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ESSAYS IN APPLIED MICROECONOMICS

Tese apresentada à Escola de Economia de São Paulo da Fundação Getulio Vargas como requisito para a obtenção do título de Doutor em Economia. Campo de conhecimento: Microeconomia Aplicada

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Resumo

Esta tese é composta por três ensaios em microeconomia aplicada.

O primeiro capítulo investiga o impacto de uma reforma eleitoral que limitou o tamanho de coalizões formadas para eleições legislativas no Brasil. Com uma estratégia de diferença-em-diferenças, o artigo mostra que houve aumento da competitividade eleitoral e uma consequente reorganização dos agentes que resultou na eleição de candidatos mais ricos.

O segundo capítulo analisa o impacto de apoio legislativo sobre a qualidade da administração pública no contexto de governos municipais do Brasil. O artigo propõe uma definição de "disputa apertada" para eleições proporcionais e mostra que maior apoio legislativo promove melhor governança quando há incentivos eleitorais ao prefeito. Do contrário, mais apoia significa pior administração.

O terceiro capítulo em economia da educação estima efeito de pares não linear sobre desempenho educacional. O artigo investiga o potencial de interação dos alunos como canal de explicação do efeito. Estima-se a partir de formas reduzidas o efeito de pares explorando a variação exógena no grupos de alunos. No entanto, leva-se em consideração padrões de interação distinto entre alunos. De fato, os efeitos estimados são mais fortes quando se consideram pares que participam dos mesmo grupos com mais frequência.

Palavras-chaves: coalizões eleitorais, coalizões governamentais, administração pública, efeito de pares

Abstract

This thesis comprises three essays in applied microeconomics.

The first chapter investigates the impact of an electoral reform that limited the size of electoral coalitions built to run in legislative elections in Brazil. With a diff-in-diff strategy, the paper shows that the reform increased electoral competitiveness and the reorganization of political players induced the election of richer candidates.

The second chapter analyzes the impact of stronger legislative support on the quality of governments looking to Brazilian municipalities. The paper proposes a definition of what is a slim margin of victory for proportional elections and shows that the greater legislative support is beneficial to governance when mayors have electoral incentives. Otherwise, more support means bad governance.

The third chapter deals with the economics of education. It is an article estimating non linear peer effects by exploring the intensity of peer interaction as a possible explanation for the effects. The effects come from reduced-form estimates exploring random variation in students' groups composition. Indeed, the strongest effects occur when peers meet more frequently in different groups.

Key-words: electoral coalitions, government coalitions, public administration, peer effects

List of Figures

List of Tables

Contents

1 The Effects of Constraining Electoral Coalitions on Political Outcomes

1.1 Introduction

Political competition can be a source of economic welfare if it promotes the selection of better politicians and the enhancement of electoral accountability. Since parties are key players in politics, the consequences of competition for the economic development can depend on what they do when facing competitive environments. When a large number of parties compete for votes it is possible that they end up engaging in special-interest politics leading to underprovision of public goods. Excessive competition in this case can be detrimental to development (BESLEY; PERSSON; STURM, 2010; BESLEY; PRESTON, 2002; LIZZERI; PERSICO, 2005). As elections are at the core of political competition, electoral rules are important constraints on what strategies are available for parties. Changing these rules can change parties' decisions on whether and how they take part in elections. This can have consequences for the incentives that potential candidates might face and for the outcomes of an election.

In this paper, we evaluate the impact of a reform that constrained the size of electoral coalitions in Brazil. This reform limited the maximum number of candidates that an electoral coalition could register to dispute seats for local legislative councils. This change targeted municipalities above the threshold of 100,000 registered voters and this is the exogenous shock we explore with a difference-in-differences strategy. The main results are that the reform increased political competition and this led to the election of substantially richer candidates and to a modest increase of spending in education.

In Brazil, during the period we consider, it was the aggregation of votes cast for parties within an electoral coalition that mattered for the distribution of legislative seats. Thus, parties' behavior changed in response to a change in a rule about coalitions. The 25% decrease in the maximum number of candidates per coalition imposed by the reform translated into a 23% decrease on the average number of candidates per coalition. However, we do not reject the absence of effect on the total number of candidates entering the dispute. Also, there was no impact in the number of parties entering the elections. This resulted from parties circumventing the limit on coalition size. In treated municipalities they formed 2 more coalitions, on average, what represents a 22.7% increase. Besides, there was a 13.6% increase in the effective number of coalitions, a measure of political competition.

We find no significant effect on average education and age of candidates. There

was small increase of 3.8% on the proportion of women running for a seat that did not translate into more women elected for the councils. A relevant results is that the more competitive environment attracted richer candidates. Citizens entering the election were, on average, 15.6% richer (measured by self-reported assets) compared to what would be expected without the reform. Further, it translated into richer candidates (+19.3%) being elected to the council. Importantly, although one of its goals was reducing the costs of electoral campaigns, the reform did not have impact on the total money raised by candidates. The tighter competition might have led parties and coalitions to a new equilibrium with the same level of revenue/spending. Finally, we find a 5.3% increase in the average budget share allocated to education. We do not find increase in party fragmentation within councils. Thus, we interpret that this could result from more electoral competition translating into increased accountability. None of these results hold in robustness exercises in which we do the analysis falsely considering the previous election as the time of the reform or considering the 2016 election as the moment of implementation but using a false threshold to define treatment.

This paper contributes with empirical evidence to a literature discussing why electoral coalitions form and what can be the consequences for policy.[1] We also speak with paper discussing the impact of political institutions, electoral rules in particular, on development (CANTONI; PONS, 2019). On this side, the paper more relevant is Avis et al. (2017), which explore the of limits on electoral campaign spending in the same context of ours to show that this cap also increased political competition.

In the sequence we explain institutional aspects to understand elections in Brazil and the reform implemented. Then, we discuss the empirical strategy of the paper. Finally, we present and discuss the results along with different robustness exercises.

1.2 Institutional Background

1.2.1 Local Elections in Brazil

Local elections in Brazil take place every four years. In these elections voters choose simultaneously a mayor and a local council in each municipality. Executive elections happen under simple plurality in municipalities with population below 200,000 registered voters. Above this threshold, either a candidate becomes mayor if she has more than 50% of votes or a runoff election takes place. Besides choosing a candidate for mayor, voters also choose a candidate to the local council. The number of councilors elected in a municipality is determined by its population one year before election. Candidates for the local council are elected under an open-list proportional representation system. In this system, voters choose either a party or a candidate. Votes cast for a candidate increase

[1] Bandyopadhyay, Chatterjee e Sjostrom (2009), Golder (2005), Debus (2009)

his party's total votes and also define the candidate's position in the party's ranking of candidates. This is important because the seats that a party eventually gets are filled by the highest ranked candidates according to their personal votes.

Up to the 2016 elections, political parties could form electoral coalitions to dispute legislative seats. It consists in an aggregation of parties formally announced before election to the Brazilian Electoral Court (TSE). This possibility adds one more layer in the system described before: votes cast for either a party or a candidate add up to the total coalition votes and council seats are distributed according to coalitions' total votes. Now, council seats assigned to a coalition go to the highest ranked candidates from each coalition irrespective of their party. For example, suppose parties A and B decide to run together in an electoral coalition. Then votes cast for both parties count to define how many seats this coalition will get. Suppose this coalition gets three seats. Then, from all candidates running for either party A or B, the three most voted ones will be elected, irrespective of their party.

It is worth mentioning why local legislative elections matter. Brazilian Constitution sets local governs as the responsible for the provision of many public services such as health and education. To provide these services mayors must submit to the local council a budget proposal describing the policies he wants to implement. In another paper, we discuss how legislative support in this step matters for good governance.[2] Also, Ferraz e Finan (2011b) show suggestive results that provision of public goods increase with legislators' effort since this makes the mayor more accountable.

1.2.2 Electoral Reform

An electoral reform was undertaken in 2015 with the explicit goal of reducing the costs of electoral campaigns and simplifying political parties' management.[3] Among several changes that would govern local elections held in October/2016, the reform set new limits on the maximum number of candidates each party or coalition could register in election for local legislative councils.[4] The law containing modifications in electoral rule was approved in September/2015 and resulted from different proposals, but the specific change we explore in the paper first appeared in drafts released in July/2015.[5]

The exogenous shock implied by the reform results from a statement saying that in municipalities above the threshold of 100,000 registered voters, the number of candidates

[2] The chapter "Tit-for-Tat between Legislative and Executive" of this thesis.

[3] The reform was implemented by the law 13165/2015 available at <http://www.planalto.gov.br/ccivil_03/_ato2015-2018/2015/lei/l13165.htm>. Another very important change in the context of these reform was the introduction of campaing spending limits. This is analyzed by (AVIS et al., 2017).

[4] Article no. 10 of the law 9504/1997. They are available at: <http://www.planalto.gov.br/ccivil_03/LEIS/L9504.htm>

[5] Proposals 2235/2015, 2259/2015, 5735/2013 can be found in a search at <https://www.camara.leg.br/buscaProposicoesWeb/pesquisaSimplificada>

presented by parties or coalitions was limited to 150% of the seats to be distributed in the city's council. In municipalities below this threshold, the limit also holds for parties, but for coalitions the limit is 200% of the seats. Figure (1.1) illustrate the change.

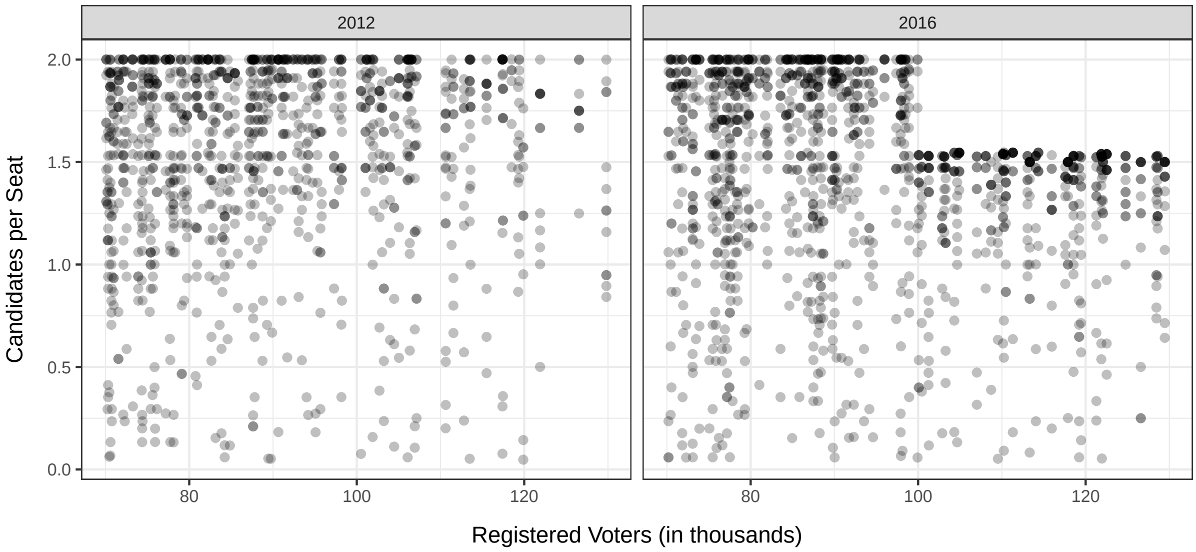


Figure 1.1 – Number of candidates registered by either coalitions or single parties in each election. Each dot is an electoral coalition or single party. In the 2016 election, coalitions in municipalities with more than 100,000 registered voters could register candidates up to the limit of 150% the number of seats in the council. However, this difference did not exist in the 2012 election.

For example, in a city with 90,000 voters that elects 17 councilors, a single party can register up to 26 candidates and a coalition can register up to 34 candidates. However, in a city with 110,000 voters that also elects 17 councilors, both parties and coalitions are limited to 26 candidates. This limit does not depend on the number of parties forming the coalition. Thus, for municipalities above the threshold of 100,000 voters there were a sharp decrease in the number of candidates per seat that an electoral coalitions could register. A final remark is that no specific reason for the choice of 100,000 voters as the threshold appears in the proposal's drafts. Besides, we found no documented evidence that local politicians somehow influenced this choice. Although we find this implausible, we can not rule out this possibility since local politics are important for the election of the congressmen that ultimately designed the law (FIRPO; PONCZEK; SANFELICE, 2015; VENTURA, 2021).

1.2.3 Voters Registration

The number of registered voters is a key variable for our analysis since it determines the rule change. So we present here some important issues with implications for our empirical strategy. Voting is mandatory in Brazil for people aged between 18 and 70 years old. It is optional for (i) illiterate people (ii) people older than 16 and younger than 18 years old, and (iii) people older than 70 years old. To be able to vote, a person

must go to an electoral office (*cartório eleitoral*) near his home to present an idetification document and some proof of his place of residence. These electoral offices are managed by a state electoral court (*Tribunal Regional Eleitoral*) and they are responsible for voters registration as well as the organization of the elections at the local level. An electoral office may be responsible for more than one municipality, but this only happens to aggregate small cities. In the sample used in the paper, there is only one case of an electoral office responsible for two different cities. In our sample, 69% of the cities have only one office, in 29% of them there are two offices and only in 2% there are more than two offices.

There are some issues regarding the number of registered voters: difficult in removing dead voters from the database as well as cases of duplicates and frauds (NICOLAU, 2004). Motivated by the problem of fraud, the Brazilian Electoral Court started in 2008 a program to have a biometric identification of voters. Each year, TSE informed state courts what municipalities should proceed with voters' registration update. In practice, voters in these municipalities had to show up in an electoral office to make their registration again. One consequence of this program is that voters not showing up during the period of re-registration had their register cancelled and so it impacted directly the number of registered voters in that municipality. During the period considered in our sample, not all municipalities updated their voters database and even some of those who did might have done only a partial update, but we can observe what happend for each case.

Two issues about this process of update in voters registration are worth mentioning. It could have had different impacts across municipalities depending on voters demographics. For example, there are cases in which regular electoral register is a requirement for signing formal job contracts and to obtain personal documents such as passports. Thus, not showing up to make the update might have been correlated with age and/or schooling (FERRAZ; VARJAO, 2015). Besides, personel in charge of the registration task are usually tenured employees from electoral courts working at electoral offices. However, the electoral court can demand public employees from the municipal administration to help in this task.[6] Also, these public servers can apply voluntarily in order to help with this kind of service. Unfortunately, there is no data on how frequently this actually happened. We discuss implications of these points when presenting the empirical strategy of the paper.

1.3 Disputing Legislative Seats

It is important to understand why the number of candidates registered in a coalition should matter in our context. The process of distributing legislative seats consists in mapping the (continuous) share of votes each coalition gets into a (discrete) number of seats. In Brazil this is done through a combination of methods.[7] For our purposes, the

[6] Law 6.999/82 available at <http://www.planalto.gov.br/ccivil_03/leis/L6999.htm>

[7] D'Hondt method and Hare quota.

crucial aspects of the rule resulting from this combination are the following. Suppose a municipality with 10 seats in a council counts a total of 10,000 votes. The assignment rule adopted in Brazil states that coalitions must have at least $10,000/10 = 1000$ votes to get a seat.[8] Thus, if parties A, B, and C enter the dispute by themselves and get 900, 450 and 450 votes respectively, none of them would get a seat. However, if they formed a coalition and had these same votes, the coalition would sum 1800 votes, ensuring one seat in the council.

But the design of the rule implies that there is a positive probability of getting one more seat since coalition votes are close to the “next seat” (2000 votes). In such a scenario, parties B and C would have incentives to make effort to get votes since they could get a seat even without reaching 1000 votes. The number of candidates is important in this effort. First, electoral campaigns are costly and more candidates in a party can bring more money. Also, a greater number of candidates can mobilize more voters. And finally, at least in Brazil, votes are somewhat personal (SAMUELS, 1999) and more candidates means reaching more people through personal ties.

To put it more concretely, suppose that the coalition formed by parties A, B and C can achieve a certain number of votes V with 10, 5 and 5 candidates, respectively, reaching the ceiling of 20 candidates allowed by law. A cap imposing a limit of 15 total candidates for the coalition could affect parties’ probabilities of election in two ways. With fewer number of candidates, the coalition could see a decrease in its share of votes and thus in the probability of getting seats. In our simple example, it could be the case that the coalition still passes the threshold, but with fewer votes. Suppose for a moment that the distribution of candidates in the coalition remained fixed after the new cap, so the share of votes of each party within the coalition was approximately constant. In this case, while party A would still have a high probability of ending up with one seat, parties B and C could face a strong decrease in the probability of electing some candidate.

This would create incentives for parties B and C to seek for other coalitions’ arrangements. But without parties B and C in A’s coalition, party A would see its probability of getting a seat significantly reduced, so A would also look for new arrangements.

1.4 Data

Here we present some remarks about the data we use. Table 1.1 shows that treatment and control municipalities are not so different in terms of several variables measured in the 2012 election. The important exception is the size of municipalities. It is the variable *registered voters* that determine treatment. Descriptive statistics are shown to the

[8]This was no longer valid for the 2020 election.

sample we consider in our main analysis, which comprises cities with 20,000 voters below or above the 100,000 voters threshold.

Elections

We use data of three municipal elections held in the period 2008-2016 provided by the Brazilian Electoral Court. From this source we retrieve information on registered voters and candidates socioeconomic characteristics (gender, age and education). Also, this is the data from which we count the number of registered voters that determined the cap on coalitions size for each municipality. It is possible to observe what parties joined into electoral coalitions for both legislative and executive elections and this allows us to compute variables related to the elections final results. Candidates data contains self-reported assets before the election as well as the amount of money used in the campaign. These monetary variables are expressed in Brazilian reais of December/2020. Finally, there is a variable measuring the proportion of voters in the municipality with registered biometric identification.

All outcomes and most of the controls are municipality averages. One point worth mentioning is that for the self-reported assets we excluded some outliers before computing the averages. These observations were candidates reporting assets above one billion reais, while the median was 19,500. Without excluding these observation, the estimated effects on this outcome were substantially higher.

Public Finance

Municipalities report to the National Treasury Department of Brazil balance sheets containing detailed info about revenue and spending. We use this database (*FINBRA*) to get info on money transfers from federal and state governments, which represent most of the revenue available to municipalities, and variables for spending in education and health. For these two areas specifically, Brazilian constitution mandates a minimum spending of 25% and 15%, respectively, calculated over a set of revenue sources. Every year, Brazilian Health and Education Ministries check the compliance with these threshold and release the audited proportion spending in each area. We use percentage points above the minimum as a measure of discretionary spending in these areas. All variables are computed for each municipality as the average over the four year of councilors/mayor term.

1.5 Empirical Strategy

To estimate the effects of limiting electoral coalition size (our treatment) we explore the exogenous change in the number of candidates per coalition valid for the 2016 election

Table 1.1 – Descriptive Statistics (2012 Election)

	Control		Treatment		Mean diff.	P-value
	Mean	SD	Mean	SD		
Municipalities characteristics						
Registered voters	86,707.2	8,069.0	103,162.3	9,923.1	16,455.1	0.000
Population	122,346.2	15,308.4	153,447.1	20,989.9	31,100.8	0.000
Money transfers (million R$)	327.6	160.5	371.4	110.4	43.7	0.192
Council seats	15.07	2.70	15.66	3.44	0.585	0.389
Mayor in 1^{st} term	0.54	0.50	0.66	0.48	0.111	0.328
Number of parties	22.68	2.47	22.83	2.73	0.143	0.806
Total candidates	209.95	57.95	220.17	45.62	10.225	0.410
Municipalities	57		29			
Voters profile (proportion)						
Between 16 and 34 years old	0.41	0.04	0.43	0.04	0.015	0.107
Between 35 and 59 years old	0.44	0.03	0.43	0.02	-0.002	0.769
College degree	0.04	0.02	0.03	0.02	-0.005	0.343
High school degree	0.19	0.05	0.18	0.05	-0.002	0.852
Women	0.52	0.01	0.52	0.01	0.002	0.455
Candidates profile						
College degree	0.26	0.07	0.25	0.08	-0.017	0.297
High school degree	0.06	0.03	0.07	0.03	0.005	0.401
Age	46.35	2.42	45.29	1.52	-1.058	0.035
Women	0.30	0.02	0.30	0.02	-0.003	0.576
Asset (in R$)	114,832	69,428	98,879	44,066	- 15,953	0.264
Funding sources (in R$)						
Self-funding	6,058	2,493	6,886	2,955	828.19	0.175
Citizen donation	8,864	4,767	9,326	5,955	461.36	0.698
Party resource	5,365	17,519	3,833	3,691	- 1,531.38	0.644

Notes: Descriptive statistics for the sample of municipalities with 20,000 voters below or above the 100,000 voters threshold. Monetary values expressed in prices of December/2020.

with a difference-in-differences strategy. As stated before, there is no documented evidence that municipalities influenced the definition of voters threshold which defined maximum coalition size. But we presented some issues that might arise from the voters registration process. We address each of these points below.

A simple regression of an outcome for the 2016 election on a treatment indicator would confound any factor covarying with municipality size and sample size is not enough to employ a regression discontinuity solution. Since we can observe a municipality for more than just one election, results throughout the paper are obtained by a fixed-effects regression described by the following equation of an outcome of interest y_{it} for municipality i in election t:

$$y_{it} = \beta T_{it} + \gamma \text{voters}_{it} + \delta \text{updated}_{it} + \boldsymbol{\theta}' \mathbf{x}_{it-4} + \boldsymbol{\lambda}' \mathbf{z}_{it} + \eta_t + c_i + u_{it} \tag{1.1}$$

In equation (1.1), $\mathrm{T}_{it} = 1$ for municipalities above the 100,000 voters threshold in $t = 2016$ and voters_{it} is the number of registered voters in i for election t. As discussed before, the update of voters' biometric identification in some municipalities between 2008 and 2016 is correlated with voters registration and makes it correlated with voters_{it}. So

we include the variable updated_{it}, which consists in all voters registered in i for the $t-4$ election that went to an electoral office to provide biometric info before the election in t. But as incentives for the voters to show up and update their registration might depend on demographics, we include the vector $\mathbf{x}_{it-4}$ which contains a profile of age, education and gender of voters registered for the previous election. We further control for a vector $\mathbf{z}_{it}$ containing federal money transfers to the municipality and the number of legislatives seats in dispute. Finally, the equation contains a time-specific shock η_t, municipality unobserved heterogeneity c_i and the error term u_{it}.

However, the update of biometric identification could still be affected by unobservables not captured by c_i. As it started in different years across the cities there could by time-varying unobservables correlated with the number of registerd voters. So instead of maintaining a strict exogeneity assumption, we state the identification hypothesis as

$$E[u_{it}|T_{it}, \mathbf{X}_{it}, c_i, \eta_t] = E[u_{it}|\mathbf{X}_{it}, c_i, \eta_t] \tag{1.2}$$

where all control variables are collapsed in $\mathbf{X}_{it}$. Thus, after controlling for factors known to determine treatment, we expect that remaining unobservables do not depend on whether the municipality gets treated or not.

To get the estimates we will restrict our sample to 86 municipalities (as shown in table 1.1) and 29 of them are the treated units. To check whether the calculation of robust standard errors in our setup is enough to perform valid tests, we proceeded with the inference assessment proposed by (FERMAN, 2019). The assessment indicates that the size of a test about our treatment coefficient is 6.5% when we expected the usual 5%. Thus, over-rejection does not seem to be a major problem.

1.6 Results

1.6.1 Effects on Political Competition

Results in this section shows that the reform implied a reorganizaton of parties entering the electoral race. There were sizeable effects, in opposing directions, on the average number of candidates per coalition and the number of coalitions. Total candidates remained stable and thus there was an increase of political competition.

Table 1.2 shows an estimate of -13.5 for the impact on total candidates, what means a 6.2% decrease relative to the total 216 candidates predicted for treated municipalities had them not been treated. However, we do not reject the hypothesis of null effect. By setting different limits on the maximum number of candidates in treatment and control municipalities, the electoral reform implied a decrease on the average number of candidates per coalition. The estimated treatment coefficient of -5.56 represent a

Table 1.2 – Effects on political competition

	Candidates		Coalitions		Parties	
	Total	Coalition average	Coalitions	Eff. Coalitions	Parties	Eff. Parties
Treatment	-13.466	-5.563*	2.070*	0.992*	-0.053	0.886
	(10.216)	(0.708)	(0.497)	(0.353)	(0.516)	(0.619)
Observations	258	258	258	258	258	258
Municipalities	86	86	86	86	86	86

+ $p < 0.1$, * $p < 0.05$. Notes: Robust standard errors in parentheses

-23.2% effect, significant at 5% and we must see this result together to what happened to coalitions.

The relative stability in total candidates with a decrease on the average candidates per coalition was possible due to a 22.7% increase on the number of coalitions, which is the estimated 2.07 coefficient compared to 9 coalitions expected in the absence of treatment. Importantly, more coalitions meant more electoral competition. This is what we analyze by regressing the "effective number" of coalitions (GOLOSOV, 2010), measured by the inverse of the Hirschmann-Herfindal Index calculated from coalitions' vote share in a given municipality.[9]

Results show an estimated impact of 0.922, what represents a 13.6% increase on the number of effective coalitions. The same does not apply to parties. Point estimates for the number of parties with some candidate in the legislative election represent a -0.2% not significat effect. The effect on the number of effective parties (7.8%) suggest more competition also in this dimension, but we do not reject the null of zero effect.

1.6.2 Entry of Candidates

The reorganization of parties and the increased competition might have changed incentives for candidacy. So we analyze if this impacted entry decision of candidates by estimating the impact of the reform on a set of candidates' characteristics. Looking to municipality averages of these characteristics, there is no significant effect on average education and age, there is a small increase in the proportion of women and a large effect on self-reported wealth.

The point estimate of 0.012 for the impact on the proportion of women means a 3.8% increase. About this effect it is worth noting that the Brazilian law requires that women represent at least 30% of a coalition's candidates. With more coalitions, this

[9]Suppose that in different cities there are two and three coalitons with vote shares given by $(0.50, 0.50)$ and $(0.49, 0.50, 0.01)$. The HHI's are 0.50 and 0.4902, respectively. Although the actual number of coalitions are 2 and 3, the effective number of coalitions are 2 and 2.04. Thus, the level of poltical competition is similar despite the different number of actual parties.

Table 1.3 – Effects on candidates' charachteristics

	High School	College	Age	Woman	Asset (log)
Treatment	-0.006	0.009	0.355	0.012*	0.145+
	(0.005)	(0.011)	(0.294)	(0.006)	(0.083)
Observations	258	258	258	258	258
Municipalities	86	86	86	86	86

+ p < 0.1, * p < 0.05. Notes: Robust standard errors in parentheses

estimate can reflect parties and coalitions adjusting their pool of candidates to comply with this law and not exactly a behavioral response from women facing a more competitive environment.[10]

Besides, the estimated effect on self-reported assets (in log) means a 15.6% increase on average wealth. This is a relevant result since an explicit goal of the reform was to reduce the costs of electoral campaigns. We analyze this in conjunction with the effects on the amount of money raised to finance the campaigns. We look separately funds from citizens that supported candidates, money from party resources and self-funding.[11]

Table 1.4 – Effects on candidates' funding

	Average (in R$)			Total (in R$ 1000)		
	Citizen	Party	Self-funding	Citizen	Party	Self-funding
Treatment	470.727	712.385	-315.884	75.465	193.399	16.560
	(733.607)	(1,629.513)	(512.100)	(60.441)	(304.046)	(42.440)
Observations	258	258	258	258	258	258
Municipalities	86	86	86	86	86	86

+ p < 0.1, * p < 0.05. Notes: Robust standard errors in parentheses

Table 1.4 show estimates of the effect on average donations received per candidate and the effect on the sum of all donations in the municipality. We do not reject the hypothesis of no effect in all cases. Therefore, at least in subsample of municipalities we use here, the reform did not reach an important outcome that it intended to. Together with the result on the attraction of richer candidates, this suggests that tighter competition demanded more resources and this moved municipalities to a new equilibrium with the same level of revenue/spending.

[10]It is possible a situation in which two parties form a coalition, only one party present women as candidates and the coalition still complies with the law. If these parties decided to run alone, both should have 30% of women. Thus, the overall number of women could increase if the first party did not adjust completely.

[11]As corporate donation were prohibited as of 2016, we aggregate it with party donations for the two previous elections.

1.6.3 Incumbents Behavior and Political Selection

A special case of candidates are those who run the election while in office. Here we analyze how incumbents behave facing a more competitive environment. The reform made incumbent councilors more likely to run for reelection as table 1.5 show. Expected incumbency rate of councilors at treated municipalities in the absence of treatment was 0.83 ($\approx$ 13 out of 16 seats on average). Thus, the 0.061 estimate means a 7.3% increase in incumbency rate. However, the reduced form estimate for the reelection rate do not indicate any effect. That is, although more incumbents become candidates, reelection keeps at the same level.

Table 1.5 – Effects on incumbents behavior

	Incumbency rate	Reelection rate
Treatment	0.061+	0.023
	(0.036)	(0.036)
Observations	258	258
Municipalities	86	86

+ p < 0.1, * p < 0.05. Notes: Robust standard errors in parentheses

Table 1.6 – Effects on incumbents' charachteristics

	High School	College	Age	Woman	Asset (log)
Treatment	-0.030	-0.014	3.195	-0.059*	0.217
	(0.022)	(0.052)	(3.043)	(0.021)	(0.135)
Observations	258	258	258	258	258
Municipalities	86	86	86	86	86

+ p < 0.1, * p < 0.05. Notes: Robust standard errors in parentheses

There was also a qualitative impact on the pool of incumbents. Though we see no effect on education and age, women in office are less likely to run for reelection in treated municipalities. The -0.059 estimate means a 27.3% decrease relative to the expected proportion of 0.21 women among incumbents without the reform. This means a potential decrease in political experience of women candidates, despite more women on the dispute according to previous result. Estimates are not statistically different from zero for results on incumbents' assets and funding sources (not reported).

In table 1.7 we analyze impacts on the profile of elected councilors. Again, the remarkable result is that they were substantially richer. The estimate of 0.177 on the log of

Table 1.7 – Effects on elected candidates

	High School	College	Age	Woman	Asset (log)
Treatment	-0.026	-0.009	0.932	-0.002	0.177+
	(0.018)	(0.033)	(0.678)	(0.017)	(0.104)
Observations	258	258	258	258	258
Municipalities	86	86	86	86	86

+ $p < 0.1$, * $p < 0.05$. Notes: Robust standard errors in parentheses

self-reported asset means a 19.3% effect, higher than the estimated effect for the whole set of candidates. There is no effect on other personal characteristics of elected councilors and, again, estimates are not statistically different from zero for results on elected candidates' funding sources (not reported). Thus, more women entering the election did not increase their representation, but richer candidates succeeded in getting to office.

1.6.4 Effects on Policy

Once legislative election became more competitive with the outcome that richer candidates were chosen as councilors, we investigate a natural question on whether this had effects on policy.

Table 1.8 – Effects on revenue and spending

	Elected Parties	Discretionary Spending		Budget Share		Revenue	
		Education	Health	Education	Health	Total	Transfers
Treatment	0.017	0.128	-0.071	0.013+	-0.002	0.041	0.011
	(0.400)	(0.529)	(0.585)	(0.007)	(0.006)	(0.028)	(0.029)
Observations	258	254	258	254	253	258	257
Municipalities	86	86	86	85	86	86	86

+ $p < 0.1$, * $p < 0.05$. Notes: Robust standard errors in parentheses

A possible mechanism for why competition should affect policy is through a potential impact on the number of parties represented in the council. However, the first result on table 1.8 shows that the average number of elected parties did not change. If we look to effects on revenues or spending in healthcare there is no significant effect. However, we see a 5.3% increase on the proportion of the public budget allocated for education, which is the 0.013 estimate compared to the expected 0.246 without the reform. It is unlikely that this is a result from councilors reacting to different electoral incentives due to more or less competition in the subsequent election. By 2017, all agents knew that electoral coalitions would not be allowed for the 2020 election, thus the difference in the rule that lead to

different levels of competition would no longer exist.[12] Thus, we conjecture that this could have resulted from an increased responsiveness of councilors to voters that elected them in a more competitive election.

1.6.5 Robustness Exercises

We reproduced our main results by running regressions based on equation 1.1 in different scenarios. First we used all elections, but restricted the sample to municipalities within a window of 20,000 voters below or above a 80,000 voters threshold instead of the actual 100,000 threshold. This excludes the treatment group from the data. Thus, we set $T_{it} = 1$ in $t = 2016$ for municipalities above the 80,000 voters threshold in 2016. This is the definition of *fake treatment*. Besides, we did the analysis falsely considering the previous election as the time of the reform. That is, we discard observations from the 2016 election and set $T_{it} = 1$ in $t = 2012$ for municipalities that would be above the 100,000 voters threshold in 2016. This is the definition of *lead treatment*. These results are reported in the appendix. Overall, the coefficients of interest are close to zero and/or not statistically significant.

1.7 Conclusion

This paper investigated the impact of an institutional reform aimed at reducing the costs of electoral campaigns that end up affecting elections through different channels. By limiting the number of candidates allowed in electoral coalitions, the reform induced parties to reorganize into more coalitions so that the total number of candidates did not fall significantly. Furthermore, there were no significant change in the amount of monetary resources mobilized for the election.

However, the reform implied an increase in political competition. This led to an unexpected outcome of the reform which was the attraction and election of richer candidates. We discussed that the difference implied by the we explored here would no longer exist for the subsequent election. Then, we conjecture that the positive impact on the budget share allocated to education might be explained by the new candidates being more responsive to voters that relied on them in a more competitive election. We think these results are relevant to illustrate that institutional reforms can have unintended consequences.

[12]Constitutional amendment no. 97 (2017).

2 Tit-for-Tat between Legislative and Executive

2.1 Introduction

Legislative and executive powers in the democratic world compose a system of checks and balances aimed at improving the welfare of society. Along with periodical elections, the interplay between legislative and executive bodies is essential for good governance (PERSSON; ROLAND; TABELLINI, 1997). A documented example is party fragmentation. The literature shows that the number of parties affects the bargain process between executive and legislative and ultimately impact the policies to be implemented.[1] More directly, the extent to which governments have legislative support is key to determine what kind of policies they will be able to choose and implement. Understanding the effects of legislative support for governments is a task with two important challenges: it is hard to measure the degree of support and even if it is possible it is difficult to disentangle the degree of support from other important drivers such as political ability and voters' preferences.

In this paper we overcome these challenges by exploring elections and governance in Brazilian municipalities. Parties form coalitions to run in the legislative elections, which happen simultaneously to the executive election, allowing us to have a formal definition of coalitions that support the elected mayor. Moreover, by leveraging the electoral rules on seats' allocation for the local council we have *as good as random* variation on the size of such support. We propose a microfounded method to identify proportional elections that were decided by a slim margin, that is, some parties were really close to lose or gain seats. This method can be seen as a generalization of the discontinuity design largely implemented in majority and plurality elections for elections under proportional rule.[2]

Our results show that legislative support matters for good governance and the effect is heterogeneous with respect to the electoral incentives that mayors face. With budget data we can measure the changes in planned spending that mayors do with legislative approval. We discuss that this variable can be interpreted as the degree of flexibility in the budget management given to mayors by local councils. Moreover, it is likely to be associated with budget mismanagement as it gives discretionary power to mayors. Stronger legislative support implies a 25% increase in this flexibility relative to municipalities in

[1] Ashworth e Heyndels (2005), Borge (2005), Schaltegger e Feld (2009), Maux, Rocaboy e Goodspeed (2011)

[2] Our method works in any system that assigns a continuum of vote shares to integers number of seats.

which mayors have less support. But the effect is strongly heterogeneous depending on the mayor's term. For mayors in second term the effect can be as large as 69%. Consistent with evidence of increased mismanagement by mayors in second term, point estimates suggest that they are more prone to receive a negative evaluation of their budget management from state courts of accounts, while the opposite happens for mayors in first term. Further, we find that one more councilor increases revenue and spending of mayors in second term. Finally, for mayors in first term there is a positive impact in mayor's party share of votes in the subsequent election, but a negative effect for mayors in second term.

Results suggest that since mayors in first term still have electoral incentives, more legislative support do not lead to worse governance outcomes. However, more support to mayors with no chance of reelection translates into worse management. We interpret our findings within the framework of a simple model in which the cost for the legal protection the councilors are able to give to the mayor decreases with more support. This increases the incentives that mayors in second term have for rent extraction. However, these incentives are balanced against reelection incentives for mayors in first term.

We contribute to a literature showing that political accountability is important in shaping good governments (FERRAZ; FINAN, 2008; FERRAZ; FINAN, 2011a). Our results show that different margins of accountability can interact to determine politicians behavior. In particular, the existence of electoral accountability (reelection) balances a potential loss in horizontal accountability (more legislative support). Another contribution is the method for defining slim margins in proportional elections. Our method is less restrictive than Folke (2014) and Fiva, Folke e Sørensen (2018) propose. The margin of the election in their method is defined by an aggregate shock in total votes needed to change the allocation of seats. However, they implicitly assume that all parties have the same probability of receiving one vote.[3] Instead, we recognize that voters have different preferences. Also, our method can be extended to identify not only the effect of increasing seats of coalitions or parties, but also the effects of minorities or individuals. A final contribution is gathering a new dataset constructed from information on budget management that allows us to measure an outcome from the legislative-executive interaction.

The paper is organized as follows. In the next section we provide the institutional context of our analysis by describing local elections in Brazil and the role of legislative and executive powers in local governance. Then, we explain the theoretical framework and our data. We then discuss our identification strategy and how we implement it. Finally, we present our results and final remarks.

[3] They compute an L^1 distance between vectors of votes resulting in different seat allocations.

2.2 Institutional Background

Brazil is a highly decentralized federal country, with 5,570 municipalities that are fundamental to supply healthcare, primary education, and other public services. To provide these public goods in a system of checks and balances, each municipality elects a mayor and a city council every four years. In this section, we focus on two features of the institutional environment that are central to politicians' behavior. Because elections are key to the interplay between the executive and legislative bodies, we describe how they work at the local level in Brazil. Then, we explain how the powers interact in the proposal, approval and inspection of the city budget every year.

2.2.1 Elections in municipalities

Once every four years, Brazilian electors choose the mayors and city councillors of all municipalities of the country. The elections of the legislative and executive members are simultaneous and a voter can cast one vote in each election. Mayors are elected by plurality rule in municipalities with fewer than 200,000 registered voters and by majority rule (with a runoff election if necessary) otherwise.

Seats of the council are assigned under an open list proportional representation system, described as follows. Around two months prior to the first round, parties form coalitions. By this time, each coalition registers its candidates for city councillor.[4] Each elector vote for one party and has the option of identifying a candidate of this party to receive this vote–in this case we say that this candidate received a vote herself. A vote cast in a party (or candidate) running in the election adds a vote to its comprising coalition and is called a valid vote. Coalitions with a number of votes greater or equal to the total of valid votes over the number of seats of the city council are eligible to compete for seats. From this point, the number of seats each coalition obtains is determined according to the D'Hondt method (see Section 2.5 for an explanation of this assignment rule). Finally, the n candidates with the largest number of votes in a coalition with n seats are elected city councillors.

An important feature induced by the Brazilian electoral design is that each seat matters. The number of seats in the councils varies from 9 to 55 depending on the municipality's population.[5] In particular, two thirds of the councils have the minimum number of seats and more than 80% of municipalities have up to 11 seats. Loosely speaking, in the vast majority of municipalities, flipping a single seat from the opposition increments the mayor's margin of support in the council by around 20 percentage points.

[4]These candidates must be members of the parties belonging to the coalition.

[5]The Brazilian Electoral Court set the rule for the 2004 and 2008 elections, but a constitutional amendment defined the current population thresholds valid from the 2012 election on.

Finally, the electoral law of Brazil creates distinct political horizons for city councilors and mayors. Members of the legislative do not have a limit for the number of consecutive terms so that their political choices are taken based in a long-term horizon. By contrast, all mayors in our sample could hold office for up to two consecutive terms. (FERRAZ; FINAN, 2011a) argue that even if a reelected mayor can return after a one-term hiatus, the average second-term mayor behaves as if it was the last period of his political horizon. This claim agrees with the electoral data from 2004 to 2020: after concluding their second consecutive term, 15% of mayors were ever reelected and only 9% ran for higher officers (at state or federal level).

2.2.2 Local public budget

Local governments in Brazil implement policies through the execution of the public budget, a legal document detailing revenue and spending for programs of different areas within the period of one year. The executive body makes a budget proposal that once agreed by the city council becomes a law the mayor must obey.[6]

At the end of the fiscal year, state courts of accounts check whether mayors complied with all the rules they should during budget implementation. Each of the 26 states of Brazil has a functionally autonomous court of accounts. However, in spite of their denomination, these courts do not judge budget executions. Instead, each court elaborates technical reports of all state's municipalities informing whether the mayor complied with the budget law and other federal legislations. These reports are submitted to the respective city councils, who vote for approving or rejecting the budget execution, not bound to follow the recommendation of the court of accounts. If this voting rejects the budget execution, mayors are prosecuted and can become ineligible or even be impeached.

Two reasons make the courts' technical reports likely to be exempt from political influence in municipalities. First, municipalities have no say in the choice of the members of the court of accounts. Each court of accounts is composed of seven members, four of which are appointed by the state legislative assemblies and the three remaining members are appointed by the executive head of the state. Second, members of these courts have no term length, but a mandatory retirement at age 75, which helps to prevent tit-for-tat political maneuvers.

[6] It is important to distinguish that the public budget is a law approved every year at the municipality level defining what policies will be implemented. However, it is legislation at the federal level that regulates the budget management. The constraints are in the article 167 of the Brazilian Constitution (<http://www.planalto.gov.br/ccivil_03/constituicao/constituicao.htm>), law 4320/1964 (<http://www.planalto.gov.br/ccivil_03/leis/l4320.htm>) and law 101/2001 (<http://www.planalto.gov.br/ccivil_03/leis/lcp/lcp101.htm>)

2.2.3 The Executive-Legislative bargain

Though public budget execution is rigid since it is a law that mayors must comply with, there exists a legal device to give them some flexibility to manage it. With proper legislative authorization, the mayor can change budget spending due to unforeseen circumstances by editing executive orders.[7] The council can give the mayor such authorization in two ways. The budget law itself may set a limit up to which the mayor can relocate spending (under further legal constraints) or the mayor ask for this authorization every time he needs. However, the mayor cannot use this device to change the budget at will. Approved spending in healthcare cannot be cancelled to be used in a new or existing project of streets' pavement, for example. This tool only allows the mayor to deal with price variations, to correct mistakes or, if there are new revenues, to increase spending already approved in the budget. Everything beyond this needs new legislative authorization.[8]

However, it seems that mayors frequently do not comply with this rule. That is, this constrained modification is used as flexibility *de facto*. Arantes (2017) analyzes municipalities from the state of Minas Gerais in the years of 2010 and 2011 whose accounts received a rejection recommendation by the state court of account. In roughly 60% of the cases there are problem related to misuse of this device.

Recall that city councillors are responsible for approving the budget proposal, authorizing changes on it, voting the court's report on budget execution, and, in case of rejection, judging a possible impeachment process. This gives councillors leverage to bargain with the executive branch. Perhaps the most dramatic example of the political use of the budget analysis in Brazil is the impeachment of the former President Dilma Roussef in 2016.[9] Although the Federal Court of Accounts recommended the federal budget execution to be rejected, only 6.5% of the voting representative alleged fiscal reasons to vote for the impeachment (PRANDI; CARNEIRO, 2017).

It is not clear to what extent the voting of the budget execution is political, but evidence that it is not merely technical abounds. If this voting were based solely on technical reasons, we would expect city councillors to almost always follow the court of accounts recommendation, but this is not the case. For instance, between 2000 and 2019, the Pernambuco state court of accounts recommended the rejection of 1,149 budged executions out of almost 3,700 analyzed accounts. About half of them (522, 14% of total) were approved by the city councils, though. Anecdotal evidence also supports the political use of budget execution voting. Arantes (2017) interviews several mayors whose budget execu-

[7] This device is called *créditos suplementares*.

[8] There is a different device to do this type of modifications (Brazilian Constitution, art. 167). See also Furtado (2005).

[9] At state and federal levels, the rules of budget proposals and execution follow the same lines as at municipal level.

tions got a rejection recommendation by the Minas Gerais court of accounts. Interviewee 1, who did not have problems to approve the budget execution, said: "[...] *Will I create a conflict with city councillors? A city councillor can destroy the mayor.* [...] *for the executive, a city councillor does not do anything,* [...] *but he can destroy the administration.*" Interviewee 2, who was waiting for the city council to vote his budget execution by the time of the interview, stated that "[...] *my budget execution was rejected* [*by the court of accounts*]*, but if the city council says 'it was not* [...]*, we think the mayor did his job well',* [...] *they approve the budget and this is it.*"

This scheme gives the council a source of bargaining power relative to the mayor. Even when properly authorized, the mayor must comply with rules to implement changes. If he does not comply, he should be charged by mismanagement in a process started by the councilors themselves. Thus, not only councilors can set the limit for this flexibility in budget management that mayors use as a discretionary power, but also they are responsible for how strict law enforcement is in cases of non compliance with law. Two of our main outcomes of interest are the proportion of budget reallocation made through this device and the state court recommendation about mayor's budget management. We discuss how we built these variable for a subsample of the municipalities in the section about data.[10]

2.3 Theoretical Framework

We present a simple model that helps us understand the channels leading to our empirical findings. In our framework, mayors only seek to extract rents as in Ferraz e Finan (2011a) and Avis, Ferraz e Finan (2018) and we introduce support in the city council as one important factor impacting mayors' behavior. In the spirit of the discussion in the last section, stronger legislative support decreases the cost a mayor faces by committing acts of mismanagement, but increases the mayor's obligations with his supporters. Since voters can punish bad mayors by giving them fewer votes, the level of rent extraction also depends on whether the mayor can run for reelection.

The model

Our economy has three types of agents: a continuum of voters with mass 1, a continuum of city councillors with mass 1, and the mayor. Since we normalize the number of seats to 1, the share of city councillors belonging to the mayor's coalition equals the number of councillors in this coalition. Mayors can hold office for at most two consecutive periods and we assume that after losing an election they retire from their political career. Thus, we set the horizon to two periods for mayors and to infinite for the other agents.

[10] We do not have a variable for the outcome of council votes about the court's recommendation yet.

The mayor's intertemporal discount factor is δ, he maximizes pecuniary gains from rents extraction, and sends a reputation signal a in the first time he runs. If the mayor extracts r in his first term, he sends a reputation signal in the second term of $a - r$ and its party inherits this signal for the next executive election. This captures the fact that voters tend to punish bad politicians/parties as discussed by Ferraz e Finan (2008). If a candidate of party j sends a reputation signal a, the reputation perceived by voter i is $a_{ij} = a + \epsilon_{ij}$, with $\mathbb{E}[\epsilon_{ij}|a] = 0$ and ϵ_{ij} being normal and i.i.d on i and j.

The timing is as follows. First, the mayor chooses r. Then, the reputation signal (only reputation, hereafter) A of the candidate against who the mayor's party runs the following race is drawn from a cumulative distribution G. We assume G is twice differentiable, strictly increasing, and strictly concave. After A is revealed, each voter chooses a candidate.

Each voter maximizes his utility by choosing the mayor with the best perceived reputation. Thus, conditional on A, if the mayor belongs to party 1 and his opponent to party 2, voter i votes for the current mayor if $a - r + \epsilon_{i1} > A + \epsilon_{i2}$. We aggregate the individual number of votes to obtain the share of votes cast for the mayor, given by $\int_0^1 \mathbf{1}_{\{a-r+\epsilon_{i1}>A+\epsilon_{i2}\}}\text{¡} = \Pr(\epsilon > A - a + r)$, where ϵ is normal with mean 0. The mayor is reelected if $\Pr(\epsilon > A - a + r) > 1/2$, an event equivalent to $a + r > A$. This implies that the mayor is reelected with a probability $\Pr(a - r > A) = G(a - r)$.

The cost of extracting r in terms of the probability of penal sanctions when the mayor has no support is $r^2/2$.[11] The cost of extracting r having a support s is given by $(1 - s)r^2/2$, so legislative support offers a legal protection against prosecution. City councillors charge the mayor for their help: a fraction θs of r is retained by them, so the mayor's pecuniary private gain with corruption is $(1 - \theta s)r$.

We evaluate the mayor's problem by backward induction. A second-term mayor utility that extracts r having a support s_2 is given by $(1-\theta s_2)r-(1-s_2)r^2/2$. This implies that the optimal level of rents extraction as function of s_2 is $r_2^\star(s_2, \theta) := (1-\theta s_2)/(1-s_2)$ and his optimal utility is $u_2^\star(s_2, \theta) := (1 - \theta s_2)^2/(2(1 - s_2))$.

The implications of these results depend on the value of θ. First, the total amount of extracted rents increases (resp. decreases) in s_2 if $\theta < 1$ (resp. if $\theta > 1$). A stronger support incentivizes rents extraction by reducing the marginal cost of mismanagement, but reduces the marginal pecuniary benefits of these acts. The knife edge case when the marginal cost cancels out the benefit occurs when $\theta = 1$ and the level of mismanagement is the same regardless of the level of legislative support. Second, the benefit of the coalition per councillor is $r_2^\star(s_2, \theta)\theta s_2/s_2 = \theta(1 - \theta s_2)/(1 - s_2)$. The sign of the derivative of this term in s_2 is the same as $\theta - 1$. Therefore, θ informs how competitive/cooperative the

[11] The functional form of this cost does not change the qualitative conclusions of the model as long as the cost is increasing and concave. We set the quadratic form for the sake of simplicity.

councillors in a coalition behave when bargaining with the mayor. The case $\theta < 1$ indicates the existence of some degree of competition since an extra councillor decreases the average benefit per councillor. If $\theta = 1$, the price to be paid for the support of each councillor is fixed. A parameter $\theta > 1$ indicates a degree of coalition's cohesion that increases the bargaining power of the coalition as the coalition becomes larger. Third, having a stronger support does not necessarily increase the second-term mayor's utility. For example, if $\theta = 1$, the mayor's coalition is in a privileged position of the bargaining process: the mayor, not the councillors, is the one who reduces his private benefit to accommodate the interests of a larger group of supporters.

We turn to first-term mayors with support s_1. We assume that the distribution of the legislative support of the mayor, S_2, conditional on being reelected, is independent of s_1 and we set $\overline{U}_2(\theta) := \mathbb{E}[u_2^\star(S_2, \theta)]$. If a is his initial reputation, s_1 his current support, and r is the amount of rents he extracts, his utility is

$$(1 - \theta s_1)r - (1 - s_1)r^2/2 + \delta G(a - r)\overline{U}_2(\theta).$$

The optimal amount $r_1^\star(s_1, \theta)$ for first-term mayors solves

$$\frac{1 - \theta s_1}{1 - s_1} - \frac{\delta \overline{U}_2(\theta)}{1 - s_1} G'(a - r_1^\star(s_1, \theta)) = r_1^\star(s_1, \theta).$$

Since $G' > 0$, we immediately conclude that $r_1^\star(\cdot, \theta) < r_2^\star(\cdot, \theta)$, that is, corruption in the first term is lower than in the second term (for each level of legislative support). This is in line with the findings of (FERRAZ; FINAN, 2011a).

Now, we analyze how the level of rent extraction varies as the support increases. Applying the Implicit Function Theorem, we obtain

$$\frac{\partial r_1^\star(s_1, \theta)}{\partial s} = \frac{\theta - r_1^\star(s_1, \theta)}{\delta \overline{U}_2(\theta) G''(a - r_1^\star(s_1, \theta)) - (1 - s_1)}.$$

The value of $\overline{\theta}(s_1)$ making the numerator equal to 0 (defining the *locus* in the plane (s_1, θ) in which $\partial r_1^\star(s_1, \theta)/\partial s = 0$) is such that $\overline{\theta}(s_1) = r_1^\star(s_1, \overline{\theta}(s_1))$, so

$$\frac{\underset{.}{\theta}(s_1)}{\underset{.}{s}} = \frac{\partial r_1^\star(s_1, \overline{\theta}(s_1))}{\partial s} + \frac{\partial r_1^\star(s_1, \overline{\theta}(s_1))}{\partial \theta} \frac{\underset{.}{\theta}(s_1)}{\underset{.}{s}}.$$

By construction, $\partial r_1^\star(s_1, \overline{\theta}(s_1))/\partial s = 0$. Since $\partial r_1^\star(s_1, \overline{\theta}(s_1))/\partial \theta < 0$ (a straightforward application of the Implicit Function Theorem), we deduce that $\underset{.}{\theta}(s_1)/\underset{.}{s} = 0$, that is, $\overline{\theta}$ is a constant. In particular, when $s_1 = 0$, we have $r_1^\star(0, \overline{\theta}(0)) = \overline{\theta}(0) = \overline{\theta}$ and applying this in the first order condition of the first-term mayor gives us $\overline{\theta} = 1 - \delta \overline{U}_2(\overline{\theta}) G'(a - r_1^\star(0, \overline{\theta}))$. Because $G' > 0$, we have $\overline{\theta} < 1$. To conclude, we observe that $G'' < 0$, so $\partial r_1^\star(s_1, \theta)/\partial s$ has the same sign as $\overline{\theta} - \theta$.

Together with the predictions for second-term mayors, this result suggests the existence of three regions of interest on which θ can fall. We say that the degree of

cooperation between members of the mayor's coalition is low when $\theta < \overline{\theta}$, is moderate when $\overline{\theta} < \theta < 1$ and is high when $1 < \theta$. Only when the coalition has a low degree of coordination, a stronger support leads to increased mismanagement for a first-term mayor. Otherwise, a larger coalition leads to *less* mismanagement in the first term. In addition, since $G(a - r_1^\star(s_1))$ is increasing in s_1, more support implies a higher probability of reelection when $\overline{\theta} < \theta$. Table 2.1 summarizes our predictions about the impact of strengthening the mayor's coalition for distinct levels of cooperation.

Table 2.1 – Predictions of causal effects of a stronger support on

	Degree of cooperation		
	Low ($\theta < \overline{\theta}$)	Moderate ($\overline{\theta} < \theta < 1$)	High ($\theta > 1$)
Legal protection	$+$	$+$	$+$
Level of corruption, 1st-term	$+$	$-$	$-$
Level of corruption, 2nd-term	$+$	$+$	$-$
Executive vote share, 1st-term	$-$	$+$	$+$
Executive vote share, 2nd-term	$-$	$-$	$+$

$+$ entries: our model predicts that an increase of the legislative support causes an increase in the variable (the lines), given a level of coalition's cooperation (columns). $-$ entries: the analogous when the variable decreases.

2.4 Data

2.4.1 Electoral Data

Brazilian Electoral Court provides all microdata related to local elections. We use elections held in the period 2004-2016 since the court itself warns they are revising data from previous elections. Mayors in first or second term are identified by tracking the their electoral history with their social security number (CPF). Also with these data, we are able to reproduce the results of all elections and then we can simulate the counterfactual elections from actual results. An important information to reproduce elections' results is the electoral coalition to which each party belonged to. This is also reported in the data.

2.4.2 Budget Data

Data on budget management is not available through a centralized organization and, to date, it is not even available for all municipalities. We gathered data from 1,963 municipalities in four states (Minas Gerais, São Paulo, Rio Grande do Sul and Ceará). In these states, the courts of accounts release systematic reports on budget modifications as discussed in section 2.2 The data is available for different periods in each state and we have a total of 7,869 observations. The variable we call *budget flexibility* is the proportion

of spending changes with legislative authorization relative to total spending in a given year. It is our measure of the flexibility *de facto* that councilors gave to mayors. We could also get data from three states regarding the reports on mayor's budget management (São Paulo, Bahia and Pernambuco). The available period is also irregular and we have a total of 15,410 observations. This variable indicates whether the state court sent to the council a rejection recommendations. This is our measure of mismanagement. On average, 29.5% of the accounts receive a negative report. Finally, from balance sheets reported by municipalities yearly and made public through the *FINBRA* database we recover data on revenue and spending of each municipality.

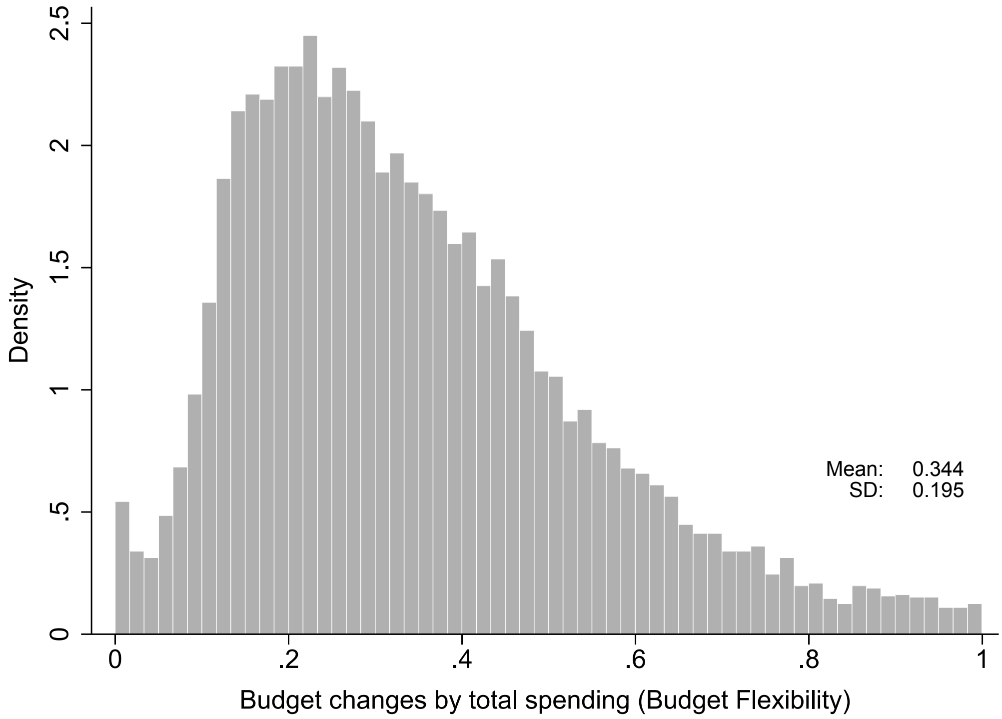


Figure 2.1 – Distribution of the variable *budget flexibility*, computed as the proportion of spending changes with legislative authorization relative to total spending in a given year.

2.5 Research Design

We are interested in testing whether the city council's composition affects the interplay between the executive and legislative. The city council's composition is clearly endogenous, likely being correlated to unobservable variables, such as voters' preferences, politicians' ability or the underlying reasons that determined the coalitions' prior to the elections. Using the fact that Brazil applies the D'Hondt method to assign seats in the city councils, we propose a research design that will generate exogenous variation in the city council composition close to the ideal experiment of randomizing the number of seats obtained by the mayor's coalition.

2.5.1 Identification strategy

The D'Hondt method assigns each seat of the council in a sequential manner. We call the *votes' allocation* the list with the number of votes each coalition obtained. In each round, each coalition has a quotient calculated as the number of votes received over the current number of seats plus one. The coalition with the largest quotient wins the seat of the round.[12] Figure 2.2 shows the seats' assignment when three coalitions compete for four seats, under two distinct votes' allocations.

(a)

Round	Coalition 1	Coalition 2	Coalition 3
1	**1920**	1000	650
2	960	**1000**	650
3	**960**	500	650
4	640	500	**650**

(b)

Round	Coalition 1	Coalition 2	Coalition 3
1	**1920**	1020	630
2	960	**1020**	630
3	**960**	510	630
4	**640**	510	630

Figure 2.2 – Each panel shows a different votes' allocation. Panel (b) is obtained from panel (a) after 20 voters swung from coalition 3 to coalition 2. Entries in bold display the winner of the round. In each row the winning coalition is indicated in bold letters. The winner is the coalition with the highest ratio between number of votes and seats already taken plus one. Every coalition starts round 1 with zero seats.

Figure 2.2 also displays the main idea of our identification strategy: the number of seats coalition 1 wins varies with a small change in the votes' allocation. Note that coalition 1 has different numbers of seats even if the turnout and the number of votes for this coalition remain constant. We posit that when the seats' allocation changes after a small variation of the votes' allocation, the assignment of this seat is "as good as random." In order to object this assumption, one would have to argue that a coalition has total and perfect control over the turnout, over its own votes *and* over each of the other parties' votes – unreasonable claims.

It remains defining our criterion to establish whether a votes' allocation is close to change the number of seats. A natural question a reader might ask is why we do not use the discountinuity design used in Lee (2008) based on the margin of victory. To answer this question, consider a close race that A won against B under the plurality rule. Note that the following statement holds: if v fewer votes would cause A to lose the race, then with v extra votes B would win the race; the share of votes, be it of the turnout or of the A's (resp. B's) votes, A would need to lose (resp. B would have to gain) to flip the election outcome is small.

These symmetries make a unidimensional running variable suitable for majority elections, but none of these properties is present in the D'Hondt method. In panel (a)

[12] According to the electoral law of Brazil, to be eligible to compete for seats, a coalition has to reach a number of votes equal to the sum of votes for all coalitions over the number of seats (*Quociente Eleitoral*).

of Figure 2.2, coalition 1 would need at least 30 extra votes to win the last seat, while coalition 3 could lose the seat with 10 fewer votes. More generally, the narrowness by which a coalition wins or lose a seat is sensitive to the normalization of each coalitions' votes. For example, suppose that in a municipality with 10,000 valid votes, 100-extra votes would make coalition 1 winning an extra seat. These 100 votes represent only 1% of the valid votes, but this would represent represent a 50%-increase in the number of their own votes if this coalition received a total of 200 votes.[13] Folke (2014) also explored the randomness in proportional elections. However, he defines a close election based on L^1 distance metric between vectors of votes that would give different allocations. This would capture an aggregate shock necessary to change seat distribution. But it implicitly assumes that all parties have the same probability of receiving one vote, what is problematic.

To abstain from these issues, we propose a probabilistic model to measure the narrowness of a race for a seat. Assume a municipality has N coalitions numbered from 1 to N and V eligible voters. Given the campaign efforts of each coalition and the distribution of the voters' preferences, a voter randomly drawn has a probability p_i for voting for coalition i and a probability p_0 of abstention. If the random variable induced by these probabilities is the same and independent across the eligible voters, the distribution of the votes' allocation is multinomial with V number of trials and $p_0, p_1, \dots, p_N$ event probabilities. That is, the probability of coalition 1 having v_1 votes, coalition 2 having v_2 votes and so on, with v_0 abstentions is

$$\frac{V!}{v_0! v_1! \cdots v_N!} p_0^{v_0} p_1^{v_1} \cdots p_N^{v_N}.$$

Although the p's are unknown to us, we can estimate them using the actual votes' allocation $(V_1, V_2, \dots, V_N)$ and the number of abstentions V_0. The non-biased estimate with minimum variance for p_i is given by $\hat{p}_i := V_i/V$.

Once these probabilities are estimated for an election, we simulate 5,000 votes' allocations. Then we focus on the subset of elections in which the mayor's coalition had a relatively high probability of ending up with s seats and, at the same time, a relatively high probability of obtaining $s+1$ seats. From this subset, our treatment group consists of the elections where the actual number of seats of the mayor's coalition is $s+1$. The control group is the analogous when the actual number of seats is s.

We will use a real example to illustrate our procedure. The municipality of Douradina (PR) in the election of 2004 had 3 coalitions running for 9 seats in the local council. For easiness of exposition we will call coalition A, B, and C and the coalition A elected the Mayor. From the 4,842 total voters of the municipality, 645 were absent (or cast invalid votes). Coalitions A,B, and C had respectively 1648, 1313, and 1236 votes yielding them 4, 3 and 2 seats.

[13]Boas, Hidalgo e Richardson (2014) also discuss problems with normalizations when exploring discontinuities within coalitions in proportional electios.

The first step is to get estimates for $\hat{p}_0, \hat{p}_A, \hat{p}_B, \hat{p}_c$, which is straightforward from the real numbers in the election: $\hat{p}_0 = \frac{645}{4842} = 0.133, \hat{p}_A = \frac{1648}{4842} = 0.340, \hat{p}_B = \frac{1313}{4842} = 0.271, \hat{p}_C = \frac{1236}{4842} = 0.255$. With this probabilities we can simulate 5,000 elections for this municipality-year. Table 2.2 presents the first 6 simulations for this case. In the first row we show the outcomes from the election, in the second row we present the first simulation where we would obtain 615 voters absenting or casting invalid votes and respectively 1683, 1303 and 1241 for coalitions A,B, C. In this draw, the number of seats would remain the same (4, 3, and 2). In the draw of the second simulation, however, even if coalition A lost only 8 votes (in comparison with the real election), it looses one seat for coalition C. It is worth noting that in every simulation the total number of eligible voters is kept constant.

Table 2.2 – Simulation: Example

		Votes				Seats		
# Sim	Total Voters	Absent	A	B	C	A	B	C
(Real)	4842	645	1648	1313	1236	4	3	2
1	4842	615	1683	1303	1241	4	3	2
2	4842	594	1640	1353	1255	3	3	3
3	4842	599	1646	1382	1215	4	3	2
4	4842	662	1698	1299	1183	4	3	2
5	4842	627	1648	1311	1256	3	3	3
6	4842	626	1613	1376	1227	3	3	3
⋮			⋮					

Notes: Simulation example for the municipality of Douradina (PR) in the 2004 election.

We draw 5,000 and compute the proportion of them that coalition A had 3 seats and the proportion of times that it has 4 seats. In this case, Coalition A had 50.4% of probability of having four seats and 49.6% probability of ending up with three seats. Therefore, Douradina in 2004 will be classified as a really competitive election. Moreover, because the coalition of Douradina's mayor ended up with four seats, this election is part of the treatment group.

Analogously, the municipality of Nova Brasilândia (MT) in 2004 the mayor's coalition had 50.7% probability of having 3 seats and 49.3% of having 4 seats. Once the mayor's coalition secured 3 seats it is classified as control. Our empirical strategy relies on the comparison of this municipalities to identifiy the causal effect of one extra councillor in the mayor's coalition. Figure 2.3 illustrates the case of four hypothetical elections, from which one is part of the treatment (B), one is part of the control (A), and two are discarded in our analysis (B and D). The axis show the votes of coalitions 1 and 2 as a fraction of coalition 3 votes and the shaded areas represent the votes' allocation that would yield coalition 1 a given number of seats. Each dot in panel (b) is a simulated election, where the color identifies if there was a seat change for coalition 1.

Our probabilistic model estimates a treatment probability that is unbiased if the

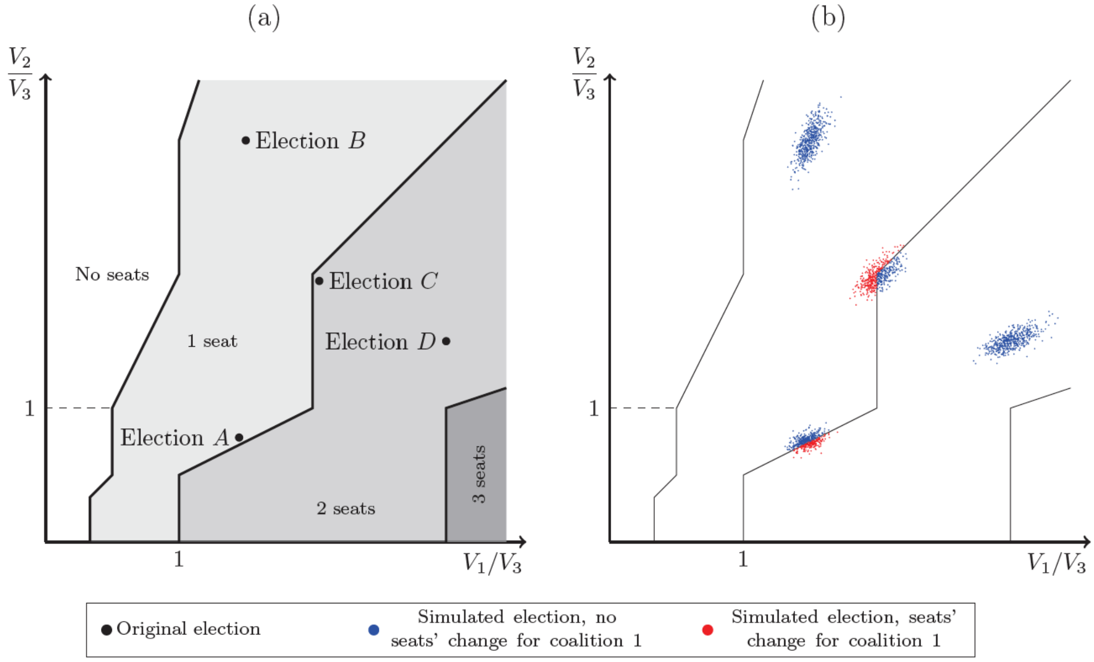


Figure 2.3 – Coalitions 1, 2, and 3 compete for a total of three seats. The value V_i is the number of votes cast for coalition i. In panel (a), we explicit the number of seats earned by coalition 1 as a function of V_1, V_2, and V_3 in the plane $V_1/V_3 \times V_2/V_3$. We set four original votes' allocations, that we call elections A, B, C, and D. In panel (b), for each election, we simulate 500 counterfactuals using the procedure described in the text. A blue point is a counterfactual holding the same number of seats coalition 1 has in the original election. A red point is the analogous when the number of seats of coalition 1 changes. If these elections were in our dataset, election A would be a control, election C a treatment, and we would discard elections B and D.

voters' preferences are independent. However, our identification strategy does not strictly depend on the independence assumption. Rather than calculating the exact probability, our multinomial model aims at identifying the elections with a high degree of uncertainty (to the candidates) of the seats' assignment.

2.5.2 Specification

We are interested on analyzing the effects of a stronger legislative in on the outcome from the interplay between municipal executive and legislative entities. Hereafter, we write *effect of an extra seat* to refer to the effect of an extra seat allocated to the elected mayor's coalition. Our simulation procedure described in the last section allow us too (i) identify competitive legislative elections, that is, elections where the mayor's coalition could end up with s or $s+1$ seats and (ii) to classify municipalities into treatment or control, if in the election they secured $s+1$ or s seats, respectively.

Our main specification is given by

$$y_{mt} = \alpha + \beta T_{mt} + \epsilon_{mt}, \tag{2.1}$$

where y_{mt} is the outcome of interest, in municipality m and election year t, T_{mt} is an indicator for whether the mayor was treated with an extra seat in his coalition, and ϵ_{mt} is a random error term for the election. Since T_{ms} is orthogonal to ϵ_{mt}, the OLS estimation for the coefficient β gives us the average effect of an extra seat on the outcome of interest.

We estimate this specification restricting the sample to those municipalities-years with competitive legislative elections for the mayor's coalition. We restrict the sample based on the treatment probabilities, using as the baseline those that lies between 35% and 65%, that is, at most 15 percentage points away from the most uncertain 50-50% scenario. We will use alternative treatment probability windows (TPW) as robustness.

For some outcomes we have several observations during the same term of the mayor, as an example, for budgetary outcomes where we have one observation per (fiscal) year, yield thus, 4 observations for each mayor. When this is the case, we adjust the above specification to:

$$y_{mt\tau} = \alpha + \beta T_{mt} + \epsilon_{mt\tau}, \tag{2.1'}$$

where τ indexes the year of the term from 1 to 4. Obviously, T_{mt} does not vary over the term and as mentioned below we clustered our standard errors at the municipality-election level, encompassing all years in the same term.

As evidence suggests (e.g., (FERRAZ; FINAN, 2011a) and (AVIS; FERRAZ; FINAN, 2018)), the limitation of two consecutive mayoral terms creates distinct incentives for a mayor depending on whether he can be reelected. As a consequence, this might affect the conditions under which a mayor and a city council negotiate. That is, the causal impact of an extra seat might be heterogeneous on the term – first or second – he is serving.

We test for this differential effect of the type of term by estimating the model

$$y_{mt} = \alpha + \beta_0 R_{mt} + \beta_1 (T_{mt} \times (1 - R_{mt})) + \beta_2 (T_{mt} \times R_{mt}) + \epsilon_{mt}, \tag{2.2}$$

where R_{mt} takes value 1 if the mayor of election mt is in his second term, and 0 otherwise. Here, β_1 is the average impact an extra seat for mayors in the first term and β_2 is the equivalent measure for reelected mayors. Once again, the OLS estimation for β_1 and β_2 is unbiased since T_{mt} and ϵ_{mt} are independent and R_{mt} is predetermined.

Following the recommendations of (ABADIE et al., 2017) we clustered our standard errors at the election level (municipal-year). The assessment proposed recently by (FERMAN, 2019) does not seem to indicate problem with the inference procedures under the assumption of independence of each election observation, which seems reasonable in our context, particularly when we restrict our sample to the close elections.

Before proceeding to the results we show in the table 2.3 some descriptive statistics from our sample. We have in the total 22,010 local valid elections between 2004 and 2016, among which 1,323 (6%) are considered close competitive elections for the elected mayor's coalition.[14]. The first two columns show the mean and standard deviation for each variable for the entire sample of elections and columns 3 and 4 for the restricted sample withing the treatment probability window of [0.35-0.65].

Table 2.3 – Descriptive Statistics

	All Elections (all)		**Sample** TPW [0.35-0.65]	
Variable	*Mean*	*SD*	*Mean*	*SD*
Population (in thousands)	28.32	145.56	18.91	47.49
Eligible Voters (in thousands)	19.84	103.95	13.48	32.43
Local Council Seats	9.76	2.10	9.50	1.58
Mayor in Second Term	0.27	0.45	0.28	0.45
# Candidates Running for Mayor	2.82	1.14	2.78	1.10
Elected Mayor Vote Share	0.56	0.13	0.56	0.13
# Candidates Running for Local Council	70.27	65.02	64.77	55.81
# Coalitions Running for Local Council	4.51	2.47	4.40	2.33
# Coalitions with Elected Councilors	3.55	1.51	3.46	1.38
# Parties with Elected Councilors	5.98	1.90	5.77	1.77
Number of Observations	22,010		1,323	

We can see that on average, our sample over represents smaller municipalities, however all the other outcomes do not exhibit much disparities. The average number of councillors is 9.5 in our sample and 28% of mayors are in second term. In the election for mayors there are on average 2.8 candidates running and the elected mayor has a vote share of 56%. In the legislative dispute, there are on average 4.4 coalitions running and around 65 candidates. From the 4.4 coalitions running, 3.4 elect councillors, which translates to, on average 5.8 parties electing city councillors.

2.6 Results

In this section, we estimate the coefficients of Equations 2.1 and 2.2 for outcomes that allow exploring the causal effect of an extra seat on governance. However, before jumping into these results, we perform several checks in our identification strategy in order to confirm that we are in safe grounds when taking our treatment as exogenous.

[14]We exclude some elections with missing or incongruous data from TSE, this affects less than 1.2% of the elections

2.6.1 Validating our identification strategy

First, we use our specifications 2.1 and 2.2 to compare the number of seats in the mayor's coalition between treated and control municipalities. By the design of our strategy we should have a mechanical effect of 1 extra seat for the treated group. In table 2.4 we report these results. The first three columns show the effects for different windows with specification 2.1, hereinafter we will call them by the short TPW (treatment probability window). Panel A shows the results for all elections, and panel B shows the results for specification 2.2 where we interact the treatment variable with the term of the mayor. We can see that in all of them we estimate an effect really close to one, in none of them we reject the null that the estimate is 1. Moreover, considering the control mean of 3.3, we can notice that the extra effect increases the mayor's coalition by more than 30%, being a sizable effect.

Table 2.4 – Mechanical Effects: Outcome Number of Seats of the Mayor's Coalition

	(1)	(2)	(3)
Panel A: Overall			
T	1.042*	1.052*	1.054*
	(0.097)	(0.081)	(0.067)
Panel B: By Mayor's Term			
T × First Term	1.017*	1.006*	1.035*
	(0.115)	(0.094)	(0.077)
T × Second Term	1.111*	1.219*	1.149*
	(0.172)	(0.151)	(0.131)
Window	[0.40-0.60]	[0.35-0.65]	[0.30-0.70]
N Obs	891	1323	1847
Control Mean	3.376	3.346	3.297

+ $p < 0.1$, * $p < 0.05$. Notes: We present the estimates of specifications 2.1 and 2.2 using as outcome the number of seats of the mayor's coalition. Panel A presents the results for all mayors (β) and Panel B interacting the treatment variable with dummies for first and second term (β_1,β_2). Each column restricts the sample for a given treatment probability window. Clustered standard errors at the election level are in parenthesis.

If the treatment is truly exogenous, we should not find difference in several other dimensions of the contemporaneous electoral variables. That is, they should be balanced across treated and control groups. We present this results in panel A of table 2.5, which has the same structure as table 2.4, except that the effect for the polled municipalities for each outcome is displayed in each of the rows. We can see that across all outcomes we dot not reject the null that treatment and control municipalities display the same average outcome. We also look at the average number of seats of the coalitions to which they

belonged in the previous election differ. If our treatment is random, this difference should be zero, which is what we see in panel B of table 2.5 below.

Table 2.5 – Placebo outcomes

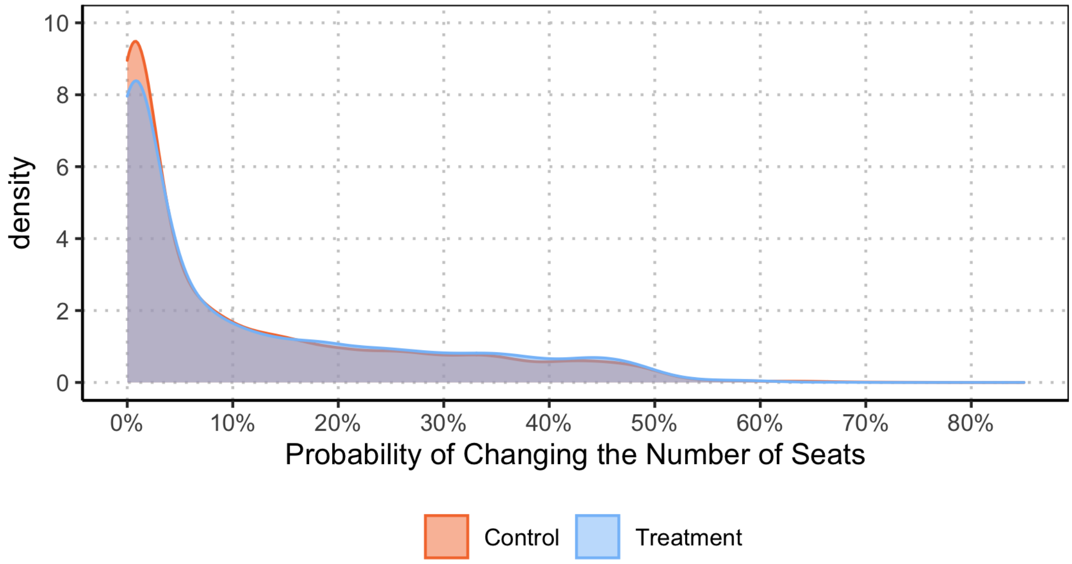
	(1)	(2)	(3)
Panel A. Contemporaneous Electoral Outcomes			
Total Seats	-0.027	0.006	-0.065
	(0.098)	(0.086)	(0.080)
Log-population	0.003	0.022	-0.051
	(0.067)	(0.057)	(0.049)
Total number of coalitions	0.035	0.074	-0.068
	(0.149)	(0.127)	(0.107)
# candidates in the mayor's coalition	-0.226	0.014	-0.088
	(0.301)	(0.258)	(0.225))
Mayor's coalition vote share	-0.003	0.000	0.007
	(0.010)	(0.009)	(0.007)
Mayor's vote share	-0.001	-0.001	0.001
	(0.009)	(0.007)	(0.006)
Panel B. Previous Election			
Coalition seats previous elections	-0.097	-0.023	-0.051
	(0.132)	(0.110)	(0.093)
Panel C. False D'Hondt Rules			
Over-estimating	-0.133	-0.070	-0.010
	(0.102)	(0.082)	(0.072)
Under-estimating	0.008	0.046	0.041
	(0.106)	(0.085)	(0.073)
Window	[0.40-0.60]	[0.35-0.65]	[0.30-0.70]

+ $p < 0.1$, * $p < 0.05$. Notes: We present the estimates of specification 2.1 for several outcomes (defined in the first column for each row). Each column restricts the sample for a given treatment probability window. Clustered standard errors at the election level are in parenthesis.

Lastly, we adapt false cutoffs of unidimensional running variables to our framework. We consider two "false D'Hondt" rules. Each of them creates a false assignment of seats and we define the set of false treatment and control groups in the same way we defined our real treatment and control groups. By design, when the mayor's coalition is on the verge of changing the number of seats in a false assignment rule, the coalition is far from changing the number of seats under the real rule. Therefore, we expect the average real number of seats of the mayors' coalitions in the false treatment group to be the same as in the false control group. The results are in panel B of table 2.5 below.

In figure 2.4 we show the density plots for treatment and control groups and we can see that they have the same pattern. In this figure we can also see that the majority of legislative elections are "stable" from the mayor coalition's point of view. That is, in most of the elections mayors' coalition have a small probability of changing the number of seats.

Figure 2.4 – Probability of the mayor's coalition changing the number of seats



Notes: The graph displays the density of the estimated probability of mayor's coalition changing the number of seats by treatment status. Treated municipalities are displayed in blue and control municipalities in orange.

In summary, in treated municipalities, an extra seat for mayor's coalition means a substantial increase (30%) in legislative support relative to control municipalities. We find no statistical difference in several variables related to or being generated in the same elections, suggesting that treatment and control groups are balanced. Moreover, we show how treatment and control are balanced, including in previous elections.

2.6.2 Main Results

Table 2.6 – Budget flexibility

	(1)	(2)	(3)
T	0.076*	0.079*	0.052*
	(0.030)	(0.025)	(0.022)
Window	[0.40-0.60]	[0.35-0.65]	[0.30-0.70]
N Obs	356	519	713
Control Mean	0.296	0.298	0.317

+ $p < 0.1$, * $p < 0.05$. Notes: We present the estimates of specification 2.1 using as outcome the budget flexibility, showing the estimate of β. Each column restricts the sample for a given treatment probability window. Clustered standard errors at the election level are in parenthesis.

Table 2.6 presents the estimates from equation 2.1 for the measure budget reallo-

cated by the mayor in a given fiscal year. We find a large and significant impact of an extra seat. Considering the TPW of $[0.35, 0.65]$, an extra seat for mayor's coalition increases the flexibility by 7.9 percentage points and the effect is highly statistically significant. The magnitude of this effect is close to the average budget flexibility per supporting councilor of 8.9 percentage points. The effect is similar if we consider alternative windows.

Therefore, a stronger legislative support translate into more discretionary power in budget management. The way mayors use this flexibility depends on their objective function. Mayors who maximize rent extraction are more likely to use it for taking advantage of mismanagement practices. By contrast, for mayors who seek to meet voters' demands, flexibility means a possibility for improving the initial budget proposal as new information becomes available. If this is the case, we should expect a stronger effect of more legislative support in the budget flexibility of first-term mayors, who are subject to electoral incentives.

Table 2.7 – Budget flexibility (II)

	(1)	(2)	(3)
T × First Term	0.056	0.053+	0.042+
	(0.034)	(0.029)	(0.026)
T × Second Term	0.154*	0.168*	0.079*
	(0.062)	(0.049)	(0.040))
Window	[0.40-0.60]	[0.35-0.65]	[0.30-0.70]
N Obs	356	519	713
Control Mean (1st term)	0.310	0.315	0.324
Control Mean (2nd term)	0.243	0.244	0.300

+ $p < 0.1$, * $p < 0.05$. Notes: We present the estimates of specification 2.2 for budget flexibility, showing treatment effect for first term mayors (β_1) and for second-term mayors (β_2). Each column restricts the sample for a given treatment probability window. Clustered standard errors at the election level are in parenthesis.

Table 2.7 presents the estimation of equation 2.2 for budget flexibility. We observe an asymmetry of the impact from an extra seat when we compare first- and second-term mayors. While treatment increases budget flexibility by 5.3 percentage points for mayors that can run for reelection (those in the first term), budget flexibility of treated second-term mayors increases by 16.8 percentage points. In percentage terms, these effects translate to 16.8% and to 68.8%, respectively for mayors in first and second term. The results are robust on changing the TPW. Therefore, this effect points to the direction that flexibility is used to ease the effort of rent extraction, following the previous argument.

Now we turn to impacts on revenue and spending. Table 2.8 reports the estimation of equation 2.2 for (log) total public revenues (columns 1, 2, and 3, for distinct TPW) and

Table 2.8 – Public revenue and spending

	log-Revenue			log-Spending		
	(1)	(2)	(3)	(4)	(5)	(6)
T × First Term	-0.058	-0.018	-0.017	-0.041	-0.011	-0.006
	(0.053)	(0.042)	(0.036)	(0.048)	(0.039)	(0.033)
T × Second Term	0.107+	0.071	0.122*	0.121+	0.078	0.116*
	(0.065)	(0.057)	(0.050)	(0.061)	(0.055)	(0.047)
Window	[0.40-0.60]	[0.35-0.65]	[0.30-0.70]	[0.40-0.60]	[0.35-0.65]	[0.30-0.70]
N Obs	842	1257	1742	842	1256	1742
Control Mean	7.909	7.903	7.894	7.848	7.842	7.832

+ $p < 0.1$, * $p < 0.05$. Notes: We present the estimates of specification 2.2, showing treatment effect for first term mayors (β_1) and for second-term mayors (β_2). Each column restricts the sample for a given treatment probability window. The first three columns show the results for log-Revenue and the last three for log-Spending. Clustered standard errors at the election level are in parenthesis.

(log) spending (columns 4, 5, and 6, for distinct TPW). The impact of an extra seat on both spending and revenues of first-term mayors is negative, but small (-1.8% and -1.1%) and statistically insignificant. By contrast, treated second-term mayors spend 7.1% more and also collect 7.8% more than second-term mayors in the control group. Although we cannot reject it to be statistically different than 0, the other TPWs, $[0.4, 0.6]$ and $[0.3, 0.7]$, yield slightly larger point estimates (more than 10% in both windows for revenues and spending) that are that are significant at 10% and 5% levels.

The results shown in Table 2.8 reinforce the previous conclusion that the differential discretionary power given by an extra councilor is not used to improve governance. Rather, it seems to be way for rent extraction. First, if more revenues were a way for improving the quality of policies, we would expect that more discretionary power would increase more revenues of mayors with electoral incentives. Second, larger expenses may facilitate the diversion of public funds through acts of mismanagement. [15]

As evidence in Ferraz e Finan (2008), Ferraz e Finan (2011a), and (AVIS; FERRAZ; FINAN, 2018) suggests, voters punish and reward politicians based on the perceived quality of government. Therefore, the results presented so far would be compatible with an extra seat allowing a relative best electoral performance for first-term mayors. Table 2.9 shows the estimation of equation 2.2 for the share of votes of the mayor's party in the next executive election, which we set as zero if the party does not run. An extra seat significantly increases the vote share by 3.0 percentage points for first-term mayors while

[15] We can use the same data on corruption as used by Avis, Ferraz e Finan (2018). However, given that the Brazilian authority inspects few selected municipalities each year and we need to restrict our analysis to places with competitive elections, we end up with a really small sample. For completeness, results are presented in appendix B.

Table 2.9 – Mayor's party vote share in the next election

	(1)	(2)	(3)
T × First Term	0.015	0.030+	0.038*
	(0.022)	(0.018)	(0.015)
T × Second Term	-0.034	-0.053+	-0.048*
	(0.034)	(0.028)	(0.023)
Window	[0.40-0.60]	[0.35-0.65]	[0.30-0.70]
N Obs	892	1324	1848
Control Mean	0.280	0.287	0.285

+ $p < 0.1$, * $p < 0.05$. Notes: We present the estimates of specification 2.2 with the outcome of mayor's party vote share in the next local election, showing treatment effect for first term mayors (β_1) and for second-term mayors (β_2). Each column restricts the sample for a given treatment probability window. Clustered standard errors at the election level are in parenthesis.

reducing the vote share by 5.3 percentage points in the TPW of $[0.35, 0.65]$. The signs of the point estimates are robust to other TPWs.

Table 2.10 – State courts of account recommending rejection of budget execution

	(1)	(2)	(3)
T × First Term	-0.029	-0.047	-0.045
	(0.054)	(0.048)	(0.041)
T × Second Term	0.085	0.052	0.071
	(0.097)	(0.089)	(0.077)
Window	[0.40-0.60]	[0.35-0.65]	[0.30-0.70]
N Obs	543	774	1108
Control Mean	0.254	0.279	0.295

+ $p < 0.1$, * $p < 0.05$. Notes: We present the estimates of specification 2.2 with the binary indicator if the state's courts of accounts recommended the rejection of the budget execution as outcome. The table shows the treatment effect for first term mayors (β_1) and for second-term mayors (β_2). Each column restricts the sample for a given treatment probability window. Clustered standard errors at the election level are in parenthesis.

Now, we turn ourselves to a more concrete evidence of an extra seat causing misconduct of mayors. Table 2.10 presents the estimation of equation 2.2 having as dependent variable the recommendation of the state's court of accounts. We set $y_{mt\tau} = 1$ whenever the court of accounts recommends the city council to reject the budget execution of the mayor elected in municipality m, election t and fiscal year τ. This recommendation could

be due to either disparities between the execution and the budget law or practices that do not conform with legislation.

Point estimates indicates that an extra seat reduces the rate of rejection recommendations of first-term mayors by 4.7 *p.p.* while it increases this rate by 5.7 *p.p.* for second-term mayors. The estimates are not statically significant, however, as mentioned in the data description, we have data on this outcome only for municipalities in the states of São Paulo, Bahia and Pernambuco. Given we are already restricting our sample to close elections, we are likely underpowered to reject the null hypothesis.

2.6.3 Discussion

To conclude we use our theoretical framework to discuss the main drivers of the empirical results. Our analysis can be summarized as follows. An extra seat gives more legal protection to mayors in both terms (Table 2.7), improves (resp. worsens) the electoral performance of first-term (resp. second-term) mayors' party (Table 2.9), and decreases the misconduct of first-term mayors (Tables 2.10 and B.1).

We discussed that if an extra seat caused better governance for second-term mayors, we would observe a better electoral performance for these mayors, opposed to our results. We restate this point in the light of the theoretical model. Our model allows for a positive or a negative impact of an extra seat on corruption in the second term, depending on the degree of cooperation of councilors. However, comparing our empirical results with the predictions of Table 2.1, our estimates inform that councilors in the mayor's coalition have a behavior of moderate degree of cooperation. Therefore, an extra seat causing increased mismanagement of second-term mayors is the only result consistent to our set of predictions.

Based in our theoretical framework, we now describe a possible set of mechanisms driving our results. We start from second-term mayors. As the mayor's coalition in the city council has an extra member, competition for diverted resources (through mismanagement) among them becomes fiercer. While they can offer more legal protection for the mayor's misconduct, the price they charge falls. Both forces push toward more extraction of rents. This yields a worse governance, which is punished by electors, who give fewer votes to the mayor's party. Now, we turn to first-term mayors. They are subject to all incentives of second-term mayors plus the electoral incentive. However, note that, with more support, legal protection and pecuniary gains become both relatively less important in the mayor's utility and the benefit for reelection becomes relatively more important (everything in marginal terms). The moderate degree of cooperation of councilors in the mayor's coalition gives the coalition relatively high bargaining power, making more advantageous for the mayor to invest in his reputation – i.e., increase his likelihood of winning the election–, which he does so by reducing the level of mismanagement.

2.7 Conclusion

We studied the effects of stronger legislative support for mayors on governance. To identify these effects we proposed a microfounded way to define proportional elections defined by slim margins. Using data from Brazil, we selected municipalities where the mayor's electoral coalition were close to winning/losing a seat in the city council. We find that more support gives mayors more flexibility in budget management, with a stronger effect for mayors with no reelection incentives. We discuss that this flexibility is associated with mismanagement. For mayors with reelection incentives we find a positive impact in their parties' share of votes in the subsequent election, but a negative effect for mayors in second term. This is consistent with evidence of increased mismanagement by mayors in second term. We interpret our findings in the framework of a simple model in which the cost for the legal protection that councilors are able to give to the mayor decreases with more support. Together, our results contribute to shed light on how political institutions are key for development.

3 Peer Effects in Active Learning

3.1 Introduction

Peer effects matter for performance in education.[1] To some extent, policies such as the provision of educational vouchers or ability tracking rely on the possibility that a student's performance could improve in a different group. From this perspective, welfare implications of peer group design are important and understanding how peer effects operate is crucial for thinking about alternatives. Several studies explored a model in which group average ability affects individual performance (*linear-in-means*), but it has two important drawbacks.[2] Any reallocation of students implemented to raise average ability for some group implies a decrease on some other group's average ability. This results in no aggregate effect (HOXBY, 2000). A more interesting scenario is when there are *nonlinear* peer effects. That is, when peers can have different impacts on a student's performance depending on both students and peers positions in the ability distribution (HOXBY; WEINGARTH, 2005). Then, regrouping policies may have some room to improve overall efficiency. But even under the simple *linear-in-means* model the identification of peer effects arising from peers' equilibrium behavior, a key parameter for optimal group design, is both theoretically and empirically challenging (MANSKI, 1993; GOLDSMITH-PINKHAM; IMBENS, 2013; LEE, 2007; BRAMOULLÉ; DJEBBARI; FORTIN, 2009).

In this paper, we estimate nonlinear peer effects in higher education by exploring random variation not only in group composition, but also in the frequency that peers meet for group work. We do not intend to identify the endogenous component of peer effects. However, in our setting it is possible to obtain reduced-form estimates more informative about the potential effects of increased interaction among peers. We perform our analysis using data from the undergraduate course in economics taught at São Paulo School of Economics. The school adopts an *Active Learning* methodology in which peer interaction is at the core of a student's work. In various disciplines, students are randomly assigned to tutorial sessions in which they have to work together in a problem solving context. The identification of peer effects relies on the random assignment of students to these sessions. Besides the random variation of peer ability across groups we also have random variation in the number of disciplines in which peers work together. We found negative peer effects from increasing the number of high ability peers that a student has in her group. Different papers also found this negative effect (FELD; ZÖLITZ, 2017; BRADY;

[1] Sacerdote (2011) and Epple e Romano (2011) are surveys on theoretical and empirical aspects of the literature.

[2] Sacerdote (2001), Zimmerman (2003), Hanushek et al. (2003) are examples.

INSLER; RAHMAN, 2017), but we further show that more intense interaction among peers can generate positive peer effects from high ability peers on low ability students.

In our analysis, students and peers are classified in three levels of ability: low, mid and high (we call peer's *type*). The baseline result show that replacing a mid ability peer by a high ability one can be detrimental to performance, on average. For low ability students, this negative effect can be as large as a decrease of 9.8% of a standard deviation. Estimates are obtained by comparing students randomly allocated to different groups of the same discipline that differ by the random number of peers by type allocated to each one.

However, it was a positive estimated peer effect from high ability peers on low ability students that motivated Carrell, Sacerdote e West (2013) to form groups that end up being detrimental to low students' performance. They designed experimental groups that ended up maximizing the number of high ability peers with which low ability students could interact. Though, they found that low and high ability peers did not interact as expected due to endogenous sorting of peers within the new groups and it was not clear from the positive reduced-form estimates.

With that in mind, we investigate what happens when pairs of students can interact in more than one group. Since we can observe students in more than one discipline, we can distinguish pairs of students by the frequency of interaction between them. First, we show evidence that when peers meet in more than one group, low ability students are 33% more likely to report high ability peers as relevant components of their groups. Then, we extend the above comparison between students across groups of different composition to use random variation in the fraction of peers that also appear in some other groups. That is, peers that are more likely to be relevant to the student. In this exercise we find that performance of low ability students would increase by 5.7% of a standard deviation if all of their high peers were also peers in some other disciplines, compared to the actual allocation in which only half of them, on average, interact in some other group. We do not find evidence that positive effects from increased interaction on performance could be explained by better group functioning. However, we find that increasing the fraction of high ability peers with more opportunities to interact in the group makes students of any level of ability to allocate a share of time approximately 5% greater to that discipline .

Our paper contribute to an empirical literature discussing peer effects as subsidy for the design of optimal groups (CARRELL; SACERDOTE; WEST, 2013; BOOIJ; LEUVEN; OOSTERBEEK, 2017; GARLICK, 2018). Two studies are close to ours in different dimensions. Feld e Zölitz (2017) analyze students interacting within an active learning framework, similar to what we observe at São Paulo School of Economics. In this context, peer interaction is an explicit requirement for good performance and so peer effects should be a meaningful mechanism determining it. In terms of the analysis we do, the paper by

Brady, Insler e Rahman (2017) using data from the U.S. Navy Academy is the most similar. All students are observed in two kinds groups with different sizes and purposes. They identify negative peer effects from variation on peer ability across groups at a broader level (companies). Then, by looking to smaller and more task-oriented groups (courses) they find that positive peer effects exist due to peers in these groups that are also peers in the broader group. This is a result qualitatively similar to ours as it highlights that interaction potentially stronger seems to matter in explaining peer effects. Thus, our paper analyze peer effects in an environment with high returns for peer interaction and at the same time we can explore an experimental allocation of students ensuring variation in both group composition and the potential strength of peer interaction.

The next two sections presents the organizational framework and the assignment mechanism of students. Then we present the data and discuss the implementation of our empirical strategy. Finally, we then present results, discuss some robustness checks and make some final remarks in the conclusion.

3.2 Organizational Framework

São Paulo School of Economics (FGV EESP) is a distinguished higher education institution in Brazil. From 2003 to 2016, the school admitted to the undergraduate course in economics up to 60 students by year from a highly selective admission exam done by around 1,500 candidates. From 2017 to 2021 the number of students admitted each year gradually increased and now the school accepts up to 120 students. The annual course fee for 2019 was approximately R$ 65,000, which was roughly 2.4 times the Brazilian per capita income calculated for that year.[3] This implies that EESP's students are on the top of country's ability and income distribution.

In 2013, the school began to replace the traditional teaching method based on lectures for a model of *Active Learning*. Students admitted from that year on have been doing all of their coursework under the *Problem-based Learning* method. Problem-based Learning (PBL) is a pedagogical strategy in which learning evolves in a problem-solving framework. A central element to this method is the goal of engaging students in their own learning process by working with concrete problems in small groups. In FGV EESP's, each student in a given discipline is allocated to a group of 12 students, on average, that will work together in sessions of two hours. In different sessions of the same discipline, group members are the same. However, across different disciplines a given student has different pools of classmates in her groups. The total number of sessions vary by discipline.

All of these sessions occur under the supervision of a tutor and each session has two parts. The *pre-discussion* part is when the problem is presented to the group without

[3]PNAD (2019)

any explicit requirement to work on the topic. The only available tools for students at the time are what they have learned up to that point. In this phase, a student is chosen to be the leader of the session. She organizes the discussion so that the group manage to bring about all the problem's latent questions. Besides, another student organizes the contributions of each student into a document made available to the group at the end of the session. Here, tutor's role is to ensure that all learning goals are clear to the group. Ideally, this should be done with minimal interventions of the tutor.

Pre-discussion happens at the end of a session, then, the *post-discussion* part happens at the beginning of next session. This way students have an interval of one day or more to make use of the pre-discussion document together with bibliographic references to get prepared for the post-discussion. This time, students should answer all issues raised from the problem by using concepts and tools they have learned from self or group study outside class. Group study in this case is not restricted to happen among students of the same tutorial group.

In the post-discussion, besides helping the session's leader in organizing the discussion, the tutor must guide students so they develop a comprehensive understanding of the topic under debate. If there is available time, he also must give additional information on the topic. Once students conclude the task, tutor must evaluate individual performance and give appropriate feedback so that students know exactly how they performed. That is, in each session each student receives a grade between zero and one. At the end of the period, the mean of these tutorial grades enters the discipline's final grade as a multiplicative factor over some combination of marks from other examinations. This way, the better the performance in group work, the better the overall performance. This provides a strong incentive for students to show up (absence implies zero) and to really participate during tutorial sessions.

From 2013 to 2017, group assignment followed a deterministic rule based on the admission ranking that, despite exogenous, implied small variation on group ability and basically repeated the same group of students across different disciplines. In 2018, to understand the role of group composition in performance and possibly redesign group assignment rules, the school allowed us to carry an experimental allocation of students in the period 2018-2019. This is what we will explore in our empirical strategy.

3.3 The Assignment Mechanism

The algorithm designed to allocate students into the tutorial groups we analyze generated random variation across groups in two dimensions. First, it created a support with large variation in terms of group composition defined as the proportions of each type (low, mid, high) in the group. Besides, as we observe students in more than on discipline,

some pairs of students meet weekly in only one group of a single discipline while others were randomly allocated to meet in groups of more than one discipline.

We placed new students of the 2018 and 2019 cohorts in their first semester at the school. During this semester, they took six mandatory courses and in every discipline each student were assigned to a tutorial group with 12 students on average. The final output of the algorithm for allocating students in a given discipline can be summarized in table 3.1 (a fictious example). The cells in the table contain the number of students in the group by ability level (blue) and who are those students (black).[4] To produce this allocation we followed these steps:

	Low Ability	Mid Ability	High Ability	Group size
A	4 33 34 35 37	3 17 21 24	5 4 10 11 12 13	12
B	8 36 39 40 41 42 42 44 47 48	2 18 32	2 3 5	12
C	1 43	6 20 22 25 28 29 30	5 2 6 8 15 16	12
D	3 38 45 46	5 19 23 26 27 31	4 1 7 9 14	12
	16	16	16	

Notes: Fictious example of the allocation procedure to put 48 students in four groups of a given discipline. Blue numbers in each cell are the number of peers from the type indicated in column's title. Conditional on group composition (blue numbers), students numbered from 1 to 48 according to their ranking in the admission exam are drawn to fill each position according to their type. The outcome of these draws are represented by the small black numbers in each cell. Thus, low students 33, 34, 35 and 37 were select to be in group A.

Table 3.1 – Group Allocation 1st Semester

- Step 1) Each student is classified as low, mid, or high ability type according to his/her position in the admission exam. This was the consistent info about ability that we always had at the time of the allocation.

- Step 2) Then the algorithm randomly chooses how many students of each type will be allocated in each group. Example: Group A of Econ 101 will have 4 low, 3 mid and 5 high students. We run this lottery without replacement in order to minimize the chance that two groups have the same composition and thus increase variation across groups. The restrictions for this step are group size and the quantity of students by type.

[4]Strictly speaking, the allocation in 2019 was done conditional on a time class rather than on a discipline. That is, if at 9 a.m. there were groups of Mathematics I and Econ 101 the allocation described here produced all the groups for the 9 a.m. class hour.

- Step 3) Finally it randomly selects the students that will be in each group. That is, if there is a total of 20 low ability students and the previous step defined that some group must have 3 of them, this step defines who are these three students.

For a given student, this procedure results in two possible sources of random variation to explore: the number of peers by type across groups (see figure 3.1) and the identity of these peers. For instance, suppose that Joe, a high ability student, has 3 low ability peers in group A of Econ 101 and 6 low ability students in group B of Math I. The algorithm could have given him 2 low ability students in Math I by placing him in a different group. Besides, the same mechanism that placed Joe in these groups also selected who are the 3 and 6 low ability peers in the same groups. It could be the case that some of these peers are in both groups while some do not and this also would have happened by chance.

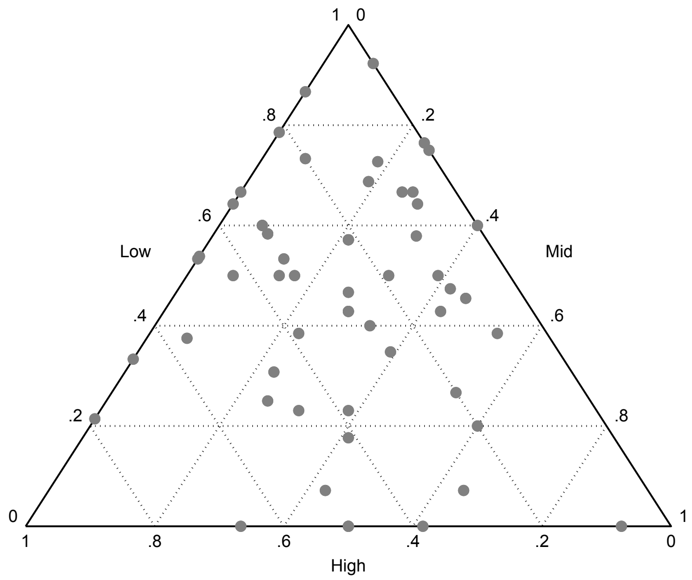


Figure 3.1 – Random variation generated in terms of group composition. Each point represents a group and its place within the simplex is given by the proportions of each type in the group.

Regarding the frequency with which pairs of students meet, table 3.2 shows that among the entrant students there were almost 10 thousand possible pairs that could be formed. However 55% of these pairs do not interact in any discipline and, conditional on meeting in at least one discipline, one third of the pairs interact in at least another group.

A final remark is about the workflow to implement the allocation. After doing the above procedure we provided to the school staff a list indicating the groups of each student. Between this information and the beginning of classes some students usually accepted offers from other institutions before knowing their allocation. These are usually high ability peers that end up being replaced by low ability students. For those who

Table 3.2 – Number of meetings by pair of students

Meetings	No. of pairs	Freq.
0	5,390	55.0%
1	2,897	29.6%
2	1,099	11.2%
≥3	416	4.2%
Total	9,802	

Notes: From all possible pair of students, 55% never meet, 29.6% meet in one group and 15.4% meet in more than one group.

actually started the course there were perfect compliance with the allocation. One further important aspect is that tutors were assigned to groups independently and in advance of the students assignment.

3.4 Data

Our analysis use administrative data from the first semester of 2018 and 2019 academic years. All variables in our data are constructed from information on: (i) students performance in the admission exam and in the discipline they take; (ii) the schedule of disciplines; (iii) the allocation of students implemented according to our guidelines. In this section we define the variables we use, present descriptive statistics and also show results on balance checks.

3.4.1 Variables

Academic performance

The final measure of performance in a given discipline may take into account different kinds of activities. Each discipline combines exams, problem sets or other evaluations with varying weights. To have a more homogeneous measure of performance we use scores of the first exam taken in each discipline (standardized by discipline). Students take all exams during a week two months after the beginning of the course. We did not use the final exam or final GPA to avoid potential endogeneity arising from students adjusting behavior after knowing their first exam. Besides, there is a discipline (Probability) that starts only after the first exam. So we use students data from five discipline only.

Students classification

We classify students according to what we call *predicted GPA*. This results from regressing first exam scores of each discipline on admission test scores (math, writinig, history, biology, etc.) for cohort t and then use the estimates to predict the scores for the cohort $t+1$. The predicted GPA here means a weighted average of the predicted scores. Then, for each student we construct variables indicating whether he belongs to top/botom 25% or mid 50% of his cohort's $\widehat{GPA}$ distribution. This is what defines *low*, *mid* and *high* ability students in the analysis. All data we used in this classification also consider exogenous group composition.

Peers classification

This time students are classified according to their predetermined ability in writing as measured by the admission exam.[5] *Low*, *mid* and *high* are defined based on the ability distribution in a way analogous to what we did for students GPA.

Number of peers

For a given student, this variable simply counts the number of other students in the group falling in each ability category (low, mid, high). There are two types of students we count separately: those doing a discipline for a second time and few that did not use school's admision exam. These types were randomly allocated in the group and appear as controls in the main regressions.

Peers interaction

As discussed before, some pairs of students take more than one discipline together. Thus, for a given student, we identify how many peers in her group are also present in other groups. This is done for each ability category from the previous variable. Then we construct a variable containing the fraction of peers in each category that are also peers in a different group. That is, suppose a student has 4 low students in Math I and 2 of them are also peers in another course. Then, the value for this variable is 50%. In our results, this is the variable we label as "*% repeated*".

Info from survey

We applied a survey to students to get information regarding their perception about the groups in which they worked. We asked questions about the learning environment, motivation and peer teaching. This is an ordinal variable ranging from 0 to

[5] Here we follow the literature that find peer effects mainly through peers verbal ability. None of the results we will present hold if we classify peers by their math ability.

5 indicating how many peers contributed to some group attribute. The value 0 means none of the peers and the value 5 means all peers. Else, students name the most relevant peers for group work. We use this information to construct a variable at the dyad level indicating whether student i reported peer j as a relevant peer in the group. About two thirds of the students answered the questionnaire and table C.1 in the appendix shows that answering is not related to ability.

3.4.2 Balance checks and descriptive statistics

To check that the assignment mechanism was indeed random we ran regressions of some predetermined ability variables on group variables plus randomization controls. Group variables include total number of peers (*Number of peers*) by type and how many of those peers are also peers in some other groups (*Number of repeated peers*). Table 3.3 shows that coefficients on variables of groups composition are usually close to zero and for only 4 out of 48 estimates (aside from controls) we reject the null of no effect at usual levels of significance.

Table 3.3 – Predetermined Ability on Group Composition

	Final Score	Math	Writing	Low $\widehat{GPA}$	Mid $\widehat{GPA}$	High $\widehat{GPA}$
Number of peers						
Low Peers	-0.00	-0.01	-0.01	0.01	-0.02^{+}	0.01
	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Mid Peers	0.00	0.02	-0.02	-0.00	0.00	0.00
	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
High Peers	-0.01	0.01	-0.00	-0.00	-0.00	0.01
	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Other Peers	0.01	0.02	-0.02	-0.00	-0.01	0.01
	(0.02)	(0.04)	(0.05)	(0.02)	(0.02)	(0.02)
Retained Peers	0.07	0.10	-0.01	-0.05	-0.02	0.08^{*}
	(0.04)	(0.08)	(0.10)	(0.04)	(0.04)	(0.04)
Number of repeated peers						
Low Peers	-0.00	-0.01	0.01	-0.03	0.03	-0.00
	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)	(0.01)
Mid Peers	-0.01	-0.02	-0.07^{*}	0.01	-0.02	0.01
	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
High Peers	0.00	0.01	-0.01	-0.03^{+}	0.02	0.01
	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)	(0.02)
Students	174	174	174	174	174	174
Observations	827	827	827	827	827	827

Randomization controls included. Robust standard errors in parentheses.
$^{+}$ $p < 0.1$, * $p < 0.05$

While table 3.3 shows that initially 174 students were assigned to the various

groups, resulting in 827 observations, table 3.4 shows that we actually observe 135 students in 56 groups, resulting in a total of 660 observations. These are the students that effectively started the course and took at least the first exam. The median group is composed by 3 low ability students, 6 mid ability students and 3 low ability students. Also, students have in their group 55% of the peers appearing in at least some other group.

Table 3.4 – Descriptive statistics

Variables	Mean	SD	Min	Median	Max	N
Student's own Ability						
Writing	5.59	0.78	3.21	5.93	6.77	135
Math	5.58	0.97	3.72	5.53	7.90	135
Final Score	5.29	0.55	4.18	5.22	6.71	135
Number of peers by group						
Low	2.61	1.30	0	3	5	56
Mid	6.25	2.04	2	6	12	56
High	2.93	1.62	0	3	7	56
Group size	11.79	2.22	8	12	16	56
Fraction of repeated peers						
Low	0.55	0.35	0	0.57	1	660
Mid	0.56	0.21	0	0.57	1	660
High	0.53	0.34	0	0.50	1	660
Grade	7.11	1.85	0	7.20	10	660

Notes: *Own ability* refers to to the the admission exam ability measures and ranges from 0 to 10. *Number of peers* counts the number of students in each groups considering their classification according to the writing ability. *Fraction of peers* contains the proportion of peers in a group that also appear in some other group.

3.5 Empirical Strategy

The empirical strategy of the paper explores the random allocation that put students into tutorial sessions of different disciplines. As discussed previously, randomization is done at the discipline level and our main results compare students within a given discipline by using random variation of peer-related variables across groups and/or across students as sources of identification. For an outcome of interest y_{sd} of student s in discipline d, we estimate peer effects based on the equation

$$y_{sd} = \mathbf{x}'_{sd}\boldsymbol{\beta} + \mathbf{w}'_{sd}\boldsymbol{\lambda} + \eta_d + u_{sd} \tag{3.1}$$

In our exercises, $\mathbf{x}_{sd}$ is a vector of some peer-related variables (e.g., the number of low/high peers). Vector $\mathbf{w}_{sd}$ controls for student's ability type and cohort (randomizaton controls.) All regressions use the number of meetings in a given discipline as weights for observations of that discipline. We report results including a vector $\mathbf{z}_{sd}$ in equation 3.1

to control for peers in the group not classified by ability, peers from past cohorts, group size and student's ability measures. This vector $\mathbf{z}_{sd}$ is used to increase precision and give appropriate interpretation for $\boldsymbol{\beta}$ since these variables play no role in ensuring exogeneity. Given the assignment mechanism, the identification hypothesis can be stated as

$$E[u_{sd}|\mathbf{x}_{sd}, \mathbf{w}_{sd}, \eta_d] = E[u_{sd}|\mathbf{w}_{sd}, \eta_d] \tag{3.2}$$

That is, when comparing students of the same discipline, cohort and ability level, mean unobservables do not depend on peer-related variables.[6] In our main results, we cannot control for group fixed-effects since we must use variation of peers across groups. Thus, it is possible that unobservables are correlated for the same student across groups or for different students in the same group due to common shocks. Then, for inference purposes we acknowledge that for students i and j in groups g and h

$$E[u_{ig}u_{jh}|\mathbf{x}_{ig}, \mathbf{x}_{jh}] \neq 0 \tag{3.3}$$

if $g = h$ or $i = j$. We deal with this issue by estimating a two-way clustered standard error at student and group levels following Cameron, Gelbach e Miller (2011)[7]. Besides, for some estimates of interest we report an inference assessment as proposed by Ferman (2019).

3.6 Results

3.6.1 Nonlinear peer effects

The first set of results is based on an empirical exercise also done in other papers. Regressions are based on the following version of equation 3.1:

$$\text{StdExam}_{sd} = \beta^L \text{LowPeers}_{sd} + \beta^H \text{HighPeers}_{sd} + \mathbf{w}'_{sd}\boldsymbol{\lambda} + \mathbf{z}'_{sd}\boldsymbol{\theta} + \eta_d + u_{sd} \tag{3.4}$$

In this equation, LowPeers_{sd} and HighPeers_{sd} are the numbers of low and high ability peers a student s has in discipline d. Note that the number of mid ability students is excluded. Thus conditional on a given discipline, the coefficients β^L and β^H give the effect of replacing a mid ability peer by a low or high ability peer, respectively. Estimates are obtained by comparing students of the same ability level across groups of the same discipline. Coefficients are reported in table 3.5 where we consider the number of peers classified by writing ability.

[6] As robustness exercises, we report results using within-student variation.

[7] The estimator is implemented through Stata's user-written command `reghdfe`. See (CORREIA, 2016).

Table 3.5 – Peer Effects on Standardized Exam Grade

	(1)	(2)	(3)	(4)
Peers by Writing Ability				
Low Peers	-0.020	-0.031		
	(0.043)	(0.039)		
High Peers	-0.054	-0.061^{+}		
	(0.037)	(0.034)		
Low $\widehat{GPA}$ × Low Peers			-0.034	-0.021
			(0.053)	(0.051)
Low $\widehat{GPA}$ × High Peers			-0.098^{+}	-0.076
			(0.051)	(0.048)
Mid $\widehat{GPA}$ × Low Peers			-0.059	-0.070^{+}
			(0.044)	(0.041)
Mid $\widehat{GPA}$ × High Peers			-0.036	-0.041
			(0.035)	(0.033)
High $\widehat{GPA}$ × Low Peers			0.062	0.032
			(0.063)	(0.062)
High $\widehat{GPA}$ × High Peers			-0.077	-0.101^{*}
			(0.047)	(0.043)
Student Ability Controls	No	Yes	No	Yes
Students	135	135	135	135
Groups	56	56	56	56
Obs.	660	660	660	660

Standard errors clustered by group and student in parentheses.
$^{+}$ $p < 0.1$, * $p < 0.05$

With controls included (column 2), results suggest that reducing the number of mid writing ability peers of one's group implies a decrease of 6.1% of a standard deviation when the new peer is a high writing ability level one. The effect is significant at 10%, and we do not reject zero effect for increasing low peers in the group. However, the inference assessment indicates that instead of 10% the actual rejection rate was 17.8%, indicating a slightly over-rejection. So we view this result with cautious.

But it is interesting to know if these effects depend on students' own ability as it is an important aspect to analyze if one is interested in redesigning groups to improve overall performance. This is what columns (3) and (4) report with estimates on interactions of LowPeers$_{sd}$ and HighPeers$_{sd}$ with indicators of student's predicted GPA level.

Estimates in table 3.5 suggest that peer effects are heterogeneous across students of different ability levels. The previous negative point estimates for the effects of high

writing ability peers seems to result mostly from effects at high predicted GPA students. The replacement of a mid peer by high peer in this case decreases performance, on average, by 10.1% of a SD, significant at 5%.[8] In the median group, a high GPA student has 3 low, 6 mid and 2 high ability peers. So this effect means moving this student to a group with 3 low, 5 mid and 4 high ability peers. For low predicted GPA students, point estimates suggest a negative effect of an analogous change and for mid predicted GPA students it is the increase of low peers in the group that seem to impact negatively their performance.

However, there may be different potential explanations for the heterogeneity of the effects. Negative peer effects from high ability peers on high ability students could be explained by a mechanisms in which individual comparisons play some role in performance. Recent evidence shows, for example, that rank concerns seems to matter (ELSNER; ISPHORDING, 2017; TINCANI, 2017). But we concentrate here on the possibility that heterogeneous effects could provide some guidance for a regrouping policy that can improve performance overall.

It was an estimated positive spillover from high ability peers on low ability students from a specification like 3.4 that motivated Carrell, Sacerdote e West (2013) to redesign groups that end up being detrimental to low students' performance. They found that low and high ability peers did not interact as expected due to endogenous sorting of peers not identified by reduced-form estimates. But in a context very similar to ours, Feld e Zölitz (2017) also find negative effect of high peers on low ability students' performance.

As these results suggest, by estimating peer effects from random variation of group composition one could not isolate the impacts it may have on the dynamics of peer interaction. But our data allows us to test an hypothesis in this direction. What we do next is to investigate whether high peers could benefit low ability students' performance had they the opportunity to strengthen their interaction *conditional* on group composition.

3.6.2 Effects of repeated interaction on *actual* interaction

The next set of results show evidence that when two peers meet in more than one discipline (we call it *repeated interaction*) the probability that some of them report the other as a relevant peer in some group increases. To do so we take all survey respondents and form every possible pair by combining each respondent s to each respondent peer p in discipline d. We define an indicator variable I_{sp} which equals one when s and p were allocated in more than one discipline. The dependent variable report_{spd} indicates whether student s reported peer p as a relevant peer in discipline d. Then we run the following

[8] The assessment indicates an actual rejection rate of 11%.

regression

$$\text{ReportPeer}_{spd} = \beta I_{sp} + c_i + \eta_d + u_{spd} \tag{3.5}$$

For a given student, within a discipline, the estimate for β gives the average effect of the repeated interaction on the probability of reporting a peer as relevant. As the error term may be correlated both across s and p, we calculate standard errors clustering by student and peer (CAMERON; GELBACH; MILLER, 2011). Table 3.6 show results for estimates of equation 3.5 and for extended versions of it by making the estimates of β to vary by student and peer ability levels.

Estimates show that repeated interaction implies a huge increase in the probability that a low student name a high peer as relevant peer in the group. Conditional on both student and peer answering the questionnaire, the proportion of high peer with a single interaction that low student reported as a peer was 38%. The estimate of 0.138 in column 3 means an increase of 36% in this probability. Thus, we take this figure as evidence that low students could strengthen their interaction with high peers they meet more frequently.

3.6.3 Effects of repeated interaction on performance

To analyze what happens to performance when the student have peers with which she meets more frequently we modify a bit the equation we estimate to improve inference. We now run

$$\begin{aligned}\text{StdExam}_{sgd} = &\beta^L \text{LowPeers}_{sgd} + \beta^H \text{HighPeers}_{sgd} \\ &+ \gamma^L (\text{LowPeers}_{sgd} \times \text{FractionLow}_{sgd}) \\ &+ \gamma^M (\text{MidPeers}_{sgd} \times \text{FractionMid}_{sgd}) \\ &+ \gamma^H (\text{HighPeers}_{sgd} \times \text{FractionHigh}_{sgd}) \\ &+ \mathbf{w}'_{sgd}\boldsymbol{\lambda} + \mathbf{z}'_{sgd}\boldsymbol{\theta} + \eta_d + \xi_g + u_{sd}\end{aligned} \tag{3.6}$$

Variables labelled with *Fraction* are the proportion of the respective type of peer that appear in at least some other discipline with student s. For example, FractionLow_{ssd} is the proportion of low peers the student s has in group d of discipline d that are also peers in some other discipline. Then, conditional on the group, within a discipline and relative to a situation in which all low peers only interact with student s in group g of discipline d, the estimate of γ^L gives the average effect of replacing all these peers by other peers that interact with s in at least some other group. Since now there is variation of *Fraction* within the group, we can include a group fixed-effect to take account of common shocks to students of the same group. This time, standard errors are clustered only at student level.

Table 3.6 – Effect of repeated interaction on peer reporting

	(1) Reported Peer	(2) Reported Peer	(3) Reported Peer
(Interaction > 1)	0.034*		
	(0.016)		
(Interaction > 1) × Low Stud		0.065*	
		(0.029)	
(Interaction > 1) × Mid Stud		0.022	
		(0.024)	
(Interaction > 1) × High Stud		0.030	
		(0.022)	
(Interaction > 1) × Low Stud × Low Peer			0.041
			(0.047)
(Interaction > 1) × Low Stud × Mid Peer			0.044
			(0.030)
(Interaction > 1) × Low Stud × High Peer			0.138*
			(0.056)
(Interaction > 1) × Mid Stud × Low Peer			-0.010
			(0.033)
(Interaction > 1) × Mid Stud × Mid Peer			0.017
			(0.028)
(Interaction > 1) × Mid Stud × High Peer			0.065
			(0.040)
(Interaction > 1) × High Stud × Low Peer			0.019
			(0.032)
(Interaction > 1) × High Stud × Mid Peer			0.038
			(0.027)
(Interaction > 1) × High Stud × High Peer			0.022
			(0.046)
Constant	0.433*	0.433*	0.410*
	(0.051)	(0.051)	(0.058)
Observations	6086	6086	6086

Two-way clustered SE by student and peer in parentheses following Cameron, Gelbach e Miller (2011).
$^{+}$ $p < 0.1$, * $p < 0.05$

Table 3.7 – Peer Effects on Standardized Exam Grade

	(1)	(2)	(3)	(4)
Low Peers	0.147	0.192	0.157	0.212
	(0.133)	(0.213)	(0.130)	(0.211)
High Peers	-0.151	-0.122	-0.136	-0.139
	(0.137)	(0.188)	(0.135)	(0.185)
Low Peers × % of Repeated Low	-0.027	-0.023		
	(0.058)	(0.058)		
Mid Peers × % of Repeated Mid	-0.007	0.003		
	(0.035)	(0.035)		
High Peers × % of Repeated High	0.052	0.049		
	(0.044)	(0.044)		
Low $\widehat{GPA}$ × Low Peers × % of Repeated Low			-0.036	-0.016
			(0.098)	(0.099)
Mid $\widehat{GPA}$ × Low Peers × % of Repeated Low			-0.044	-0.028
			(0.069)	(0.070)
High $\widehat{GPA}$ × Low Peers × % of Repeated Low			-0.021	-0.032
			(0.083)	(0.084)
Low $\widehat{GPA}$ × Mid Peers × % of Repeated Mid			-0.068	-0.047
			(0.046)	(0.048)
Mid $\widehat{GPA}$ × Mid Peers × % of Repeated Mid			0.014	0.022
			(0.038)	(0.037)
High $\widehat{GPA}$ × Mid Peers × % of Repeated Mid			0.053	0.049
			(0.051)	(0.049)
Low $\widehat{GPA}$ × High Peers × % of Repeated High			0.123^{+}	0.162^{*}
			(0.065)	(0.066)
Mid $\widehat{GPA}$ × High Peers × % of Repeated High			0.041	0.033
			(0.053)	(0.055)
High $\widehat{GPA}$ × High Peers × % of Repeated High			-0.017	-0.038
			(0.068)	(0.063)
Ability Controls	No	Yes	No	Yes
Students	135	135	135	135
Groups	56	56	56	56
Observations	660	660	660	660

Standard errors clustered by students in parentheses.

$^{+}$ $p < 0.1$, * $p < 0.05$

Results on columns 1 and 2 of table 3.7 show that, conditional on the group, point estimate suggest that an increase in the fraction of high writing ability peers with repeated interaction have an average positive effect on performance. The average fraction of repeated high peers is 53% across groups. The estimate on column 1 means that replacing the non repeated high peers would increase performance, on average, by $0.47 \times 0.052 = 0.024$ of a SD. But what columns 3 and 4 show is that this effects comes basically from the positive impact on low ability students. For this subgroup of students, replacing the non repeated high peers by others that also interact somewhere else would raise performance by $0.47 \times 0.123 = 0.057$ of a SD. The inference assessment for this test now indicates a rejection rate of 11.8%, close to what would be expected.

Using information about student's time allocation from the survey we construct a variable indicating the share of time spent in studying for discipline d. Table 3.8 shows results using this variable as the outcome in equation 3.6. Interacting with repeated high ability peers leads students to allocate more time in that discipline. The effect of more interaction with high peer seems to be driven by impacts on low and high ability students. The magnitude of the interaction coefficient for high peers in columns 3 and 4 means a positive effect of 5% compared to an average share of 20% of the time spent in each discipline. Then, it could be the case that this increase in time allocated to the discipline explain the positive impact of this type of high peers on the performance of low ability students. For these students, the cost in terms of time could be justified by the returns of such a connection with more skilled peers. We investigate other aspects we collected in the survey, but none of them seem to explain the positive spillover from high peers to low students. For completeness, this is reported in the appendix.

3.7 Robustness checks

We report in the appendix the same set of results but using a different identification hypothesis. Since allocation across disciplines are independent, we identify peer effects using the variation of peer-related variables for the same student across disciplines. We estimate the effects by modifying equation 3.1 to run

$$y_{sd} = \mathbf{x}'_{sd}\boldsymbol{\beta} + \mathbf{w}'_{sd}\boldsymbol{\lambda} + c_i + u_{sd} \tag{3.7}$$

with the corresponding hypothesis that

$$E[u_{sd}|\mathbf{x}_s, \mathbf{w}_s, c_i] = E[u_{sd}|\mathbf{x}_{sd}, \mathbf{w}_{sd}, c_i] \tag{3.8}$$

Estimates for β are qualitatively similar and actually larger in magnitude. However, we view this result with caution. Hypothesis 3.8 says that u_{sd} should not be related to $\mathbf{x}_{sd'}$

Table 3.8 – Peer Effects on Time Allocation (Share by discipline)

	(1)	(2)	(3)	(4)
Low Peers	-0.011^{+}	-0.013	-0.011^{+}	-0.012
	(0.005)	(0.012)	(0.006)	(0.012)
High Peers	-0.012^{+}	-0.010	-0.010	-0.009
	(0.007)	(0.009)	(0.007)	(0.009)
Low Peers × % of Repeated Low	0.008	0.008		
	(0.005)	(0.005)		
Mid Peers × % of Repeated Mid	-0.008*	-0.009*		
	(0.003)	(0.003)		
High Peers × % of Repeated High	0.010^{+}	0.010^{+}		
	(0.005)	(0.005)		
Low $\widehat{GPA}$ × Low Peers × % of Repeated Low			-0.005	-0.005
			(0.007)	(0.007)
Mid $\widehat{GPA}$ × Low Peers × % of Repeated Low			0.012*	0.012*
			(0.005)	(0.005)
High $\widehat{GPA}$ × Low Peers × % of Repeated Low			0.007	0.007
			(0.006)	(0.006)
Low $\widehat{GPA}$ × Mid Peers × % of Repeated Mid			-0.005	-0.006
			(0.005)	(0.005)
Mid $\widehat{GPA}$ × Mid Peers × % of Repeated Mid			-0.009*	-0.009*
			(0.003)	(0.003)
High $\widehat{GPA}$ × Mid Peers × % of Repeated Mid			-0.011*	-0.011*
			(0.005)	(0.005)
Low $\widehat{GPA}$ × High Peers × % of Repeated High			0.011*	0.011^{+}
			(0.005)	(0.006)
Mid $\widehat{GPA}$ × High Peers × % of Repeated High			0.006	0.006
			(0.006)	(0.006)
High $\widehat{GPA}$ × High Peers × % of Repeated High			0.014^{+}	0.015^{+}
			(0.007)	(0.008)
Ability Controls	No	Yes	No	Yes
Students	86	86	86	86
Groups	56	56	56	56
Observations	429	429	429	429

Standard errors clustered by students in parentheses.

$^{+}$ $p < 0.1$, * $p < 0.05$

once we control for $\mathbf{x}_{sd}$ and c_i. But the estimate might not be robust to some mechanism in which students adjust their unobserved behavior in d due to some peer effects in d'. This could explain the larger magnitude in the coefficients.

3.8 Conclusion

Using data of students in higher education we showed that negative peer effects from high ability peers on low ability students do not reflect an important heterogeneity in the potential interaction between these two groups. Although the literature have already raised this issue, we could present empirical evidence that increased opportunity for peers to meet might be a way to strengthen peer interaction that could generate positive spillovers.

In a context of high returns for peer interaction on performance – the active learning setting – we find that when low and high ability students can meet more frequently it becomes more likely the low ability student recognize the high ability peer as someone relevant for group work. Then, according to our estimates, maximizing the interaction between low student and this type of high ability peer could increase average performance for low ability students by 5.7% of a standard deviation.

A back of the envelope calculation in our data, assumming an homoegeneous effect for students in this subgroup, indicates a 1.6% increase in average performance of low ability students. This is a modest increase, but it would be important to understand if this is a persistent effect, since we analyze students in their first exams at university. Finally, a word of cautious is needed since it is still a reduced-form estimate and as evidence shows it may fail to uncover endogenous responses.

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Appendix

APPENDIX A - Appendix from First Chapter

A.1 Spillover on Executive Elections

Since legislative and executive elections happen simultaneously, there is strong relationship between performance of mayoral candidates and legislatives coalitions supporting them. Also, mayoral candidates' parties usually take part also in legislative elections. Table A.1 shows this strong correlation between votes in a party both in legislative and executive elections, controlling for supporting coalitions and municipality fixed-effects.

Table A.1 – Correlation bewteen executive and legislative votes

	Mayoral candidate's votes
Mayoral candidate's party votes (legislative)	2.055*
	(0.059)
Mayoral candidate party's coalition votes (legislative)	1.195*
	(0.125)
Election year=2012	-29.038
	(750.643)
Election year=2016	471.924
	(678.671)
Constant	915.213
	(585.953)
Observations	1057

Robust standard errors in parentheses. Municipality fixed-effects included.
$^{+}$ $p < 0.1$, * $p < 0.05$

This highlights that competition in legislative elections can be relevant for mayoral candidates' strategies. We explore this point in table A.2.

Table A.2 – Effects on mayoral elections

	Candidates	Eff. Candidates	Incumbency	Reelection
Treatment	0.374	0.167	-0.054	0.014
	(0.432)	(0.217)	(0.163)	(0.178)
Observations	258	258	172	172
Municipalities	86	86		

+ p < 0.1, * p < 0.05. Notes: Robust standard errors in parentheses

There is no evidence of more candidates entering the mayoral race. Point estimates for actual and effective number of candidates are positive but imprecise. Also, there is no effect on the probability that incumbent mayors in first term run for reelection or even get reelect (reduced-form).

A.2 Robustness results and DID plots

Table A.3 – Effects on political competition

Panel A: Fake treatment in 2016

	Candidates		Coalitions		Parties	
	Total	Coalition average	Coalitions	Eff. Coalitions	Parties	Eff. Parties
Fake treatment	12.026	0.630	0.349	0.191	0.258	0.267
	(8.019)	(0.641)	(0.379)	(0.290)	(0.466)	(0.393)
Observations	348	348	348	348	348	348
Municipalities	116	116	116	116	116	116

Panel B: Treatment in 2012

	Candidates		Coalitions		Parties	
	Total	Coalition average	Coalitions	Eff. Coalitions	Parties	Eff. Parties
Lead treatment	-6.741	0.339	-0.428	-0.188	-0.663	-0.473
	(9.526)	(0.863)	(0.449)	(0.274)	(0.470)	(0.414)
Observations	172	172	172	172	172	172
Municipalities	86	86	86	86	86	86

Table A.4 – Effects on candidates' characteristics

Panel A: Fake treatment in 2016

	High School	College	Age	Woman	Asset (log)
Fake treatment	0.001	-0.006	-0.095	0.000	-0.162+
	(0.005)	(0.009)	(0.244)	(0.005)	(0.096)
Observations	348	348	348	348	348
Municipalities	116	116	116	116	116

Panel B: Treatment in 2012

	High School	College	Age	Woman	Asset (log)
Lead treatment	0.003	0.003	-0.642	0.007	-0.015
	(0.008)	(0.011)	(0.446)	(0.010)	(0.102)
Observations	172	172	172	172	172
Municipalities	86	86	86	86	86

Table A.5 – Effects on elected candidates

Panel A: Fake treatment in 2016

	High School	College	Age	Woman	Asset (log)
Fake treatment	0.017	-0.024	-0.905+	-0.013	-0.195*
	(0.014)	(0.024)	(0.505)	(0.016)	(0.097)
Observations	348	348	348	348	348
Municipalities	116	116	116	116	116

Panel B: Treatment in 2012

	High School	College	Age	Woman	Asset (log)
Lead treatment	0.009	-0.017	0.967	-0.039+	0.141
	(0.020)	(0.040)	(0.715)	(0.020)	(0.152)
Observations	172	172	172	172	172
Municipalities	86	86	86	86	86

Table A.6 – Effects on on revenue and spending

Panel A: Fake treatment in 2016

		Discretionary Spending		Budget Share		Revenue	
	Elected Parties	Education	Health	Education	Health	Total	Transfers
Fake treatment	0.262	-0.184	0.120	-0.004	-0.005	-0.029	-0.038
	(0.348)	(0.438)	(0.545)	(0.006)	(0.006)	(0.020)	(0.023)
Observations	348	345	348	344	343	344	345
Municipalities	116	116	116	115	116	115	115

Panel B: Treatment in 2012

		Discretionary Spending		Budget Share		Revenue	
	Elected Parties	Education	Health	Education	Health	Total	Transfers
Lead treatment	0.294	0.602	-1.337	0.004	-0.002	0.040	0.032
	(0.326)	(0.482)	(0.852)	(0.007)	(0.010)	(0.025)	(0.027)
Observations	172	172	172	170	167	172	172
Municipalities	86	86	86	85	86	86	86

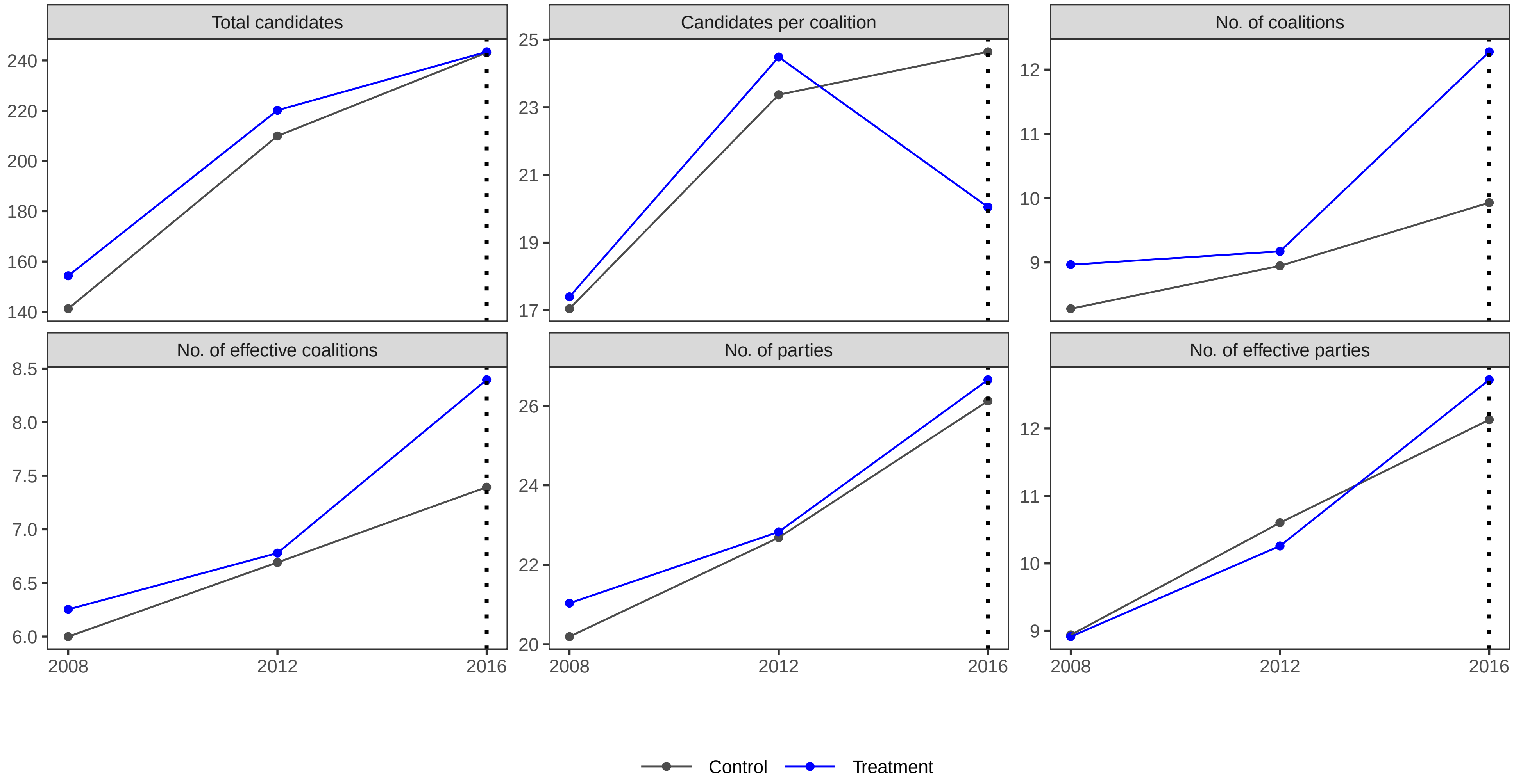

Figure A.1 – Municipality averages for total candidates, candidates per coalition, number of (effective) coalitions and number of (effective) parties.

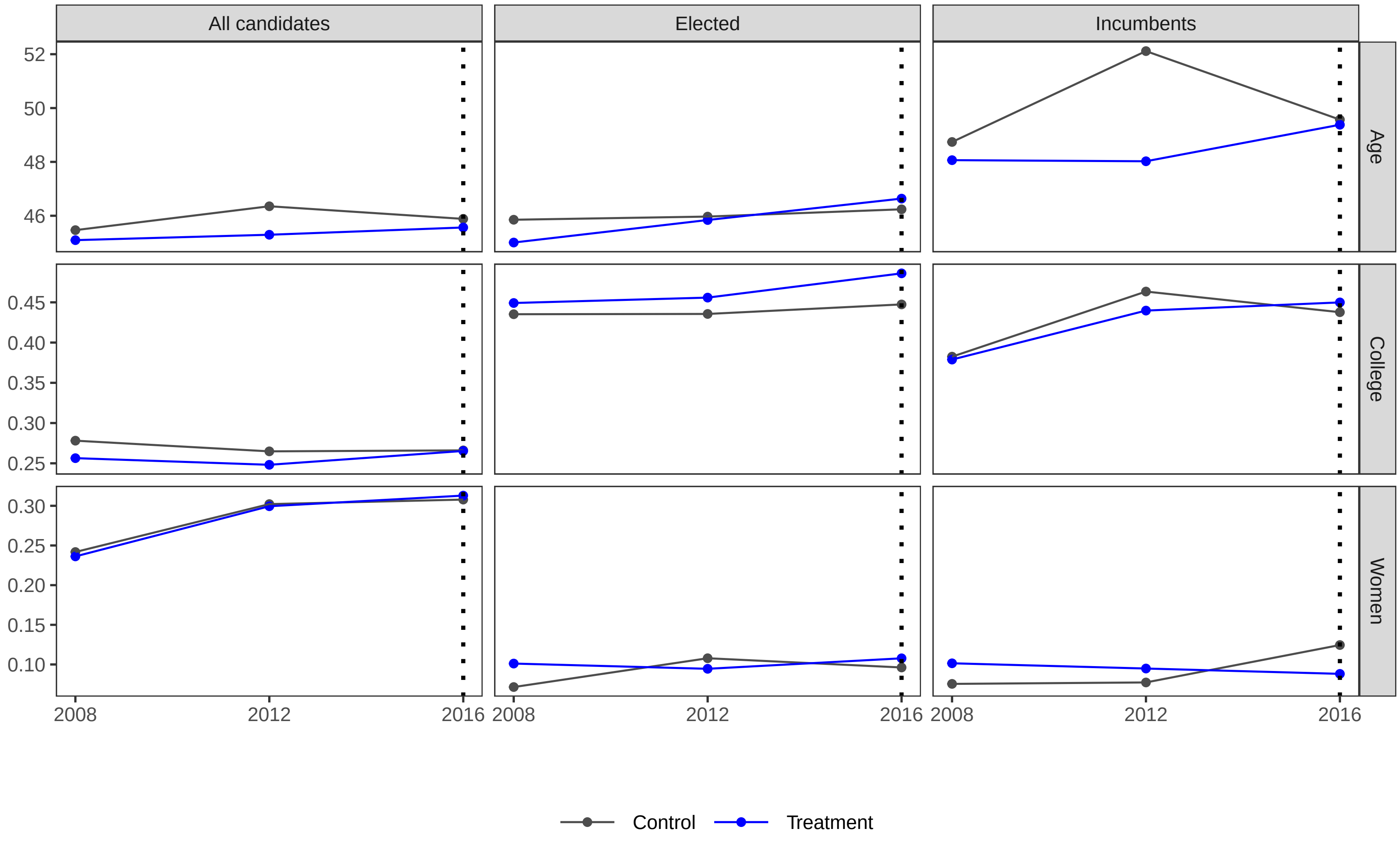

Figure A.2 – Municipality averages for candidates' mean characteristics (age, gender and education) by status (all candidates, incumbents and elected).

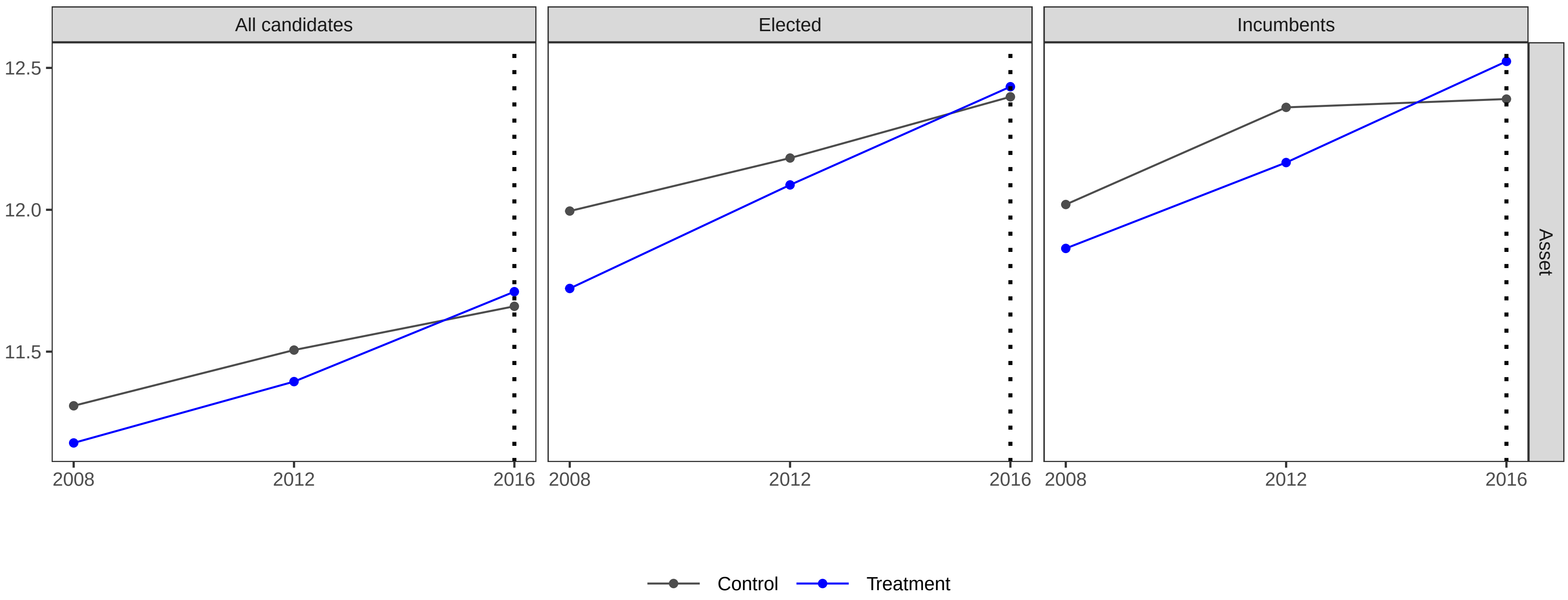

Figure A.3 – Municipality averages for (log) self-reported assets.

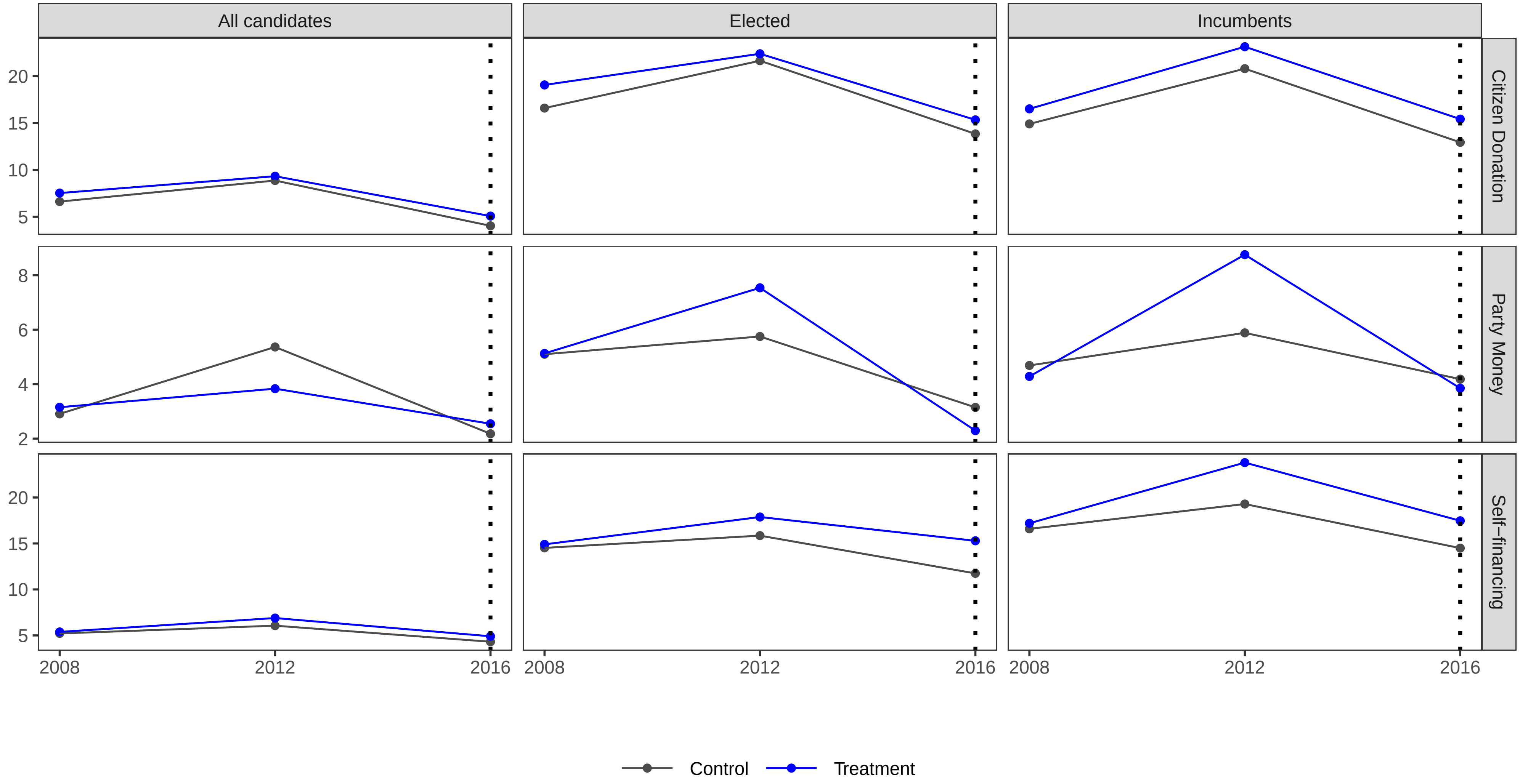

Figure A.4 – Municipality averages for the average revenue per candidate (in R$1000). Plots by status (all candidates, incumbents and elected) and funding source (citizen donation, party resources and self-funding).

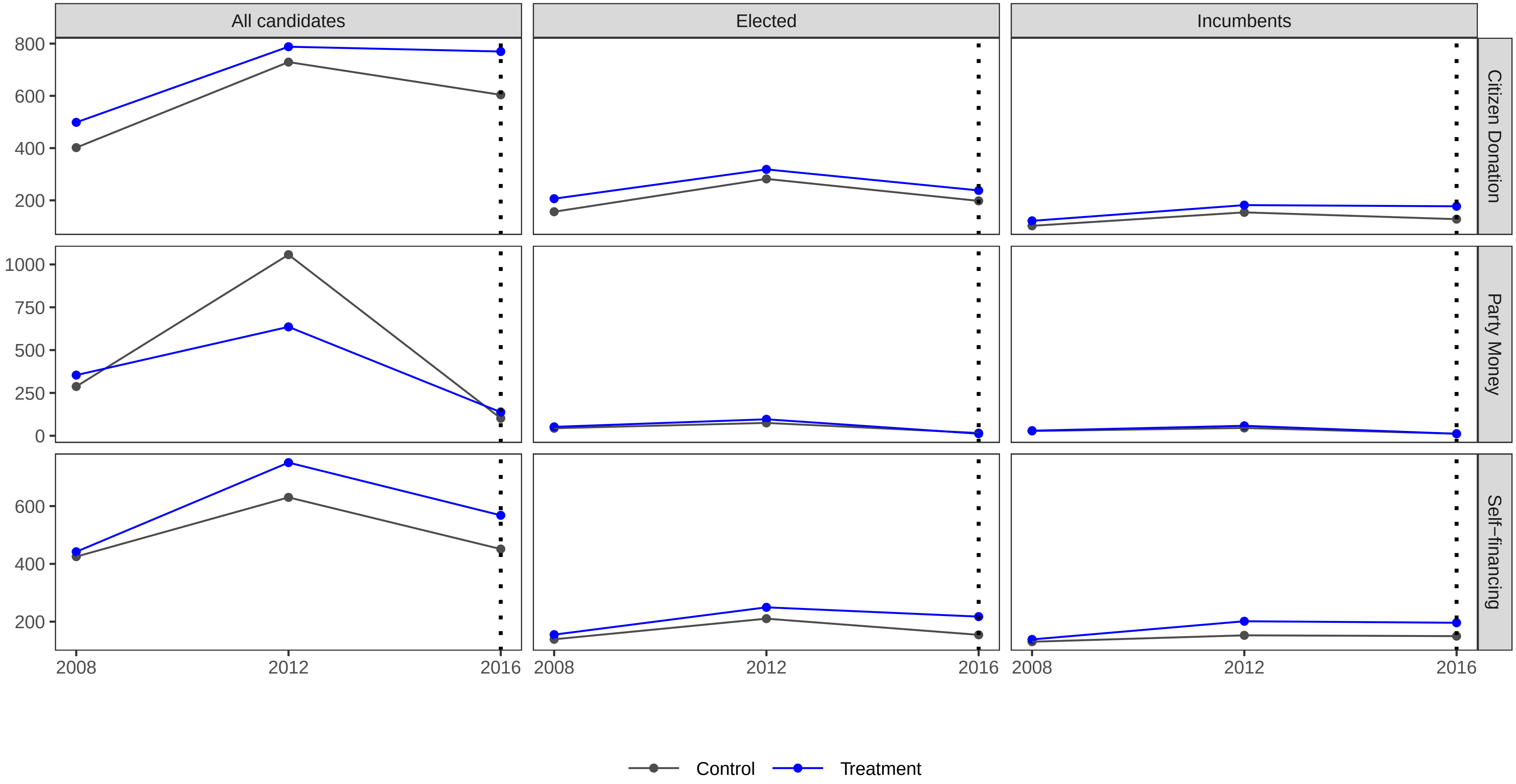

Figure A.5 – Municipality averages for the total revenue (sum for all candidates in 1000 R$). Plots by status (all candidates, incumbents and elected) and funding source (citizen donation, party resources and self-funding).

APPENDIX B - Appendix from Second Chapter

B.1 Generating false D'Hondt rules

The false "false D'Hondt" rule operates in the following way. As in the original method, seats are sequentially assigned. In each round, we calculate a coefficient for coalition i given by the number of votes for i over some $\gamma_i(n_i)$, where n_i is the current (round-wise) number of seats held by i. The coalition with the largest quotient wins the seat of the round. We set $\gamma_i(n_i) = n_i + 1$ for each coalition i that is not the mayor's coalition j, exactly as in the D'Hondt rule. As for the mayor's coalition, we set $\gamma_j(n_j) = \sqrt{n_j(n_j+1)}$, which overestimates the actual number of seats j obtains. As an alternative "false D'Hondt" rule, we set $\gamma_j(n_j) = \sqrt{(n_j+1)(n_j+2)}$, underestimating the actual number of seats. Figure B.1 depicts the real and two false seats assignments in elections where three coalitions compete for a total of three seats.

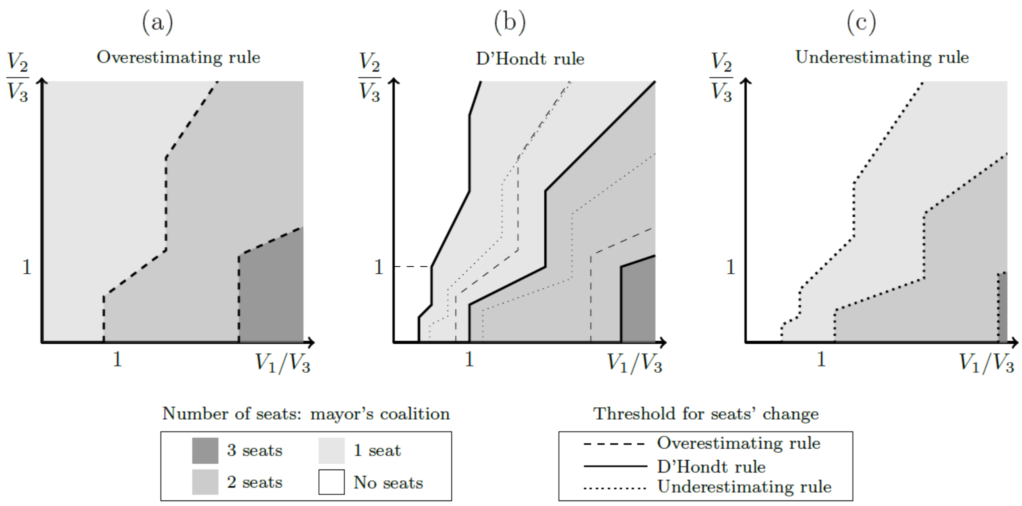

Figure B.1 – Coalitions 1, 2, and 3 compete for a total of three seats. The value V_i is the number of votes cast for coalition i. Each panel indicates the number of seats obtained by the mayor's coalition as a function of V_1, V_2, and V_3 under a specific assignment rule: the "false D'Hondt" rule with $\gamma_j(n_j) = \sqrt{(n_j+1)(n_j+2)}$ in panel (a); the D'Hondt rule in panel (b); and the "false D'Hondt" rule with $\gamma_j(n_j) = \sqrt{n_j(n_j+1)}$ in panel (c). Panel (b) displays the thresholds for seats' change in the mayor's coalition under all the rules.

B.2 Results on corruption

Table B.1 – log number of faults (corrupt and total)

	log-Corrupt faults			log-Total faults		
	(1)	(2)	(3)	(4)	(5)	(6)
T × First Term	-0.220	-0.122	-0.120	-0.171	-0.120	-0.110
	(0.172)	(0.164)	(0.153)	(0.164)	(0.151)	(0.135)
T × Second Term	0.021	-0.061	-0.103	-0.037	-0.097	-0.104
	(0.376)	(0.283)	(0.238)	(0.359)	(0.272)	(0.226)
Window	[0.40-0.60]	[0.35-0.65]	[0.30-0.70]	[0.40-0.60]	[0.35-0.65]	[0.30-0.70]
N Obs	51	67	102	51	67	102
Control Mean	3.850	3.749	3.680	3.864	3.786	3.723

+ $p < 0.1$, * $p < 0.05$. Notes: We present the estimates of specification 2.2, showing treatment effect for first term mayors (β_1) and for second-term mayors (β_2). Each column restricts the sample for a given treatment probability window. The first three columns present the results for the outcome log-Corrupt faults and the last three columns for log-Total faults. Clustered standard errors at the election level are in parenthesis.

Table B.1 shows the estimation of equation 2.2 having as the outcome the log of the number of faults reported in the audits of Brazil's anticorruption program. Columns 1, 2, and 3 present, for distinct TPW, the estimate for serious or moderate faults, described by Avis, Ferraz e Finan (2018) as corrupt faults. Columns 4, 5, and 6 also include mismanagement faults. Although we fail rejecting the coefficients differ from zero, we find that an extra seat decreases the number of corrupt faults by 12.2% for first term mayors, while reducing by 6.1% for second-term mayors in the $[0.35, 0.65]$ TPW. We have a similar result looking at corrupt plus mismanagement faults, with a reduction of 12.0% of these faults for first-term mayors and 9.7% for second-term mayors. In addition, sign of the estimates are negative in all TPW for first-term mayors. As for second-term mayors, the sign changes in the narrowest TPW for corrupt faults (which has a total of 51 observations) and the magnitude of the effect is always lower then the one for first-term mayors. The double sample restriction, on municipalities that were audited with close legislative elections, imposes a low power for our estimates.

APPENDIX C - Appendix from Third Chapter

C.1 Survey responses

The regression below show that survey responses were not related to student's ability.

Table C.1 – Probability of responding to the survey

	Answered
Math Ability Score	0.01
	(0.06)
Writing Ability Score	-0.04
	(0.05)
Low Student	-0.12
	(0.12)
Mid Student	-0.16
	(0.12)
High Student (Baseline)	0.79***
	(0.10)
Observations	131

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

C.2 Effects of repeated interaction on group functioning

Table C.2 show results for equation 3.6 using as outcome students answers about their perception about group interaction. The positive effect from high ability peers on low ability students does not seem to be explained by these attributes. What results show is that when low students have a higher fraction of low peers with stronger interaction, then they report that more peers of *any* ability level contribute to the group learning environment and their self-motivation. As students did not name individual peers in this answer we can not look to specific groups of peers. Below we present the exact question made to students. In the answers, they indicated and ordinal variable ranging from 0 to 5 indicating how many peers contributed to that group attribute. The value 0 means none of the peers and 5 means all peers.

Table C.2 – Peer Effects on Group Perception

	(1) Environment	(2) Motivation	(3) In Help	(4) Out Help	(5) Discussion
Low $\widehat{GPA}$ × Low Peers × % of Repeated Low	0.246^{*}	0.318^{*}	0.024	0.078	-0.024
	(0.112)	(0.136)	(0.121)	(0.127)	(0.112)
Mid $\widehat{GPA}$ × Low Peers × % of Repeated Low	-0.015	0.163^{+}	0.069	0.097	0.133
	(0.093)	(0.097)	(0.093)	(0.092)	(0.093)
High $\widehat{GPA}$ × Low Peers × % of Repeated Low	0.153	0.093	-0.027	0.021	0.048
	(0.100)	(0.115)	(0.103)	(0.118)	(0.095)
Low $\widehat{GPA}$ × Mid Peers × % of Repeated Mid	-0.151^{*}	-0.205^{*}	-0.107^{+}	-0.143^{*}	-0.008
	(0.058)	(0.060)	(0.059)	(0.059)	(0.057)
Mid $\widehat{GPA}$ × Mid Peers × % of Repeated Mid	0.022	-0.034	0.004	-0.018	0.018
	(0.051)	(0.055)	(0.055)	(0.050)	(0.047)
High $\widehat{GPA}$ × Mid Peers × % of Repeated Mid	-0.005	-0.059	-0.030	-0.004	0.002
	(0.065)	(0.074)	(0.072)	(0.064)	(0.053)
Low $\widehat{GPA}$ × High Peers × % of Repeated High	0.095	-0.042	0.056	0.092	0.070
	(0.104)	(0.130)	(0.112)	(0.109)	(0.082)
Mid $\widehat{GPA}$ × High Peers × % of Repeated High	-0.026	-0.035	-0.066	-0.038	-0.037
	(0.100)	(0.111)	(0.088)	(0.101)	(0.090)
High $\widehat{GPA}$ × High Peers × % of Repeated High	-0.002	0.009	-0.034	-0.026	0.045
	(0.088)	(0.106)	(0.086)	(0.102)	(0.060)
Ability Controls	Yes	Yes	Yes	Yes	Yes
Students	88	88	88	88	88
Groups	56	56	56	56	56
Observations	439	439	439	439	439

Standard errors clustered by students in parentheses.
$^{+}$ $p < 0.1$, * $p < 0.05$

Dos colegas indicados acima, quantos ...

- *contribuem para um ambiente de aprendizado mais favorável ao seu desempenho na disciplina?* **Environment**
- *contribuem para melhorar sua motivação/confiança em relação a essa disciplina?* **Motivation**
- *contribuem para o seu desempenho pela ajuda que oferecem na compreensão dos tópicos da disciplina?* **In Help**
- *recebem frequentemente a sua ajuda para entender tópicos da disciplina?* **Out Help**
- *são pessoas com quem você compartilha tempo de estudo/discussão também no contexto de outras disciplinas?* **Discussion**

C.3 Robustness exercises

Since allocation across disciplines are independent, here we identify peer effects using the variation of peer-related variables for the same student across disciplines by using equation 3.7.

Table C.3 – Peer Effects on Standardized Exam Grade

	(1)	(2)	(3)	(4)
Peers by Writing Ability				
Low Peers	-0.044	-0.044		
	(0.044)	(0.044)		
High Peers	-0.051	-0.051		
	(0.036)	(0.036)		
Low $\widehat{GPA}$ × Low Peers			0.093	0.093
			(0.075)	(0.075)
Low $\widehat{GPA}$ × High Peers			-0.038	-0.038
			(0.068)	(0.068)
Mid $\widehat{GPA}$ × Low Peers			-0.108^{+}	-0.108^{+}
			(0.054)	(0.054)
Mid $\widehat{GPA}$ × High Peers			-0.017	-0.017
			(0.042)	(0.042)
High $\widehat{GPA}$ × Low Peers			-0.071	-0.071
			(0.097)	(0.097)
High $\widehat{GPA}$ × High Peers			-0.165^{*}	-0.165^{*}
			(0.076)	(0.076)
Student Ability Controls	No	Yes	No	Yes
Alunos	133	133	133	133
Grupos	56	56	56	56
Obs.	658	658	658	658

Standard errors clustered by groups in parentheses.

$^{+}$ $p < 0.1$, * $p < 0.05$

Table C.4 – Peer Effects on Standardized Exam Grade

	(1)	(2)	(3)	(4)
Low Peers	-0.032	-0.032	-0.037	-0.037
	(0.052)	(0.052)	(0.053)	(0.053)
High Peers	-0.087*	-0.087*	-0.096*	-0.096*
	(0.043)	(0.043)	(0.044)	(0.044)
Low Peers × % of Repeated Low	0.003	0.003		
	(0.057)	(0.057)		
Mid Peers × % of Repeated Mid	0.023	0.023		
	(0.031)	(0.031)		
High Peers × % of Repeated High	0.098+	0.098+		
	(0.050)	(0.050)		
Low $\widehat{GPA}$ × Low Peers × % of Repeated Low			0.119	0.119
			(0.086)	(0.086)
Mid $\widehat{GPA}$ × Low Peers × % of Repeated Low			-0.052	-0.052
			(0.069)	(0.069)
High $\widehat{GPA}$ × Low Peers × % of Repeated Low			-0.066	-0.066
			(0.106)	(0.106)
Low $\widehat{GPA}$ × Mid Peers × % of Repeated Mid			0.032	0.032
			(0.054)	(0.054)
Mid $\widehat{GPA}$ × Mid Peers × % of Repeated Mid			0.028	0.028
			(0.037)	(0.037)
High $\widehat{GPA}$ × Mid Peers × % of Repeated Mid			0.004	0.004
			(0.062)	(0.062)
Low $\widehat{GPA}$ × High Peers × % of Repeated High			0.180*	0.180*
			(0.083)	(0.083)
Mid $\widehat{GPA}$ × High Peers × % of Repeated High			0.154*	0.154*
			(0.063)	(0.063)
High $\widehat{GPA}$ × High Peers × % of Repeated High			-0.060	-0.060
			(0.083)	(0.083)
Ability Controls	No	Yes	No	Yes
Students	133	133	133	133
Groups	56	56	56	56
Observations	658	658	658	658

Standard errors clustered by groups in parentheses.

$^{+}$ $p < 0.1$, * $p < 0.05$

Table C.5 – Peer Effects on Time Allocation (Share by discipline)

	(1)	(2)	(3)	(4)
Low Peers	-0.009	-0.009	-0.006	-0.006
	(0.007)	(0.007)	(0.007)	(0.007)
High Peers	-0.014*	-0.014*	-0.012*	-0.012*
	(0.005)	(0.005)	(0.005)	(0.005)
Low Peers × % of Repeated Low	0.009	0.009		
	(0.008)	(0.008)		
Mid Peers × % of Repeated Mid	-0.011*	-0.011*		
	(0.005)	(0.005)		
High Peers × % of Repeated High	0.015*	0.015*		
	(0.007)	(0.007)		
Low $\widehat{GPA}$ × Low Peers × % of Repeated Low			-0.027*	-0.027*
			(0.009)	(0.009)
Mid $\widehat{GPA}$ × Low Peers × % of Repeated Low			0.022*	0.022*
			(0.010)	(0.010)
High $\widehat{GPA}$ × Low Peers × % of Repeated Low			0.015	0.015
			(0.010)	(0.010)
Low $\widehat{GPA}$ × Mid Peers × % of Repeated Mid			-0.020*	-0.020*
			(0.007)	(0.007)
Mid $\widehat{GPA}$ × Mid Peers × % of Repeated Mid			-0.009+	-0.009+
			(0.005)	(0.005)
High $\widehat{GPA}$ × Mid Peers × % of Repeated Mid			-0.007	-0.007
			(0.006)	(0.006)
Low $\widehat{GPA}$ × High Peers × % of Repeated High			0.003	0.003
			(0.007)	(0.007)
Mid $\widehat{GPA}$ × High Peers × % of Repeated High			0.016+	0.016+
			(0.009)	(0.009)
High $\widehat{GPA}$ × High Peers × % of Repeated High			0.020*	0.020*
			(0.010)	(0.010)
Ability Controls	No	Yes	No	Yes
Students	86	86	86	86
Groups	56	56	56	56
Observations	429	429	429	429

Standard errors clustered by groups in parentheses.

$^{+}$ $p < 0.1$, * $p < 0.05$