




# Group size, member selection, and performance: Evidence from legislative elections in Brazil

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## ABSTRACT

This paper provides empirical evidence on the causal effects of group size on member selection and performance. Using data from local legislative elections in Brazil in a difference-in-differences framework, I explore an electoral reform that reduced the maximum number of candidates allowed in coalitions disputing legislative seats. The reform did not change the number of candidates and parties in the election, which was possible because in municipalities affected by the new cap parties reorganized into 21% more coalitions than would be expected without the reform. I discuss how the heterogeneity of the candidates combined with the coalitions' adjustment to the reform led to the election of city councilors who were 17% wealthier, on average. Although the reform achieved the goal of reducing campaign costs, which fell by 9%, the election of wealthier politicians was an unintended consequence that weakened the representation of economically disadvantaged groups in the legislative body.

## 1. Introduction

When teams compete with one another for a prize to be split among the winning group members, team size is a relevant choice variable. Adding a member to the team creates a disincentive for other team members because it decreases each member's share of the potential prize. If the additional member's contribution to team production does not outweigh the reduced effort caused by this disincentive, increasing the team size will not increase its winning probability. However, two characteristics of teams might work to reverse this result.

Esteban and Ray (2001) show that if team members' effort costs are sufficiently convex, the reduction in effort in response to one additional member is compensated by the effort exerted by this new member, making larger groups more productive. Besides, Cheikbossian and Fayat (2018) show that larger groups might have higher winning chances if the degree of complementarity between group members' efforts is sufficiently large relative to the degree of rivalry over the prize. Finally, Crutzen et al. (2020) link these two dimensions by showing that when parties compete for legislative seats, and all candidates have the same cost function, increasing the degree of complementarity between members' efforts in getting votes reinforces the role played by the convexity in making larger groups more effective.

In this paper, I consider the hypothesis that potential team members might be heterogeneous in their productivity instead of having the

same cost function. Then, complementarity could establish a causal link between team size and group composition: if a team needs to adjust size, complementarity favors recruiting relatively more productive individuals. Specifically, I contribute to this literature by providing empirical evidence on the causal effects of team size on member selection and performance. I explore an electoral reform adopted in Brazil that reduced the maximum number of candidates allowed in coalitions disputing legislative seats. Using data from three local elections held between 2008 and 2016, I employ a difference-in-differences framework to show that by constraining the size of coalitions, the reform implied a reorganization of parties into more coalitions. Under this rearrangement, coalitions constrained by the cap on coalition size selected wealthier candidates and elected more councilors.

## 2. Legislative elections and the electoral reform

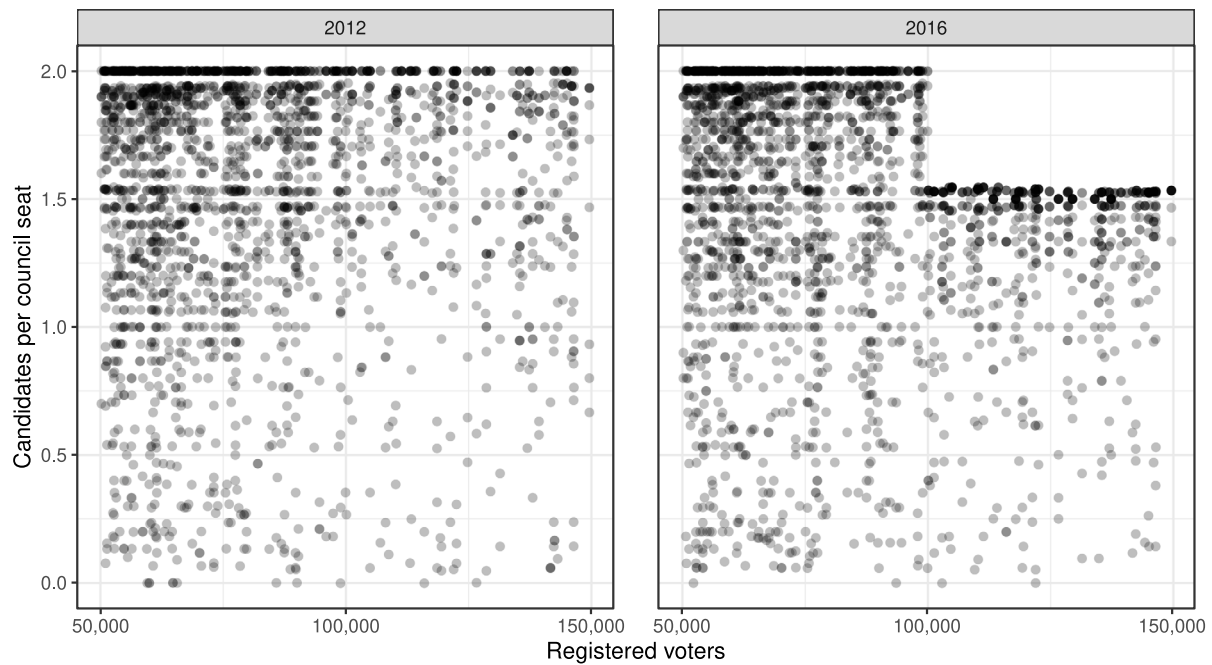
Brazilian municipalities elect city councilors every four years. Instead of pure competition among political parties, they often form electoral coalitions. These coalitions are announced before the election but do not demand any post-election agreements.<sup>1</sup> Candidates are chosen via an open-list proportional representation system. The coalition's vote share determines the number of seats it gets, while individual votes affect a candidate's ranking within the coalition and, thus, who gets

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<sup>1</sup> In the 2008 election, considering all parties in all Brazilian municipalities, only 10% chose not to join a coalition.

<sup>2</sup> If a party runs alone, the same mechanism applies, and the party plays the role of the coalition in the argument.

<sup>3</sup> Law 13,165, available [here](http://www.planalto.gov.br/ccivil_03/leis/2006/l13165.htm)



**Fig. 1.** The Electoral Reform – Each dot is an electoral coalition or single party. In the 2016 election, coalitions in municipalities with more than 100,000 registered were limited to registering candidates up to 150% of the number of seats in the council.

**Table 1**  
Political competition.

	Treatment	$\hat{Y}(0) \mid \text{Treatment} = 1$
<i>Panel A. Political Competition</i>		
No. of parties	0.36 (0.41)	26.35* (0.95)
Total candidates	-1.39 (6.81)	225.59* (13.18)
Candidates per coalition	-4.82* (0.43)	23.09* (0.77)
Total coalitions	2.26* (0.34)	10.72* (0.59)
Share of large coalitions	0.17* (0.03)	0.31* (0.06)
Large coalition's seat share	0.22* (0.04)	0.46* (0.07)

Notes: Estimates produced with 927 observations from 309 municipalities. Standard errors in parentheses are clustered at the municipality level. Significance levels: + 0.1 \* 0.05. Candidates' wealth expressed in thousands of Brazilian reais of December/2020. Large coalitions are coalitions that used the maximum number of candidates.

elected.<sup>2</sup> An electoral reform in 2015 reduced the limit on the number of candidates that coalitions could register.<sup>3</sup> In municipalities with less than 100,000 voters, parties could register candidates up to the limit of 150% of the council size and coalitions up to 200%. In places above the 100,000 threshold, parties and coalitions could register candidates for up to 150% (see Fig. 1).

### 3. Data and empirical strategy

**Electoral data.** I use data from municipal elections held between 2008 and 2016 provided by the Brazilian Electoral Court. This data set contains the total registered voters in each municipality, which defines its position relative to the 100,000-voter threshold. The data contains information regarding parties' organization into electoral coalitions and

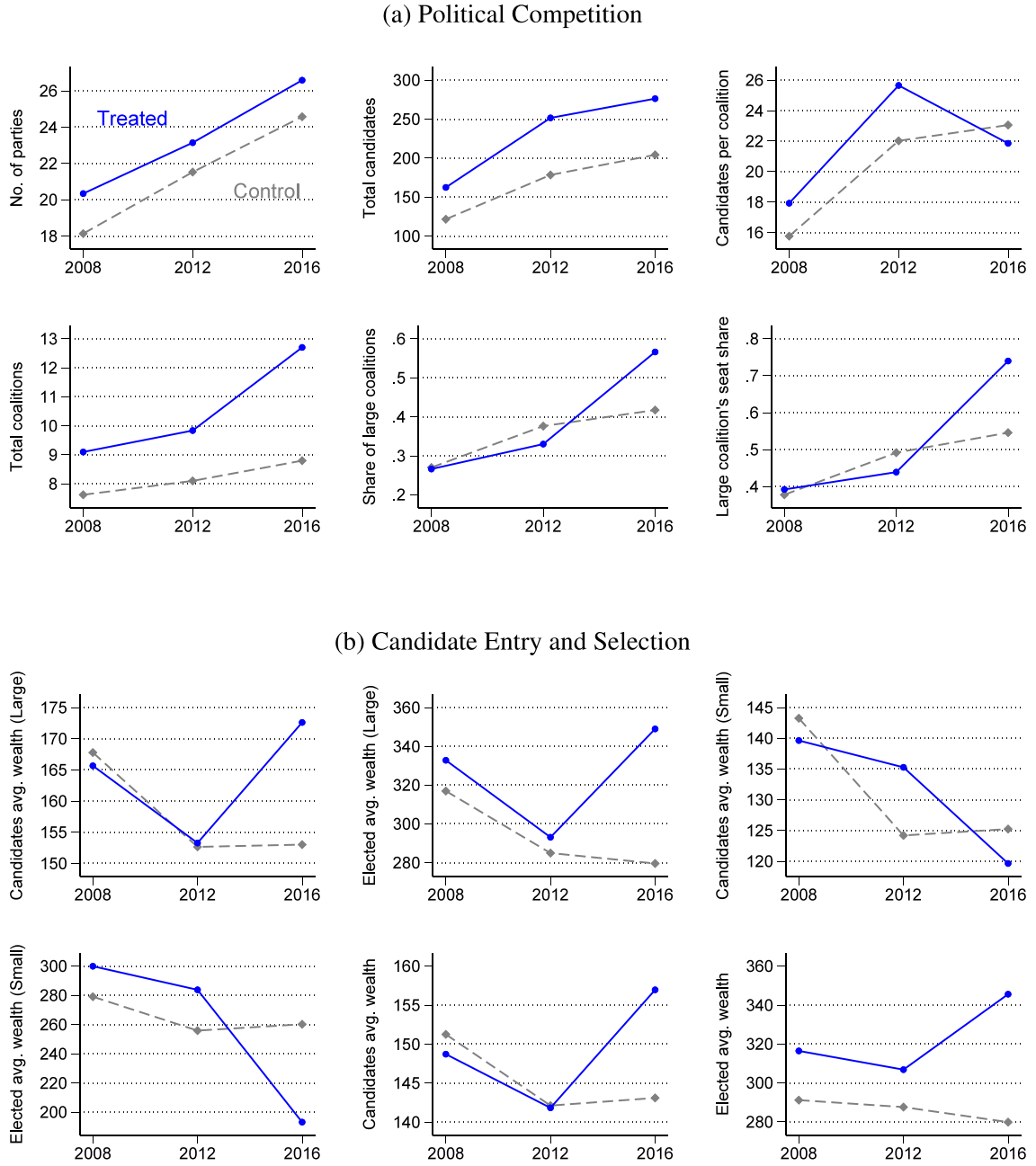
**Table 2**  
Candidate entry and selection.

	Treatment	$\hat{Y}(0) \mid \text{Treatment} = 1$
<i>Panel A. Large Coalitions</i>		
Candidates' average wealth	38.75* (15.28)	185.84* (29.87)
Elected candidates' average wealth	122.96* (49.92)	368.63* (86.78)
<i>Panel B. Small Coalitions</i>		
Candidates' average wealth	-21.11* (9.29)	105.29* (20.08)
Elected candidates' average wealth	-112.23* (45.25)	210.01* (75.00)
<i>Panel C. All coalitions</i>		
Candidates' average wealth	20.49* (9.89)	162.47* (20.09)
Elected candidates' average wealth	55.99* (30.08)	318.80* (51.49)

Notes: In Panels A and B, the sample contains municipalities that had each type of coalition in all elections. Large coalitions were always present in 221 municipalities and small coalitions in 293. Estimates in Panel C were obtained using a sample of 927 observations from 309 municipalities. Standard errors in parentheses are clustered at the municipality level. Significance levels: + 0.1 \* 0.05. Candidates' wealth expressed in thousands of Brazilian reais of December/2020.

candidates' characteristics such as self-reported wealth and educational level. Variables are defined as totals or averages at the municipal level.<sup>4</sup>

<sup>4</sup> Before computing the averages for self-reported wealth, I removed the richest candidate from control and treatment groups in each election (6 candidates) plus two other candidates out of more than 165,000 candidates. The average self-reported wealth of these outliers exceeded 900 million reais, while the mean self-reported wealth of the remaining candidates is approximately 154 thousand reais. Besides, to run regressions of wealth-related variables, I further removed some municipalities responsible for the highest 3 average wealth in each election for different wealth measures. In the online appendix, I explain the procedure to remove them.



**Fig. 2.** Raw means of outcomes indicated in the vertical axis of each plot. The solid lines display the mean outcomes of treated municipalities, and the dashed lines the control municipalities' mean outcomes. Sample of 309 municipalities in each election within the 50,000-voter window around the 100,000-voter threshold. Plots of average wealth in 2(b) for small and large coalitions use samples restricted to municipalities that had each type of coalition in all elections: 221 municipalities with large coalitions and 293 with small coalitions.

**Empirical strategy.** Since the 100,000-voter threshold does not determine other policies, an RD design would be valid to estimate the effects of interest. However, in the sample of 309 municipalities within a 50,000-voter window around this threshold, municipalities above the threshold (treated) are, on average, larger and richer. The sample size decreases rapidly as this window narrows. Thus, to deal with unobservables properly and ensure the statistical power of the analysis, I employ a difference-in-differences strategy, which I implement using the equation

$$y_{i,t} = \alpha_i + \lambda_t + \beta \text{Treatment}_{i,t} + \mathbf{x}'_{i,t} \gamma + \varepsilon_{i,t} \quad (1)$$

where  $\text{Treatment}_{i,t}$  equals 1 for municipalities above the 100,000-voter threshold in 2016 and is 0 otherwise. The parameters  $\alpha_i$  and  $\lambda_t$  are

municipality and year fixed-effects, respectively. The socioeconomic profile and the council size of municipalities likely influence some of the outcomes I analyze. For this reason, I include a vector of controls  $\mathbf{x}$  that contains council size, the number of registered voters, money transfers from the federal government, and voters' age and educational profiles.

Under the assumption that treated and control municipalities had similar evolution of unobservables, on average,  $\hat{\beta}$  estimates the average treatment effect on the treated. When presenting the results, I report plots for each outcome to assess the plausibility of the assumption of parallel trends. I use the sample of municipalities within a 50,000-voter window around the threshold and cluster standard errors at the municipal level. In the online appendix, I present the estimates for

several robustness exercises: results from Eq. (1) with no controls, regressions putting more weight on municipalities close to the threshold, and regressions using the sample within a 30,000-voter window. I also report an RD analysis, which produces qualitatively similar estimates or estimates with large standard errors.

#### 4. Results

**Political competition.** The first estimates in Table 1 shows that the reform did not affect the average number of parties and total candidates present in the elections but implied a 20% decrease in the number of candidates per coalition, in line with Fig. 1. This could happen because parties in treated municipalities were expected to form 11 coalitions under the previous cap on coalition size, but the reform caused them to form an average of 13 coalitions.<sup>5</sup> Large coalitions, those with the number of candidates at the limit allowed by the law, comprised 37% of the coalitions in the 2012 election.

Table 1 shows that in treated municipalities, 31% of the coalitions were expected to be large, and the reform caused a 17 percentage point increase. Besides, these coalitions were expected to elect less than half of city councilors, but they elected two-thirds of them.

**Candidate entry and selection.** Panel A of Table 2 indicates that the average wealth of candidates in large coalitions increased by 20%, whereas it declined by 20% for those in small coalitions, according to Panel B. Among elected councilors, this disparity became even larger: councilors from large coalitions were, on average, one-third richer, while those from small coalitions had half the wealth expected without the reform.<sup>6</sup> Finally, Panel C shows that after the reform, candidates and elected councilors in treated municipalities were, on average, 13% and 17% richer, respectively. In the appendix, I show there was no effect on the quality of politicians, measured by the share of candidates or elected councilors with at least a high school degree. Also, the share of women and the politicians' average age were unaffected.

#### 5. Discussion and concluding remarks

Candidates in a coalition maximize their (expected) utility net of the cost of using resources in the election. The candidates' utility depends on the coalition's expected seats and their individual probabilities of getting one seat. Coalitions want to maximize the probability of winning seats and only add one more candidate if it increases the vote share, which, in turn, depends on how other candidates in the coalition adjust their use of resources facing a new competitor.

If every candidate in a coalition makes the optimal choice, one could only increase utility by moving to a coalition where the election probability would be higher without using more resources. In equilibrium, coalitions and candidates should not be able to find such opportunities. However, the reform obliged parties in coalitions for which the old cap would be binding to adjust by either removing candidates or forming new coalitions that complied with the new cap.

There is no evidence that parties adjusted by simply removing candidates from their lists since there was no effect on the total number of candidates in the election. Instead, the elections became more competitive, with more coalitions, and the increase in the share of large

coalitions is consistent with larger parties reorganizing themselves and forcing smaller ones out of the large coalitions.

Large coalitions have always had candidates who were, on average, wealthier than those in smaller coalitions (see plot 2(b) of Fig. 2). This type of candidate is likely the most productive one, either because the cost of using resources increases more slowly or because they are more effective in getting votes.<sup>7</sup> Thus, the observed increase in the average wealth of candidates in large coalitions caused by the reform is consistent with the hypothesis that when they need to adjust their size, they choose to keep the more productive individuals since this is the vote-maximizing behavior. Therefore, an increase in the average productivity of large coalitions explains the increase in their average seat share and, ultimately, the election of richer councilors. To conclude, it is worth noting that electoral finance rules usually intend to promote a "more egalitarian playing field in party competition" (Norris and Van Es, 2016). The reform analyzed here intended to do so by reducing the costs of electoral campaigns, which it successfully reduced by 9% (see the appendix's section B.6). However, the endogenous response of political actors to the new rule resulted in an adjustment that unintentionally favored the selection of wealthier politicians, which ultimately heightened the barriers to the representation of economically disadvantaged groups. Therefore, local legislative bodies affected by the reform became perhaps less representative of the interests of lower-income constituents, demonstrating how challenging it is to design formal rules that ensure a more equitable political environment.

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#### Appendix A. Supplementary data

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.econlet.2024.112153>.

#### Data availability

Data will be made available on request.

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<sup>5</sup> Alternative measures of political competition are the number of effective coalitions in the elections and the average number of effective candidates per coalition. This type of measure is calculated as the inverse of the Herfindahl–Hirschman Index computed from the distribution of votes (Golosov, 2010). In the appendix's Table A6, I show that these measures yield similar results as those displayed in Table 1.

<sup>6</sup> To produce the results using wealth variables displayed in both Fig. 2(b) and Table 2, I restricted the sample to municipalities that always had large or small coalitions. In the appendix's Table A10, I show that the results in Panel A of Table 1 also hold in these restricted samples.

<sup>7</sup> For instance, elected councilors were more than twice as rich as non-elected candidates from the same party in the same election during the period I analyzed.