	YES	YES	YES	YES

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

Table 5.4: Vote with party: parties joining the govt. coalition in 2016

	PSDB	PSDB	DEM	DEM	PPS	PPS
Coalition, pre May/16	-0.025	-0.035	-0.044	0.005	-0.036	-0.033
	(0.015)*	(0.016)**	(0.016)***	(0.018)	(0.017)**	(0.019)*
Coalition, post May/16	0.137	0.175	0.081	0.123	0.158	0.182
	(0.029)***	(0.029)***	(0.036)**	(0.030)***	(0.035)***	(0.033)***
R^2	0.42	0.46	0.41	0.48	0.36	0.38
N	223,501	178,826	228,396	148,445	232,860	182,732
Avg. y out of coalition	0.550	0.552	0.582	0.583	0.564	0.588
Equality of effects	0.000	0.000	0.000	0.000	0.000	0.000
Threshold	50%	85%	50%	85%	50%	85%
Congressman FE	YES	YES	YES	YES	YES	YES
Bill FE	YES	YES	YES	YES	YES	YES

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

Table 5.5: Vote with party: parties staying in the govt. coalition in 2016

	PMDB	PMDB	PP	PP	PR	PR
Coalition, pre May/16	0.138	0.108	0.104	0.084	0.111	0.124
	(0.021)***	(0.024)***	(0.020)***	(0.023)***	(0.020)***	(0.021)***
Coalition, post May/16	0.304	0.283	0.212	0.233	0.255	0.305
	(0.046)***	(0.047)***	(0.039)***	(0.037)***	(0.039)***	(0.040)***
R^2	0.25	0.30	0.24	0.32	0.24	0.28
N	217,536	142,796	225,571	128,197	229,667	156,227
Avg. y out of coalition	0.611	0.665	0.644	0.703	0.634	0.679
Equality of effects	0.000	0.000	0.000	0.000	0.000	0.000
Threshold	50%	85%	50%	85%	50%	85%
Congressman FE	YES	YES	YES	YES	YES	YES
Bill FE	YES	YES	YES	YES	YES	YES

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

Table 5.6: Summary statistics, government pressure

	mean	50th perc.	75th perc	90th perc
No. of congressmen voting	339.084	340	388	423
No. of congressmen voting yea	191.578	196	264	316
No. of congressmen in coalition	306.098	305	322	346
No. of coalition members voting for bill	210.308	214	243	262
No. votes reversed due to govt pressure	22.667	13.439	32.271	59.768
% coalition votes reversed due to govt pressure	.112	.066	.170	.296

Table 5.7: Evolution over time of votes reversed due to government pressure

	Avg. # coalition	# Votes reversed			Share of coalition votes reversed		
Year	members voting	50th perc	75th perc	90th perc	50th perc	75th perc	90th perc
2008	230.8	64.7	84.9	98.9	0.291	0.363	0.402
2009	197.9	39.4	62.1	76.5	0.208	0.311	0.369
2010	204.2	36.8	69.2	75.5	0.179	0.378	0.424
2011	203.0	19.0	36.8	47.6	0.101	0.203	0.235
2012	184.3	16.3	34.8	44.6	0.093	0.203	0.236
2013	159.5	13.9	27.1	44.7	0.101	0.168	0.255
2014	152.0	12.5	27.6	36.1	0.088	0.184	0.249
2015	238.1	9.5	16.1	24.8	0.038	0.068	0.098
2016	228.9	5.0	11.0	19.8	0.024	0.046	0.075
Total	210.3	13.4	32.3	59.8	0.066	0.170	0.296

Table 5.8: Evolution over time of votes reversed due to government pressure

	# Votes reversed				Share of coalition votes reversed			
Bill type	Mean	25th perc	50th perc	75th perc	Mean	25th perc	50th perc	75th perc
Constit. Ammendment	15.6	5.6	9.1	15.5	0.066	0.024	0.038	0.067
Ordinary laws	21.9	5.4	12.8	33.9	0.109	0.028	0.065	0.149
Provisional/transform. laws	26.4	6.2	18.0	39.5	0.135	0.031	0.098	0.225
Supplemental laws	22.6	4.2	13.5	24.5	0.113	0.024	0.063	0.142
Total	22.7	5.6	13.4	32.3	0.112	0.026	0.066	0.170

Table 5.9: First order conditions for pressure

			Terciles of $\int \Phi(a_b + c_b^T x_l) dl$		
	Basic	Extension	1st Terc.	2nd Terc.	3rd Terc.
$\frac{\partial^2 P}{\partial V^2}$	-0.004				
	(0.001)				
$\frac{\partial^2 P}{\partial (V^{GOV})^2}$		3.774	3.226	3.625	4.103
		(0.105)	(1.030)	(0.212)	(0.195)
$\frac{\partial^2 P}{\partial V^{GOV} \partial V^{OPP}}$		-0.001	-0.001	-0.003	-0.001
		(0.0004)	(0.001)	(0.001)	(0.0006)
$\frac{\partial^2 P}{\partial V \partial u}$	0.029	0.051	-0.048	0.070	-0.028
	(0.033)	(0.019)	(0.034)	(0.031)	(0.025)
$\frac{\partial P}{\partial V}$	0.305	0.098	-0.076	0.198	0.148
	(0.083)	(0.030)	(0.067)	(0.071)	(0.061)
R-2	0.060	0.197	0.127	0.388	0.113
N	569	569	188	188	193

Table 5.10: First order conditions for pressure: no government/government coalition authors

Sample:	No exec. author		No govt. coal. authors	
	Basic	Extension	Basic	Extension
$\frac{\partial^2 P}{\partial V^2}$	-0.002		-0.002	
	(0.001)		(0.001)	
$\frac{\partial^2 P}{\partial (V^{GOV})^2}$		3.616		4.408
		(0.290)		(0.212)
$\frac{\partial^2 P}{\partial V^{GOV} \partial V^{OPP}}$		-0.001		-0.001
		(0.001)		(0.0001)
$\frac{\partial^2 P}{\partial V \partial u}$	-0.020	0.011	0.002	0.016
	(0.025)	(0.017)	(0.032)	(0.008)
$\frac{\partial P}{\partial V}$	0.173	0.083	0.223	0.074
	(0.092)	(0.046)	(0.153)	(0.014)
R-2	0.035	0.062	0.052	0.456
N	222	222	112	112

Table 5.11: Evolution over time of votes reversed due to government pressure

	Expect. # votes, pressure	Prob. approv., pressure	Expect # votes, no pressure	Prob. approv., no pressure
Pre May 2016, $\lambda_b < 0$	242.579	0.667	279.642	0.795
Pre May 2016, $\lambda_b > 0$	310.412	0.858	270.626	0.750
Post May 2016, $\lambda_b < 0$	314.500	0.816	324.705	0.816
Post May 2016, $\lambda_b > 0$	197.216	0.483	184.909	0.379
Total	277.020	0.757	273.577	0.754

Figures

Figure 5.1: Ideology measures: structural model vs. legislator surveys

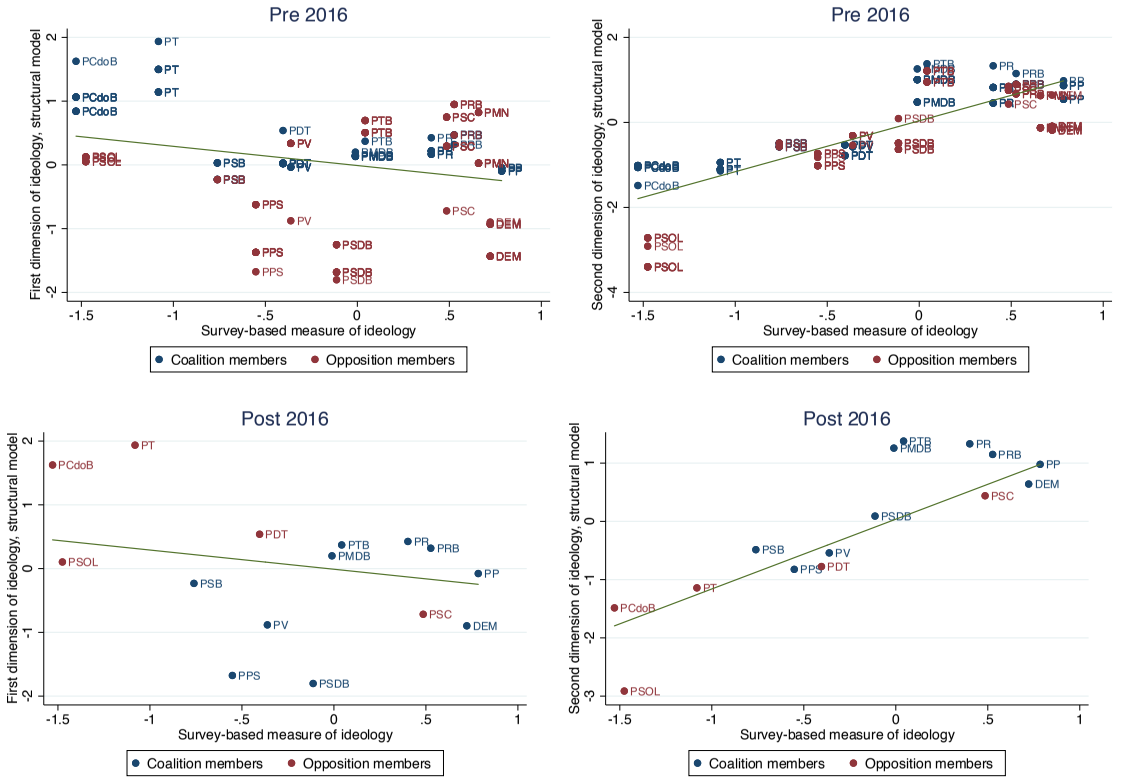


Figure 5.2: Density of measures of government pressure

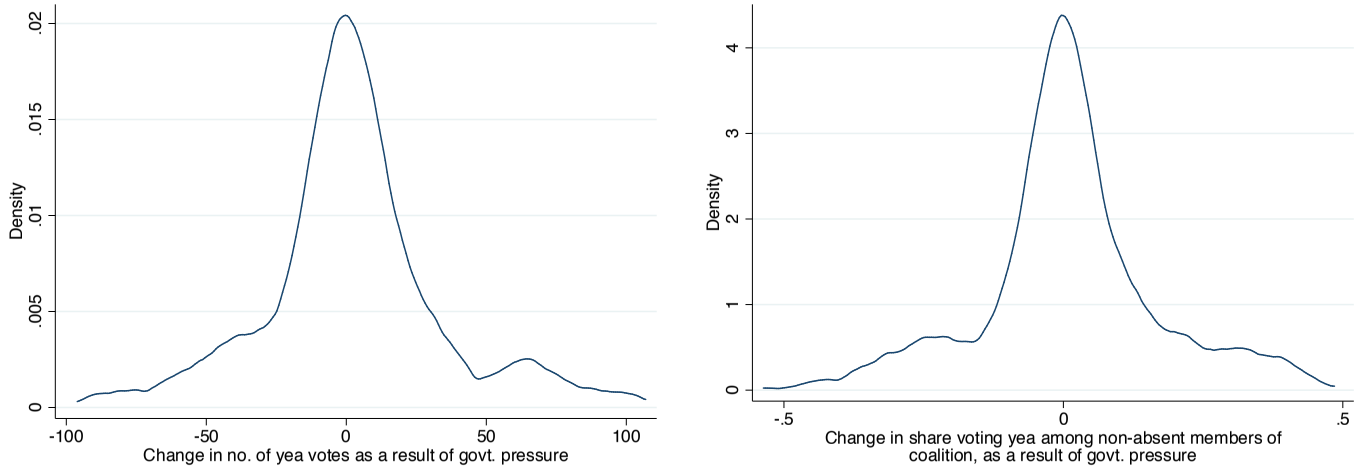


Figure 5.3: Government pressure in close vs. lopsided votes

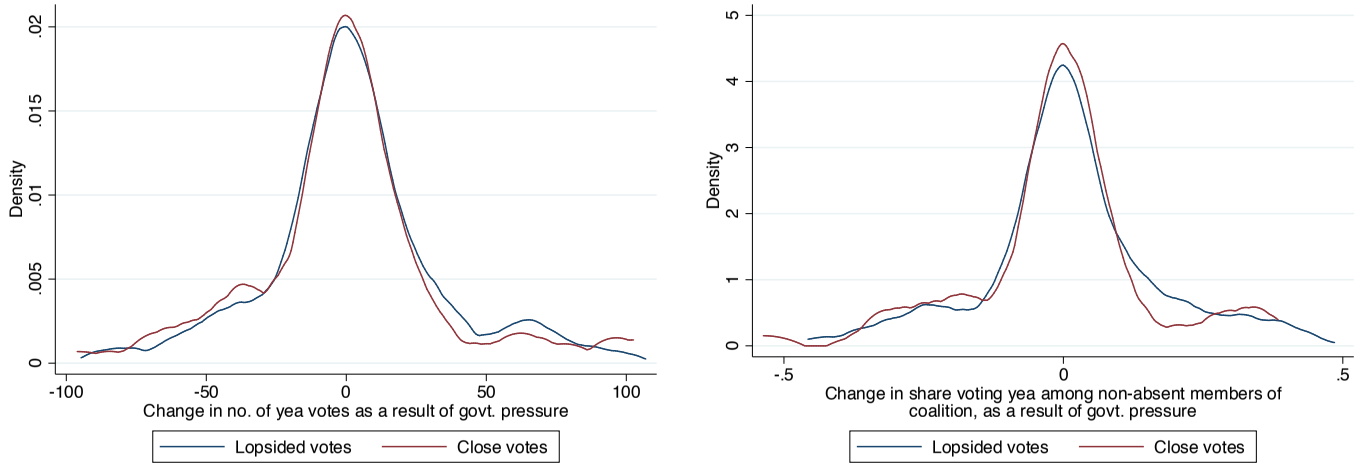


Figure 5.4: Government coalition and president's party's preferences

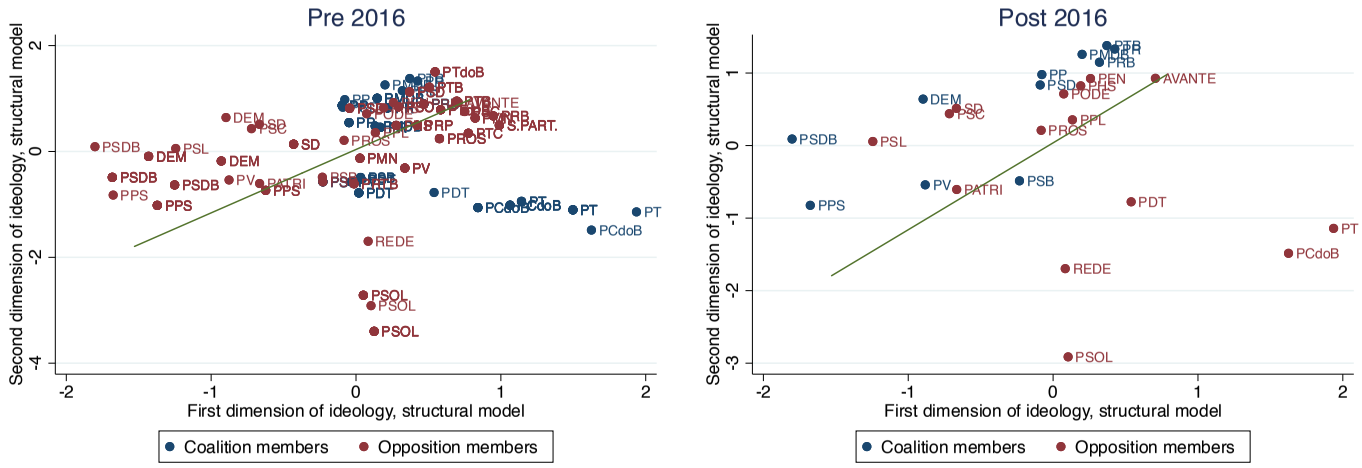
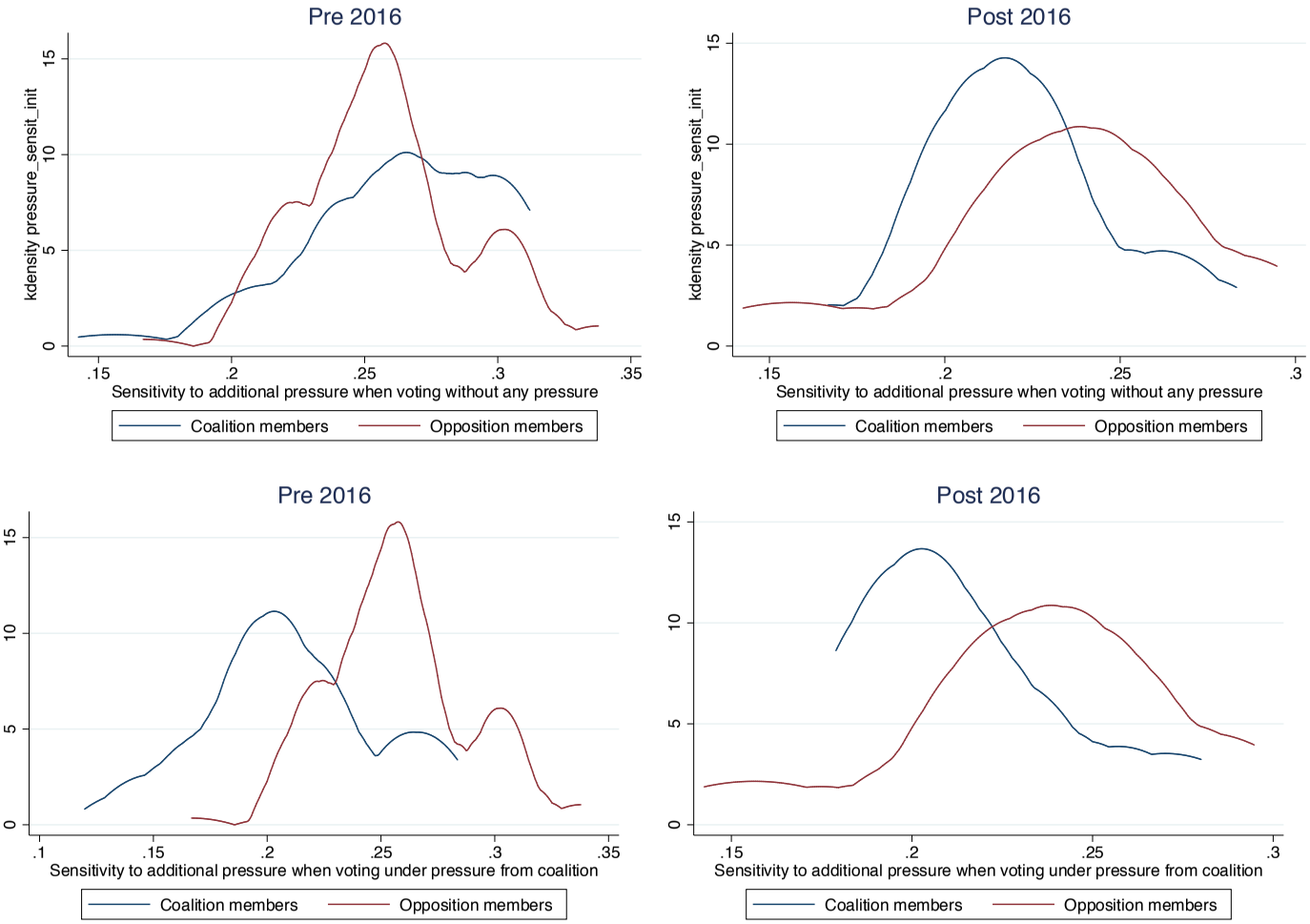


Figure 5.5: Government coalition and government pressure



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